How Negativity Can Increase and Decrease Voter Turnout: The Effect of Timing

YANNA KRUPNIKOV

Negative ads dominate campaign communication, but scholars continue to disagree over the effects of negativity on voter turnout. While some studies show that negativity leads to a lower likelihood of turnout, others find precisely the opposite. In this article, I leverage the role of timing to unify findings that were heretofore perceived as largely conflicting. I use the same data to show that at a certain time exposure to negativity can be mobilizing, but at other points in time exposure can be demobilizing. A combination of observational data and experimental results highlight these crucial conditions.

Keywords advertising, voter turnout

Over the last decade, negative ads have not only grown to be one of the most frequently employed campaign techniques (Fowler & Ridout, 2013), but media coverage of elections has increasingly fixated on negative campaigning (Geer, 2012). One of the factors driving media coverage is the fear that negativity is harmful to American democracy. A particular worry is that negativity turns voters away from the polls.

Over time, research on negativity has produced conflicting results. While some scholars show that negativity leads to a lower likelihood of turnout (e.g., Ansolabehere & Iyengar, 1995; Ansolabehere, Iyengar, Simon, & Valentino, 1994), others find that negativity mobilizes voters (e.g., Djupe & Peterson, 2002; Goldstein & Freedman, 2002). Attempts at reconciling these findings suggest that negativity has no effect on turnout at all (e.g., Finkel & Geer, 1998; Krasno & Green, 2008). Adding to these mixed findings, recent work has focused on the context in which negativity is transmitted, showing that negative ads can demobilize, but only when individuals are exposed to them at a certain time (Krupnikov, 2011).

In this article, I focus on temporal context and consider how the timing of exposure to negativity can alter its effects on individual behavior. I show that under certain conditions exposure to negativity can mobilize individuals, but under other conditions it can demobilize. This article not only brings the importance of timing of exposure to the foreground, but also offers one of the first theoretical and empirical accounts to show evidence of both mobilization and demobilization—and does so using both experimental and observational data. This approach to analyzing the role of negativity sets the foundation for understanding the sometimes counterintuitive effects of political information.

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Negativity and Voter Turnout

The Differential Power of Negativity

While previous scholars have shown evidence for either mobilization or demobilization, here I will show evidence of both effects. When considering both mobilization and demobilization, I focus on research that has specifically addressed the relationship between negativity and turnout. This is not to ignore work on the broader effects of negativity, such as the way campaign negativity shapes American political discourse (Jamieson, 1992) or the strategic use of negativity in campaigns (Skaperdas & Grofman, 1995), but simply to guide the discussion toward the key empirical conflict directing this article. Moreover, it is important to note that this is a rich area of research, and thus the results I highlight are only a sample of the full set of scholarship on the topic (see Lau & Rovner, 2009).

My argument hinges on the individual’s decision process: An individual first considers which candidate he or she will vote for (Lau & Redlawsk, 2006), and subsequently, whether he or she will implement this choice with a vote (Krupnikov, 2011; Svenson, 1996). I argue that the effect of negativity will depend on the timing of exposure relative to this decision process: Negativity will have a different effect depending on whether it is encountered before or after an individual makes a candidate choice.

Negativity as Mobilizer

Although negative ads are designed to weaken support for the targeted candidate, some scholars have found that negativity increases voter turnout (Djupe & Peterson, 2002; Goldstein & Freedman, 2002; Kahn & Kenney, 2004; Wattenberg & Brians, 1999). Scholars offer different explanations for this finding, including the fact that individuals are more likely to remember negative ads (Wattenberg & Brians, 1999) and that negative ads are more likely than other types of ads to increase interest in politics (Freedman, Franz, & Goldstein, 2004). Heightened levels of interest could work to increase the likelihood of turnout.

Research on advertising demonstrates the superior memorability of negative ads (Brians & Wattenberg, 1996; Geer & Geer, 2003). Further, scholars have shown that information processing is characterized by a negativity bias, with more attention paid to negative than positive messages (Lau, 1982). However, enhanced attention or memorability is insufficient to connect exposure to negativity and mobilization. It is unclear why a heightened interest in negativity should necessarily translate to higher turnout. After all, individuals may pay more attention to negativity only to find it so repugnant that they no longer wish to vote. I suggest that the missing connection between heightened interest and mobilization is the idea that negativity helps individuals make choices (Krupnikov, 2012). Following Kahneman and Tverksy (1979), Skowronski and Carlston (1989) argue that the presence of negativity is more likely than the presence of positivity to signal to an individual that he or she has sufficient information to make a selection. Further, as Krupnikov (2012) demonstrates, higher exposure to campaign negativity increases the likelihood of choice.

Making a choice is an important component of the voting decision. Lau and Redlawsk (2006) provide extensive evidence that during campaigns, individuals shift from candidate evaluation to making a candidate choice. This choice, they argue, is more definitive than an expressed preference or positive evaluation, as it implies more commitment to a candidate. This choice need not be an emotional attachment reserved only for the ideologically strong (Kogen & Gottfried, 2011; Meffert et al., 2006; Lau & Redlawsk, 2006); it simply marks the selection of one candidate over another. This choice point is also the time at which an individual begins focusing on how he or she may act on his or her particular choice (Jewell,
As a result, an individual who has made a choice is more likely to act—in this case vote—than one who has not (Jewell, 2003).

It is the timing of candidate choice, then, that connects the memorability of negativity with its mobilizing potential. Not only are people naturally inclined to pay more attention to negativity, but negativity signals informational sufficiency and helps individuals move from evaluation to choice. In turn, those who make choices are more likely to turn out than those who have not. Thus, I expect that individuals who are exposed to negativity while evaluating the candidates—before a choice is made—will be more likely to vote than those who are exposed to little or no negativity during that same time in the choice process.

If exposure to negativity during evaluation is mobilizing, how and why would we expect negativity to ever have a demobilizing effect? To consider the potential of negativity to demobilize, I turn my focus to what happens after that choice is made.

