

ARTICLE TYPE

What Drives Perceptions of the Political in Online Advertising? The Source, Content, and Political Orientation

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Abstract

As digital platforms become a key channel for political advertising, there are continued calls for expanding regulation of digital political ads as a distinct content category. However, designing policies to meet these demands requires us first to decipher what the public perceives a ‘political’ ad to be. In this article, we report two preregistered experiments to understand factors that drive public perceptions of what makes an ad political. We find that both advertiser-level cues and content-level cues play an independent role in shaping perceptions. To a lesser extent, participants also attribute political meaning to ads that clash with their own preferences. These patterns were replicated in a conjoint study, using artificial ads, and an experiment using real-world ads drawn from the Facebook Ad Library. Our findings serve as an important benchmark for evaluating proposed definitions of political ads from policymakers and platforms.

Word Count: 3864

1. Introduction

Digital advertising has become an increasingly important part of electoral politics in the United States and generates growing revenue streams for platforms. In the 2020 U.S. election cycle, for instance, approximately 18 % of all political ads spending went to digital ads (\$1.6 billion), a dramatic increase from the 2-3 % in the 2016 cycle (Homonoff, 2020). Unsurprisingly, this change has come with calls for greater regulation. This would place digital media on a more even footing with broadcast media, where political ads have long been subject to specific rules. It is also consistent with growing evidence that the public thinks of political advertising as a distinct category and has different expectations and tolerances for it compared to commercial advertising (Herder and Dirks, 2022; Zeng, Kohno and Roesner, 2021). Thus far, regulations have primarily come from the digital platforms themselves, but there is significant interest in expanding government oversight (e.g., Coons, 2021).

However, meeting demands for new regulations in this domain is complicated by uncertainty about what people mean when they refer to a ‘political’ ad. If platforms and

government officials wish to benchmark their policies to public expectations, a crucial step must be to establish what actually makes an ad political to the public. Are there identifiable dimensions of content that drive these perceptions and, if so, which factors are most important? Despite the centrality of this question for understanding public preferences in this domain, we are aware of no previous studies that tackle it directly and the existing literature provides little guidance.

In this article, we use two large nationally representative surveys to study what factors contribute to perceptions of ‘politicalness’ in digital ads. Specifically, we compare the importance of ad source, content, and political orientation in shaping public perceptions of the political. The first two features are important to understand because they are the primary features that platforms¹ have used to define political advertising (although which dimension is essential has varied significantly across platforms and over time). The latter feature is important to study to rule out the possibility that the public merely views content they disagree with as being ‘political’.

Methodologically, we first use a conjoint design that allows us to isolate the independent causal effects of each dimension (Hainmueller, Hopkins and Yamamoto, 2014). We further conduct a within-between experiment asking respondents to evaluate real ads drawn from the Facebook Ad Library. This allows us to replicate the findings from our conjoint using authentic stimuli, and we refer to this as the real ads experiment.

We find that both sponsor-level cues (e.g., sponsorship by candidates) and content-level cues (e.g., stronger messages advocating for government action) play an independent role in shaping perceptions. Participants attribute more political meaning to ads that clash with their own preferences or are sponsored by ideologically unaligned entities, but these effects are modest. These patterns replicate across studies, although sponsorship is more important in the real ads experiment.

Our results are important because they indicate that the public on average does have a systematic, if implicit, understanding of the political. Although there is no universally accepted definition of political advertising, there are clearly characteristics that drive these perceptions on average. Thus, our results serve as an important benchmark for policymakers and platforms wanting to align their approaches with public expectations on how political ads should be defined. In particular, these results suggest that definitions that rely on sponsor-level or content-level features in isolation do not match the public’s understanding.

2. What makes an ad political?

Debates on the definition of “political” have a long history, from Aristotle to 19th and 20th-century theorists (e.g., Arndt, 2019; Weber, 2004). However, empirical research often lacks an operational definition (Guess et al., 2019), with scholars noting they have “largely ignored the problem of vague definition” (Settle, 2018, p. 120).

Yet, for social media companies, defining and operationalizing this concept has become unavoidable. Social media has become a sought-after delivery platform for political advertising. And as this has happened over the last decade in particular, public calls for platforms to place limits on political advertisers have grown ever louder. Starting in 2018, social media companies have responded to this pressure by creating new policies to govern the delivery of political advertising on the platforms they control. Exactly which ads should platforms subject to these new policies? And what definitions should they draw upon?

One answer is to use legal definitions. However, in the United States, government entities

¹Ad sponsorship and content have also been the primary features of proposed legislation in Congress.

can offer little legal guidance due to First Amendment constraints on government action in digital spaces and a near-vacuum of federal regulation (Fowler, Franz and Ridout, 2021). Another approach is to draw from political theory and qualitative insight. Here, while there is broad agreement on core political speech (e.g., candidate ads), the boundaries are politically contested (e.g., the feminist adage that the “the personal is political”). To the extent there is a consensus, it is that the definition is contextual. Politicalness, it seems, is highly subjective, “created through the process of developing and clarifying one’s social identity” (Cramer Walsh, 2004, p.4). This, as one may imagine, is a difficult conceptualization for platforms to execute at scale.

Existing quantitative research on public views of the political offers little better guidance. Fitzgerald (2013) used a convenience sample of U.S. Americans and Canadians to investigate which issues were considered political across contexts. Settle (2018) relied on a larger representative survey of U.S. Facebook users in 2016 to study how respondents learned the political orientation of friends. Both studies report that what the public views as political is contingent on factors such as ideology, gender, and political engagement, but it is unclear how exactly this translates to online ads. More recently, Groenendyk and Krupnikov (2021) show that politics is associated with adversarial thoughts (see also Groenendyk et al., 2024). Finally, in research most similar to our own, Sosnovik and Goga (2021) examine which Facebook ads were considered to be about elections, politicians, or social issues by a non-representative set of labelers. While this study did not attempt to identify which components of ads motivated their choices, its goal of attempting to detect political ads based on user labeling is related to ours (see also Guess et al., 2019).

Therefore, platforms have by necessity been forced to develop definitions themselves with little concrete guidance. To better understand these policies, we believe it is informative to turn to a method frequently employed by both independent and platform-affiliated human-computer interactions (HCI) and social media researchers when studying politics-adjacent issues: user studies (Zeng, Kohno and Roesner, 2021; Redmiles, Bodford and Blackwell, 2019; Ur et al., 2012; Redmiles, Chachra and Waismeyer, 2018; Zeng et al., 2021; An, 2020; Wei et al., 2020; Darke and Ritchie, 2007; Gomez-Mejia, 2020). While public opinion should not necessarily dictate platform activities, it seems reasonable that it should at least inform policy design in this area, as it does in so many others. In this way, our study aligns with research that often relies on respondents’ implicit understanding of “political” (e.g., Guess et al., 2019; Settle, 2018; Sosnovik and Goga, 2021), but leverages our experimental design to more directly interrogate the dimensions that drive these perceptions.

Thus, to motivate our design, we begin by considering how the major social media platforms have defined the political. Definitions on each platform have shifted significantly over the past several years, but broadly speaking, there are two dimensions that platforms use to identify an ad as political or not. First, an ad may be political because of the sponsor, regardless of the ad content itself. For example, platforms identify all ads paid for by federal candidates for office as political (Facebook, 2022d; Google, 2022a; Twitter, 2022c). Second, an ad may be identified as political, regardless of its sponsor, due to its content. For instance, almost all platforms identify ads that are substantially focused on candidates for national office as being political.

Between different platforms, definitions vary about specifically which sponsors or what content make an ad political. A useful comparison for our experiments are the policies at Facebook, Twitter, and Google that were in place during the 2020 U.S. presidential election (and motivated this study). The relevant policies in place at all three companies are indicated in Table 1. Of the three major platforms, Facebook used the broadest definition of political content, including ads about federal, state, or local elections (Facebook, 2022c) and policy

Table 1. Electoral and issue ad policies of major social media platforms during the 2020 U.S. election period

	Google	Facebook	Twitter
Federal Electoral Ads Allowed?	Yes	Yes	No
State Electoral Ads Allowed?	Yes	Yes	No
Local Electoral Ads Allowed?	Yes	Yes	No
Issue Ads Allowed?	Yes	Yes	Yes
			w/Limitations
Ads Classed as Political Based on Advertiser?	No	Yes	Yes
Ads Classed as Political Based on Content?	Yes	Yes	Yes
Commercial exemption?	Yes	No	Yes
News exemption?	Yes	Yes	Yes

issues (Facebook, 2022a). It refers collectively to ads that fall under this umbrella as ads on “social issues, elections, and politics.” However, Facebook (as well as Google and Twitter) exempted ads from verified news publishers from their political ad policies (Facebook, 2022b; Twitter, 2022b; Google, 2021).

During this period, Google included only federal and state ‘electoral’ content, defined as ads featuring candidates or officeholders on the state or federal level, political parties, and ballot measures (Google, 2022b). It also exempted ads for products and services from these policies, which we refer to as a commercial exemption. Twitter defined two separate categories of political ads. The platform referred to electoral ads on the federal, state, or local level as ‘political’ (Twitter, 2022c) and called issue ads ‘cause-based’ (Twitter, 2022a) advertising. Unlike Google, whose policy for electoral ads was focused solely on content, Twitter defined ‘political’ advertising both in terms of content² and in terms of the speaker: content from political candidates, office holders, and parties, as well as political action committees (PACs) was always considered political (Twitter, 2022c). By contrast, Twitter defined cause-based advertising as content that seeks to “educate, raise awareness, and/or call for people to take action in connection with civic engagement, economic growth, environmental stewardship, or social equity causes.” This is what we refer to as issue ad content. During the 2020 U.S. presidential election period, Twitter prohibited ‘political’ ads entirely, and limited how ‘cause-based’ ads could be targeted.

