Mobilization and Backlash: Asymmetric Updating in Response to Campaign Ads

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Abstract

Applying a media market boundary approach to individual survey data, I show that political advertising on television increases the probability that viewers who identify with a party will espouse its positions, prefer its candidates, and turn out to vote. This is true no matter which party sponsored the ad, suggesting that an ad consolidates and motivates the sponsor's partisans while simultaneously engendering a countervailing consolidation and mobilization among supporters of the other party. My results are consistent with agents who judge a source's quality by their priors and highlight the importance of targeting supporters.

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1 Introduction

In the wake of convincing evidence that voters' policy preferences are significantly and durably affected by news media consumption (e.g. DellaVigna and Kaplan 2007; Chiang and Knight 2011), economists have conducted many high-quality studies of persuasion, often in a political context. Early influential studies include Gentzkow and Shapiro 2006; Mullainathan, Schwartzstein, and Shleifer 2008; Glaeser and Sunstein 2009; and Kamenica and Gentzkow 2011. As Della Vigna and Gentzkow point out in an early review, "distinguishing different models of persuasion is particularly important because they have quite different implications for policy and welfare... Welfare effects are harder to evaluate if receivers are not Bayesian or if persuasion works though emotion or preference change." (DellaVigna and Gentzkow 2010)

I study persuasion in a political context, analyzing the vote choice and policy preferences of individuals who are exposed to exogenously varying political advertising on television during recent American federal elections. I find that while political ads effectively rally the faithful, they provoke an equally powerful backlash from the opposition, suggesting that the reception of messages is contingent on prior beliefs. I highlight three important findings. First, ads consistently drive voters to their ideological predispositions. The effect of an ad depends not on the sponsoring party but on the partisanship of the viewer. The magnitude of the effect on the sponsoring party's own supporters, which I refer to as *mobilization*, is statistically indistinguishable from the magnitude of the effect on the other party's supporters, which I refer to as *backlash*. Second, exposure to ads on a particular subject leads voters to align with their party's positions on policies related to that subject. As before, the effect of the ad depends not on the identity of the sponsoring party, but on the ideological leaning of the viewer. These results hold across dozens of policies. Third, ads for other federal offices have just as strong an effect on pre-election candidate preference and election-day vote choice as ads for the candidate in question, suggesting that the effect of these short campaign ad spots operates at the party level rather than pertaining to characteristics of the candidate in question. In sum, political ads consolidate and galvanize the opposition as well as one's supporters. They polarize. Interestingly, there is no consistent effect on the ideologically uncommitted that might tip the balance in favor of the ad sponsor by conversion of the itinerant. Thus, political advertisements may do their sponsors little good unless appropriately targeted to their own supporters. They are a call to political prayer best reserved for the eyes and ears of the faithful.

The political advertising that I study is of particular interest because the receivers have not chosen whether to receive the message, as they do when selecting a news channel. Thus, rather than the typical combination of selective exposure and selective attention, I am observing the effects of only the latter (Gerber and Green 1999). As such, I can compare the differing responses of partisans to receipt of identical messages. This is a different setting than the quasi-Bayesian updating of Ortoleva and Snowberg 2015, in which polarization results from a misinterpretation of one's idiosyncratic information set. My findings could be evidence that ads cue latent pre-conceptions, a view espoused by Gerber et al. 2011. They are also consistent with agents who judge the quality of the news by the degree to which it fits their preconceptions, as in the seminal work of Gentzkow and Shapiro 2006.[1] I discuss this further in section 3.

My most important result in this regard is robust evidence of asymmetric updating in response to a given message. There are hints of asymmetric updating in a pair of recent empirical papers focused on specialized settings. Faia et al. 2022 find that receivers update their beliefs less strongly in response to evidence they prefer to receive less. This is asymmetric

[1]In the setting studied by Kolotilin et al. 2017, privately tailored messages are just as effective as public messages. This is strongly at odds with my finding that targeting would usefully avoid backlash. Kolotilin et al. 2017 do not consider whether receivers evaluate the quality of the information source, which is what makes polarization from a single message possible. (Gerber and Green 1999)

updating but without evidence of the backlash that I find. Baysan 2022 finds something similar to backlash in an experiment on political attitudes in Turkey. Providing common, consistent information about executive branch performance resulted in polarized opinions regarding the advisability of constraining the executive branch in an upcoming referendum. I provide robust, multi-faceted evidence spanning both candidate selection for multiple races and evaluation of policy proposals over two election cycles in a mature democracy with an uncensored media environment.

My analysis centers on the individual respondents to the Cooperative Congressional Election Study (of US voters) for the years 2012 and 2016. My dependent variables include post-election recollections of vote choice, pre-election expressions of preferences over candidates, and preferences over specific policies. Pre-election candidate preferences are a useful check given the potential inaccuracy of post-election recollections. Policy preferences provide a more direct measure of the channel whereby vote choice is affected and are far less studied than vote choice and turnout.[2] I measure them using a variety of questions asking whether a respondent agrees or disagrees with a certain policy, such as increasing the number of patrols on the US-Mexico border. These questions range across a variety of issues from gun control to gay marriage and can be matched to broad classifications of the issues addressed by each ad. For example, I evaluate whether exposure to ads that discuss immigration affects respondents' opinions about policies related to immigration.

My identification strategy mirrors that of several other papers in the recent literature on the effects of campaign advertising (Huber and Arceneaux 2007; Krasno and Green 2008; Spenkuch and Toniatti 2018; Sides, Vavreck, and Warshaw 2021), though unlike most of these papers, I analyze individual responses rather than geographically aggregated (county level) data. Namely, I make use of the exogeneity of media market boundaries, a strategy pioneered by Dube, Lester, and Reich 2010 and Snyder Jr and Strömberg 2010 among others.

[2] Important recent studies on persuasion to specific policy positions include Huber and Arceneaux 2007 and Kalla and Broockman 2022.

By comparing the attitudes of two individuals within the same congressional district yet on either side of a media market boundary, thus exogenously subjected to differing levels of advertisements, I plausibly estimate a causal effect of exposure to campaign advertising. I discuss this strategy in the context of individual data and explore potential confounds in sections 2.2 and 3.5.

My results are robust to altering several design choices including the length of the window during which ads are counted, the choice of estimator, the strictness of the classification of partisans and independents, the distance metric employed to form pairs, and the formation of pairs by matching on interview date or demographic characteristics. Fowler, Franz, and Ridout 2021 have pointed out that, from an advertising perspective, the 2016 Presidential race was rather different from both prior Presidential races and concurrent congressional races. I confirm that my results remain when restricting to 2012 and are thus not driven by the peculiarities of the 2016 race in which one candidate advertised very little and both candidates were reviled by partisans of the other side. The remainder of the paper is structured as follows. In section 2, I detail the data and the empirical approach including the method of sample construction and the identification strategy. I present the various results in section 3 and conclude in section 4 by placing them in a broader context including relating them to important prior results.

2 Data and Research Design

As previously noted, I follow the burgeoning literature in exploiting the discontinuity in advertising exposure exogenously imposed by pre-existing media market (DMA) boundaries. Spenkuch and Toniatti 2018 note the development of this literature from labor economics (Card and Krueger 1994) and were the first to rigorously apply the approach to political advertising, though they also note the prior existence of several papers using media market boundaries to address other questions in political economy, Ansolabehere, Snowberg, and Snyder Jr 2006 and DellaVigna and Kaplan 2007 being early examples. Sides, Vavreck, and

Warshaw 2021 employ this strategy to study political advertising alongside simpler panel methods, which frequently generate notably different results.

While prior studies of political ads using observational data focus on county-level aggregates of vote returns, my dependent variables are the survey responses of individuals. Thus, rather than pairing counties that are adjacent but falling on either side of a DMA boundary, I pair individuals on either side of a DMA boundary within the same congressional district. Naturally, this requires a new definition of adjacency to identify the set of admissible pairs, which I discuss in section 2.2.[3]

2.1 Econometric Specification

I investigate the effects of advertising on three different dependent variables: vote choice, pre-election candidate preference, and policy preferences. For vote choice and candidate preference, my specifications are built on the following:

$$y_{ip}^{o} = \alpha_p + \beta * AdExposure_i + \gamma * X_i + e_{ip}^{o} \tag{1}$$

where y_{ip}^{o} is the vote choice or candidate preference of CCES respondent i for office o. The p subscript is necessary because individuals appear in the dataset multiple times, once for each valid pair, p, that they are matched into. Fixed effects at the level of each pair, α_p, ensure the identifying variation comes from within-pair differences in ad exposure. X_i is a set of demographic controls including age, gender, ethnicity, and education. $AdExposure_i$ is a measure of ad exposure of individual i whose construction is detailed in section 2.2. The coefficient of interest is β.

As I am interested in how the effect β varies according to the partisan identity of the

[3] I prefer this to the method of Huber and Arceneaux 2007 of focusing on within-DMA variation between battleground and non-battleground states. As noted by Hill et al. 2013, restriction to battlegrounds results in substantial loss of cases and thus precision.

issuer and viewer of the ad, I split ad exposure by party and race (e.g. minutes of ads shown by the Republican candidate for Senate) and interact ad exposure with the partisan identity of the survey respondent. Thus the specification for my main results reported is:

$$y^o_{ip} = \alpha_p + \sum_r \sum_t \sum_v \beta^{r,t,v} AdExposure^{r,t}_i VoterParty^v_{ip} + \gamma * X_i + e^o_{ip} \tag{2}$$

with a separate regression for each office $o \in$\{President, Senate, House\}.