**Negativity as Demobilizer**

Scholarly interest in the demobilizing potential of negativity began with research conducted by Ansolabehere and Iyengar (Ansolabehere & Iyengar, 1995; Ansolabehere et al., 1994) showing that exposure to negative ads decreased voter turnout. The process underlying this effect is that negativity makes individuals less efficacious, which in turn decreases turnout (Ansolabehere & Iyengar, 1995; Ansolabehere et al., 1994). More recently, Krupnikov (2011) has documented a similar demobilizing effect, by which exposure to negativity after a choice is made makes individuals less confident in their selected candidate, leading to a decline in voter turnout. Though specifying a different mediator, this reasoning is quite similar: Individuals are less likely to turn out because they, somehow, feel less certain about their role in the political process.

While the dampening effects of negativity on efficacy and confidence are intuitive, they are difficult to reconcile with the arguments supporting mobilization. Even if we accept the argument about demobilization being a post-choice phenomenon, it is still unclear why this type of campaigning would have such a profoundly different effect after a choice is made. If negativity is more memorable and signals informational sufficiency, why would its role in an individual’s decision process suddenly change?

To show that negativity can be both demobilizing and mobilizing, I return to the idea of choice. While it may be comforting to think that individuals continue to adapt their evaluations and update their candidate preferences as they gain more information during a campaign, there is little evidence to suggest that this is the case (Lau & Redlawsk, 2006). During the last month of an American campaign, for example, when most people have already made candidate choices (Holbrook, 1996), the propensity to change one’s mind is less than 10%; the propensity to change one’s mind in the last 10 days is 6% (Blais, 2004).

The low likelihood of preference reversal during the late months of the campaign is symptomatic of a focus on one candidate at the expense of another. Indeed, research suggests that in making a choice individuals come to view the unselected alternative as a poor option and a bad course of action (Holyoak & Simon, 1999; Simon, Pham, Le, & Holyoak, 2001). Tracing the process of decision making, Simon et al. (2001) find that as people make choices, one alternative becomes increasingly devalued until it is viewed as unusable and inferior. Given that we see a similar effect in experiments that trace the process of selecting a candidate (Lau & Redlawsk, 2006), we can expect that once voters commit to a candidate, they also reject the unchosen candidate.

The selection of one candidate—and rejection of the unselected candidate—is what gives negativity its demobilizing power. As decision patterns show, many potential voters
make choices well before they have a chance to act on these choices, even if we account for the possibility of early voting. Yet, in numerous states and media markets across the country, individuals continue to be bombarded with negative ads regardless of whether or not they have chosen a candidate. Even if they do not want to see these ads, incidental exposure in the heat of the campaign is quite likely (Franz, Freedman, Goldstein, & Ridout, 2007), particularly as more ads are embedded in Web sites.3

How does this post-choice but pre-action exposure affect individuals? After an individual selects one candidate over another, he or she is likely to pay more attention to new information about the selected candidate (Fischer et al., 2010; Meffert et al., 2006). As Meffert et al. (2006) assert, this attention pattern is not inconsistent with theories of motivated reasoning and other perceptual biases (Taber & Lodge, 2006) arguing that people are more drawn to any information about candidates they already prefer (see also Donsbach, 1991). At first, people may be more responsive to information about their candidate that suggests that they made the right choice (Fischer et al., 2010; Redlawsk, Civettini, & Emmerson, 2010). Yet as time passes, individuals become more responsive to information that is “incongruent”—or suggests something negative about their chosen candidate. Fischer et al. (2010), for example, show that over time people actually come to perceive the incongruent information to be of higher quality. Similarly, in an experiment where individuals were asked to select a preferred candidate, Redlawsk et al. (2010) found that over time systematic exposure to incongruent information leads to decreasing candidate evaluations. Redlawsk et al. (2010) are not the only scholars to see unexpected responses to incongruent information. Stevens, Sullivan, Allen, and Alger (2008), for example, show that partisans can indeed be responsive to negativity about their own candidate. Thus, if negativity is already more memorable, and individuals are more likely to focus on their selected candidate and over time they become more responsive to the effects of incongruent information, then we can predict that negativity will eventually lead to declining evaluations of the chosen candidate.

How will these declining evaluations affect individuals? One possibility is that declining evaluations lead a person to switch to the other candidate. This is a possible, though unlikely outcome, as part of the commitment of choice is the rejection of the unselected alternative, and moreover, the propensity for preference reversals is very low during the latter point of a campaign (Blais, 2004). A second possibility integrates the commitment to one candidate with the power of negativity: While negativity may not lead a person to pick a new candidate, it will decrease the likelihood of turnout (Krupnikov, 2011).

In short, research suggests that exposure to negativity post-choice is more likely to lead to inaction rather than a shift in candidate allegiance. Since the unselected candidate has already been rejected as a viable option, as evaluations of the selected candidate decline, individuals become less likely to act on their choices by turning out to vote. This effect occurs regardless of the type of information an individual relied on to make a choice, which means that an individual who made a choice following exposure to negativity is just as likely to suffer its demobilizing post-choice effects. Indeed, if a campaign is likely to have a consistent flow of negativity, exposure to negativity during evaluation can help individuals reach choices but, in this way, set the stage for post-choice demobilization.4

Negativity: A Differential Effect

To reconcile the differential effects of negativity, I have highlighted the theoretical conditions that could lead to differing effects. Specifically, mobilization will occur when an individual’s only exposure to negativity happens during evaluation, before a choice
Table 1
Conditional effect of negativity on individual behavior

<table>
<thead>
<tr>
<th>Individual-level condition</th>
<th>Aggregate Pattern</th>
<th>Campaign Example</th>
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<tbody>
<tr>
<td>Mobilization</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre-choice exposure, little or no post-choice exposure</td>
<td>Bulk of ads early, few ads late</td>
<td>Chattanooga media market: pre-Oct. 1, 2004, 96% of ads neg., post 15.4%</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Tucson media market: pre-Oct. 1, 2004, 73.3% of ads neg., no neg. post</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Demobilization</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-choice exposure</td>
<td>Ads aired evenly over course of campaign</td>
<td>Columbia media market: pre-Oct. 1, 2004, 70% of ads neg., post 73.5%</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Lexington media market: pre-Oct. 1, 2000, 87% of ads neg., post 75.6%</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Dallas media market: pre-Oct. 1, 2004, 17.3% of ads neg., post 85.3%</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Fresno media market: pre-Oct 1, 2000, 26.4% of ads neg., post 71.6%</td>
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</table>

Empirical Tests

I conduct several empirical tests of my predictions. The first test relies on experimental data. The second test relies on observational data. I rely on this methodological approach as scholars in the past have suggested that the conflicting negativity findings may be a function
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of reliance on either observational or experimental analyses (Clinton & Lapinski, 2004). Furthermore, previous work on timing of exposure (Krupnikov, 2011) did not present any experimental evidence—a critical omission. The present analysis also addresses positivity, highlighting that the effects I observe are a function of negativity, rather than ad volume or general ad exposure.