Motivated by these policies, our study focuses on better understanding how these core features – the source and content – drive perceptions of the political. Specifically, we pre-registered³ two main hypotheses.

H1: Ads from sources viewed as being overtly political (i.e. presidential candidates) will be rated as more political than other sources (i.e. corporations and advocacy groups).

H2: Ads with messages that engage in stronger content will be rated as more political, regardless of source.

The term “stronger” refers to content that more expressly advocates for a candidate or public policy. As we discuss in Appendix A in more detail, this definition comes with some ambiguity. However, it was intended to echo the platforms’ content-based definitions.

In addition, our third hypothesis is that respondents will be more likely to label ads as political when they are non-congruent with their own prior attitudes. This expectation

²An ad meets Twitter’s political content test if it references a candidate, political party or election.

³The wording of H2 deviates somewhat from the pre-registration. See Appendix E for a full discussion.

builds on past research showing individual heterogeneity in how individuals define “political” (Fitzgerald, 2013), as well as the extensive literature on motivated reasoning (Bolsen, Druckman and Cook, 2014; Lodge and Taber, 2013).

H3: Prior beliefs will influence opinions, in that, participants will perceive ads with messages that do not conform to their beliefs to be more political than those that do.

This dimension is important to consider to rule out the possibility that the public simply sees content they disagree with as political.

Finally, one concern is that our results may be driven by the artificial nature of the political ads created for the conjoint experiment. Therefore, we tested if these patterns replicate when using authentic political ads in the following research question.

RQ1: Do the effects measured in the conjoint analysis replicate when respondents evaluate real social media ads?

3. Methods and materials

Our analyses rely on two nationally representative samples from the American Social Survey (TASS). The samples are collected by the National Opinion Research Center (NORC) at the University of Chicago. TASS draws a national sample of 1,000 from the large panel of 35,000 people already recruited by NORC. These are general population samples of U.S. adults aged 18 and older and were selected from NORC’s AmeriSpeak Panel for this study to match national benchmarks. The NORC1 survey was fielded July 1 – 24, 2020 ($N = 1,006$). This survey included the conjoint experiment and the real ads experiment. However, due to a programming error, the conjoint study from this wave was not usable. The NORC2 survey was fielded to re-administer the NORC1 wave with the correct programming for the conjoint. This was fielded from March 31 to April 19, 2021 ($N = 1,013$). The conjoint and real ads experiments were both preregistered.⁴ We analyze both the conjoint and real ads data from this survey.⁵

We also conducted two smaller surveys of U.S. adults recruited via Amazon’s Mechanical Turk (AMT). These additional studies were designed to validate our coding of the message strength needed to test H2. These samples are discussed in Appendix D.

Conjoint Experiment

We constructed realistic stimuli by recombining a source, message, and image selected at random from a pre-selected set of components shown in Figure 1. We then constructed ads representing every possible combination of these features. Respondents were shown two of the possible ads at random and were asked to choose the one that was more political. An example task is shown in Appendix C. Respondents completed this task eight times.

In choosing the ad components, we sought to make a compromise between ad elements that were somewhat realistic and ad elements that lent themselves to clean randomization as part of the conjoint experiment. Since respondents were asked to make eight choices, we also wanted variation in wording and appearance.

⁴Link to pre-registration: <https://osf.io/cnd9y/>

⁵Data available on the Harvard Dataverse (Edelson et al., 2025).

To begin, we chose a single broad issue area. We did not want to have ads discussing wildly different topics as this would have introduced yet another dimension. We chose energy and environmental policy, which has the advantage that commercial companies (e.g., Exxon and Patagonia) regularly advertise in this issue area. In addition, choosing one issue area allowed us to create more plausible stimuli since we can match aspects like images and sponsor to the content. It would be implausible, for instance, for the Sierra Club to release ads on abortion rights. We discuss this limitation further in our concluding discussion.

Next, we selected real advertisers who had ads in the Facebook Ad Library related to either oil sector development or environmental protection. The categories of ad sources that we chose included political candidates, non-profit groups, and politically active companies, such that there was one pro-development⁶ and one pro-environmental source in each category. Candidates were selected by choosing the presidential candidate from the Republican and Democratic parties. Non-profit organizations were selected by first identifying the top-spending non-profit organizations on environmental and business regulatory issues and then selecting the highest spending pro-environment (The Sierra Club) and pro-development (Power the Future) organizations whose advertising was solely focused on those issues. Politically active corporations, Exxon and Patagonia, were identified by looking for corporations that were top spenders on this topic, and then selecting entities with PACs that had more than 75% of their spending identified as being for one party according to Open Secrets.

Message texts were taken from the Ad Library and edited lightly to make them more comparable in terms of message strength. We chose three pro-development and three pro-environment messages of varying strength, where stronger ads more explicitly advocate for government action. To validate our coding of strength, we showed these messages to a small convenience sample. These results are shown in Appendix D and support our coding.

Images were necessary to make the ads seem authentic but are not our focus. We chose relatively generic images that might seem appropriate next to a variety of messages. Recall, however, that the conjoint design means that any effect of the images is marginalized out.

Prior orientation for respondents' opinions on development was measured based on responses to two items related to global warming and environmental protection. The specific question items are shown in Appendix C. We combined these items additively and used a median split to identify "pro-development" and "pro-environment" respondents.⁷

In our analysis, we use the statistical methods described in Hainmueller, Hopkins and Yamamoto (2014) and implemented them in the `cjoint` R package. This allows us to calculate the average marginal component effect (AMCE) for our main hypotheses. To test how message orientation interacts with prior beliefs, we calculate the average component interaction effect (ACIE).⁸

Real Ads Experiment

To study how respondents judge the politicalness of more realistic ads, we designed a second experiment using real ads selected from Facebook's Ad Library. This study was repeated on both the NORC1 and NORC2 waves, which we pool in our analysis below.⁹ Respondents were asked to evaluate ads from eight different sources. We selected ads from the same six

⁶For the sake of exposition, we refer to ad content that promoted further economic activity in the energy industry as "pro-development" throughout.

⁷We also conduct an exploratory analysis as to whether there are also heterogeneous effects based on partisanship. The results are consistent with findings in the main text (Appendix F.2).

⁸To ease interpretation, we fit two separate models to study the heterogeneous effects of message orientation and source orientation. However, the substantive results remain nearly identical if we instead run a single model, as we show in Appendix F.

⁹In Appendix G, we show that these patterns hold when disaggregating by survey.

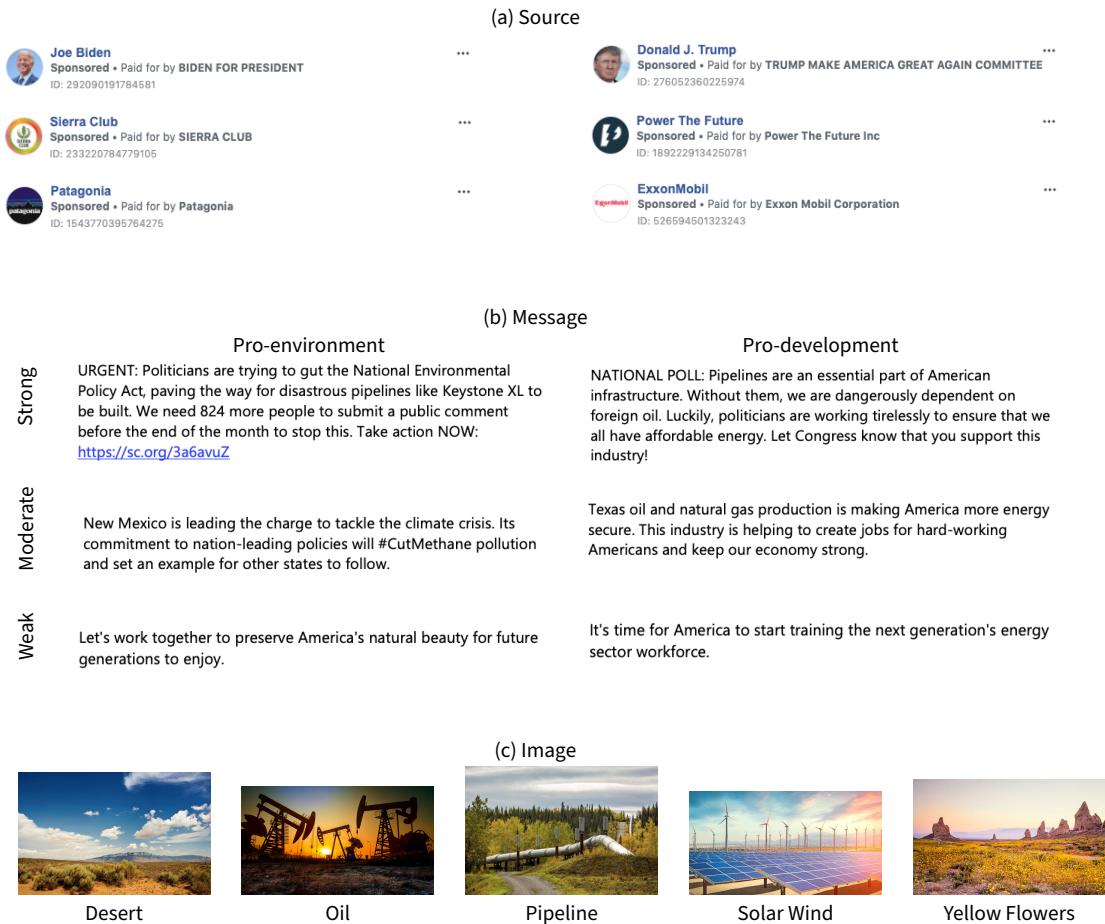


Figure 1. All source, message, and image components used to combine the $6 \times 6 \times 5 = 180$ ads of the conjoint experiment.

advertisers as in the conjoint experiment and added an additional category: non-political corporations. Our non-political corporations, MSC Industrial and Colgate, were identified by looking for advertisers that had a small number of non-declared political ads, and no declared political ads in Facebook's Political Ad Library.

