The Small Effects of Political Advertising are Small Regardless of Context, Message, Sender, or Receiver: Evidence from 59 Real-time Randomized Experiments

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Evidence across social science indicates that average effects of persuasive messages are small. One commonly-offered explanation for these small effects is heterogeneity: persuasion may only work well in specific circumstances. To evaluate heterogeneity, we repeated an experiment weekly in real time using 2016 U.S. presidential election campaign advertisements. We tested 49 political advertisements in 59 unique experiments on 34,000 people. We investigate heterogeneous effects by sender (candidates or groups), receiver (subject partisanship), content (attack or promotional), and context (battleground versus non-battleground; primary versus general election; early versus late). We find small average effects on candidate favorability and vote. These small effects, however, do not mask substantial heterogeneity even where theory from political science suggests we should. During the primary and general election, in

**battleground states, for Democrats, Republicans, and Independents — effects
are similarly small. Heterogeneity with large offsetting effects is not the source
of small average effects.**

Efforts by one actor to influence the choices of others pervade the social world. Political campaigns aim to persuade voters, firms aim to persuade consumers, and public service groups and governments aim to persuade citizens. Because persuasion is attempted in so many settings, the conditions for effective persuasion have been studied by scholars across many fields. The resulting set of theories and evidence points in the same direction: study by study, social scientists have reported that persuasive influence tends to be small on average but have theorized that those small average effects mask large differences in responsiveness.

While the details vary across disciplines, persuasion is thought to require a specific mix of message content and environmental context along with a special match of features of the sender with features of the receiver. In other words, persuasion is presumed to be conditional on who says what to whom and when and getting this recipe right is thought to be critical for changing minds.

Across fields, scholars have elaborated different elements of this mixture. Psychologists laid out an initial model of attitude change (*1*) and demonstrated how characteristics of people (*2*) and pathways of thought (*3*) affect acceptance of messages and, therefore, persuasion. Contemporary work in psychology has taken context seriously, suggesting that culture, habit, social networks, and the framing of messages affect the magnitude of persuasion (*4, 5*). In marketing and consumer research, persuasive success has been shown to increase with shared social or ethnic identities among senders and receivers (*6*) and to depend upon receiver experience with promotional appeals (*7*) and sender level of expertise (*8*). Work in management science shows similar heterogeneity: successful transfers of information within firms depends upon the capacity of the recipient to absorb information, the ambiguity of the causal process at issue, and the

personal relations between the speaker and the audience (*9*). In economics, the magnitude and effectiveness of persuasion is argued to vary with the preferences of the speaker and the prior beliefs of the audience (*10*).

In our discipline of political science, a large body of work focuses on how much campaigns affect voter preferences (*11–21*). Recent work has focused on the relatively small size of the persuasive effects of campaign ads and the rapid decay of these effects (*22, 23*). Persuasion is generally believed to vary by characteristics of the messages such as advertising content (*17, 24, 25*), identity of the sender (*26, 27*), differences across receivers like partisanship or knowledge about the topic at hand (*28*), and contextual factors including whether competing information is present (*28, 29*).

Synthesizing results across fields into a coherent theory of heterogeneity in persuasive effects is frustrated by difficult-to-overcome challenges of research design. For example, in order to understand the relative importance of message content, context, sender, and receiver, we need a design that allows each of the features to vary independently while holding others constant at the same levels each time. Typically, experiments vary one feature and hold all others constant at an idiosyncratic level in a single setting. When subsequent experiments turn to investigate a different attribute, the other features of the experiment become fixed at their own idiosyncratic levels — most likely *different* levels than in previous experiments. Further, owing to the decades that have been spent researching persuasion, not only do studies investigate different targets and types of persuasion while holding other factors constant at varying levels, they do so with different designs, instruments, and sampling methods. Aggregating results from this set of studies — executed in largely uncoordinated ways — is challenging. Adding to the difficulty of forming a coherent theory of heterogeneity is the fact that publication and career incentives reward evidence of difference more highly than evidence of similarity (*30, 31*). As a result, the empirical record may overemphasize findings of treatment effect heterogeneity.

We have designed a series of unique tests spanning 8 months in which the sample, design, instrument, and analysis are all held constant. We measure the effects of 49 unique presidential advertisements made by professional ad-makers during the 2016 presidential election among large, nationally representative samples using randomized experiments (we tested some of the 49 unique ads in multiple weeks). This design allows us to examine possible differences in persuasion related to message, context, sender, and receiver.