**Differential Effects: The Experimental Results**

My first step is to analyze the relationship between negativity, timing, and turnout in a controlled setting. While an experimental approach is, admittedly, a less generalizable analysis of advertising effects than a test relying on observational data, it is nonetheless an important analysis. Only in an experimental test can I be fully certain of exposure, and only in an experimental test can I directly measure mechanisms. In this way, experiments are particularly beneficial for analyzing the direct effects of exposure to various types of political communication (Brader, 2005).

The experimental studies follow previous experimental approaches to analyzing the effect of various stimuli on candidate choice (McGraw, 2011). The first study focused on conditions for mobilization, the second study focused on conditions underlying demobilization, and the third study served as a check to ensure the robustness of the results. All three studies relied on the same fictitious candidates, identical campaign appeals, subjects who were recruited in an identical manner, as well as the same measures and experimental mode. All three were designed such that subjects received the same number of appeals. As a result, this experimental set-up is a controlled, balanced, and conservative test of my predictions. Given the near-identical set-up of the studies, I first present an overview and then discuss the results of each one in turn.

In all studies, subjects were told that they would be receiving campaign appeals from two candidates who were very similar to candidates who may run for the United States Senate in upcoming national elections. My appeals follow those used in previous experiments and are the types of appeals that people may encounter during an actual campaign (stimuli and measures are detailed in Appendix 1, available in the supplemental online appendices). All appeals focus on issues present in the major party platforms. To increase the applicability of these appeals to a national sample and to maintain control in the study, the two candidates were called “Candidate A” and “Candidate B.”

In order to focus on the tone of the message (i.e., its negativity or positivity), I follow Brooks and Geer (2007) and present the appeals to the subjects as written statements. This presentation of candidate statements sets a higher barrier to obtaining results, as often images and sounds work to increase effect size in experimental settings. Further, I conducted a small study prior to the fielding the main experiments; in this study, more than 90% of subjects (convenience sample of 299 adults) were correctly able to identify the intended tone of the appeal as either negative or positive and reported that the appeals were similar to those they had seen during recent campaigns.

To measure choice, I rely on previous experimental research as a foundation. In previous studies, subjects were presented with a stimulus that offered differential information about candidates (in my case, appeals) and were subsequently asked which candidate they would select or would vote for in an upcoming election. Indeed, numerous experimental studies on campaign advertising rely on this approach (for example, Ansolabehere & Iyengar, 1994; Brader, 2005). Much like in previous studies, subjects in my experiments could take as much or as little time as they wished to make a choice. Further, in my
measure subjects could report that they were not ready or did not want to pick either of the candidates.

In each of my studies, I manipulate when subjects were exposed to particular appeals relative to this choice measure. To analyze the mobilizing power of negativity, exposure happens before subjects encounter the candidate selection question. To analyze the demobilizing power of negativity, exposure to negativity happens after the selection question.

The fact that these candidates are fictitious bolsters the internal validity of the study. Had I relied on real candidates, I could not be certain that subjects made choices during the experiment (i.e., they could have begun the experiment having already selected one real politician over another). Relying on fictitious candidates identified simply as A and B helps to control for this potential confound.

In order to measure subjects’ likelihood of turning out to vote in this particular case, rather than their general turnout propensity, I ask subjects how likely they would be to vote in a special election that only included the two candidates whose appeals they saw earlier. The full design of the studies is shown in Table 2.

Each of the experimental studies was conducted over the Internet, and subjects were recruited from the YouGov panel. The YouGov panel includes over 1 million adult respondents; results are weighted to produce a representative sample. Randomization checks—using demographic characteristics measured prior to treatment—were conducted in all studies.

**Study 1: Conditions for Mobilization**

To test the conditions under which negativity can mobilize, my first study focused on exposure to negativity during candidate evaluation. In this study ($N = 279$), some subjects were randomly assigned to receive two positive appeals (“positivity” condition), while others were randomly assigned to receive a positive appeal about one candidate and a negative appeal about the other candidate (“negativity” condition). If my predictions hold, I should observe that subjects in the negativity condition are more likely to report that they will vote than those in the positivity condition.

The results reinforce my predictions. First, subjects exposed to negativity were generally more likely to report selecting one of the candidates than those exposed only to

<table>
<thead>
<tr>
<th></th>
<th>Appeals</th>
<th>Total appeals seen</th>
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<tr>
<td><strong>A: Study 1, mobilization</strong></td>
<td></td>
<td></td>
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<tr>
<td>Group 1</td>
<td>Positivity</td>
<td>2</td>
</tr>
<tr>
<td>Group 2</td>
<td>Negativity</td>
<td>2</td>
</tr>
<tr>
<td><strong>B: Study 2, demobilization</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Group 1</td>
<td>Negativity pre-choice</td>
<td>3</td>
</tr>
<tr>
<td>Group 2</td>
<td>Negativity post-choice</td>
<td>3</td>
</tr>
</tbody>
</table>

*a*This study also includes a check with a double-positivity condition to ensure that it is the presence of negativity that has the key effect.  
*b*Study to consider robustness follows the same set-up as Study 2.
positivity (group difference of 6 percentage points). This effect was particularly large among weaker partisans, who were 13 percentage points more likely to make a choice when exposed to negativity than when they only saw positivity. Subjects exposed to negativity were also significantly more likely to report that they would vote, and again the effect was larger for weaker partisans (see Figure 1). An ANOVA reinforces these group differences in turnout (Table 3).