From each advertiser, we selected two ads. One was coded as a relatively strong political message and imagery, while the other was relatively weaker. As noted above, we define strong ads to be those that more expressly advocated for a candidate or public policy (see Appendix A). All ads are shown in Appendix C. For the non-political corporation category, 'strong' ads were selected from the source's non-disclosed political ads and 'weak' ads were selected from currently running non-political ads from Facebook's Library for all ads. The relative strength of the messages was validated with the second Mechanical Turk convenience sample ($N = 302$), which is discussed in Appendix D.

In the experiment, each respondent was shown one ad from each sponsor (for a total of eight ads) and asked, "How political is this ad?" Responses were on a five-point scale ranging from 'not at all political' (1) to 'extremely political' (5). For each source, respondents were randomly assigned to see either the 'strong' or 'weak' ad. Thus, this can be considered a

within-between experiment since respondents rated multiple ads but the exact composition of the ads was randomized for each respondent. In the main text, we estimate the effects of ad strength separately for each source, essentially treating them as eight separate between-subject experiments.¹⁰

4. Results

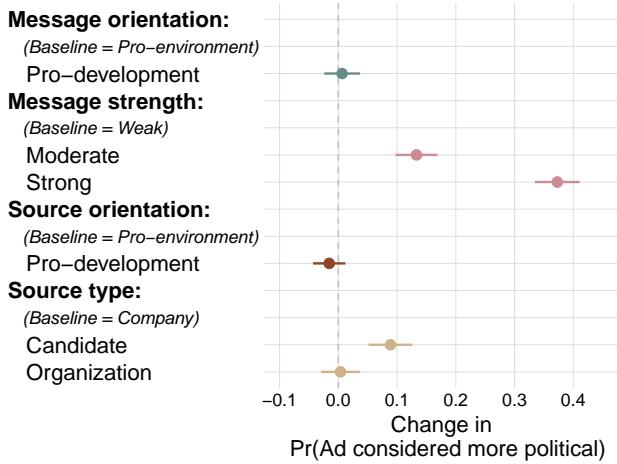


Figure 2. Effect of advertisement's attributes on the perception of ad's politicalness (Average Marginal Component Effects). Estimates of the effects of the randomly assigned ad attributes on the perceived politicalness of the ad in a paired conjoint experiment. The full model includes an attribute accounting for the images displayed in each ad. See Table F.1 for full results. (Source: Sample NORC2; $N = 1,013$)

We begin by reporting the AMCE for the conjoint. There are 8,008 observations from 1,013 respondents. The AMCE estimates are shown in Figure 2 and the full table of results is shown in Appendix F. While both the ad source and message matter, it is the content of the ad messages that has the largest AMCE on perceived politicalness. Specifically, moving from the baseline of a weak message (e.g., “Let’s work together to preserve America’s natural beauty for future generations to enjoy”) to a moderately strong message (e.g., “Texas oil ... production is making America more energy secure.”) has an effect of 13.3% ($se = 0.02$, $p < 0.001$). Moving from the baseline to a strong message (e.g., “Politicians are trying to gut the National Environmental Policy Act”) has a an effect of 37% ($se = 0.02$, $p < 0.001$). Substantively this effect size is large. In all, this is clear evidence in favor of H2.

In terms of ad source, the AMCE for moving from the corporate baseline (Patagonia and Exxon) to a political candidate (Biden or Trump) is 9% ($se = 0.02$, $p < 0.001$). Moving from a corporate source to a political organization (Sierra Club or Power the Future) has a modest effect of 0.5% ($se = 0.02$, $p = 0.83$). Thus, the effects of the source are more modest but still support H1.

We next turn to analyzing the effects of ad components on perceived politicalness conditional on respondents’ prior beliefs. Consistent with our pre-registered expectations, the

¹⁰To better approximate the conjoint analysis, we also fit a pooled model combining six of the sources, leaving out the non-political companies since those were not in the conjoint study. These results are discussed in Appendix G.4 and conceptually replicate the findings in the conjoint.

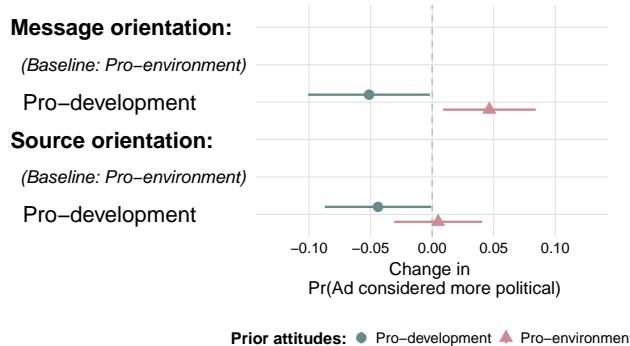


Figure 3. Effect of pro-development message and source on perception of ad's politicalness, conditioned on pro-development prior orientation. Estimates are Average Component Interaction Effects in a paired conjoint experiment. The dependent variable is a dummy variable indicating whether an ad was selected as more political when presented with two ads. See Table F.2 for full results. (Source: Sample NORC2; $N = 1,013$)

results in Figure 3 show that among respondents with more pro-development attitudes, messages more in favor of oil development were perceived as less political than messages with more pro-environment messages ($\beta = -0.05$, $se = 0.03$, $p = 0.04$). Likewise, ads from pro-oil sources (e.g., Exxon) were viewed as being less political relative to ads from pro-environmental sources such as Patagonia ($\beta = -0.04$, $se = 0.02$, $p = 0.05$). Respondents with pro-environmental prior beliefs viewed pro-development ads as being more political ($\beta = 0.05$, $se = 0.02$, $p = 0.02$), although there was no measurable effect for source orientation for this group ($\beta = 0.005$, $se = 0.02$, $p = 0.79$). This evidence largely supports H3, although these effect sizes are modest.

Turning to the real ads experiment, we first analyze the effect of each treatment (strong versus weak ad) separately for all eight sources. The means are shown in Figure 4, and simple bivariate regressions are reported in Appendix G. Full results of summary statistics are reported in Appendix Table G.1. For each of the eight sources, the ad with the stronger message was rated as being more political with treatment effects in Table G.2 ranging from 0.60 ($se = 0.07$, $p < 0.001$) for Biden to a remarkable 1.93 on the five-point scale ($se = 0.06$, $p < 0.001$) for Patagonia.

As in the conjoint, the real ads experiment suggests that the public does appear to view ads from politicians as generally more political than ads from advocacy groups, which in turn are perceived as more political than those from politically active businesses. Indeed, Figure 4 illustrates that even ads from politicians with weak messages (e.g., selling t-shirts or coloring books) are on average viewed as political with mean scores of 3.62 for the weak/Biden ad and 3.38 for the weak/Trump ad, placing them somewhere between “somewhat political (3)” and “very political (4).” We confirm that these source-based cues are statistically distinguishable in our pooled analysis reported in Appendix G.3.¹¹

To understand how prior attitudes shape evaluations of politicalness, we ran an additional analysis of each study controlling for party affiliation (and excluding independents) and gender. The full results are shown in Table G.3 and Table G.4, respectively. The results show that Republican respondents (relative to Democrats) found ads from pro-development/Republican sources less political, while the pro-environment/Democratic ads were viewed as more politi-

¹¹We also examine the heterogeneous effect of conditioning on gender suggested in literature (Settle, 2018). We show in Appendix F.2.3 that there is no statistically significant difference between treatment effects among female and male respondents.