The summary finding from our study is that, at least in hard-fought campaigns for the presidency, substantial heterogeneities in the size of treatment effects are *not* hiding behind small average effects. Attack and promotional ads appear to work similarly well. Effects are not substantially different depending on which campaign produced the ads or in what electoral context they were presented. Subjects living in different states or who hold different partisan attachments appear to respond to the advertisements by similar degrees.

While we do not claim absolute homogeneity in treatment effects, our estimates of heterogeneity are substantively small. We estimate an average treatment effect of presidential advertising on candidate favorability of 0.05 scale points on a five point scale, with a standard deviation across experiments of 0.07 scale points. On vote choice, the average effect is 0.7 percentage points with a standard deviation of 2 points. Advertising effects do vary around small average effects, but the distribution of advertising effects in our experiments excludes large persuasive effects. These results suggest scholars may want to revisit previous findings and re-evaluate whether current beliefs about heterogeneity in persuasion rest on heterogeneity of studies and designs rather than an essential conditionality of persuasion.

Materials and Methods Our experiments were fielded from March to November (Election Day) in 2016, covering the primary elections of both major American political parties and the general election. Each week for 29 (not always consecutive) weeks, a representative sample of

Americans was divided at random into groups and assigned to watch campaign advertisements or a placebo advertisement before answering a short survey. This sample period strengthened our design because it covered both the primary election, when voters lacked information about candidates or strong partisan cues, and the general election when information about the two major party candidates was more easily available.

Our subjects were recruited by YouGov, who furnished samples of exactly 1,000 (or 2,000, depending on the week) complete responses. Participants were part of YouGov's ongoing survey research panel and were invited to this particular survey after agreeing to take surveys for YouGov generally. This process was approved by the University of California, Los Angeles Institutional Review Board (IRB#16-000691). Some subjects (42%, on average) started the survey but did not finish, which can cause bias away from our inferential target, the U.S. population average treatment effect (PATE). We rely on YouGov's post-stratification weights to address the problem that some kinds of people are more likely to finish the survey than others. A second source of bias is the possibility that our treatments *caused* subjects to stop taking the survey. Using information on the full set of respondents who began each survey we find no evidence of differential attrition by treatment condition. Specifically, we conduct separate χ^2 tests of the dependence between response and treatment assignment within each week of the study. Of the 29 tests, three return unadjusted p-values that are statistically significant. When we adjust for multiple comparisons using the Holm or Benjamini-Hochberg corrections (*32, 33*), none of the tests remain significant. Although this analysis does not prove the missingness is independent of assignment, we proceed under that assumption.

We chose treatment ads from the set of ads released each week during the 2016 campaign by candidates, parties, or groups. We picked ads to test on the basis of real-time ad-buy data from Kantar Media and news coverage of each week's most important ads. See the supplemental materials for more information about the treatments, including transcripts, date of testing, and

the ads themselves. In total, we tested 30 ads attacking Republican candidate Donald Trump, 11 attacking Democratic candidate Hillary Clinton, eight promoting Clinton, and three promoting Trump. The remainder were promotional advertisements for other primary candidates fielded prior to the general election.

The design of each weekly experiment was consistent. During the primaries (March 14th to June 6th), 1,000 respondents were randomly assigned to one of four conditions: watch either one of two campaign ads, both, or neither. Subjects who saw neither treatment ad were shown a placebo ad for car insurance. Subjects who saw two ads sometimes saw pairs of ads with competing messages and sometimes saw ads with reinforcing messages. We also showed some ads in multiple weeks to estimate whether the effect of the same advertisement varies with changing context. In analyses reported in the supplemental materials, we find no significant differences in effectiveness over time: the same ad works approximately equally as well regardless of when during the campaign we test it.

During the period surrounding the conventions before the traditional start of the general election campaign (June 20th to August 15th), 2,000 respondents were recruited every other week. These subjects could be assigned to control or one of the two video conditions, but we removed the "both" condition during this period. In the fall, we returned to 1,000 respondents a week and re-introduced the "both" condition on September 26th. We used Bernoulli random assignment to allocate subjects to treatment conditions with equal probabilities. We are confident treatments were delivered as intended: tiny fractions of the control groups claimed to have seen campaign ads (2 to 4%) compared with large majorities of the treatment groups (92 to 95%).