This experiment also allows for a more direct test of my theoretical argument that as individuals are selecting between candidates, negativity increases the likelihood of choice and this likelihood of choice leads to turnout. Thus, as a next step, I conduct a mediational analysis that treats choice as the mediator between exposure to negativity and willingness to vote. As there are numerous methodological questions about studies of mediation (Bullock & Ha, 2011), I use multiple approaches to conduct this test: I rely on the Baron and Kenny (1986) multimodel test and the Imai et al. (2011) average causal mediation (ACME) test. Although both tests lead to similar substantive results, I present the results from the Imai et al. (2011) ACME test as it is the more conservative test. This test breaks treatment effects

![Figure 1](image_url)

**Figure 1.** Percentage of subjects reporting that they are likely to turn out, by negativity. *p < .05.

<table>
<thead>
<tr>
<th>Study, mobilization</th>
<th>ANOVA</th>
<th>Mediation</th>
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<tbody>
<tr>
<td></td>
<td>$F$</td>
<td>$p$</td>
</tr>
<tr>
<td>Study 1</td>
<td>3.49</td>
<td>.06</td>
</tr>
<tr>
<td>Study 2, demobilization</td>
<td>6.66</td>
<td>.01</td>
</tr>
<tr>
<td>Study 3, positivity</td>
<td>0.53</td>
<td>.47</td>
</tr>
</tbody>
</table>

*Note. CI = confidence interval. In Study 1 and Study 3 CI = 90% confidence interval; in Study 2 CI = 95% confidence interval.*
into the direct effect and indirect, or mediated, effect. This test, then, estimates the direction and relative size of the effect. Further, it allows me to control for covariates that may affect the relationship between negativity, choice, and turnout both before and after treatment. Thus, in my meditational analyses, I control for factors such as partisanship, political interest, knowledge, and previous voting behavior.

Using the ACME method (Imai et al., 2011), I show that choice plays a significant meditational role in the relationship between negativity and turnout (Table 3). Specifically, the positive ACME estimate reinforces the argument: Exposure to negativity increases the likelihood of choice, which in turn increases the likelihood of turnout. In sum, when individuals are exposed to negativity while evaluating the candidates, they are more likely to actually make a choice. In turn, in the absence of post-choice negativity, this choice increased the likelihood of voting.

**Study 2: Conditions for Demobilization**

While the first study set the conditions for mobilization, the second study ($N = 532$) sets the conditions for demobilization. In this study, *all* subjects received three appeals: a positive appeal about each candidate and a negative appeal about one of the candidates. The manipulation was the timing of the negative appeal. Some subjects were randomly assigned to first receive the two positive appeals, then to receive the choice question, and finally to receive the negative appeal (Table 2B, Group 2); others were randomly assigned to receive all three appeals at once on a single screen and then asked about their candidate choice (Table 2B, Group 1). Subsequently, all subjects were asked about likelihood of turnout. For ease of discussion, I call the group exposed to post-choice question as the “post group” and the group exposed only to pre-choice question as the “conventional group.”

For subjects in the post group, the negative appeal focused on their selected candidate. For subjects in the conventional group, the negative appeal focused on the candidate of the subject’s partisanship.¹¹

A comparison of my two groups shows that of subjects who made choices, nearly 6 percentage points more subjects in the post group reported that they would be highly unlikely to vote. This difference is significant at $p \leq .1$. This difference is again somewhat larger for weak partisans, where 10 percentage points more subjects in the post group report that they will be highly unlikely to vote; this difference is significant at $p \leq .01$. I reinforce the significance of the results across the whole sample with an ANOVA (Table 3).

These results, as expected, go in the opposite direction than the results presented in the mobilization study. This effect is highlighted in Figure 1, which traces the mobilizing effect of negativity pre-choice, during evaluation, and the demobilizing effect of post-choice negativity. This figure presents the percentage of individuals who report that they would be highly likely or likely to turn out, showing a decrease in this percentage when subjects are exposed to negativity after a choice and an increase when exposure is pre-choice.

I next turn to an additional theoretical check. Specifically, I theorize that this decline in action is a function of declining evaluations of the selected candidate. Again, I use a meditational analysis to trace this effect, calculating the ACME (Imai et al., 2011) while controlling for the same potentially confounding factors as in the first study. I show that declining evaluations are a significant mediator of the relationship between timing of negativity and turnout likelihood (Table 3). The negative sign demonstrates that exposure to negativity decreases evaluation, which further decreases turnout likelihood. Furthermore, this approach shows that nearly 40% of the group shifts in turnout behavior can be explained by negativity-driven decreases in the evaluation of the selected candidate.
Study 3: Effects of Positivity

To this point, I have demonstrated that identical campaign appeals, offered to identically recruited study participants, can lead to divergent effects depending on the conditions of exposure. A final check on these experimental results is a study that presents subjects with positivity, rather than negativity, after they have made a choice (N = 500). Previous research suggests that positive ads may be distinctly less informational (Craig, Kane, & Gainous, 2005) and are less likely to present evidence in support of their claims (Geer, 2006). As a result, their effect on individuals may be weaker. Further, I have argued that individuals are generally more likely to focus on negativity. The next step, then, is to show that this is indeed the case. This is also a crucial check: If there is something specific to negativity—as my theoretical framework suggests—then we should see that positivity has little effect no matter when it is offered.

The results reinforce the power of negativity. The timing of the positive statement has no effect on turnout behavior, and there are no mediational effects in this study (Table 3). In other words, the effects we see in the mobilization and demobilization studies are a function of a combination between conditions for exposure and negativity.

Timing and Negativity: Observational Results

Although experimental results reinforce predictions, I next ensure that similar patterns can be observed outside of the experimental setting. Using observational data to test the specific conditions under which negativity is mobilizing and demobilizing, however, poses a challenge. While in an experimental setting I can create the necessary conditions for a participant, a non-experimental test depends on the availability of a survey that not only reinterviews the same respondents pre- and post-election, but does so at campaign points that allow for the necessary variance in choice state. If a survey first interviews the majority of respondents early in the campaign process, it is possible that many respondents still will not have made choices, which would make it difficult to observe the post-choice demobilizing effects of negativity. Conversely, if a survey interviews most respondents just days before the election, it would be nearly impossible to observe the mobilizing pre-choice effects of negativity. Yet, accounting for the effects of negativity before and after respondent choice is critical, as previous research on timing only considered whether the ads themselves were aired late in a campaign, without accounting for individual choice (Krupnikov, 2011). Fortunately, the 2000 National Annenberg Election Study (NAES) offers a rare opportunity to conduct my analysis. The NAES Multiple Re-interview Panel B is a smaller panel where respondents were first interviewed starting in September 21, 2000, and then reinterviewed after the election. I rely on a wave of this panel to trace the differential effects of negativity.