Here race, r, captures whether the ads pertain to the office, o, over which the respondent is expressing a preference (e.g. Presidential) or other races for federal office (e.g. Senate and House). This allows me to distinguish between candidate-specific effects and party spillovers. The tally, t, can be either the total minutes of ad exposure (Dem+Rep) or the Democratic advantage in ad exposure (Dem-Rep). The voter's partisan identification, v, is either Republican, Democrat, or independent. There are thus 2*2*3=12 βs for each of the three regressions. One of the twelve would, for example, tell us how exposure to an additional minute of ads from Presidential candidates affected the vote choice over candidates for the Senate race by voters who identify as Republicans (o=Senate, r=ads for another federal office, t=total ads, v=Republican viewer).

For policy issues, the dependent variable is a binary indicator of whether the voter expressed support for proposal j. Ad exposure is a tally of those ads about the issue related to policy question j. For example, the policy question "Should the US increase patrols along the US-Mexico border" is connected to ads coded as covering the issue "immigration". I interact the partisanship of the viewer, v, and ad sponsor, s, resulting in the following set of specifications for the regressions behind Figures 6 and 7:

$$y^j_{ip} = \alpha_p + \sum_s \sum_v \beta^{s,v} AdExposure^{j,s}_i VoterParty^v_{ip} + \gamma * X_i + e^j_{ip} \tag{3}$$

with a separate regression for each issue, j.

As Dube, Lester, and Reich 2010 point out, the existence of a single individual in multiple pairs and thus multiple observations necessarily implies correlation among the residuals which is taken care of via two-way clustering over each individual involved in the pair. This two-way clustering is important as the difference between the responses of individuals i and i' for $i, i' \in p$ are affected by the idiosyncrasies of both individuals, and thus the error, e^j_{ip} of this difference will be correlated with that of all other pairings made by i and i'. I also cluster on the DMAs of each member of the pair, in recognition that the content of the ads (persuasive vs. combative, say) may vary by DMA in unobservable ways that nonetheless affect the reactions of all respondents within a DMA.

I estimate a linear regression rather than a logistic to facilitate this necessary multi-way clustering. To allay fears of misspecification, I have confirmed the magnitude of the coefficients with a logit employing one-way clustering on i.

2.2 Construction of Pairs

The purpose of Spenkuch and Toniatti's border county pairing is to remove the influence of geographically varying omitted effects, such as canvassing. Unfortunately, the characteristic distance over which such omitted variables remain reasonably constant is, by the very nature of their unobservability, not possible to establish. I address this by repeating my primary analysis for three different samples. To achieve an individual-level analog of the border-county pairs used by Spenkuch and Toniatti 2018 and Sides, Vavreck, and Warshaw 2021, I generate every possible pairing in which the individuals are from the same state, from different DMAs, from neighboring counties, but from the same congressional district. For the full sample including 2012 and 2016, this results in 670,152 pairs.

As an alternative to using counties, I define proximity by distance. I have individuals' zip codes but not precise street addresses, thus I locate individuals at the centroid of their zip code. I then generate every possible pairing in which individuals are from the same state, from different DMAs, from the same congressional district, and whose zip code centroids are within 100km of each other. I chose the 100km figure as a round number nearest the average distance in the sample generated by the first method. Finally, I repeat method two with a threshold of 50km. These samples include 642,293 and 66,173 observations respectively.

Figure 1 shows how adopting different methods affects the size and weight of the sample. The key point is that counties and DMAs vary in size across the country, tending to be smaller where population density is greater. Thus, using county-adjacency is akin to an uneven distance metric. I prefer this dynamic distance metric for two reasons. First, it has the result of including far more pairs from the west than a uniform distance metric, which I feel is important for representation. Second, I feel the relevant unobservables are likely to vary over distance at a rate related to population density. Political strategy and communication with the electorate are candidate-specific and thus vary between Congressional Districts. Thus, political culture and strategy are probably uniform over greater distances where the population sprawls than where people are tightly packed, implying that a dynamic distance cutoff is preferable and using counties is a simple mechanism to achieve it. Results presented in the main body of the paper derive from this county-adjacency sample.

2.3 Measuring Advertising Exposure

Generating a measure of advertising exposure requires defining an event window. Based on prior studies, results presented in the main body of the paper employ a 4-week window (as did Huber and Arceneaux 2007), but I have performed robustness checks for 2- and 6-week windows as well. Ad exposure is measured as the ratings-weighted sum of political ads for federal offices aired in the respondent's DMA during the 4-week window.

Data on advertisements aired for and during the federal elections of 2012 and 2016 come from the Wesleyan Media Project (WMP) and Media Analysis Group (CMAG) partnership. I use all ads for President, Senate, and House campaigns. According to the WMP, the advertisements dataset has complete data for all media markets, termed Designated Market Areas (DMAs).[4] For each political ad aired, the date, time, DMA, TV station, and length

[4]As Spenkuch and Toniatti 2018 note, small-sample audits have found that the CMAG

are listed. The overwhelming majority of ads are 30 seconds long, but I do weight by spot length.

The mere act of airing an ad does not guarantee that it is seen. The viewership for an ad depends on channel and time of day and varies by demographic group. I employ Nielsen Ad Intel Data, which, from a sample of households in each DMA, measures the fraction of households watching each station at approximately ten-minute intervals throughout the day. These data can be broken out by age and gender. Thus, when later tallying the expected number of ads watched by a particular individual, I weight each ad according to how likely it is that an individual of the gender and age in question, living in the DMA in question, is watching. Age- and gender-based rating is especially important as program ratings vary significantly along these dimensions. By conditioning on these demographic categories, I move closer to an individually accurate measure of ad exposure.[5]

WMP human-codes ads along several helpful dimensions including sponsoring party (Republican/Democrat), tone (Positive/Negative/Contrast), race (Pres/Sen/House), and which issues are mentioned. Thus, I can calculate ad exposure for subcategories such as negative ads aired by Democrats in Senate races. When analyzing specific issues, I restrict my attention to dimensions which can be matched to CCES questions. The thirteen such subjects are: "gun control", "abortion", "taxes", "immigration", "budget deficit", "social security", "environment", "jobs", "crime", "race relations", "healthcare", "gay marriage", and "government corruption".[6]

The dependent variables derive from questions asked by the Cooperative Congressional

data are highly correlated with invoice data from television stations. Hagen and Kolodny 2008 found that less than 2% of ads were missing from the CMAG sample.

[5]In my sample, ratings-weighted measures and unweighted tallies of ads exhibit bi-variate correlations of 0.79.

[6]The identification strategy remains equally valid when applied to sub-categories of ads such as ads on a specific subject, ads by a certain party, or ads of a given tone.

Election Survey. For questions asked during the pre-election waves, I measure ad exposure covering 4 weeks prior to the survey date (which varies by respondent). For questions asked during the post-election waves, I measure ad exposure in the 4 weeks prior to the election. The proper survey weights vary by wave and are employed as directed by CCES.

2.4 Dependent variables

I analyze three sets of dependent variables. First, I look at both vote choice from the post-election wave and candidate preference from the pre-election wave. The former has the disadvantage of potentially flawed recall, but the advantage of measuring final outcomes. Both are subject to forms of response bias. In these cases, my dependent variable is the net democratic vote (or preference) of the individual, defined as follows

$$\text{Net Democratic Vote} = \begin{cases} 1 & \text{Democratic candidate} \\ 0 & \text{Third party, don't know, will not vote} \\ -1 & \text{Republican candidate} \end{cases}$$

Estimating a linear regression both presumes a linear probability model and restricts the marginal effects to be equal between each step. That is, I presume a regressor X has the same likelihood of changing a voter from Republican to "will not vote" as it does of changing a voter from "will not vote" to Democrat. I perform robustness checks using ordered logit and using a binary measure of vote choice.

Second, I look at validated turnout. In the post-election waves, the CCES asks voters whether they voted. As has been noted, these statements cannot be taken at face value as both recall bias and response bias lead to improbably high reported turnout rates in all such surveys (Selb and Munzert 2013). For both the 2012 and 2016 waves, the CCES conducted validation studies to match respondents to official voting records. I use the validated turnout as directed by Enamorado and Imai 2018. The dependent variable in this case is binary.

Third, I look at questions asking whether the respondent supports or opposes specific policy questions. I limited to those questions with a clear support/oppose answer, that could be linked to a particular ad subject (e.g. immigration), where the proposal could be considered independent of other questions, and where the proposal had a clear partisan orientation (e.g. "do you support a ban on assault rifles?"). These criteria admit 21 policy proposals from the 2016 survey, listed in Table 1.

2.5 Balance Tests

The CCES sample is stratified at the state level but not at finer geographies such as DMA. Thus, it is theoretically possible that, say, the sample skews Democratic on one side of the line but Republican on the other. That sample imbalance may then result in differential responses to ads disrupting my identification.[7] Balance tests of political affiliation (Table 2) show no imbalance in political affiliation between members of the pairs with one exception. I first split the pairs according to which individual received the most total ads, looking for whether individuals receiving more total ads for a given office had different party affiliation than those receiving fewer ads (columns 1-3). I then split the pairs according to the partisan tilt of the ads received by the individual (columns 4-6), looking for whether individuals receiving a more heavily Democratic set of ads for a given office have a different partisan affiliation than those receiving a more heavily Republican set of ads. All six coefficients are insignificant indicating no partisan imbalance. Thus the individuals in a pairing are interchangeable and

[7]The first stage sampling frame comes from the American Community Survey. Frame development is built at the census block level starting from postal addresses with a Community Address Updating System to update between censuses and address discrepancies between rural and urban areas. Thus, the sampling frame ought to be equally representative across DMAs. Thus, it would be random sampling variation that would lead to a CCES sample that is differentially representative across DMA within a state.

the identification strategy remains valid.[8] [9]

Fowler, Franz, and Ridout 2021 note that third-generation targeting in which campaigns select television programs based on the expected viewership has been widespread since 2012, thus covering my entire sample. Because I measure ad exposure at the individual level, I can also control for the individual demographics—ethnicity, education, age—by which campaigns target. Age, education, and ethnicity explain roughly 20% of the individual-level variation in ad exposure, with virtually all of this due to age-related television viewing habits.