After watching the videos, subjects took a brief survey, the full text of which is available in the supplemental materials. Here we focus on two main outcome variables: subjects' favorability rating of the target candidates on five point scale and their general election vote intention. Favorability and vote intention can be thought of as two points along a spectrum of candidate

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evaluation running from more to less pliable. If ads are effective at all, they should move candidate favorability before choice (*34*). Vote choice (as measured by vote intention) is the more consequential political outcome but may be more difficult to change via persuasive appeal. Our design allows us to measure whether ads move one, the other, both, or neither of these outcomes.

We estimate treatment effects separately for each week's experiment using ordinary least squares (OLS) and robust standard errors. OLS is a consistent estimator of the ATE under our design (*35*). In order to increase precision (*36*), we control for the following covariates measured prior to treatment: seven-point party identification, five-point ideological self-placement, voter registration status, gender, age, race, income, education, region and a pre-treatment question about whether the country was on the right track.

The treatment effects in any single 1,000-person study are estimated with a fair amount of sampling variability but pooling across weeks via random-effects meta-analysis allows us to sharpen the estimates considerably. Random-effects meta-analysis allows us to estimate directly the extent of heterogeneity with respect to features of the persuasive environment.

Main Results   In Figure 1, we plot average treatment effect estimates (and 95% confidence intervals) for each of our 59 experimental comparisons for our two dependent variables. The top facet presents ATE estimates on candidate favorability, the bottom on vote intention. The x-axis represents calendar time to show when each experiment happened over the course of the campaign. Each point is one ATE estimated by OLS. To the right of each time-series we plot the meta-analytic estimate. On average, the ads moved target candidate favorability 0.049 scale points (1-5 scale) in the "correct" direction – the analysis is scaled such that the treatment effect of promotional ads is in the direction of favorability and of attack ads the direction opposite. This estimate, though small, is statistically significant owing to the large size of our study. The effect on target candidate vote choice is also small at 0.7 percentage points, but is not statistically
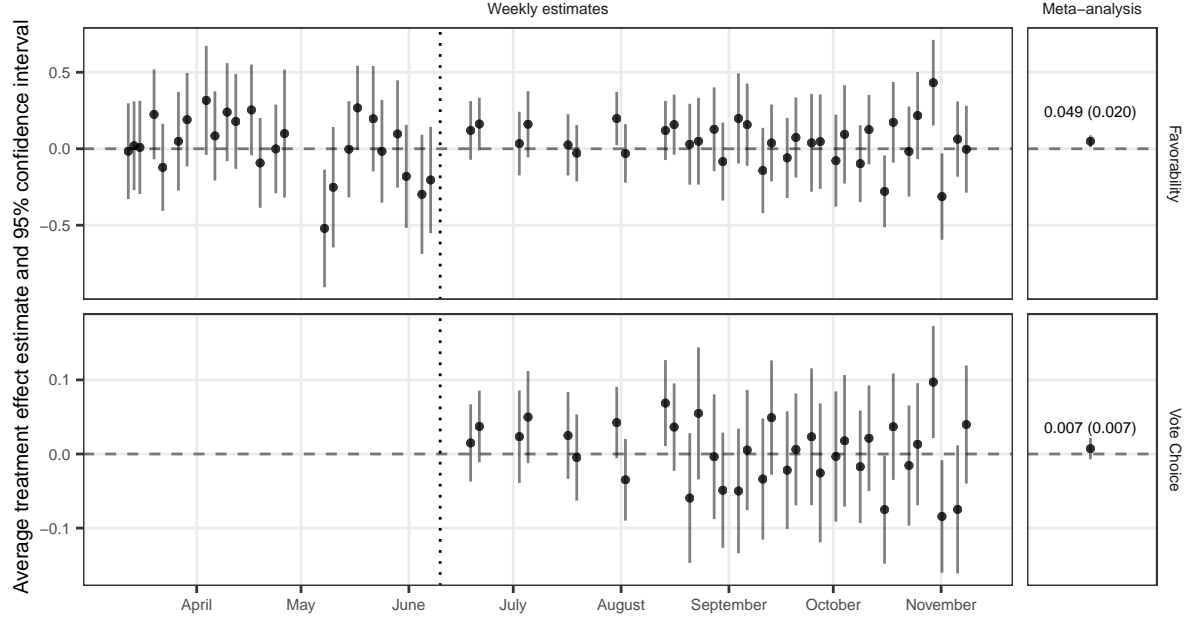
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significant.