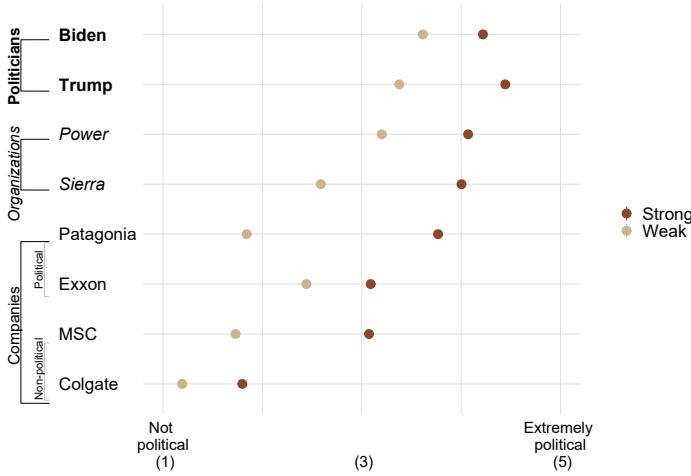


Figure 4. Average perceived politicalness of political ads by source and message strength. For each of the eight ad sources, each participant saw an ad with either a weak or strong political message. Point estimates represent weighted means (with 95% confidence intervals). The variables are measured on a five-point scale ranging from not political (1) to extremely political (5). Confidence intervals are sufficiently small that they are not always visible behind the point estimates. See Table G.2 in Appendix G for full results. (Source: NORC1 and NORC2 $N = 1,963$)

cal. The indicator for Republican respondents is positive and statistically significant for the Biden ($\beta = 0.42$, $se = 0.07$, $p < 0.001$), the Patagonia ($\beta = 0.21$, $se = 0.07$, $p = 0.002$) and the Sierra Club ($\beta = 0.14$, $se = 0.07$, $p = 0.04991$) experiments.¹² Consistent with expectations, the relevant coefficients are negative and significant for the Trump ($\beta = -0.53$, $se = 0.07$, $p < 0.001$), Power the Future ($\beta = -0.32$, $se = 0.07$, $p < 0.001$), and Exxon ads ($\beta = -0.15$, $se = 0.07$, $p = 0.039$). Here again, this effect is consistent with our results from the conjoint.¹³

In summary, candidate ads are viewed as inherently political, in contrast to sources such as politically active companies and advocacy organizations, where message strength appears to matter far more in order for an ad to be considered political. This differs from our finding in the conjoint analysis, where ads from companies and advocacy organizations were viewed as equally political. This may reflect the artificial nature of ads in the conjoint experiment, the fact that we did not include non-political companies in the conjoint experiment, or the different nature of the ads available from different source types in the Ad Library in the real ads experiment. Despite these differences, however, these results largely confirm our findings in the conjoint experiment, and thereby provide a positive answer to RQ1.

¹²Meanwhile, there is no statistical difference between female and male respondents for any but the Power the Future ads. The indicator for female respondents is negative and statistically significant for Power the Future ($\beta = -0.13$, $se = 0.07$, $p < 0.05$).

¹³The indicator is (unexpectedly) positive for both the MSC experiment ($\beta = 0.15$, $se = 0.07$, $p = 0.03$) and for Colgate ($\beta = 0.09$, $se = 0.06$, $p = 0.12$).

5. Conclusion

Our results suggest that the message, messenger, and the viewer’s prior beliefs all impact the public’s determination of what qualifies as a political ad. In particular, either an ad sponsor affiliated regularly with political advocacy or a strong message is enough to affect respondents’ perceptions. The effect of the prior beliefs of respondents, while significant, is much smaller.

Our work sheds light on citizen preferences around online political advertising and also highlights how these differ from both existing and proposed operationalizations. In particular, definitions that rely solely on whether the ad is sponsored by a candidate or advocacy group clearly do not meet public expectations. Further, definitions focused only on elections also fail to meet citizen expectations. Both our experiments tested public perceptions of non-electoral ad content and found that when these ads are backed by a strongly political figure or simply contain strong messages, respondents consider them political. When we compare our results to platform policies, Facebook’s definition of political advertising, which includes issue advertising, appears to be closer to the public’s definition.

These results may aid policymakers in defining political ads in ways that align with public perceptions. While regulation of political advertising online does not seem imminent in the United States, all major platforms have specific policies around digital political ads. As with other areas of social media policy, it is in platforms’ interest to align their policies with user preferences, and they frequently use user studies (Goodrow, 2021; Horwitz, 2021) (as we do) to understand user perceptions.

Although these findings represent the most comprehensive exploration of this question to date, future work could expand upon our findings in several ways. First, we based our coding of message strength on whether the ad advocated for a candidate or issue. However, there is surely a continuum of message strength that we cannot fully explore here. Other important factors may include whether the ad mentions a politician, whether it asks viewers to engage in a political act, or whether the issue at hand is viewed as partisan.

Relatedly, we tried to restrict our study to the single issue area of energy/environmental policy. We did so to isolate the effects of other ad features (e.g., message strength, source, orientation), because this is an area of active political debate where many “non-traditional” organizations are active, and because environmental policy provided a middle ground between topics that are by their nature inherently political (e.g., voting rights) and topics that are largely absent from current political discourse (e.g., soap). This, of course, raises the question of which issue areas are considered more or less political in the first place and is an area for future investigation (but see Fitzgerald, 2013; Settle, 2018).

Finally, the questions we raise and our design strategy are focused on the U.S. setting. Platform rules and definitions already differ significantly across countries. Likewise, the very nature of the “political” may vary across contexts (Fitzgerald, 2013). More work is needed to understand these issues from a cross-national perspective.

6. Data Availability

The data, code, and any additional materials required to replicate all analyses in this article are available at the Journal of Experimental Political Science Dataverse within the Harvard Dataverse Network, at doi: 10.7910/DVN/DW6ZGZ (Edelson et al., 2025).

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Competing Interests None

Ethics Statement Data collection was approved by our Institutional Review Boards (IRBs) at New York University (#IRB-FY2020-4335) and Washington University in St. Louis (IRB #201905178). The studies related to this publication adhere to APSA's Principles and Guidance for Human Subjects Research.

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A. Additional Discussion of “Message Strength”

We operationalize “message strength” as the degree to which a message “more expressly advocates for a candidate or public policy.” We use this definition for several reasons. This definition is inspired by the content-based definitions from the platforms discussed in the main text. However, there are two additional reasons this definition of strength is appropriate. First, ads that explicitly call for government action (“tell congress ...”) or (“vote for Smith”) leave little ambiguity about their purpose. Express advocacy makes the ad’s intention—supporting a candidate or policy—clear to the audience. Second, express advocacy frames the rest of the content as political even when the underlying issue itself may not be obviously political (Chong and Druckman, 2007). Ads supporting environmental protection may simply seem more political when framed in the context of express advocacy.

To validate our definition, we conducted a supplementary survey with Amazon Mechanical Turk workers. Participants rated a set of political ads on a scale of how strongly they perceived each ad to advocate for a candidate or policy. This approach provides external validity to our definition, as it anchors it in public perception rather than purely theoretical constructs.

However, we acknowledge potential limitations in this validation. There is a risk of circularity in defining “strong” ads as those perceived as more political, given that our primary study also measures perceptions of political content. This concern echoes debates in political psychology about the interplay between message features and audience perceptions (Chong and Druckman, 2007).

To mitigate this somewhat, we also consider the structure of the ads. We observed that ads rated as “stronger” tend to be longer and contain more arguments. Therefore, we might more fairly think of this as a dual conceptualization of message strength: (1) the degree of explicit advocacy, as perceived by an external sample, and (2) the number of arguments presented. This approach balances subjective perceptions with a more objective, structural measure, reducing the risk of definitional circularity while capturing key aspects of persuasive political communication.

In summary, our definition of message strength, validated by public perception and reinforced by message structure, provides a solid (if imperfect) operationalization for analyzing the impact of these dimensions of ad content.

B. NORC Samples

The NORC1 survey was fielded July 1 –24, 2020 ($n= 1,006$). 56 people responded via phone and the rest online. 28.4% of recruited panelists completed the survey, and the panel itself had a weighted recruitment rate (AAPOR RR III) of 23.6%. This survey included the conjoint experiment and the real ads experiment. However, due to a programming error, the conjoint study from this wave was not usable and we do not discuss this data further.

The NORC2 survey was fielded to re-administer the NORC1 wave with the correct programming for the conjoint. This was fielded from March 31 to April 19, 2021 ($N = 1,013$). 28.4% of recruited panelists completed the survey and the weighted recruitment rate (AAPOR RR III) of the panel was 19.5 %. We analyze both the conjoint and real ads data from this survey.

Both surveys are re-weighted to national benchmarks, and all results in the main text and appendix are calculated using survey weights.

C. Study Instruments

In this section, we provide details on the studies and surveys we fielded in July 2020 and April 2021. In the main text, these are referred to as NORC1 and NORC2, respectively. Included here are the questions we asked, how we asked them, and how we recorded responses.

C.1 Conjoint analysis

The conjoint study was originally fielded as part of the NORC1 survey, but a programming error made the results unusable. It was refielded for the NORC2. All respondents were online respondents.

Prior attitudes and partisanship First, we administered two items to measure respondents' prior attitudes towards environmental regulation. The first item is from the American National Election Study. The second item relates specifically to global warming attitudes.

Item 1

Some people think the federal government needs to regulate business to protect the environment. They think that efforts to protect the environment will also create jobs. Let us say this is point 1 on a 1–7 scale.

Others think that the federal government should not regulate business to protect the environment. They think this regulation will not do much to help the environment and will cost us jobs. Let us say this is point 7 on a 1–7 scale.

Where would you place yourself on this scale?

1. – Regulate business to protect the environment and create jobs
- 2.
- 3.
- 4.
- 5.
- 6.
7. – No regulation because it will not work and will cost jobs

Item 2

If global warming is happening, is it caused mostly by human activity, mostly by natural causes, or equally by both?

1. Mostly by human activity
2. Mostly by natural causes
3. Equally by human activity and natural causes

Partisanship was assessed using the standard two-item branching party ID scale when the panel was constructed.

Which of these two ads is more political?**Donald J. Trump**

Sponsored • Paid for by TRUMP MAKE AMERICA GREAT AGAIN COMMITTEE

ID: 276052360225974

NATIONAL POLL: Pipelines are an essential part of American infrastructure. Without them, we are dangerously dependent on foreign oil. Luckily, politicians are working tirelessly to ensure that we all have affordable energy. Let Congress know that you support this industry!

**Sierra Club**

Sponsored • Paid for by SIERRA CLUB

ID: 233220784779105

New Mexico is leading the charge to tackle the climate crisis. Its commitment to nation-leading policies will #CutMethane pollution and set an example for other states to follow.

**Figure C.1.** Example pairwise comparison of constructed ads in the conjoint experiment.

Paired comparisons Respondents were next given the following prompt.