A stronger potential confound is unobserved social media ads which are individually targeted and correlated with television ads. By 2016, ads on social media could be micro-targeted by demographics, location, or interests expressed in browsing habits. On some platforms, campaigns can also provide email lists of people they wish to target, have the social media platform find those people in their user base, and then recommend out-of-sample similar users (Fowler, Franz, and Ridout 2021).

Controlling for age, ethnicity, and gender mitigates some of these concerns by controlling for the factors most likely to correlate with differential exposure to political ads on social media. Forming pairs from individuals who live within the same congressional district

[8]If one does not restrict pairs to being formed within a single congressional district, the results change slightly. Individuals whose Presidential or Senate Ad exposure skewed more Democratic are indistinguishable from their partners whose exposure was more Republican. However, individuals whose House ad exposure was more Democratic are significantly different from those whose House exposure was more Republican. Presumably, this arises because the candidate that dominates the airwaves is also facing more friendly voters and thus my sample is more likely to select a voter of that party.

[9]The correlation between total ad exposure and democratic ad advantage, when calculated for respondents who identify as Democrats, is statistically indistinguishable from the same correlation calculated for respondents who identify as Republicans.

and relatively close to each other limits the likelihood that they fall on either side of a geographically targeted social media ad buy. I am also helped by the inability of campaigns to effectively target hidden characteristics with television ads. The effects of social media targeted to characteristics that cannot be observed in a television audience, and are thus not subject to targeting via television ad buys, constitute a safely orthogonal component of the error term in my regression. While I cannot rule out this source of influence, I note that such targeting was not possible in 2012, and find that my results do not differ much between 2012 and 2016.

2.6 The Sample

Aggregating the years 2012 and 2016 delivers a sample of 118,863 individuals from which I form 670,152 pairs using the border counties method described above. I report sample statistics for the 60,491 individuals who are involved in at least one pairing (Table 3). These demographics are broadly in line with US averages for voting-age adults over the period though, of course, I use sample weights.

Summary statistics on pre- and post-election wave advertising exposure (Table 4) are broadly in line with prior studies. Respondents see over twice as many ads in the 4 weeks prior to the election as they do in the 4 weeks prior to the pre-election interview, consistent with the observed back-loading of ads close to election day. There is a slight Democratic advantage across the board for all races and both windows. But as Sides, Vavreck, and Warshaw 2021 note, the difference varies widely across geographic places and thus across my individuals. In accordance with the concentration of ads in particular races, which I will illustrate shortly, the distributions of ad exposure are truncated at zero and exhibit a right tail encompassing saturated jurisdictions. Negative ads are predominant at a rate of three to one.

DMAs vary a great deal in the number of households, spanning more than two orders of magnitude from a few tens of thousands of households to several million. This size distribution interacts with the geography of county adjacency and CCES sampling to produce the geographic distribution of my pairs (Figure A.1). The geographic distribution looks broadly representative though there are no qualifying data points in Utah or Rhode Island because these states contain a single DMA (Salt Lake City and Providence, respectively) thus there can be no pairs in neighboring DMAs within these states.[10]

Figure 1 displays both the 50km and the border county pairs samples for Massachusetts, Rhode Island, and Connecticut. From it, I can see that the distance-based metric selects from Metropolitan Boston while the border-county pairs method does not. However, this is not always the case as some metropolitan centers, especially out west, sit in counties that are nonetheless at the boundary of their DMA. Figure 2 shows a close-up of Oregon and Washington with the connections between pairs explicitly drawn. Note that while individuals from central Portland are not selected; those from central Seattle are. Results are robust to dropping observations from DMAs whose central city, which may be driving campaign advertising strategy, is in a border county.

Figure 3 shows, for a randomly chosen four of the issues under study, the DMA-wide average exposure to political ads on the issue run by campaigns for federal office during a 60-day period prior to the federal election. Measured in expected minutes of exposure, the maps make clear that there is a great deal of variation between DMAs on exposure to any given issue and a great deal of variation across issues as to which DMAs received heavy exposure. Thus my regressions that draw on ads for specific issues are each identifying off a very different set of geographic boundaries, increasing confidence that results are not driven by outliers or misconstructions. Ads on any given subject tend to be concentrated in a minority of DMAs.

In the online appendix, I include Figures A.2-A.5 showing that while certain issues command the bulk of both parties' advertising, parties also differ markedly in which issues

[10]Nielsen does not record data for Alaska and Hawaii thus these states are not part of my sample.

they choose to emphasize. Republicans favor economic issues: jobs, taxes, and the deficit. Democrats outspend their counterparts on social security and abortion. While there is some change between 2012 and 2016 and some difference between Presidential and House campaigns, the rough ordering of which issues take precedence is stable.

3 Results

3.1 Vote Choice

My first and most direct evidence of backlash comes from declared vote choice. The specification is defined in Equation 2 and the dependent variable is the net democratic vote defined above. While the full tables are available in the online appendix (Table A1) with accompanying hypothesis tests (Table A2), I present the results visually in Figure 4 to illustrate their essential character.

Positive (negative) coefficients indicate that the respondent became more inclined to vote Democratic (Republican). The effect of total ads on Democratic voters is always positive and strongly significant ($\hat{\beta}^{o,D+R,D} > 0$); the effect on Republican voters is always negative and strongly significant ($\hat{\beta}^{o,D+R,R} < 0$). Moreover, the effects come not only from ads run by the candidates in question; ads from other races have effects of similar magnitude ($\hat{\beta}^{\sim o,D+R,D} > 0$, $\hat{\beta}^{\sim o,D+R,R} < 0$). On the other hand, the partisan mix of ads has no clear effect ($\hat{\beta}^{\cdot,D-R,D} \approx \hat{\beta}^{\cdot,D-R,R}$), indicating that voters respond identically to ads from either party[11]. In sum, political advertisements from either party consistently drive voters to their ideological predispositions. Relatedly, I find that ads increase straight-ticket voting: the propensity of the viewer to vote for the same party in all three federal races (Table A3). Together these suggest that the effect operates via party brand rather than individual candidate characteristics.

To contextualize the magnitude of these coefficients, I convert the effect of total advertisements into persuasion rates, f_P^O, for ads pertaining to an office, O, of a respondent identifying with party P (DellaVigna and Gentzkow 2010). Defined in percentage terms

[11] Formal hypothesis tests are reported in Table A2.

$$f_P^O = 100 * \frac{1}{u} * \hat{\beta}_P^O \tag{4}$$

where $\hat{\beta}_P^O$ is the estimated coefficient of advertisements on the vote choice for a particular office, O, of a respondent who identifies with party P. In this case, u stands for the fraction of the population available to be convinced, operationalized as those who do not already identify with the party in question. For respondents who identify with the Democrats, and for whom I find ads will generally convince them to vote for Democrats, I approximate this with the share who chose to vote Republican. For respondents who identify with the Republicans, and for whom I find ads will generally convince them to vote for Republicans, I approximate this with the share who chose to vote Democrat. For Independents, whom ads very marginally convince to vote Democrat, I approximate with the share who choose to vote Republican. In Table **??** (top half), I report the rate at which a group is persuaded to vote Democratic, with negative signs indicating persuasion to vote Republican. Finally, while the estimated coefficients displayed in Figure 4 are for a single minute of ad exposure, I have shown persuasion rates for 20 minutes of total advertising as this is approximately the average 4-week level of exposure to Presidential ads for a respondent in my sample (20.8 mins). Each row of Table **??** pertains to vote choice for a different office. The left-hand panel concerns ads aired by candidates running for that office while the right-hand panel concerns ads aired by campaigns for other federal offices.

Two results are consistent with persuasion that operates mainly through cueing of partisan loyalties. First, for respondents who identify with one party or the other, the absolute value of these persuasion rates averages 9.9% while for independents the average is 0.6%. Second, the persuasion rates of ads for the race in question (10.2%) are only slightly stronger on average than the persuasion rates from ads for other federal races (9.7%), a difference too slight to be statistically significant. Additionally, among partisan voters, persuasion rates for votes for the Presidency (12.7%) are stronger on average than persuasion rates for Congressional races (8.6%) and these differences are statistically significant (two-sample Z-test statistic 3.64).

How do these persuasion rates compare to those of prior studies? In their review, DellaVigna and Gentzkow 2010 summarize four studies that report persuasion rates for vote shares varying from 2% to 20% in response to exposure to specific television stations and newspapers.[12] These are aggregate effects from non-targeted media exposure over a period of at least a few weeks and as many as a few years. These strong aggregate effects might be consistent with my study if media exposure is influencing the formation of partisan identity.

Broockman and Kalla 2020 do measure responses of individual voters rather than aggregates, and disaggregate results by the partisan content of the message and the partisan identity of the respondent. I use post-election reports of vote choice by partisan affiliation from Igielnik, Keeter, and Hartig n.d. to proxy for u and thus convert Broockman and Kalla's raw regression coefficients to persuasion rates, which I report in table 6. The effect of pro-Biden statements on Republicans' votes is the only one of these coefficients estimated sufficiently precisely to be statistically significant. The persuasion rate of 4.1% is compatible with the range of my estimates. Transforming the coefficients into persuasion rates makes clear that their results do exhibit asymmetric updating. I will return to their results in section 4.