Figure 1: Average treatment effect of advertising on candidate vote choice and favorability

Figure 1 also provides a first indication of our main finding of low treatment effect hetero-geneity. While ATE estimates vary from ad to ad, this variability is no greater than what would be expected from sampling variability. Using formal tests, we fail to reject the null of homo-geneity in both cases (favorability: $p = 0.09$, vote choice: $p = 0.07$). As we discuss below, other statistical tests for large (non-sampling) variability generate similarly weak evidence.

We pause here to reflect on what Figure 1 would mean if experimental results were subject to a statistical significance publication filter. Imagine that Figure 1 represents the sampling dis-tribution of persuasive treatment effects. If only statistically significant results were published, we would be left with one large negative result (nearly -0.5 points on favorability in early May), one large positive result towards the end of the campaign in October (more than +0.5 points), and two other negative results in October. If the remaining experiments were not published, one could imagine theorists hypothesizing that the effects depend on specific features of the content

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of these ads, the context of the campaign on these dates, or something about the senders and receivers in these experiments.

Our research design of repeated experiments provides context for these estimates. The main story of the graph is that treatment effects are similarly small over time but sample sizes of 1,000 or 2,000 generate week-to-week sampling variability. Without multiple experiments, or much larger experiments, publication bias could lead to a distorted view of the heterogeneity in persuasive effectiveness, which in turn could lead to overfit theories of persuasion.

Conditionality of persuasive effects We now more directly evaluate whether treatment effects are conditional on theoretically-posited drivers of difference across message, context, sender, and receiver. In Table 1, we regress conditional average treatment estimates (conditioning on subject partisanship and battleground state residence) on a set of predictors of heterogeneity common to political science studies of campaigns. Altogether, we consider seven sources of heterogeneity, one feature of the receiver (respondent partisanship), three features of context (time to election day, whether the ad was aired during the primary or the general, and battleground state residency), and three features of message or sender (whether the ad was attack or promotional, if sponsored by a campaign versus an outside group (PAC), and the target candidate).

Turning first to features of receiver, we see that the differences in treatment response across Democrats, Independents, and Republicans are small with standard errors greater than coefficients. Independents do not appear to be more malleable than partisans and neither partisan group is more responsive than the other. We consider below if partisanship of respondent interacts with partisanship of sender.

Of the three features of context, the slope with respect to the timing of the ad is negative in three of four specifications and statistically distinct from zero in one. The estimate in the third

	Candidate Favorability		Vote Choice	
Average effect	0.056*	0.062*	0.007	0.008
	(0.020)	(0.020)	(0.007)	(0.007)
Democratic respondent (vs. Republican)	0.035	0.022	0.011	0.006
	(0.035)	(0.036)	(0.010)	(0.011)
Independent respondent (vs. Republican)	0.023	0.015	0.009	0.007
	(0.051)	(0.052)	(0.020)	(0.020)
Battleground state (vs. nonbattleground)	−0.008	−0.007	−0.017	−0.017
	(0.033)	(0.033)	(0.010)	(0.010)
PAC sponsor (vs. campaign sponsor)	−0.012	0.026	−0.023	−0.016
	(0.043)	(0.047)	(0.013)	(0.014)
Time (scaled in months)	−0.023	0.005	−0.009*	−0.008
	(0.014)	(0.010)	(0.004)	(0.004)
Attack ad (vs. promotional ad)	−0.017		0.028	
	(0.046)		(0.016)	
General election (vs. primary election)	0.123			
	(0.067)			
Pro Trump Ad (vs. Pro Clinton ad)		−0.124		−0.016
		(0.101)		(0.034)
Anti Clinton Ad (vs. Pro Clinton ad)		−0.105		0.012
		(0.070)		(0.023)
Anti Trump Ad (vs. Pro Clinton ad)		−0.041		0.026
		(0.058)		(0.021)
Pro Sanders Ad (vs. Pro Clinton ad)		−0.075		
		(0.089)		
Pro Cruz Ad (vs. Pro Clinton ad)		0.047		
		(0.116)		
Pro Kasich Ad (vs. Pro Clinton ad)		−0.182		
		(0.145)		
Num. obs.	354	354	204	204

*$p < 0.05$. Observations are CATE estimates for each ad, conditional on subject partisanship and battleground residency. The signs of the outcomes are scaled with respect to the valence of the ad: higher values indicate that promotional ads had positive effects on target candidate favorability or vote choice and that attack ads had negative effects. All meta-regressors have been demeaned so the intercept always refers to the estimate of the average treatment effect but the coefficients still refer to the average difference in the effectiveness of the ad associated with a unit change in the regressor relative to the omitted category.