In this section, you will see several pairs of ads, and be asked to select which of the two is more political. Please be sure to look closely at both ads before making a choice. There are no right or wrong answers, simply select based on what feels right to you.

Respondents were then asked to compare two ads and choose the one that was most political. An example item is shown in Figure C.1. Each respondent evaluated eight pairs of ads.

C.2 Real ads experiment

The real ads experiment study was originally fielded as part of the NORC1 survey. The total number of respondents was $N = 1,006$, but we excluded 56 CATI (telephone) respondents because we relied on visual stimuli. Because of the programming error for the conjoint, the same set of experiments was fielded again as part of NORC2.

The real ads experiments occurred directly after the conjoint study. Respondents were given the following prompt:

In this next section, you will see several ads. Please note that these ads are images and not videos.

Respondents then evaluated eight ads, one chosen at random for each of the eight sources, and shown in a randomized order, with the prompt “How political is this ad?” Response options were: (5) Extremely political; (4) Very political; (3) Somewhat political; (2) Slightly political; (1) Not at all political. This has the advantage relative to paired comparisons that the response scale itself is somewhat interpretable, as scores above 3 are quite firmly viewed as being ‘political’ content. An example item is shown in Figure C.2.

The actual ads used for each of the eight sources are shown in Figures C.3 for candidates, C.4 for advocacy organizations, C.5 for politically active companies, and C.6 for less-politically active companies, respectively.

How political is this ad?



Nancy doesn't care if Americans suffer as long as she can be on vacation eating \$13 pints of ice cream from her \$24,000 freezer!

...



- Extremely political
- Very political
- Somewhat political
- Slightly political
- Not at all political

Figure C.2. Example item for the real ads analysis



(a) Strong/Biden



(b) Weak/Biden



(c) Strong/Trump

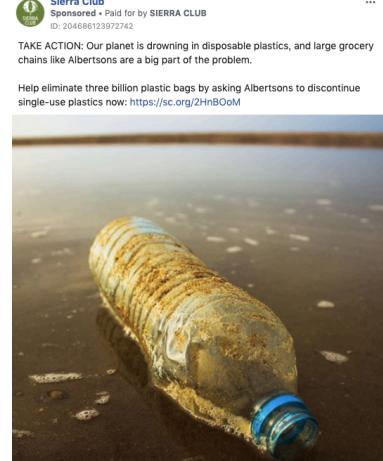


(d) Weak/Trump

Figure C.3. Real ads for candidates



(a) Strong/Sierra Club



(b) Weak/Sierra Club



(c) Strong/Power the Future



(d) Weak/Power the Future

Figure C.4. Real ads for advocacy organizations

 ExxonMobil
Sponsored • Paid for by Exxon Mobil Corporation
ID: 518949815645676

NATIONAL POLL: Approximately 840,000 American jobs are supported by offshore drilling and exploration. Do you support American offshore drilling?



(a) Strong/Exxon

 ExxonMobil
Sponsored
ID: 452326435699710

Chips and guacamole are an ideal combination. Kind of like natural gas and renewable energy. Watch how.

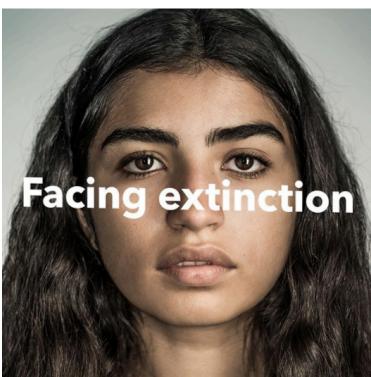


WHEN IT COMES TO
CLEANER ENERGY

(b) Weak/Exxon

 Patagonia
Sponsored • Paid for by Patagonia
ID: 2281430505299971

Tell Congress there is no room in government for climate deniers. Text CLIMATE to 71333



(c) Strong/Patagonia

 Patagonia with Rocky Mountain Wild
Sponsored • Paid for by PATAGONIA, INC.
ID: 92878517541205

Tune in to Patagonia grantee Rocky Mountain Wild's live discussion on May 11 to learn about the importance of gray wolves, threats to their survival, reintroduction initiatives and how you can help protect wolf populations near you.



(d) Weak/Patagonia

Figure C.5. Real ads for politically active companies



MSC DIRECT.COM
Expert Insights on What Phase 1 Deal & USMCA Mean for Manufacturers
Recent breakthroughs in trade tensions give U.S. manufacturers a measure of relief, experts share.

(a) Strong/MSC



MSCDIRECT.COM
Touchscreen Gloves Designed to Perform and Protect
Manufacturers asked for long-lasting, hardworking touchscreen gloves, so Ironclad went back to the lab.

(b) Weak/MSC



(c) Strong/Colgate



(d) Weak/Colgate

Figure C.6. Real ads for companies not engaged in political activities

D. Validating Coding of Message Strength

In this section, we provide the results from the message strength tests we conducted with convenience samples from Mechanical Turk to ensure that message strengths for our conjoint and real ads experiments were adequately calibrated. The conjoint messages we tested were identical to the messages in Figure 1 in the main text. The real ad image/message combinations were the same as those presented in Figures C.3, C.4, C.5, and C.6 with the advertisers' name (but not politician likeness) removed or obscured.

We emphasize that the Mechanical Turk samples were not used for our primary analyses, but rather to provide additional external validity for our survey instruments. Given this goal, we viewed Mechanical Turk as a relatively simple way to receive fast feedback from a diverse sample of respondents. While imperfect (Chmielewski and Kucker, 2020), the platform has and continues to be used widely in the social sciences for data collection (Berinsky, Huber and Lenz, 2012; Thomas and Clifford, 2017), and we viewed it as appropriate for this limited task.

D.1 Validation for the conjoint

In advance of implementing our conjoint analysis, we recruited 218 respondents who were U.S. adults in May 2020. We refer to this sample as AMT1. These respondents were asked to evaluate the political messages included in the conjoint experiment on a five-point Likert scale ranging from “not at all political” to “extremely political.” These messages were shown without additional context (e.g., ad source or image) and the respondents were asked “How political is this ad?”

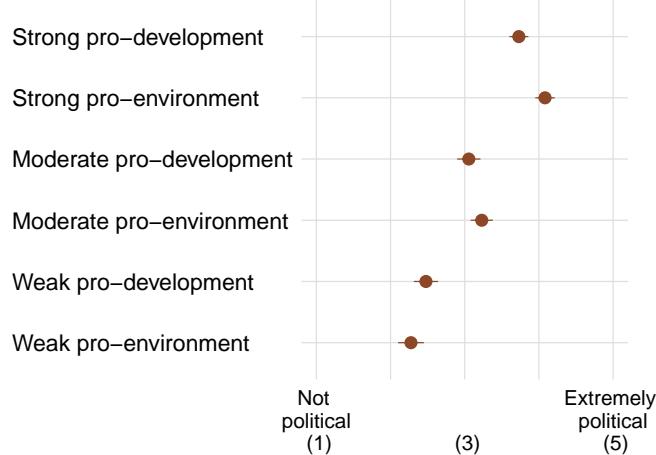


Figure D.1. Mean politicalness scores (with 95% confidence intervals) from the validation of coded message strength for the conjoint experiment. (Source: AMT1; $N = 218$)

Figure D.1 confirms our ordering of the stimuli with all strong messages scoring higher than moderate messages, which in turn scored higher than all weak messages. There are

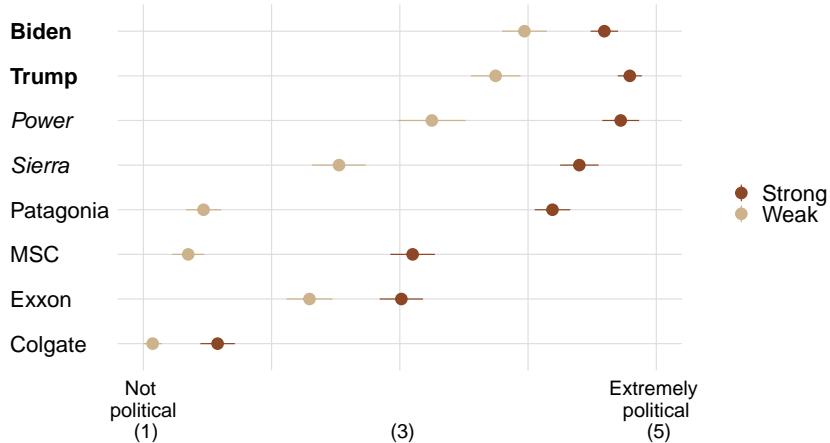


Figure D.2. Mean politicalness scores (with 95% confidence intervals) from the validation of coded ad message strength for the real ads experiment. Candidates are in **bold**, non-profits are in *italics*, and companies are in plain text. (Source: AMT2; $N = 302$)

some remaining differences in perceived politicalness within our assigned categories. For instance, the weak pro-environment message was given an average score of 2.28, while the weak pro-development message received an average score of 2.48. We view this as an inevitable consequence of our effort to build ads based on real-world content. Reassuringly, there is no consistent pattern (e.g., not all pro-environmental ads are viewed as less political than their counterpart) and the within-category differences are substantively modest.

D.2 Validation for real ads experiment

We conducted a separate validation survey ($N = 302$) of the messages' strength in the real ads experiment in July 2022. We refer to this sample as AMT2. Respondents were U.S. adults and were asked to evaluate the message in isolation from the sponsor (although the images were included). Ads were evaluated on a 5-point Likert scale. Respondents were asked to read each statement and then asked, "How political is this statement?"