3.2 Candidate preference

Because of the well-known dangers of relying on self-reported vote choice, I complement my analysis of vote choice with an identical regression of the respondent's professed candidate preference in the pre-election wave on ads seen prior to the pre-election survey (Figure 5 and Table A4). The results maintain the same pattern and are of similar magnitude. Here too I converted the coefficients to persuasion rates (Table **??**, bottom half). In this case, I did so for a treatment of 10 minutes of total advertising as the average level of exposure to Presidential ads for a respondent in my sample over the 4 weeks prior to the pre-election survey was 9.56 minutes. Interestingly, pre-election persuasion rates average 9.8% among

[12]DellaVigna and Gentzkow 2010, Table 1.

those who identify with one party or the other, which is almost exactly the same magnitude as those for the post-election vote choice. However, this persuasion comes from a treatment half as large, corroborating the general finding that early advertising is more effective.[13] Here too, persuasion rates for the Presidency (15.7% on average) are stronger than those for Congressional races (6.5%), with a two-sample Z-statistic of 6.78. Once again, independent voters show virtually no systematic persuasion.

3.3 Policy Preferences

Analyzing support for specific policies provides additional evidence for my thesis that ads rally viewers to their ideological leanings. I find that ads on the relevant issue from either party lead a viewer's attitudes about virtually any policy proposal to be more aligned with their ideological leanings. Moreover, I once again find that an ad will just as strongly crystallize the viewpoint of the sponsor's ideological foe as that of the sponsor's ideological friend.

Figure 6 reports the point estimates and 95% confidence intervals from 21 separate regressions: one for each of the policies listed along the y-axis. The point estimates show clearly that, for any given policy, the effect of advertisements depends on the partisanship of the viewer and *not* on the party showing the ad. First, for a viewer of a given affiliation, changing the party showing the ad does not change the sign of the effect.

$$sgn(\hat{\beta}^{R,R}) = sgn(\hat{\beta}^{D,R})$$

$$sgn(\hat{\beta}^{R,D}) = sgn(\hat{\beta}^{D,D})$$

Second, when the affiliation of the viewer is switched– R for D or vice versa– the sign switches, demonstrating that the response to any ad is strongly determined by the viewer's partisan identity.

[13] With the caveat that all persuasion is short-lived, as I confirm in section 3.5.

$$sgn(\hat{\beta}^{R,R}) \neq sgn(\hat{\beta}^{R,D})$$

$$sgn(\hat{\beta}^{D,R}) \neq sgn(\hat{\beta}^{D,D})$$

Third, the coefficients almost always indicate a strengthening of support for the position naturally espoused by the viewer's preferred party. That is, when viewing ads aired by either party, Democrats (Republicans) are more likely to support Democratic (Republican) policies and less likely to support Republican (Democratic) proposals. Finally, the mobilization of one's own supporters (solid line) is roughly equal in magnitude (and of opposite sign) to the backlash by supporters of the other party (short-dashed line) indicating that an ad by a specific party on that issue would garner no net support if shown to a balanced audience. (One clear exception is Republican ads regarding gay marriage which mobilize Republican support against this policy more than they result in Democratic backlash in favor.)

$$\hat{\beta}^{R,R} \approx -\hat{\beta}^{R,D}$$

$$\hat{\beta}^{D,R} \approx -\hat{\beta}^{D,D}$$

For instance, the fourth row pertains to whether the Clean Air and Clean Water Act should be more vigorously enforced. Exposure to Republican ads on the environment clearly lowers support for this proposal among Republicans (solid-R is negative). On the other hand, exposure to those same Republican ads raises support for the proposal among Democrats (dashed-D is positive). The backlash among Democrats is nearly as strong as the mobilization of Republicans, indeed they are statistically indistinguishable. Exposure to Democratic ads produces the same effect: viewers who self-identify as Democrats are more likely to support the proposal (solid-D) while those who self-identify as Republican are less likely to do so (dashed-R).

What is striking is that the sign of the response is nearly always opposite for individuals of opposing ideological leanings. That is, if ad exposure raises Democratic support, it will lower Republican support and vice versa. With 21 issues and two parties sponsoring ads, I have 42 cases to consider. In only 3 of these cases are the estimated effects on Republican and Democratic voters of the same sign, and in none of these 3 cases are the coefficients statistically significant. The 21 policies under consideration span gun control, immigration, the environment, abortion, crime, and gay marriage. With two parties issuing ads to be viewed by voters of two ideological leanings, there are 84 coefficients of interest. Of the 62 coefficients that are statistically significant, *every single one* is of the sign consistent with my prediction of polarized response. Each of the signs is consistent with the partisan alignment of the issue: ads from either party increase the likelihood that a voter will espouse preferences in line with their partisan leanings. Thus, the overwhelming pattern is a polarized response based on the viewer's partisan identity, regardless of who sponsored the ad.

Moreover, I once again find no clear advantage accruing to the party sponsoring the ad. In 31 of the 42 cases, there is no statistically significant difference in magnitude between the mobilization and the backlash. Of the eleven cases where one effect is significantly larger, seven show a larger backlash while only four show a larger mobilization.

3.4 Validated Turnout

Finally, I turn to an analysis of validated turnout. Mindful of a long literature contesting whether the tone of an ad is consequential for political engagement (e.g. Ansolabehere, Iyengar, and Simon 1999; Lau et al. 1999), I split the ads into positive and negative.[14] I find that positive ads increase turnout among partisan voters with a much weaker effect on independent voters (Figure 7 and Tables A5 and A6). This comports with the overall theme that ads deliver a call to profess one's tightly-held beliefs. Independents, by their very definition, do not feel moved by the partisan call.

But is there a backlash? When I break out ads by the sponsoring party as well as the

[14]Contrast ads are dropped.

tone, I find that Democrat-sponsored positive ads seem to have a larger mobilizing effect than Republican-sponsored positive ads. However, these are the only types of ads for which I can reject the hypothesis that the effects are equal across audiences. Generally speaking, ads mobilize the other party's voters as strongly as they motivate one's own.

3.5 Robustness

The online appendix details eight robustness checks. Table A7 re-estimates the vote choice specifications of Table A1 and Figure 4 using an ordered logit instead of a linear regression. This comes at the cost of being able to cluster only on one of the individuals in the pair so the standard errors are surely wildly optimistic. Nonetheless, the pattern of coefficient signs and relative magnitudes continues to support the main hypotheses. Changing the dependent variable from ternary {Republican, Democrat, Neither} to binary {Republican, Democrat} also does not affect the conclusions (Table A8)

Prior studies have shown that the effects of Presidential ads decay quickly, albeit remaining significant for several weeks (Hill et al. 2013). Moreover, Bartels 2014 has found that persuasion and motivated reasoning may take place on different time frames. Thus, one might question whether my results equating mobilization and backlash are due to the choice of a particular window. By adjusting my baseline 4-week window to estimate both 2- and 6-week windows, I confirm that my patterns hold, while corroborating that the effects decay swiftly (Table A9).

As discussed above, matching individuals from adjacent counties is my preferred method. However, I might also match by distance, pairing an individual with all other individuals within a specified distance (albeit from the same state but different DMAs). A radius of 100km yields a number of pairs comparable to the baseline. I also investigate a tighter radius of 50km which greatly restricts the sample. Table A10 shows that the results, while weaker when restricted to the much smaller 50km sample, nonetheless retain the original character.

My baseline classification of the partisan affiliation of respondents classifies those who report leaning Democrat or leaning Republican as belonging to the independent category.

Table A11 reports the baseline results when I reclassify leaners as partisans. I can also drop the Nielsen ratings weightings and measure ad exposure by the raw number of ads shown in the DMA over the window in question. Table A12 shows the results retain their original character.

Because interviews are conducted at different dates, subjects will have different information sets for reasons other than differential ad exposure. While differences in interview dates between members of a pair should be orthogonal to the issues of interest, I check to be sure. First, I control for the date of the interview. Second, I rerun the main specification on the subset of pairs who were interviewed in the same week (Tables A13 and A14). Similarly, one may worry that within-pair differences in demographic markers such as gender and education may not be safely orthogonal. While these are controls in the baseline regressions, here too I can restrict to pairs with the same level of education, the same gender, or the same race. The character of the results remains unaffected (Tables A15).[15]

4 Discussion

By looking at individual voters rather than aggregates, I can see that, rather than persuading those who identify with the opposing party, ads reinforce a viewer's existing partisan inclinations. My most striking result is that the mobilization of supporters is evenly balanced by the backlash from the opposition. This may, as hypothesized by Hillygus 2005, explain why the net effects studied by previous scholars are small and unstable. Perhaps more surprisingly, there is no significant net persuasion of independent voters. Together, these results highlight the critical importance of targeting one's natural supporters, questioning the wisdom of targeting programs with an ideologically balanced viewership, such as the local news (as Fowler, Franz, and Ridout 2021 note is common practice), and point to the

[15] I am grateful to an anonymous reviewer for the suggestions discussed in this paragraph.

potential attractiveness of social media.[16]

These results fit with prior findings in the literature on political communication noting that motivated reasoning from partisans on both sides implies that viewing of information during a campaign results in increased polarization of attitudes (Meffert et al. 2006; Taber, Cann, and Kucsova 2009). They are congruent with what Broockman and Kalla 2020 term the "partisan intoxication" thesis of Achen and Bartels 2016: that voters resist crossing partisan lines.[17] My results on the effect of ads on policy positions are also strongly consistent with Zaller's receive-accept-sample framework in which messages are accepted contingent on prior beliefs. Rather than persuasive messages, political ads contain mostly cues that activate the viewer's partisan predisposition, leading to either adoption or rejection of the issue position discussed. The backlash that I document is a particularly strong example of this contingent nature of acceptance.