Table 1: Meta-analysis of average treatment effects of ads on target candidate favorability and vote choice

column suggests effects do decline as the campaign progresses. Difference in effectiveness for subjects who do and do not live in battleground states is 0.008 scale points. On average, general election ads move candidate favorability by 0.121 scale points more than primary ads, though the estimate is not precise enough to achieve statistical significance. Even though this point estimate is large compared with the others in the table, it remains substantively small. The upper bound of the 95% confidence interval for the average difference between primary and general election ads is about 0.25 scale points on a five-point favorability scale. If there is heterogeneity by election phase, it is not of large political importance.

Characteristics of the ads themselves do not correlate strongly with estimated effects. We find that attack ads are about as effective in achieving their goals as promotional ads and that PAC or SuperPAC sponsored ads are no more effective than those sponsored by candidates. While Pro-Clinton ads tended to be more effective than ads in support of or in opposition to other candidates, these differences cannot be distinguished from zero. Overall, there is little evidence that the magnitude of persuasion is conditional on the moderators that we evaluate motivated by political science theory.

Table 1 estimates average differences in response by features of the subjects and the ads *separately*, but it is possible that effectiveness depends instead on the *interaction* of receiver and message, as argued by some existing theories of persuasion. For example, some theories posit that partisan respondents will view in-party messages more favorably than out-party messages, or more generally, that treatments need to "match" subjects on important dimensions in order to be effective. Figure 2 provides partial support for the "partisan match" theory: Democratic subjects respond more strongly to pro-Democratic ads than to pro-Republican ads. However, we do not observe a corresponding pattern among Republican respondents: both pro-Democratic and pro-Republican ads have approximately the same small, positive, nonsignificant effect.

Table S.2 in the Supplementary Information presents formal tests of homogeneity of treat-
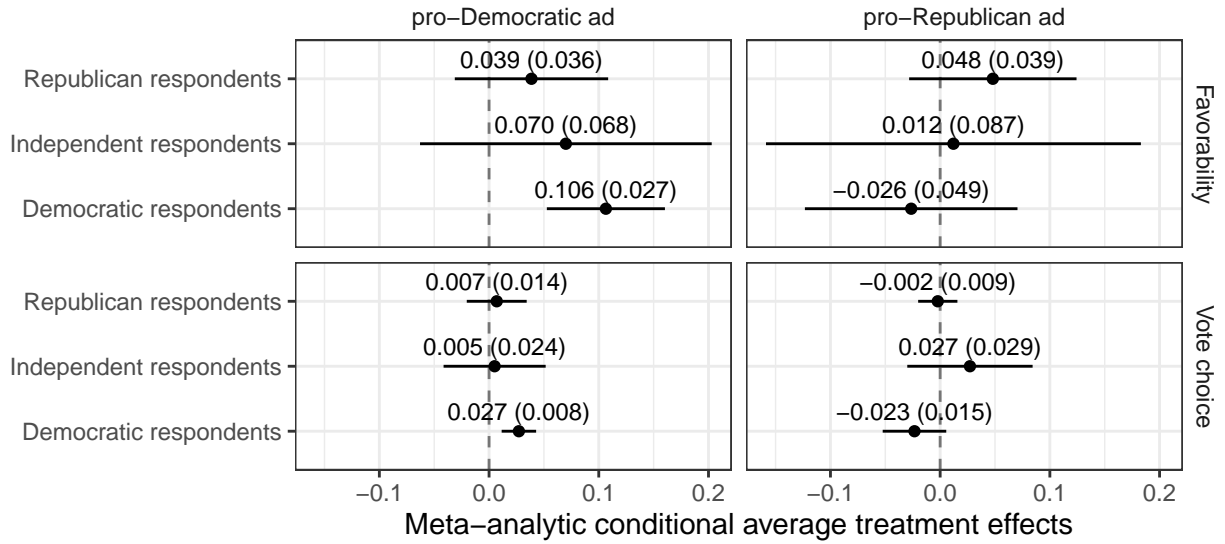
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Figure 2: Average effects of ads on favorability and vote choice, conditional on subject partisanship and ad target

ment effects across experiments and across subgroups. For the sample average treatment effects across experiments, p-values from tests against the null of treatment effect homogeneity are 0.09 for favorability and 0.07 for vote choice. For the set of conditional average treatment effects, p-values are 0.0002 and 0.96.