Data about ads aired during the 2000 campaign come from the Wisconsin Advertising Project (Goldstein, Franz, & Ridout, 2002). These ad data rely on tracking data provided by the Campaign Media Advertising Group (CMAG) and were coded for content at the University of Wisconsin. CMAG tracked the top 75 media markets in America in 2000. The data provide information about the date of airing, the media market of airing, the television program, and the number of times of airing. I merge the NAES data with ad data by media market and date airing relative to interview.

This combination of data is well suited for the task at hand. First, the Wisconsin data currently offer the best measure of the amount of negativity aired during a campaign. In addition, merging the ad data with the NAES by media market and airdate will allow
me to estimate a respondent’s potential for exposure to negativity with a high level of geographic precision (Freedman & Goldstein, 1999).

Independent Variables

Choice. I measure choice using a two-part question. The first part asks respondents which presidential candidate they intend to vote for in the general election. The second part is a follow-up: Is there any chance that they will vote for the other candidate? This two-part question is similar to measurement approaches used in previous research on the timing of choice during a campaign (Chaffee & Choe, 1980; Kogen & Gottfried, 2011). Just as in previous research, I code respondents who say there is no chance that they will vote for the other candidate as having made a choice; I code those who report that they are “undecided” in the first part of the question or say there is still a chance that they may vote for the other candidate as having not yet made a choice.15

Doubtlessly, there is heterogeneity within those who have made choices—some will have made a choice well before their interview, while others will have made choices more recently. Nonetheless, there are several reasons why choice at time of interview is a useful proxy. While the interview point does not give a precise time at which a choice was made, it is a better proxy than simply asking a respondent to recall when he or she had made a choice (Chaffee & Rimal, 1996). Consequently, scholars have used the interview point as a measure of whether a choice has been made (e.g., Chaffee & Choe, 1980; Fournier, Nadeau, Blais, Gidengil, & Nevitte, 2004). Indeed, Fournier et al. (2004) find that individuals who are coded as having made a choice at the time of the interview remain stable over the course of the election after that interview point, while those coded as not having made a choice fluctuate. Finally, what is critical for my study is whether a choice was made at the time of interview. The goal is to consider whether negativity will lead to differential behaviors relative to choice, and using the interview date as a proxy provides the most controlled baseline to observe this effect.

Advertising. In considering the ads aired in the 2000 presidential election, I rely on coding by the Wisconsin Advertising Project. This approach uses the full ad as the unit of analysis; Franz et al. (2007) explain that “this approach makes particular sense when one is making causal inferences about the impact of an ad, as viewers are more likely to think about an ad in more or less global terms, rather than in terms of its constituent parts” (p. 40). Although this is not the only approach to coding negativity (see Geer, 2006), the “ad as unit” measure has been used in studies of negativity (see Goldstein & Ridout, 2004). Indeed, Krasno and Green (2008) offer direct evidence for the validity of the coding.

I specify the relationship between negativity exposure and choice as an interaction between the amount of negativity aired after a respondent’s interview, his or her television exposure, and his or her choice state at the time of that interview. This means that for respondents who had not made choices at the time of the interview, it is likely that post-interview negativity exposure occurred during the evaluation phase (i.e., pre-choice); for those who had made a choice at the time of the interview, negativity exposure occurred post-choice. Although my theoretical foundation focuses on negativity about the chosen candidate, here I use all negative ads. I do so in order to consider pre- and post-choice effects in one model—after all, I cannot account for negativity about the selected candidate for individuals who have not yet made choices. Undoubtedly, this limits the precision of the analysis, but also makes my task more difficult, as respondents could have been exposed to negativity that likely had no effect on them. To ensure that the results I observe are, in
fact, a function of negativity about the chosen candidate, I conduct an additional analysis focusing only on subjects who have made a choice. This analysis is included alongside numerous robustness checks in online Appendix 5.

*Exposure.* There are numerous approaches to measuring exposure. To ensure that my results are robust to these different approaches, I estimate a series of models that rely on different exposure measures. First, I estimate a model that treats exposure as the interaction between the level of negativity in a given media market and television viewership rates. Second, I estimate exposure using the method presented in Franz et al. (2007); this method is similar to the approach above, but uses television viewership as an adjustment on the number of ads shown rather than treating the relationship as an interaction. Next, I consider exposure by including the percentage of negative ads (Finkel & Geer, 1998) and separately accounting for individual media habits and measures of television habits. Finally, I account for exposure by using the raw numbers of ads, unadjusted for individuals’ television viewing habits. My results are robust to all of these differential approaches. While I present the approach that relies on the interaction in the main text, the additional analyses are included in online Appendix 5.

*State Differences.* As it is unlikely that negative ads are randomly distributed across states, I also control for state differences that may contribute to the level of negativity. My measure of state differences comes from Shaw’s (2006) measure of competitiveness, which is built using interviews with operatives from both the Gore and Bush campaigns and details whether each of the campaigns classified a particular state as a battleground, a “possible leaner,” solidly Republican, or solidly Democrat. To ensure that these patterns do not lead to any simultaneity problems, I also estimate my results using a two-stage approach that first predicts the likelihood that a media market sees negativity (see online Appendix 5); this approach leads to similar substantive conclusions as the model presented here. To address the possibility that errors may be correlated, I cluster by media market. Additional Controls. I control for other determinants of turnout, including demographic characteristics (age, race, education, income, gender, employment status, church attendance, and years at current residence), political characteristics (strength of partisanship, cares about electoral outcome, affect for presidential candidates, and efficacy), mobilization (Rosenstone & Hansen, 1993), and measures of interest (political interest, media habits, knowledge). Finally, I control for the advertising the respondents observed prior to the interview. My dependent variable is the individual’s self-report of turnout in the post-election interview. Given the reliance on self-reports, I also conduct a robustness check that considers aggregate voting patterns (see online Appendix 6).