Interestingly, the recent literature on endogenous media bias with a rational audience can produce results with a similar flavor. Gentzkow and Shapiro 2006 presume that viewers, when uncertain about the quality of news, rationally judge the quality of the source by the degree to which it matches their preconceptions. In their model, consumers purchase the news and would not purchase from the media outlet that, because it does not match their priors, they perceive to be lower quality. Thus, there is no room for backlash. In the case of political advertising on television, however, the viewer is subjected to both types of messages as the unintended consequence of viewing habits. Upon receipt of conflicting messages, a

[16] I say "potential" because recent studies show that despite an apparently greater ability to target, political advertising via social media may not be terribly effective (Broockman and Green 2014.)

[17] Broockman and Kalla 2020 find behavior at odds with the partisan intoxication thesis. While appreciating their rigorous experiment, I note it concerns a single issue in a lab setting and is thus not irreconcilable with my broader results.

rational viewer will lend greater weight to the message arriving from the source that matches her priors (see Gerber and Green 1999, p200 for a development of this point). Indeed, in a binary message space, she will presume the source matching her priors is the truth and conclude the other source is incorrect. This would lead to precisely the dynamics I observe where ads from either party simply reinforce ideological predispositions.

Surprisingly, the degree of political awareness, as captured by various self-reported measures of political participation and news consumption, was not, in my sample, related to the strength of the effects I observed. I suspect that, unlike op-eds and other elite discourse, 30-second campaign ad spots are deliberately produced such that the overwhelming majority of the audience understands the cues contained therein. The fact that ads for other federal races are just as potent as ads for the candidate in question supports the notion that it is not candidate-specific information (on valence dimensions, say) that is being communicated. Rather, viewers would seem to be learning (or re-learning) about the party.

I noted above that transforming the results of Broockman and Kalla 2020 into persuasion rates delivers clear evidence of asymmetric updating. Nonetheless, as they point out, the sign of the effect they find is determined by the identity of the sender rather than that of the receiver. In other words, they find no evidence of the backlash that is so clear and consistent in my results. I suspect the difference is due to the difference in character between the 30-second campaign ads I analyze and the experimental manipulations they design which consisted of two or three sentences of informative text to be read by the respondent. Their factual statements, sitting in black-and-white text without further cues, while accepted at a rate depending on partisan affiliation, did not cue partisan sentiment and thus did not engender backlash.

Spenkuch and Toniatti 2018 find that while overall turnout is not affected by advertising, the partisan composition of the electorate does respond to the partisan mix in advertising, to the advantage of the party showing more ads. This would suggest that ads from one's own party make one more likely to vote whereas ads from the opposition party make one less likely to vote. I do find modestly differential effects of Democratic ads that could be consistent with their finding.

Meanwhile, Sides, Vavreck, and Warshaw 2021 find that ads have a larger effect on down-ballot races which they interpret as the receipt of information. I have shown evidence that ads, by consolidating and triggering viewers' partisan identities, lead to greater straight-ticket voting intention for Federal offices. I hypothesize that voters, reminded of their partisan identities, are more likely to vote in races that, absent a partisan motive, might not have interested them.

Huber and Arceneaux 2007 directly ask whether advertising exposure leads voters to adopt their party's positions (their Table 2b), looking at four policy questions from the 2000 Presidential election: personal social security accounts, school vouchers, universal healthcare for children, and patient rights to sue HMOs. In stark contrast to my results, they find inconsistent partisan reinforcement. I suspect their results differ from mine because the partisan position on their four proposals is somewhat difficult to infer from basic partisan principles. By contrast, CCES questions on banning assault weapons, legalizing abortion, the right of gay couples to marry, and increasing patrols on the US-Mexico border are all quite straightforward. Thus, their results might be reconciled with mine to suggest that partisan reinforcement via campaign advertising is limited by the complexity of a policy position.

If partisanship is so easily cued by political advertisements, my results might seem to suggest such ads as a source of polarization. However, I note that previous studies find the effects of advertising are short-lived and my results do not contradict this view. For instance, the effect of ad exposure smoothly declines as the window lengthens from 2 weeks to 4 weeks to 6 weeks (Table A9). Given the extreme concentration of such ads in the weeks before an election, it seems more likely that such ads briefly activate a latent polarization that is generated by other factors. On the other hand, this does not preclude the possibility that a steady diet of such ads may have a cumulative effect akin to the Fox News effect noted by DellaVigna and Kaplan 2007.

My results, which make clear that the weak net effects of television ads hide powerful counter-veiling mobilization of subgroups, clearly demonstrate the enormous potential benefits of targeting in the context of television viewing. This may be why the frontier of political advertising is the delivery via social media of spots that are micro-targeted according to age, gender, ethnicity, zip code, and browsing habits, which can all be used to infer partisan affiliation. Yet current studies suggest that advertising via social media comes with other disadvantages (Coppock, Green, and Porter 2022) likely connected to the manner in which social media are consumed via active selection and dismissal of unwanted information, as opposed to the relatively passive, focused attention given to TV. It is thus of great relevance that streaming services such as Hulu, Netflix, and Amazon Video, whose content is consumed in a manner more akin to television, have collected a great deal of information about their customers and are exploring targeted advertising.

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Table 1: Policy questions (2016)

Policy Proposal (2016)	**CCES code**	**Party Favoring**
Require background checks for all gun sales	330a	Democratic
Prohibit publishing gun owners' addresses	330b	Republican
Ban assault rifles	330d	Democratic
Easier concealed carry permits	330e	Republican
Legalize tax-paying, non-felony immigrants	331_1	Democratic
Increase US-Mexico border patrols	331_2	Republican
Legalize child immigrants graduated from hs	331_3	Democratic
Deport more illegal immigrants	331_7	Republican
Make abortion always legal	332a	Democratic
Abortion legal only for rape, incest, danger to mother	332b	Republican
All abortions illegal after 20 weeks	332c	Republican
Allow employers to deny coverage of abortion	332d	Republican
Empower EPA to regulate C02	333a	Democratic
Raise fuel stds from 25mpg to 35mpg	333b	Democratic
Require renewables in electricity	333c	Democratic
Strengthen enforcement of Clean Air/Water Act	333d	Democratic
No mandatory minimum sentences for non-violent	334a	Democratic
Require police to wear body-cams	334b	Democratic
Increase # of police by 10%	334c	Republican
Harsher sentences for 3rd time offenders	334d	Republican
Legalize gay marriage	335	Democratic

Table 2: Partisan Balance Tests

Dependent variable:	**Binary Party identification [1 = Dem, 2 = Rep]**					
	[1]	[2]	[3]	[4]	[5]	[6]
Greater Total President Ad Exposure	-0.00253 (0.0126)					
Greater Total Senate Ad Exposure		-0.0196 (0.0117)				
Greater Total House Ad Exposure			-0.0103 (0.0116)			
Greater Democratic President Ad Exposure				-0.00863 (0.0143)		
Greater Democratic Senate Ad Exposure					-0.0202 (0.0117)	
Greater Democratic House Ad Exposure						0.00137 (0.0125)
Constant	1.426** (0.0092)	1.433** (0.0084)	1.430** (0.0084)	1.430** (0.0101)	1.434** (0.0085)	1.424** (0.0091)
Observations	627,546	693,320	693,758	524,222	682,998	622,506
R-squared	0	0	0	0	0	0

Regressions of party id (1 = Dem, 2 = Rep) on an indicator of whether the respondent received more or fewer of the ads in question than the respondent with which he is paired. First I look at which member of the pair received more total ads [1]-[3]. Second, I look at which member of the pair received more ads from Democratic campaigns [4]-[6]. Statistical significance would suggest that ads are being targeted at one member of the pair, which would violate my identification strategy. In no case do I find significance. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$

Table 3: The Respondents Summary Statistics

Variable	**Obs**	**Mean**	**Std. dev.**	**Min**	**Max**
gender	60,491	1.54	0.50	1	2
nonwhite	60,491	0.25	0.43	0	1
age	60,491	50.28	16.80	18	99
HS or 2yr degree	60,491	0.66	0.47	0	1
4 year BA	60,491	0.21	0.41	0	1
Graduate degree	60,491	0.12	0.33	0	1
Democrat	60,491	0.36	0.48	0	1
Republican	60,491	0.26	0.44	0	1
Independent	60,491	0.28	0.45	0	1
Other	60,491	0.04	0.20	0	1
Not sure	60,491	0.05	0.23	0	1

Table 4: Ad Exposure (minutes), 4-week window

	Pre-election wave			Post-election wave		
Variable	**Obs**	**Mean**	**SD**	**Obs**	**Mean**	**SD**
President						
Total	60,162	10.9	24.8	60,491	24.4	38.5
Dem	60,162	7.1	15.4	60,491	14.2	22.4
Rep	60,162	3.8	10.4	60,491	10.2	17.6
Dem - Rep	60,162	3.2	8.6	60,491	4.1	11.8
Senate						
Total	60,162	12.8	23.3	60,491	25.2	38.7
Dem	60,162	6.6	12.4	60,491	13.9	20.9
Rep	60,162	6.2	11.4	60,491	11.3	19.0
Dem - Rep	60,162	0.4	4.8	60,491	2.6	9.7
House						
Total	60,162	9.2	20.4	60,491	22.3	35.2
Dem	60,162	5.2	11.4	60,491	12.2	19.1
Rep	60,162	4.0	9.6	60,491	10.1	17.3
Dem - Rep	60,162	1.3	5.3	60,491	2.0	9.5

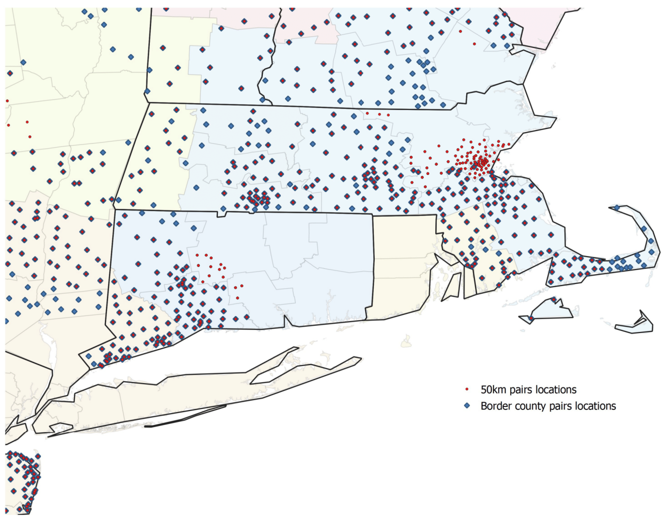


Figure 1: Comparing sample-building methods

Each dot is the location of one CCES respondent who has been selected for a pair. Lines are not drawn connecting the two members of a pair so as not to obscure the locations. Using the 50km method generates a different set of pairs than does the border counties method, as shown by the difference in spatial distribution between the large blue and small red dots.