While we fail to reject the null hypothesis of treatment effect homogeneity in three of four opportunities, we do not affirm that null. Instead, we rely on the direct measure of treatment effect heterogeneity provided by the random effects estimator. The square root of the τ^2 statistic is an estimate of the true standard deviation of the treatment effects. We estimate this value to be 0.07 (SATEs) and 0.15 (CATEs) for favorability on a 5-point scale, and 0.02 and 0.02 for the vote choice. Since most estimates can be expected to fall within 2 standard deviations of the average, this analysis suggests small substantive effects even for the largest of CATEs. Across our many experiments that vary content, context, sender, and receiver, we find very little evidence that large persuasive effects occur even under a specific mix of features.

Discussion   Across social science fields, a common pattern has emerged: persuasive attempts tend to produce small average effects. Our 59 experiments demonstrate this clearly. A persistent worry is that these small average effects mask large and offsetting conditional effects. Scholars from many traditions have forwarded theories to predict the circumstances under which such conditionality will obtain. Theories of heterogeneity have tended to outpace empirical demonstrations and confirmations of such heterogeneity due to basic constraints of research design. We need fine control over the many features presumed to cause heterogeneity, large sample sizes to measure small variations in response, and repeated experiments to confirm generality.

The present study is unusual in its size (34,000 nationally-representative subjects) and breadth of treatments (a purposive sample of 49 of the highest-profile presidential advertisements fielded in the midst of the 2016 presidential election), allowing us to systematically investigate how variations in message, context, sender, or receiver, condition persuasive effects — while holding all other tools of the research design constant.

We do not find strong evidence of heterogeneity. The magnitude of the effects of campaign ads on candidate favorability and vote choice does not appear to depend greatly upon characteristics of the ad like tone, sponsor, or target; characteristics of the information environment such as timing throughout the election year or battleground state residency; or characteristics of actors such as partisanship.

Of course, we have not tested all potential sources of heterogeneity. There may well be a mix of message features and subject characteristics that generates politically important persuasion. We have not considered here some hypothesized moderators such as need for cognition, need for evaluation, need for cognitive closure, moral foundations, personality type, or interest in politics for the main reason that we allocated our budget to many shorter surveys rather than fewer longer surveys that could have measured these possible sources of variation in treatment response. But even if we had measured these and found they did not predict heterogeneity, we

still would not affirm complete homogeneity because future scholarship could always discover as-yet unknown and unmeasured sources of variation. All that said, we have tested many of the key theoretical ideas from political science in the context of a presidential campaign and found little evidence of large differences.

Despite these small effects, campaign advertising may still play a large role in election outcomes. Our intervention delivers one additional ad in the heart of a dramatic presidential campaign that aired hundreds of thousands of such ads. This promotes external validity, but we are measuring the *marginal* effect of one additional advertisement. We do not measure the impact of an entire advertising campaign. If effectiveness were to increase linearly in ads viewed (or if the marginal returns diminished slowly enough), these small effects could be highly consequential, consistent with the observed level of spending by candidates on advertising. Our data cannot speak to this question of scale, though the result in Table 1 that effects do not vary by battleground status (where people see many more ads than those who live in non-battleground states) suggests that marginal effectiveness may not depend upon ambient levels of advertising.

How should scholars add our evidence to the science of political persuasion and to persuasion more generally? We suggest two conclusions. First, the marginal effect of advertising is small but detectable, thus candidates and campaigns may not be wrong to allocate scarce resources to television advertising because, in a close election, these small effects could be the difference between winning and losing. Second, the expensive efforts to target or tailor ads to specific audiences requires careful consideration. The evidence from our study shows that the effectiveness of ads does not vary greatly from person to person or from ad to ad.

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Supplementary materials

Treatments

Repeat Advertisements

Analysis of Heterogeneity

Ad Transcripts

Random Assignment

Survey Questionnaire

Tables S1 to S3

Figure S1