On a five-point scale, average message strengths ranged from a high of 4.8 for Trump's strong message and a low of 1.07 for Colgate's weak message. Full validation results are presented in Figure D.2 and show that the selected "strong" messages are indeed always ranked higher than the selected "weak" messages from the same advertiser. However, there is clearly important heterogeneity across ad sources. For instance, we were unable to locate a "weak" Biden ad with a message scoring below even the most political Colgate ad.

E. Pre-registration

Data collection was approved by our Institutional Review Boards (IRBs) at New York University (#IRB-FY2020-4335) and Washington University in St. Louis (IRB #201905178). The conjoint and real ad experiments were preregistered (<https://osf.io/cnd9y/>). All analyses described in the preregistration were conducted and have been included in the main body of

the article or the appendix. The primary deviation from our study preregistration was the necessity to collect additional data. When the preregistration was submitted, the NORC1 survey had been completed, but not examined or even opened. Upon discovering that the conjoint task contained a programming error and had failed to be administered properly, we initiated a subsequent round of data collection (i.e., NORC2). The design and analysis remained unchanged.

Compared to the preregistration, this paper contains two wording changes to improve clarity. To begin, the pre-registered version of Hypothesis 2 is “More explicitly political messages will be rated as political, regardless of source.” We chose to reword the hypothesis and use the term “stronger” throughout the manuscript for expositional clarity although our pre-registered procedures remain unchanged.

We also changed our pre-registered Hypothesis 4 to a research question. We feel this terminology is a more accurate reflection of our pre-registered expectations and procedures, which did not specify precisely how the results in the conjoint and real-ad analysis were to be compared. These changes do not affect the interpretation of our study.

F. Additional Results for Conjoint Experiment

In this section, we report additional results for our conjoint experiment and analysis.

F.1 Unconditional effects

First, we report the full results associated with Figure 2 in the main text. These are reported in Table F.1.

F.2 Heterogeneous Effects

To test the extent to which prior beliefs impact the perception that an ad is political in our conjoint experiment, we calculate the average component interaction effect (ACIE), which estimates an attribute’s marginal effect conditional on the respondent’s position on environmental concerns versus energy development. Consistent with our preregistration, we also conduct an exploratory analysis as to whether there are also heterogeneous effects based on partisanship.

F.2.1 Prior environmental attitudes

The main results can be found in Figure 3 and Table F.2, and are discussed more fully in the main text. We also include an alternative specification where both interactions are included simultaneously. The results, shown in Table F.3, are essentially identical.

F.2.2 Partisanship

Next, we look to partisanship as a potential source of heterogeneous treatment effects. Table F.4 demonstrates that pro-development messages were perceived as more political by Democrats ($\beta = 0.04$, $se = 0.02$, $p = 0.03$) and as less political by Republicans, although the latter ($\beta = -0.04$, $se = 0.03$, $p = 0.18$) does not meet traditional measures of significance.

A similar pattern emerges when we turn to the source of the message with pro-development sources being evaluated as more political among Democrats ($\beta = 0.04$, $se = 0.02$, $p = 0.03$) and as less political among Republicans ($\beta = -0.07$, $se = 0.02$, $p = 0.002$). This is largely consistent with the results divided by prior attitudes and supports Hypothesis 3.

Table F.1. Effects of advertisement's attributes on the perception of ad's politicalness (Average Marginal Component Effects). Estimates of the effects of the randomly assigned advertisement attributes on perceived politicalness of advertisement in a paired conjoint experiment. Corresponds to Figure 2. (Source: Sample NORC2; $N = 1,013$)

	Average marginal component effect
<i>Images (Baseline: Yellow Flowers)</i>	
Desert	-0.005 (0.02)
Oil rig	0.09*** (0.02)
Pipeline	0.08*** (0.02)
Solar wind	0.07** (0.02)
<i>Message orientation (Baseline: Pro-environment)</i>	
Pro-development	0.01 (0.02)
<i>Message Strength (Baseline: Weak)</i>	
Moderate	0.13*** (0.02)
Strong	0.37*** (0.02)
<i>Source orientation (Baseline: Pro-environment)</i>	
Pro-development	-0.02 (0.01)
<i>Source type (Baseline: Corporation)</i>	
Candidate	0.09*** (0.02)
Organization	0.004 (0.02)
N	1013
Total Responses	8008

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table F.2. Effect of pro-development message and source on perception of ad's politicalness, conditioned on pro-development prior orientation. Average Component Interaction Effect (ACIE) of pro-development prior orientation on perceived politicalness of advertisements in a paired conjoint experiment. Dependent variable is a dummy variable indicating whether an ad was selected as more political when presented with two ads. The first column estimates ACIE for the message and the second column estimates the ACIE for the source. Corresponds to Figure 3. (Source: NORC2; $N = 1,013$)

	Message × Priors	Source × Priors
Images (Baseline: Yellow Flowers)		
Desert	-0.005 (0.02)	-0.005 (0.02)
Oil rig	0.09*** (0.02)	0.09*** (0.02)
Pipeline	0.08*** (0.02)	0.08*** (0.02)
Solar wind	0.07** (0.02)	0.07** (0.02)
Message orientation (Baseline: Pro-environment)		
Pro-development	0.01 (0.02)	0.01 (0.02)
Message Strength (Baseline: Weak)		
Moderate	0.13*** (0.02)	0.13*** (0.02)
Strong	0.37*** (0.02)	0.37*** (0.02)
Source orientation (Baseline: Pro-environment)		
Pro-development	-0.02 (0.01)	-0.02 (0.01)
Source type (Baseline: Corporation)		
Candidate	0.09*** (0.02)	0.09*** (0.02)
Organization	0.004 (0.02)	0.004 (0.02)
Interactions		
Pro-development message conditioned on prefers development	-0.05* (0.03)	
Pro-development message conditioned on prefers environment	0.05* (0.02)	
Pro-development source conditioned on prefers development		-0.04* (0.02)
Pro-development source conditioned on prefers environment		0.005 (0.02)
N	1013	1013
Total Responses	7945	7945

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table F.3. Effect of pro-development message and source on perception of ad's politicalness, conditioned on pro-development prior orientation. Average Component Interaction Effect (ACIE) of pro-development prior orientation on perceived politicalness of advertisements in a paired conjoint experiment. Dependent variable is a dummy variable indicating whether an ad was selected as more political when presented with two ads. (Source: NORC2; $N = 1,013$)

	Average marginal component effect
<i>Images (Baseline: Yellow Flowers)</i>	
Desert	
Desert	-0.005 (0.03)
Oil rig	0.09*** (0.02)
Pipeline	0.08*** (0.02)
Solar wind	0.07** (0.02)
<i>Message orientation (Baseline: Pro-environment)</i>	
Pro-development	0.006 (0.02)
<i>Message Strength (Baseline: Weak)</i>	
Moderate	0.13*** (0.02)
Strong	0.37*** (0.02)
<i>Source orientation (Baseline: Pro-environment)</i>	
Pro-development	-0.02 (0.01)
<i>Source type (Baseline: Corporation)</i>	
Candidate	0.09*** (0.02)
Organization	0.004 (0.02)
<i>Interactions</i>	
Pro-development message conditioned on prefers development	-0.05* (0.03)
Pro-development message conditioned on prefers environment	0.05* (0.02)
Pro-development source conditioned on prefers development	-0.04* (0.02)
Pro-development source conditioned on prefers environment	0.006 (0.02)
N	1013
Total Responses	7945

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table F.4. Effect of pro-development message and source on perception of ad's politicalness conditioned on party identification (ID). Average Component Interaction Effect of party ID on perceived politicalness of advertisements. Dependent variable is a dummy variable indicating whether an ad was selected as more political when presented with two ads. The first column estimates ACIE for the message and the second column estimates the ACIE for the source. (Source: Sample NORC2; $N = 1,013$)

	Message × Party ID	Source × Party ID
Constant	0.23*** (0.02)	0.22*** (0.02)
Desert	-0.005 (0.02)	-0.05 (0.02)
Oil rig	0.09*** (0.02)	0.09*** (0.02)
Pipeline	0.08*** (0.02)	0.08*** (0.02)
Solar wind	0.07** (0.02)	0.07** (0.02)
Message orientation: Pro-development	0.01 (0.02)	0.01 (0.02)
Moderate	0.13*** (0.02)	0.13*** (0.02)
Strong	0.37*** (0.02)	0.37*** (0.02)
Source orientation: Pro-development	-0.02 (0.01)	-0.02 (0.01)
Candidate	0.09*** (0.02)	0.09*** (0.02)
Organization	0.004 (0.02)	0.004 (0.02)
Pro-development message conditioned on Democrat	0.04* (0.02)	
Pro-development message conditioned on Republican	-0.04 (0.03)	
Pro-development source orientation conditioned on Democrat		0.04* (0.02)
Pro-development source conditioned on Republican		-0.07** (0.02)
N	1013	1013
Total Responses	7111	7111

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

F.2.3 Gender

Finally, we investigate gender as a potential source of heterogeneous treatment effects. Figure F.1 shows no significant statistical evidence of differences in perceived politicalness between female and male respondents. See Table F.5 for full results.