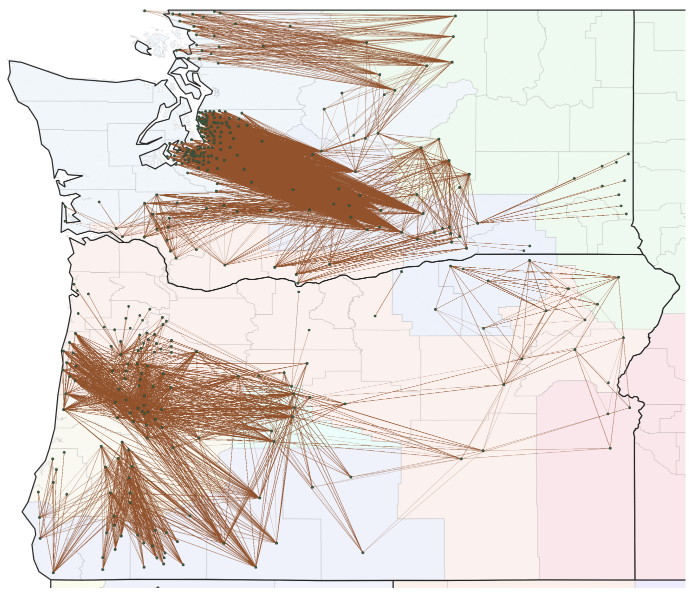
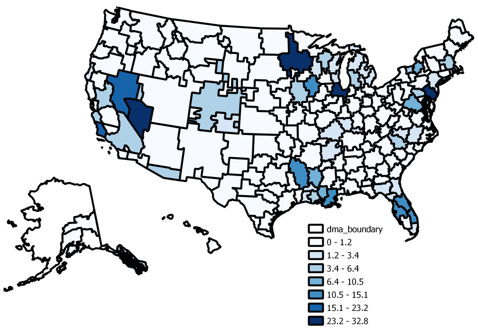
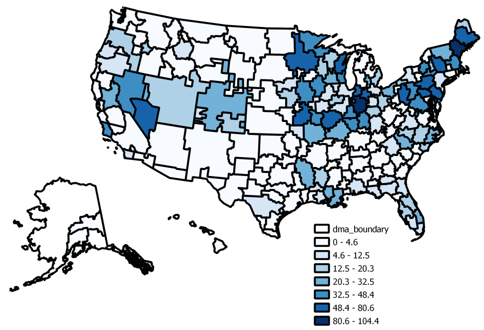


Figure 2: Pairwise connections

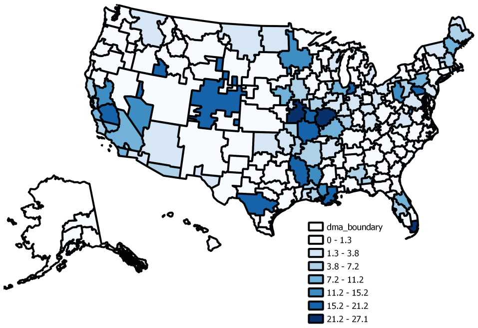
Border county pairs method. Each line represents one pair in the sample, with the endpoints showing the locations of the paired individuals. Pairs may not cross state lines but must cross media market boundaries, which are indicated by lightly shaded background color.



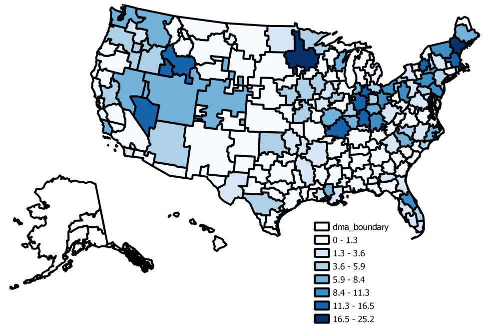
(a) abortion



(b) taxes



(c) immigration



(d) budget deficit

Figure 3: Minutes of exposure to ads on four different subjects over the last 60 days of the 2016 campaign. Because different campaigns chose to emphasize different issues, each issue provides identifying variation from distinct geographic boundaries.

Vote Choice

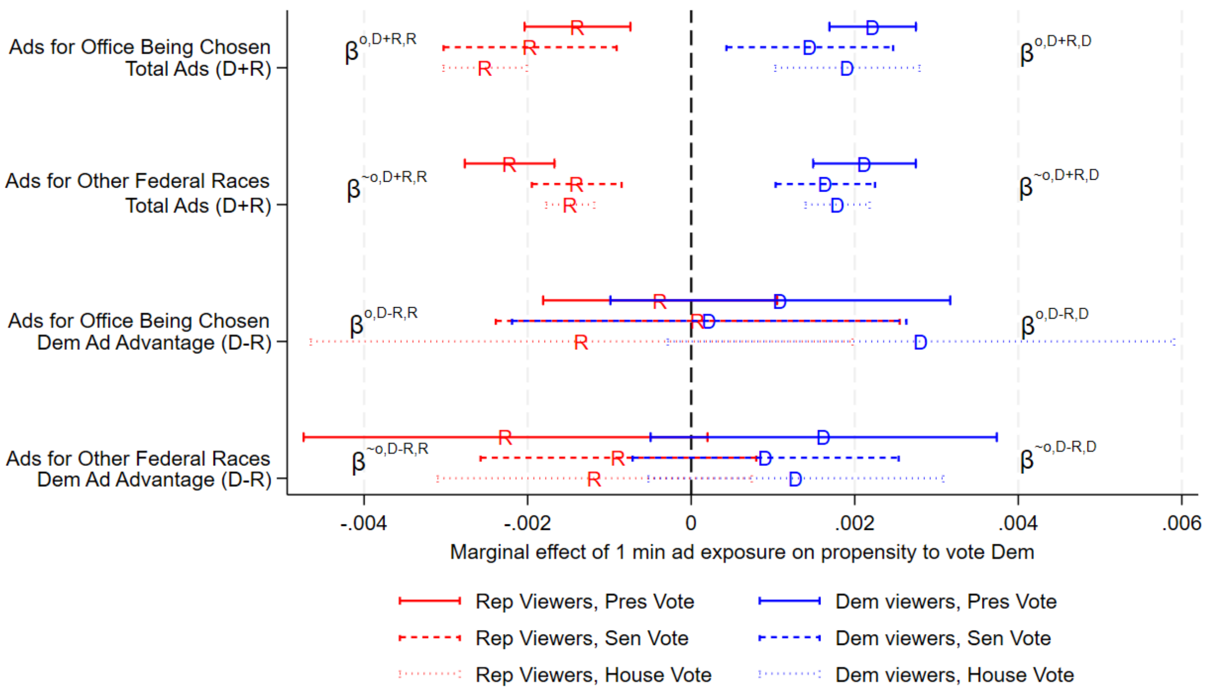


Figure 4: This graph represents the point estimates and 95% confidence intervals from the same 8 dependent variables across 3 separate regressions. The left-hand side variables are the respondents' vote choice for the Presidential, Senatorial, and House elections. I have stacked the regressions to show the congruity of results across the three offices. Confidence intervals are solid lines for Presidential vote choice, dashed lines for Senatorial vote choice, and dotted lines for House vote choice. Thus the solid lines come from one regression, the dashed lines from a second, the dotted lines from a third. The independent variables are four different measures of ad exposure, labeled on the y-axis. Each of these types of exposure is allowed to have a different effect on viewers according to the viewer's partisan affiliation, thus separate coefficients for viewers who lean Democrat and those who lean Republican. Point estimates are indicated by a D or an R depending on the partisan leaning of the viewer. The point estimate is the marginal effect of one minute of ad exposure on the viewer's propensity to vote for the Democrat. Formal tests of whether the coefficients for Democratic and Republican viewers are statistically equal are reported in Table A2.