*Group Comparisons.* Before turning to the results, it is important to consider any differences between respondents who have and have not made choices at the time of the interview that may make one group more likely to be exposed to advertising or more likely to turn out and vote in general. I find few such differences. The groups appear equally likely to follow national and local news, are equally likely to have been contacted by campaigns, and have nearly equal levels of knowledge. Where the two groups differ is in partisanship strength (those who made choices are more partisan) and income (the same group has higher incomes). If anything, though, these differences should make those who have made choices more likely to turn out and vote (partisanship strength), or at least more likely to say
they have voted (higher income), lowering the likelihood of obtaining a result that confirms my prediction. In short, these comparisons suggest that any patterns of turnout observed in the next section are a function of the theoretically defined relationship between selection and negativity, rather than preexisting differences between those who have and have not made choices.

**Results: Mobilization and Demobilization**

I predict differential effects of negativity: During evaluation negativity increases the likelihood of choice, which may increase turnout, but after choice negativity can be demobilizing. Given that I consider exposure to negativity as the interaction between TV viewership and level of negativity, I model the differential effect of negativity exposure by choice through a triple interaction. As a result of the triple interaction, my model also includes all of the constitutive terms and the remaining interactions between my key variables. Overall, however, we should observe that for those who had made a choice at the time of the interview, exposure to negativity post-interview will lead to lower turnout likelihood. In contrast, we should observe that exposure to negativity has a positive, mobilizing effect for those who had not made choices at the time of the interview. As I estimate this relationship using a triple interaction, this implies the following expectations. First, we should observe the strongest effects for those individuals who have higher levels of television exposure—after all, negative ads are unlikely to have an effect if individuals do not see them. Indeed, seeing null or minimal effects for individuals who report watching no television (TV = 0) would reinforce the theoretical argument in this case. Next, this would mean a positive coefficient on the interaction between negativity and TV viewership alone—as this would mean a mobilizing effect when an individual has not made a selection (i.e., choice = 0). In contrast, we should see a negative coefficient on the triple interaction itself.

The results are shown in Table 4. The estimated coefficients on the control variables are generally consistent with previous work on voter turnout (Rosenstone & Hansen, 1993). Turning to the focal variables in this analysis, we see the predicted pattern across both the triple interaction, as well as the interactions with the constitutive terms. First, we see a positive, significant coefficient on the interaction between negativity and television exposure, suggesting that when an individual has not made a choice (choice = 0), increasing exposure to negativity leads to a greater likelihood of turnout. Even more importantly, there is a negative, significant coefficient on the triple interaction, suggesting that increasing exposure to negativity for those who have made choices leads to a decline in turnout likelihood.

Given that a triple interaction can be difficult to interpret, I consider the substantive effects of this model through two figures. In both of these figures, I focus on individuals who have above median television viewership—given that exposure to advertising is necessary for this effect. First, I consider the predicted probability of turnout given choice state and negativity (Figure 2A). Second (Figure 2B), I present the marginal effects of an increase in negativity for those respondents who have made a choice and those who have not (Brambor, Clark, & Golder, 2005). If the results follow from predictions, plotting this average marginal effect of an increase in negativity should show that at equivalent rates of television exposure, negativity has a positive marginal effect on turnout for those who had not made choices and a negative marginal effect for those who had already made choices. This is the pattern in Figure 2B; further, there is no overlap in the confidence intervals,
Table 4
Differential effects of advertising exposure

<table>
<thead>
<tr>
<th>Variable</th>
<th>Negativity Coefficient</th>
<th>SE</th>
<th>Positivity Coefficient</th>
<th>SE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ad tone</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Choice × Neg × TV</td>
<td>-0.785* (0.390)</td>
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<td>Choice × Neg (TV = 0)</td>
<td>0.871 (1.493)</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Choice × TV (Neg = 0)</td>
<td>0.298 (0.221)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Neg × TV (Choice = 0)</td>
<td>0.659* (0.331)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Neg (Choice = 0, TV = 0)</td>
<td>-1.333 (1.469)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Choice (Neg = 0, TV = 0)</td>
<td>-0.633 (0.914)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>TV (Neg = 0, TV = 0)</td>
<td>-0.307 (0.162)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Choice × Pos × TV</td>
<td>0.012 (0.255)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Choice × Pos (TV = 0)</td>
<td>-0.326 (0.897)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Choice × TV (Pos = 0)</td>
<td>-0.186 (0.145)</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Pos × TV (Choice = 0)</td>
<td>0.011 (0.230)</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Pos (Choice = 0, TV = 0)</td>
<td>0.291 (0.948)</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Choice (Pos = 0, TV = 0)</td>
<td>0.170 (0.536)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>TV (Pos = 0, TV = 0)</td>
<td>0.076 (0.130)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Other controls