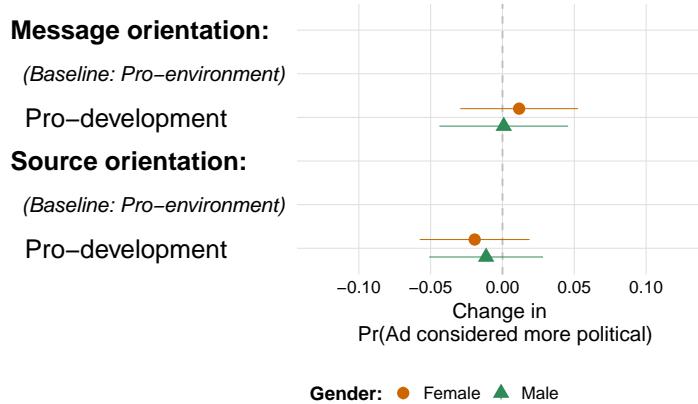


Figure F.1. Effect of pro-development message and source on perception of ad's politicalness, conditioned on respondents' gender. Estimates are Average Component Interaction Effects in a paired conjoint experiment. The dependent variable is a dummy variable indicating whether an ad was selected as more political when presented with two ads. (Source: Sample NORC2; $N = 1,013$)

Table F.5. Effect of pro-development message and source on perception of ad's politicalness conditioned on gender. Average Component Interaction Effect of gender on perceived politicalness of advertisements. The dependent variable is a dummy variable indicating whether an ad was selected as more political when presented with two ads. The first column estimates ACIE for the message and the second column estimates the ACIE for the source. (Source: Sample NORC2; $N = 1,013$)

	Message \times Gender	Source \times Gender
Constant	0.23*** (0.02)	0.22*** (0.02)
Desert	-0.005 (0.02)	-0.05 (0.02)
Oil rig	0.09*** (0.02)	0.09*** (0.02)
Pipeline	0.08*** (0.02)	0.08*** (0.02)
Solar wind	0.07** (0.02)	0.07** (0.02)
Message orientation: Pro-development	0.01 (0.02)	0.01 (0.02)
Moderate	0.13*** (0.02)	0.13*** (0.02)
Strong	0.37*** (0.02)	0.37*** (0.02)
Source orientation: Pro-development	-0.02 (0.01)	-0.02 (0.01)
Candidate	0.09*** (0.02)	0.09*** (0.02)
Organization	0.004 (0.02)	0.004 (0.02)
Pro-development message conditioned on Male	0.0009 (0.02)	
Pro-development message conditioned on Female	0.01 (0.02)	
Pro-development source orientation conditioned on Male		-0.01 (0.02)
Pro-development source conditioned on Female		-0.02 (0.02)
N	1013	1013
Total Responses	7111	7111

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

G. Additional Results for Real Ads Experiment

In this section, we report additional results for our real ads experiment and analysis. We begin by providing basic summary statistics for ad evaluations.

G.1 Summary Statistics for Ads

The full result of summary statistics is reported in Table G.1.

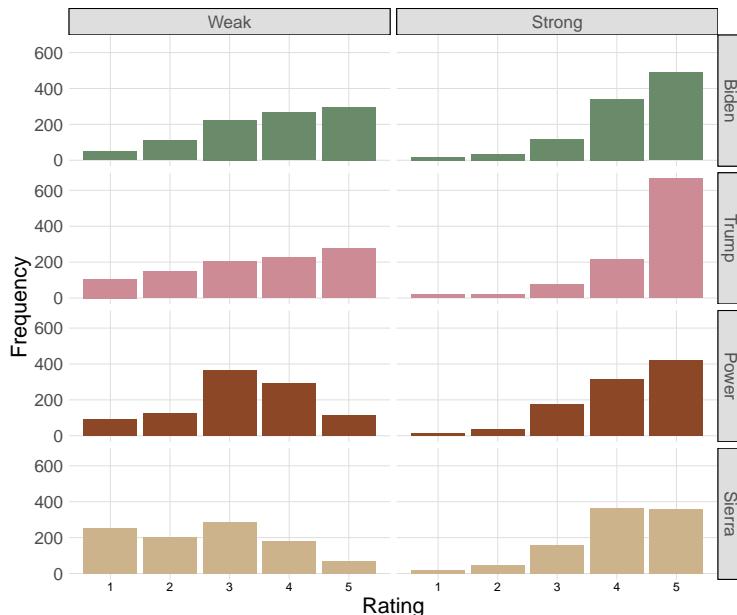


Figure G.1. Full response distributions of politicalness for strong and weak-message ads from candidates and organizations in the real ads experiment. Values on a scale from (1) not at all political to (5) very political. Each participant saw either an ad with strong or weak message from each ad source. (Source: NORC1 and NORC2; $N = 2,019$)

Table G.1. Summary statistics for the real ads experiment by condition

Estimate	N	Lower CI	Upper CI	Standard Error	Ad Source	Message Strength
3.63	949	3.61	3.66	0.04	Biden	Weak
1.25	1023	1.25	1.25	0.02	Colgate	Weak
2.48	976	2.46	2.50	0.03	Exxon	Weak
1.78	1000	1.76	1.80	0.03	MSC	Weak
1.89	974	1.87	1.91	0.03	Patagonia	Weak
3.23	986	3.21	3.24	0.03	Power	Weak
2.62	994	2.59	2.65	0.04	Sierra	Weak
3.40	956	3.36	3.43	0.04	Trump	Weak
4.23	1001	4.22	4.23	0.03	Biden	Strong
1.84	929	1.82	1.87	0.03	Colgate	Strong
3.12	975	3.10	3.13	0.03	Exxon	Strong
3.10	945	3.08	3.12	0.04	MSC	Strong
3.78	977	3.77	3.80	0.03	Patagonia	Strong
4.08	960	4.07	4.09	0.03	Power	Strong
4.02	951	4.01	4.02	0.03	Sierra	Strong
4.45	993	4.45	4.45	0.03	Trump	Strong

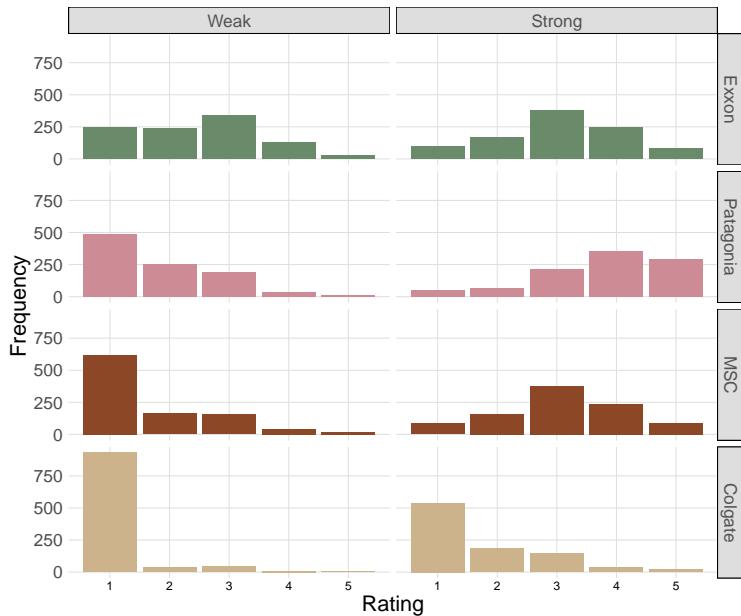


Figure G.2. Full response distributions for companies. Distribution of respondents' rating of the politicalness of advertisements. Values on a scale from (1) not at all political to (5) very political. The figure displays advertisement from company sources. For each of the eight sources (Trump, Exxon, etc.), each participant saw either an ad with a strong or weak message. (Source: NORC1 and NORC2; $N = 2,019$)

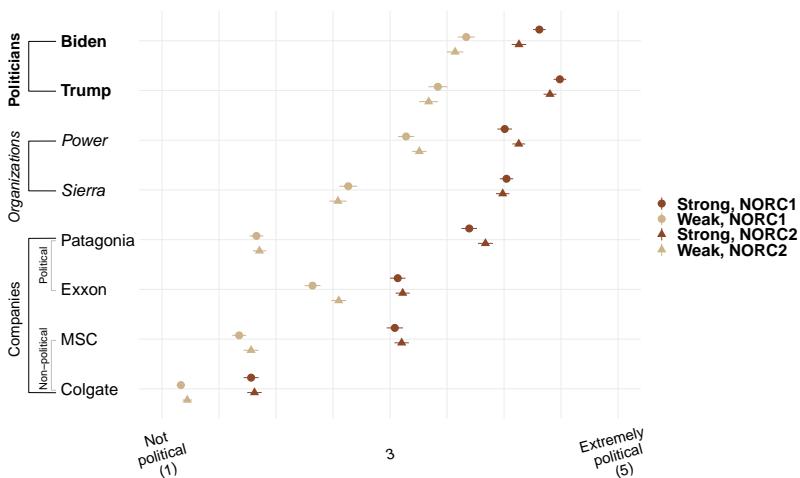


Figure G.3. Disaggregated averages of perceived politicalness of ads by source and message strength. Respondents' average rating of the politicalness of advertisements. For each of the eight ad sources (Trump, Exxon, etc.), each participant saw either an ad with a strong or weak message. Point estimates represent weighted means (with 95% confidence intervals) from the NORC1 and NORC2 surveys. (Source: NORC1 and NORC2; $N = 2,019$)

G.2 Effects of message strength, party and gender

Here we report the full table of results for the between-subjects experiments discussed in the main text. First, we report the full tables of results. The main treatment effects are shown in Table G.2. The results including an indicator for party identity and gender are shown in Table G.3 and Table G.4, respectively.