Table 5: Persuasion Rates by Office and Identity of Viewer

Panel A: Post-election survey	Persuasion rates arising from viewing 20 minutes of ads (from either party) cumulative exposure 4 weeks prior to the election					
	ads for the race in question prefs over candidates for			ads for other federal races prefs over candidates for		
	President	Senate	House	President	Senate	House
Viewers who identify with						
Democrats	12.4	6.0	8.9	11.9	6.8	8.3
Republican	-10.2	-8.3	-15.3	-16.3	-5.9	-9.0
Independents	1.0	-1.1	-0.3	-0.1	0.7	0.3

Panel B: Pre-election survey	Persuasion rates arising from viewing 10 minutes of ads (from either party) cumulative exposure 4 weeks prior to the survey					
	ads for the race in question prefs over candidates for			ads for other federal races prefs over candidates for		
	President	Senate	House	President	Senate	House
Viewers who identify with						
Democrats	13.4	6.4	8.2	21.9	4.5	7.3
Republican	-8.9	-6.7	-14.9	-18.6	-5.3	1.4
Independents	0.5	-0.6	-0.2	-0.5	0.5	0.1

Panel A displays the persuasion rates from 20 minutes of ads during the 4 weeks prior to the election. Panel B displays the persuasion rates from 10 minutes of ads during the 4 weeks prior to the pre-election survey. These are each roughly the mean exposure for the period in question (table 4). Persuasion rates are defined as the percentage of unconvinced who will vote for the Democratic (+) or Republican (-) candidate as a result of the treatment. They are calculated from coefficients in tables A1 and A4.

Table 6: Implied persuasion rates from Broockman and Kalla 2020

	Coefficients		Base Rates		Persuasion Rates	
Sender	Dem	Rep	Dem	Rep	Dem	Rep
Pro-Trump	0.024	0.011	0.04	0.92	2.5	13.75
Pro-Biden	-0.018	-0.039	0.94	0.06	-30	-4.14894

Persuasion rates: % of unconvinced who will vote for the Democratic (+) or Republican (-) candidate as a result of the treatment. Rates calculated by the author using coefficients from Broockman and Kalla's Table 6 plus base rates from Igielnik, Keeter, and Hartig n.d.

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Candidate Preference (Pre-Election)

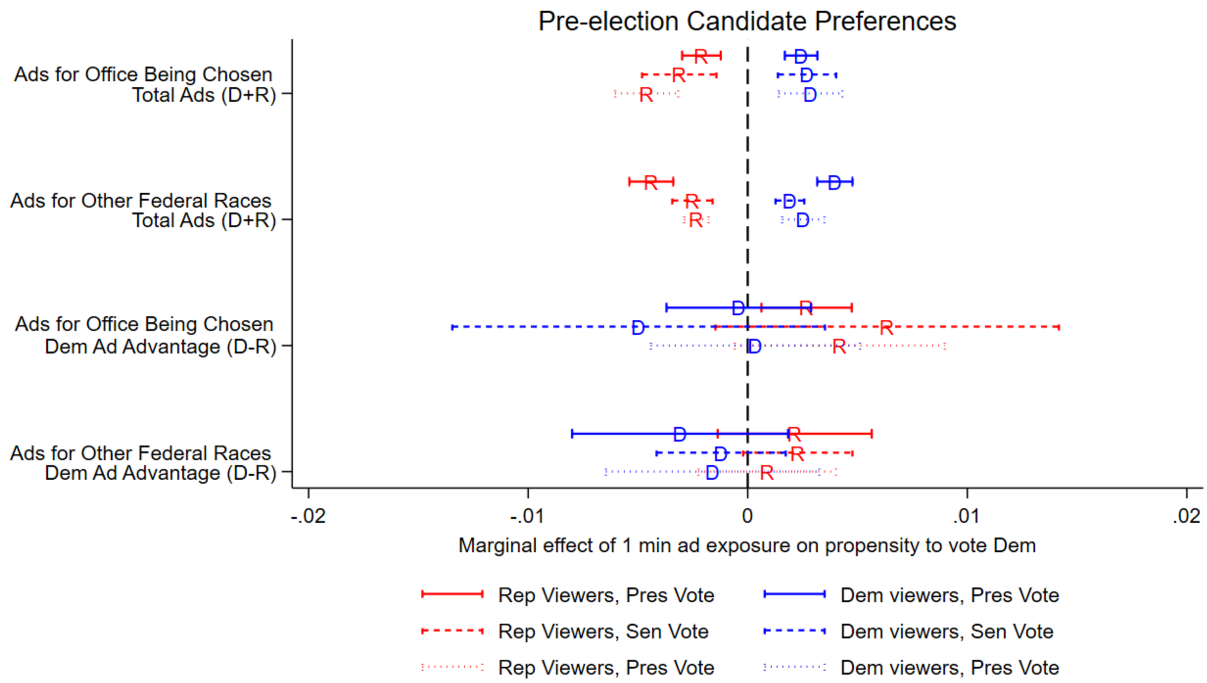

Figure 5: This graph represents the point estimates and 95% confidence intervals from the same 8 dependent variables across 3 separate regressions. The left-hand side variables are the respondents' indications of candidate preference regarding their Presidential, Senatorial, and House races during pre-election interviews. I have stacked the regressions to show the congruity of results across the three offices. Confidence intervals are solid lines for Presidential preference, dashed lines for Senatorial preference, and dotted lines for House preference. Thus the solid lines come from one regression, the dashed lines from a second, and the dotted lines from a third. The independent variables are four different measures of ad exposure, labeled on the y-axis. Each of these types of exposure is allowed to have a different effect on viewers according to the viewer's partisan affiliation, thus separate coefficients for viewers who lean Democrat and those who lean Republican. Point estimates are indicated by a D or an R depending on the partisan leaning of the viewer. The point estimate is the marginal effect of one minute of ad exposure on the viewer's propensity to prefer the Democrat.

Policy Preferences

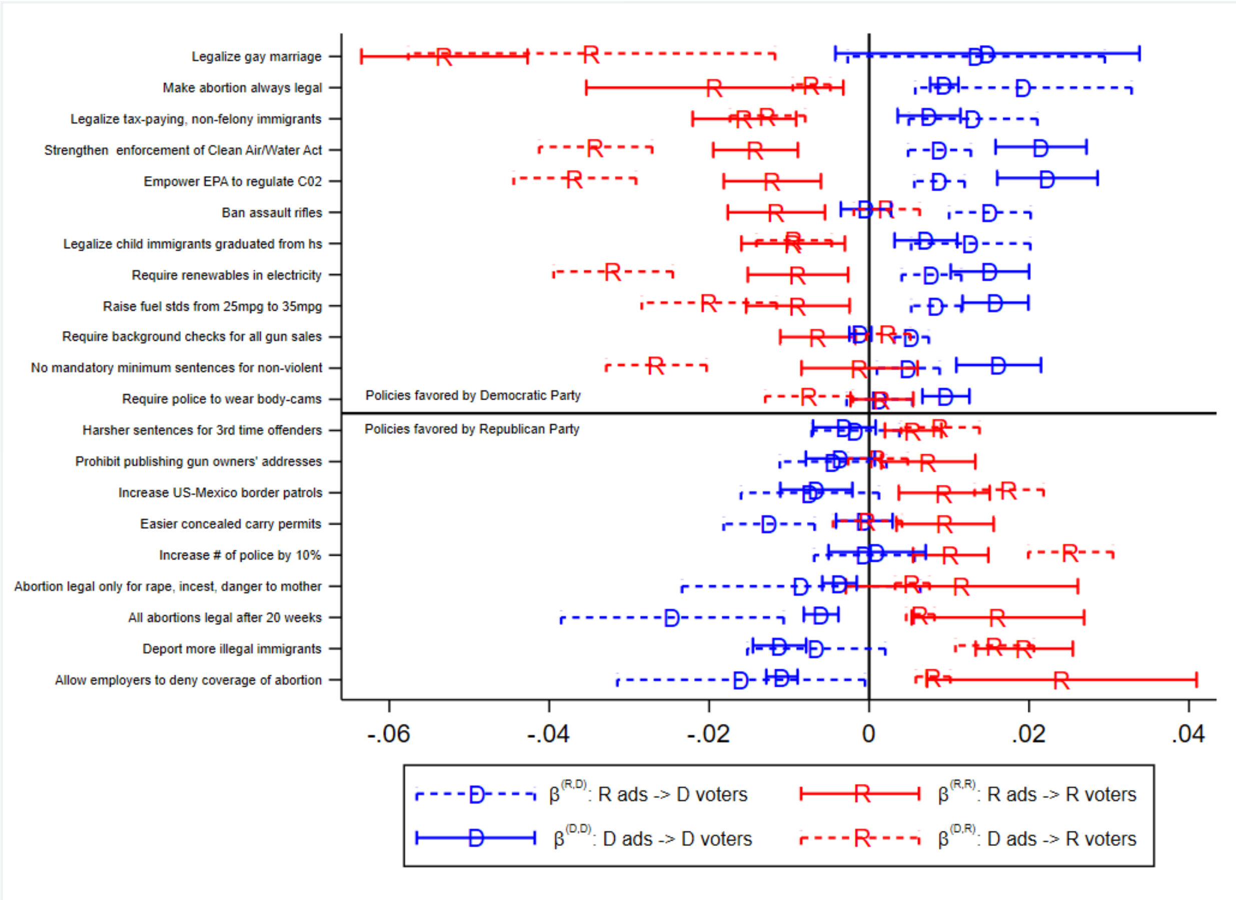

Figure 6: This graph represents the point estimates and 95% confidence intervals from 21 separate regressions: one for each of the policies listed along the y-axis. In each case, the binary dependent variable was support for the policy in question and the independent variable was exposure to ads on the relevant broad topic (e.g. gun control, immigration, etc.) For each policy, the party issuing the ad {D,R} was interacted with the partisan identity of the viewer {D,R,I} resulting in six coefficients. For clarity of exposition, I have left out the Independents, displaying the other four combinations. The marker label {D,R} and color {blue,red} correspond to the partisan identity of the viewer. The line style corresponds to the party showing the ad: solid lines are the party with which the viewer affiliates while short-dashed lines are the other party. Policies have been ordered according to the point estimate of the effect of Republican ads on Republican viewers. The resulting ordering happens to naturally segregate those policies supported by the Democratic party (upper) from those supported by the Republican party (lower).