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient</th>
<th>SE</th>
<th>Coefficient</th>
<th>SE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Income</td>
<td>0.977* (0.397)</td>
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<td>0.762 (0.408)</td>
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</tr>
<tr>
<td>Education</td>
<td>1.460* (0.564)</td>
<td></td>
<td>1.158* (0.558)</td>
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</tr>
<tr>
<td>Age</td>
<td>0.006 (0.010)</td>
<td></td>
<td>0.007 (0.010)</td>
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</tr>
<tr>
<td>Int. efficacy</td>
<td>0.017 (0.152)</td>
<td></td>
<td>0.085 (0.139)</td>
<td></td>
</tr>
<tr>
<td>Ext. efficacy</td>
<td>0.041 (0.105)</td>
<td></td>
<td>0.034 (0.105)</td>
<td></td>
</tr>
<tr>
<td>Employed</td>
<td>0.201 (0.320)</td>
<td></td>
<td>0.045 (0.366)</td>
<td></td>
</tr>
<tr>
<td>Years</td>
<td>0.355* (0.141)</td>
<td></td>
<td>0.390** (0.124)</td>
<td></td>
</tr>
<tr>
<td>Church</td>
<td>0.171 (0.114)</td>
<td></td>
<td>0.167 (0.119)</td>
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</tr>
<tr>
<td>Strength of PID</td>
<td>1.184* (0.433)</td>
<td></td>
<td>1.206** (0.448)</td>
<td></td>
</tr>
<tr>
<td>Affect for candidate</td>
<td>0.005 (0.004)</td>
<td></td>
<td>0.004 (0.003)</td>
<td></td>
</tr>
<tr>
<td>Care</td>
<td>0.598* (0.257)</td>
<td></td>
<td>0.495 (0.290)</td>
<td></td>
</tr>
<tr>
<td>Mobilization</td>
<td>0.321 (0.228)</td>
<td></td>
<td>0.370 (0.259)</td>
<td></td>
</tr>
<tr>
<td>State compt.</td>
<td>0.462 (0.472)</td>
<td></td>
<td>0.376 (0.501)</td>
<td></td>
</tr>
<tr>
<td>Interest</td>
<td>0.015 (0.042)</td>
<td></td>
<td>0.012 (0.038)</td>
<td></td>
</tr>
<tr>
<td>Knowledge</td>
<td>0.106 (0.145)</td>
<td></td>
<td>0.131 (0.143)</td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>0.113 (0.385)</td>
<td></td>
<td>0.001 (0.342)</td>
<td></td>
</tr>
<tr>
<td>Hispanic</td>
<td>-1.229* (0.535)</td>
<td></td>
<td>-1.267* (0.525)</td>
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</tr>
<tr>
<td>Gender</td>
<td>-0.250 (0.259)</td>
<td></td>
<td>-0.174 (0.265)</td>
<td></td>
</tr>
<tr>
<td>Overall volume</td>
<td>0.0001 (0.002)</td>
<td></td>
<td>0.0001 (0.0002)</td>
<td></td>
</tr>
<tr>
<td>Intercept</td>
<td>-0.753 (1.674)</td>
<td></td>
<td>-2.084 (1.499)</td>
<td></td>
</tr>
</tbody>
</table>

*p < .05; **p < .01.

underscoring the differential effects depending on whether a choice was made at the time of the interview.

Further reinforcing my argument, however, is that the differential effect of negativity on choice increases—and reaches statistical significance—as TV viewership grows.
Figure 2. Relationship between negativity, choice, and turnout. Other variables set at means and mode when appropriate. (A) Predicted probability of turnout; (B) marginal effect of increase in negativity. In B, the confidence interval is at 90%. Increase in negativity is from median to maximum.

Negativity, for example, has no significant effect on respondents who report watching no television, regardless of their decision state. This reinforces the theoretical argument of the article by reinforcing the importance of exposure to the predicted effect.

Results: Positivity

To complete this analysis, I turn to positivity, much as I did in the experimental analysis. To do so, I replicate my main model using positivity rather than negativity. Indeed, this analysis acts as a useful foil: If the effects observed are simply a function of splitting any type of informational exposure by choice, it would suggest that my theoretical foundation does little to unify the previously conflicted literature on negativity.

Reestimating my main model with positivity (Table 4) reinforces the distinct role of negativity. Positivity has no effect on individual behavior, under any conditions. Further, the effect of positivity pre-choice and the effect of positivity post-choice are not only non-significant, but they are largely identical. For example, moving from minimal to mean positivity exposure decreases the likelihood of turnout for those who have made choices by $-0.009$ and, similarly, decreases the likelihood of turnout of those who have not made choices by $-0.002$; neither change is significant.

Robustness Checks

As a last step in my observational data analysis, I consider the robustness of my results and address potential alternative explanations (all presented in online Appendix 5). I first consider alternative exposure measures. Next, since I rely on presidential ads, I ensure that my results are indeed driven by this particular election. To do so, I follow Goldstein and Freedman (2002), who were faced with a similar context: Their data focus on the presidential race, yet there are other races going on simultaneously. Following Goldstein and Freedman (2002), I include pre-election measures of House and Senate competitiveness...
Negativity and Voter Turnout

by state. Further, I estimate a model that controls for other participatory behaviors that a respondent may have performed during the campaign to ensure that individuals are not simply acting on their choices in other ways. Next, I instrument the amount of negativity in a media market.24

My results are robust to these differences in model specification, reinforcing the differential effect of negativity. Those who have selected and those who have not selected a candidate were exposed to similar campaign advertisements after their NAES interview. Yet, exposure to negative ads leads them in opposite directions: For those who have made a choice, these ads lead them away from the polls; for those who have not made a choice, these ads increase turnout likelihood.25

Conclusions

Do negative ads affect voter turnout? This is a question that “political scientists should be able to answer” (Lau & Pomper, 2004, p. 10). If large groups of individuals do not turn out and vote, then the representativeness of American democracy is in question. If elected officials are not an accurate representation of the public will, can the policies of the government still be legitimate? On the other hand, if negative campaigns prove mobilizing, providing citizens with useful political information, then there is no need to despair about the increase in negative campaigning over the last 30 years.

The results of this research suggest that the effect of negativity on individual behavior may be more nuanced than basic mobilization and demobilization, and may depend on the context in which an individual is exposed to negative ads. Put another way, negativity is not either mobilizing or demobilizing, it is both. My results, then, suggest that the effects of negativity are highly conditional; even an identical ad can have profoundly different effects depending on a person’s particular point of exposure.

Acknowledgments

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Supplemental Material

Supplemental data for this article (Appendices 1–6) can be accessed on the publisher’s website at http://dx.doi.org/10.1080/10584609.2013.828141.

Notes

1. Except in cases of mudslinging.
3. In 2012, there were increases in ads embedded in entertainment programs shown online. Even if individuals can fast forward through televised political ads, most embedded online ads require that the viewer watch all the way through in order to watch the show.
4. This argument assumes that individuals are voting for their preferred candidate rather than against their disliked candidate. It is possible that people do vote based on candidate threat, meaning that they turn out to vote against an opponent. Recent research on voting by threat suggests, however, that this is most likely to happen when people have a very high sense of self-efficacy and a strong attachment to a particular candidate (Schuck & de Vreese, 2012). Thus, it is unlikely that these individuals would have been moved by post-choice negativity no matter their voter motivations.