G.3 Testing Difference Across Source Types

Here we simply conduct difference in mean tests comparing the perceived politicalness of ads from different source categories. These results are shown in Table G.5 and simply compare the average rating for ads in each of the four categories. The results show that respondents indeed rated the political content of ads from different sources (candidates, advocacy groups, political businesses, and non-political businesses) as distinct.

G.4 Complete replication of the conjoint

Finally, we create a single model for the entire real-ads experiment in a way that more closely replicates the conjoint study. Since the conjoint did not have non-political ad sponsors, we excluded these ads from this analysis.

Specifically, we estimate two linear regressions pooling across sources. Let respondent i evaluate the ad from source j and provide evaluation y_{ij} . We estimate the model

$$y_{ij} = \alpha_i + \beta_1 \text{Cand}_j + \beta_2 \text{Org}_j + \beta_3 \text{Strong}_{ij} + \beta_4 \text{Pro-dev}_j + \epsilon_{ij}$$

where α_i is the respondent-level fixed effect, and the β coefficients are the effect of the respective ad feature relative to the baseline. We use clustered standard errors at the respondent level to account for correlated errors within respondents. To estimate heterogeneous effects, we rely on partisanship instead of environmental policy positions since some sources (e.g., Biden) did not have appropriate ads in this issue area.

The results (Model 1 in Table G.6) show the same general pattern discussed above; both the content (message strength) and source matter for the perceived politicalness of an ad. However, we can see more explicitly that in this less controlled setting, the source effects on politicalness ratings are similar to the message strength treatments. The coefficient for candidate ads (with ads from Exxon and Patagonia again serving as the baseline) is $\beta = 1.13$ ($se = 0.04$, $p < 0.001$), while the aggregate effect of the strong ads treatment is $\beta = 1.10$ ($se = 0.03$, $p < 0.001$). Thus, the evidence suggests that the source type (company, advocacy organization, or candidate) does have a significant correlation with politicalness ratings given more natural content. Finally, the effect for ads with a pro-development orientation is ($\beta = 0.11$, $se = 0.03$, $p < 0.01$).

The results in Figure 4 suggest that the effect of the ad message on perceived politicalness is generally much smaller for political candidates than for organizations or companies. We, therefore, conduct an exploratory analysis where we interact the ad strength variable with the source type (adding Strong ads \times candidate and Strong ads \times organization interaction terms to Model 1). This allows us to explicitly test whether the ad strength treatment differs across source types. This result is reported as Model 2 in Table G.6. This shows that the effects are significantly smaller for candidates ($\beta = -0.49$, $se = 0.07$, $p < 0.001$) relative to the baseline of companies. Again, we cannot interpret these naively because the “treatments” differ in important ways across sources. However, the results are consistent with the idea that for some sources (e.g., candidates), all content is viewed as political, regardless of the strength of the message.

Table G.2. Effect of exposure to strong (relative to weak) ads on perceived politicalness in the *real ads* experiment. Dependent variable is on a five point scale ranging from not political (1) to extremely political (5). For each of the eight ad sources (Trump, Exxon, etc.), each participant saw either an ad that was strong or weak in message strength. Point estimates based on regression estimators (standard errors in parentheses). Values represent the change in perceived politicalness on a five-point Likert scale. (Source: NORC1 and NORC2 pooled)

	Politicians		Organizations		Political companies		Non-political companies	
	Biden	Trump	Power	Sierra	Exxon	Patagonia	MSC	Colgate
Constant	3.62*** (0.05)	3.38*** (0.06)	3.20*** (0.05)	2.59*** (0.05)	2.44*** (0.04)	1.84*** (0.04)	1.73*** (0.04)	1.19*** (0.03)
Strong message (Baseline: Weak)	0.60*** (0.07)	1.07*** (0.07)	0.87*** (0.06)	1.42*** (0.07)	0.65*** (0.06)	1.93*** (0.06)	1.34*** (0.06)	0.60*** (0.05)
R ²	0.07	0.18	0.15	0.29	0.08	0.47	0.29	0.11
N	1950	1949	1946	1945	1951	1951	1945	1952

*** * $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table G.3. Effect of exposure to ads with strong (relative to weak) message strength on perceived politicalness in the *real ads* experiment. Control included for party identification. Dependent variable is on a five-point scale ranging from not political (1) to extremely political (5). For each of the eight ad sources (Trump, Exxon, etc.), each participant saw either an ad with strong or weak message strength. Point estimates based on regression estimators (standard errors in parentheses). Values represent the change in perceived politicalness moving from weak message to strong message. (Source: NORC1 and NORC2 pooled).

	Politicians		Organizations		Political company		Non-political company	
	Biden ads	Trump ads	Power ads	Sierra ads	Exxon ads	Patagonia ads	MSC ads	Colgate ads
(Intercept)	3.39***	3.62***	3.39***	2.51***	2.52***	1.74***	1.65***	1.15***
Strong ad	(0.07)	(0.07)	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)	(0.04)
Republican	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)
R ²	0.11	0.23	0.16	0.30	0.09	0.47	0.29	0.11
Num. obs.	1666	1665	1661	1660	1668	1665	1662	1668
N Clusters	1666	1665	1661	1660	1668	1665	1662	1668

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table G.4. Effect of exposure to ads with strong (relative to weak) message strength on perceived politicalness in the *real ads* experiment. Control included for gender. The dependent variable is on a five-point scale ranging from not political (1) to extremely political (5). For each of the eight ad sources (Trump, Exxon, etc.), each participant saw either an ad with strong or weak message strength. Point estimates based on regression estimators (standard errors in parentheses). Values represent the change in perceived politicalness moving from weak message to strong message. (Source: NORC1 and NORC2 pooled).

	Politicians		Organizations			Political company		Non-political company	
	Biden ads	Trump ads	Power ads	Sierra ads	Exxon ads	Patagonia ads	MSC ads	Colgate ads	
(Intercept)	3.67*** (0.07)	3.41*** (0.07)	3.27*** (0.06)	2.64*** (0.06)	2.47*** (0.05)	1.89*** (0.05)	1.71*** (0.06)	1.21*** (0.04)	
Strong ad	0.60*** (0.07)	1.06*** (0.07)	0.87*** (0.06)	1.42*** (0.07)	0.65*** (0.06)	1.93*** (0.06)	1.34*** (0.06)	0.60*** (0.05)	
Female	-0.11 (0.07)	-0.05 (0.07)	-0.13* (0.06)	-0.10 (0.07)	-0.05 (0.06)	-0.09 (0.06)	0.04 (0.06)	-0.03 (0.05)	
R ²	0.07	0.18	0.15	0.29	0.08	0.47	0.29	0.11	
Num. obs.	1950	1949	1946	1945	1951	1951	1945	1952	
N Clusters			1949						

*** p < 0.001; ** p < 0.01; * p < 0.05

Table G.5. One-tailed T-Test results for differences in mean politicalness score between ad source categories

Ad Source Type	Comparison Source Type	T-Test Result	p-value
Candidates	Advocacy Groups	$t(1952) = 18.5$	< 0.01
Advocacy Groups	Political Businesses	$t(1953) = 29.1$	< 0.01
Political Businesses	Non-political Businesses	$t(1955) = 35.8$	< 0.01

Table G.6. Pooled model for the real ads experiment. Ads from less politically active companies excluded to better reflect the design of the conjoint analysis. Dependent variable is on a five-point scale ranging from not political (1) to extremely political (5). For each of the six ad categories (excluding non-political companies), each participant was randomly shown an ad with a strong or weak message. Point estimates based on regression estimators with clustered standard errors (standard errors in parentheses). Model 1 is the baseline model, Model 2 explores heterogeneous effects of ad strength by ad source type, Model 3 explores heterogeneous effects of ad orientation by respondent partisanship, and Model 4 explores heterogeneous effects of ad orientation by respondent gender. All models include respondent-level fixed effects.
(Source: NORC1 and NORC2 pooled)

	Model 1	Model 2	Model 3	Model 4
Candidate	1.13*** (0.04)	1.35*** (0.06)	1.13*** (0.04)	1.13*** (0.04)
Organization	0.68*** (0.03)	0.74*** (0.04)	0.70*** (0.03)	0.68*** (0.03)
Strong ads	1.10*** (0.03)	1.29*** (0.05)	1.11*** (0.04)	1.10*** (0.03)
Pro-development source	0.11** (0.03)	0.11** (0.03)	0.39*** (0.04)	0.09 (0.05)
Strong ads × candidate		-0.45*** (0.07)		
Strong ads × organization		-0.11 (0.06)		
Pro-development source × Republican			-0.59*** (0.07)	
Pro-development source × gender				0.03 (0.06)
R ²	0.51	0.51	0.52	0.51
Adj. R ²				
N Respondents	1962	1962	1675	1962
N	11692	11692	9985	11692

Model 3 in Table G.6 provides evidence consistent with H3 that respondents tend to view content they disagree with as being more political. Here, we include an interaction between an indicator for pro-development/Republican ads and Republican identifiers and exclude independents. (Recall that all of these models include respondent-level fixed effects, thus the base term for Republican identifiers is excluded due to co-linearity.) The results show that Democrats identify ads from pro-development sources as being more political. For Republicans, the effect of a pro-development orientation is -0.2.¹ Here again, we see a significant but relatively modest effect for source orientation on perceptions of politicalness.

Finally, Model 4 in Table G.6 examines the interaction between gender and treatment effect. We do not find evidence supporting gender heterogeneous treatment effects by respondents' gender.

¹This is calculated by adding the pro-development coefficient (0.39) with the interaction term (-0.59) in Model 3. For Democrats, the effect is simply 0.39, which is the 'pro-development' coefficient in Model 3.