5. Aggregate turnout patterns reinforce these predictions. Relying on the turnout information Krasno and Green (2008) present, the 2000 turnout rate in the Fresno media market—a market where the bulk of negativity was aired late in the campaign—was 47%. In the Lexington media market, where negativity aired evenly over the course of the campaign, turnout was at 50%. In contrast, however, within the same year, in the Albany-Schenectady media market, the bulk of negativity was aired early; within that media market, turnout was between 58% and 61% (depending on state). Constructing aggregate turnout data for 2004 shows that in the Chattanooga media market, where negativity was early, turnout was 63%. Meanwhile, that same year, in the Dallas media market—where the bulk of negativity came late in the election—turnout was 51%. In sum, although these results are not as consequential as the individual-level analyses in this article, they do hint at an important turnout pattern.

6. I rely on issue ads following previous scholars (Ansolabehere & Iyengar, 1994; Clinton & Lapinski, 2004). Further, the majority of negativity is issue based: In 2000, 93.44% of negative ads were issue ads; in 2002, 88.14% were issue ads; and in 2004, 96.59% were issue ads.

7. To ensure that observed results are not due to the fact that the candidates are simply called “A” and “B,” I replicate this study with an experiment that used real names (online Appendix 3).

8. Party of candidate was randomized.

9. Effect significant at \( p \leq .01 \).

10. Effect significant at \( p \leq .1 \) for the sample.

11. Pretests showed that partisanship is a good approximation of the candidate the individual will select absent any information. I conducted additional checks excluding subjects in the post group who identified as members of one party yet selected a candidate of a different party. The final result holds even when this group is excluded.

12. The NAES is commissioned by the Annenberg School for Communication and the Annenberg Public Policy Center of the University of Pennsylvania (Romer et al., 2004).

13. This is a smaller wave of the campaign than the NAES cross-section. As a result, the \( N \) in my analyses will naturally be smaller than the \( N \) of the full cross-section. Further, the \( N \) is smaller due to the interview dates necessitated by the structure of my analysis. I conduct checks to ensure that there is no distinction in the respondents interviewed on particular dates. All models are robust to controls measured in either pre- or post-election waves. I rely on the 2000 NAES for two key reasons. First, the timing of the pre-election interviews allows for variation in choice state that is highly beneficial for my tests. Second, this particular survey asks choice questions most appropriate for the analysis at hand.

14. Use of the Wisconsin Advertising Project data requires the following acknowledgment: “The data were obtained from a joint project of the Brennan Center for Justice at New York University School of Law and Professor Kenneth Goldstein of the University of Wisconsin-Madison, and includes media tracking data from the Campaign Media Analysis Group in Washington, D.C. The Brennan Center-Wisconsin project was sponsored by a grant from the Pew Charitable Trusts. The opinions expressed in this article are those of the author(s) and do not necessarily reflect the views of the Brennan Center, Professor Goldstein, or the Pew Charitable Trusts.”

15. Given that these are panel data, tracing respondents’ eventual voting behavior provides additional validation of this measure. Notably, for this measure to be useful, the majority of respondents who are considered “undecided” need not change their minds—but the percentage of preference reversals should be significantly and substantively higher among these respondents than among those who have made choices. Of those individuals who first report that they would vote for Gore and that there is no chance they would vote for Bush, only 1.7% intended to cast ballots for Bush on the day prior to the election. In contrast, of those who first reported that they would vote for Gore, yet said
there was still a chance they might vote for Bush, 25% intended to vote for Bush at the end of the campaign. Similarly, of those who initially reported that they would vote for Bush and said there was no chance they would vote for Gore, 1.6% intended to vote for Gore at the end of the campaign; of those who stated they preferred Bush but said there was still a chance they might vote for Gore, 20% reported that they intended to vote for Gore. These results are robust to controls for partisan strength.

16. I consider the level of negativity in two ways—as a percentage and as the raw number of negative ads. Both lead to substantively similar results, but the models in the main text rely on percentages of negativity.

17. The Franz et al. (2007) measure also relies on the natural log of the value obtained when the number of ads is adjusted by television viewship.

18. I also estimate a random effects probit; both models lead to the same conclusions.

19. See online Appendix 4.

20. I do not make predictions about the effects of pre-interview negativity due to data structure, though one can argue that in a media market with a consistent flow of negativity, there could be conditions where pre-interview negativity could set the stage for post-choice demobilization.

21. As Vavreck (2007) notes, there are errors in turnout self-reports. My analyses suggest that while self-reports are by no means a precise measure, there is nothing to suggest that the factors that lead to misreporting are confounded with factors crucial to my theoretical premises. Moreover, Berent, Krosnick, and Lupia (2011) suggest that validated votes do not lead to better data than self-reports.

22. For respondents who have not made choices and watch no television, the marginal effect of negativity is $-0.19, p = .431$. For respondents who have made choices and watch no television, the marginal effect of negativity is $-0.07, p = .656$.

23. I also obtain similar patterns of substantive results relying on raw negativity in the triple interaction. The average marginal effect of a choice decreases likelihood of turnout by 0.10 at average levels of negativity and by 0.23 at levels of negativity in the upper quartiles of the negativity distribution (both significant decreases in turnout likelihood).

24. I also leverage the panel structure of the data to consider whether the relationship between advertising and choice can explain changes in turnout intention over the course of the campaign. Using a measure of intention to vote at the start of the campaign and a measure of intention to vote near the end of the campaign, I create a dependent variable that considers whether intention to vote declined. Then I use the relationship between negativity, exposure, and choice to trace patterns in this shift. The results (presented alongside other tests in online Appendix 5) show that when individuals are exposed to negativity after a choice has been made, their intention to vote is significantly likely to decline. In contrast, when individuals are exposed pre-choice, their intention to vote is likely to either remain constant or increase. These results further reinforce that negativity has distinct effects before and after choice.

25. Another robustness check is the aggregate patterns in the same year, by media market. Specifically, using aggregate-level turnout data (Krasno & Green, 2008), I again use October 1 as a proxy for early and late negativity and consider whether we see the expected differential pattern across aggregate turnout results. As controls, I rely on demographics gathered via the U.S. census, previous turnout information, as well as competitiveness information. Using these data, I estimate a series of models relying on different estimation approaches: OLS, a two-stage model that initially uses media market factors to predict negativity patterns within the market and then considers turnout factors, and a model with state-level fixed effects (Krasno & Green, 2008). All results reinforce previous conclusions obtained with individual-level data: Early negativity increases turnout, while late negativity decreases it. The coefficients on the critical negativity variables are included in online Appendix 6.

References