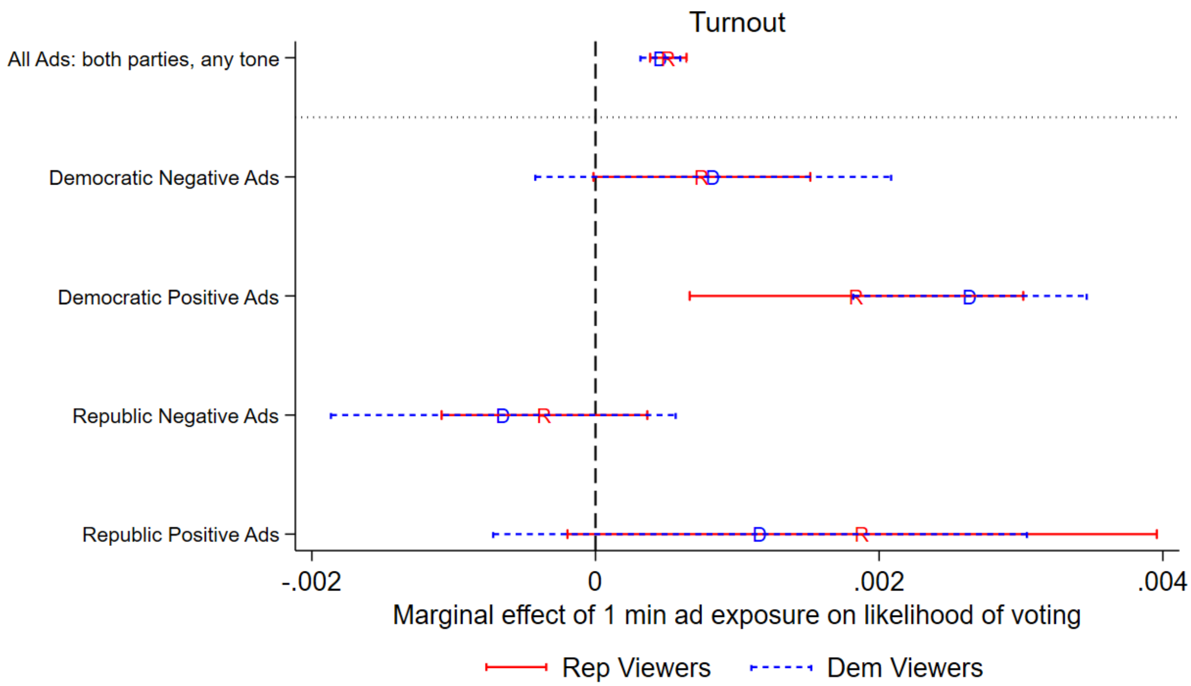

Figure 7: This graph represents the point estimates and 95% confidence intervals from two separate regressions. In each case, the left-hand side variable is the respondents' turnout. The first regression, whose coefficient of interest is illustrated above the horizontal dotted line, has a single measure of total ad exposure. A second regression, whose coefficients are illustrated below the dotted line, breaks this into four different ad types. In each regression, I allow the independent variable(s) to have separate effects on viewers according to their partisan affiliation. Coefficients and formal tests are available in Table A6

.

A Appendix: for online publication

A note about tables A1, A4, and A7 through A15 . These tables report estimates from specifications detailed in Equation 2.

The dependent variable in each column tracks which party the respondent voted for in the race in question. For each specification in this table, there are four independent variables: total ads run from either party by candidates for the race in question, total ads run from either party by candidates for other federal races, the net democratic advantage in ads run by candidates for the race in question, the net democratic advantage in ads run by candidates for other federal races. Each of these four independent variables is interacted with three dummy variables indicating the partisan affiliation of the respondent, thus delivering twelve coefficients. The top coefficient in the left column gives the estimated marginal effect on a Democratic-affiliated voter's likelihood of voting for the Democratic candidate for President of viewing another minute of ads– from either party– by candidates for Presidency. This is positive indicating that viewing ads from either side make this viewer more likely to vote Democrat. The next coefficient down is the marginal effect of any ad for President on a Republican-affiliated voter's likelihood of voting Democrat for President. This is negative, showing ads from either side make this voter more likely to vote Republican. The top panel shows that more ads viewers to vote their partisan leanings. The second panel shows this is true whether the ads are for the race in question (Pres) or for other races (Senate and House). The third and fourth panels show that it doesn't matter whether the ads are produced by Republicans or Democrats.

Table A1: Vote Choice (post)

Dep Var:	**Net Democratic vote -1, 0, 1**		
Race:	**Pres**	**Senate**	**House**
Effect of Total Ads (D+R)			
Ads from own race			
Effect on Democratic Voters	0.00222**	0.00145**	0.00191**
	(0.00027)	(0.00052)	(0.00045)
Effect on Republican Voters	-0.00139**	-0.00197**	-0.00252**
	(0.00033)	(0.00054)	(0.00026)
Effect on Independents	0.00051	-0.000508	-0.000158
	(0.00026)	(0.00026)	(0.00029)
Ads from other Federal Races			
Effect on Democratic Voters	0.00212**	0.00164**	0.00179**
	(0.00032)	(0.00031)	(0.00020)
Effect on Republican Voters	-0.00222**	-0.00140**	-0.00148**
	(0.00028)	(0.00028)	(0.00015)
Effect on Independents	-3.13E-05	0.000318*	1.42E-04
	(0.00020)	(0.00015)	(0.00012)
Effect of Democratic Ad Advantage (D-R)			
Ads from own race			
Effect on Democratic Voters	0.00109	0.000216	0.00281
	(0.00106)	(0.00123)	(0.00158)
Effect on Republican Voters	-0.000377	0.0000839	-0.00134
	(0.00073)	(0.00126)	(0.00169)
Effect on Independents	-0.00215	0.00165*	0.000319
	(0.00118)	(0.00080)	(0.00127)
Ads from other Federal Races			
Effect on Democratic Voters	0.00162	0.000905	0.00128
	(0.00108)	(0.00083)	(0.00092)
Effect on Republican Voters	-0.00227	-0.000892	-0.00118
	(0.00126)	(0.00086)	(0.00098)
Effect on Independents	-0.000392	-0.00132	-0.00136*
	(0.00079)	(0.00070)	(0.00064)
Observations	670,152	670,152	670,152
R-squared	0.146	0.106	0.119

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** $p<0.01$, * $p<0.05$. See note at beginning of appendix for interpretation.

Table A2: Vote Choice: Tests of Equality of Coefficients on Rep and Dem Voters

Measure	**Subject of Ads**	**Dependent Var**	**F-stat**	**P value**
Total Ads	for President	Presidential Vote	45.58	0.000
Total Ads	for Senate	Senate Vote	11.74	0.001
Total Ads	for House	House Vote	56.40	0.000
Total Ads	for S or H	Presidential Vote	61.21	0.000
Total Ads	for P or H	Senate Vote	30.67	0.000
Total Ads	for P or S	House Vote	138.15	0.000
Dem Adv.	for President	Presidential Vote	0.97	0.325
Dem Adv.	for Senate	Senate Vote	0.00	0.953
Dem Adv.	for House	House Vote	1.93	0.166
Dem Adv.	for S or H	Presidential Vote	3.21	0.075
Dem Adv.	for P or H	Senate Vote	1.65	0.201
Dem Adv.	for P or S	House Vote	2.19	0.141

These are tests of the coefficients in Figure 4. Each row of this table corresponds to one of the rows of Figure 4 in the same order from top to bottom. Each row is a test of whether the marginal effects on Republican and Democratic viewers are the same for a particular set of ads. The ads in question are specified in the two leftmost columns. The third column specifies which of the three dependent variables (three regressions). In reference to the figure, this is whether the point estimates from the same row, illustrated with "D" and "R", are statistically equal. The top half of the table shows that the effect of ads in general is always dependent on the viewer. The bottom half of the table shows that which party shows the ads is not important. These are consistent with my view that mobilization equals backlash.

Table A3: Straight Ticket Voting

Vote Choice (post)	
Dependent Variable: Straight Ticket Voting	
Effect on Democrats	
By Tone	
Total Positive Ads	0.000863*
	(0.00042)
Total Negative Ads	0.000108
	(0.00011)
Effect on Republicans	
By Tone	
Total Positive Ads	0.00151**
	(0.00045)
Total Negative Ads	-0.00004
	(0.00013)
Effect on Independents	
By Tone	
Total Positive Ads	-0.00061
	(0.00040)
Total Negative Ads	-0.00008
	(0.00010)
Observations	670,152
R-squared	0.011

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** p<0.01, * p<0.05

Table A4: Candidate Preferences (Pre)

Dependent Variable:	**Net Democratic vote -1, 0, 1**		
Race:	**President**	**Senate**	**House**
Effect of Total Ads (D+R)			
Ads from own race			
Effect on Democratic Voters	0.00243**	0.00270**	0.00286**
	(0.00038)	(0.00068)	(0.00074)
Effect on Republican Voters	-0.00211**	-0.00312**	-0.00460**
	(0.00045)	(0.00086)	(0.00073)
Effect on Independents	0.000494	-0.00057	-0.00015
	(0.00042)	(0.00048)	(0.00058)
Ads from other Federal Races			
Effect on Democratic Voters	0.00397**	0.00192**	0.00253**
	(0.00041)	(0.00033)	(0.00049)
Effect on Republican Voters	-0.00439**	-0.00252**	-0.00234**
	(0.00051)	(0.00047)	(0.00028)
Effect on Independents	-0.00047	4.39E-04	0.000141
	(0.00036)	(0.00035)	(0.00025)
Effect of Democratic Ad Advantage (D-R)			
Ads from own race			
Effect on Democratic Voters	-0.00041	-0.00497	0.000352
	(0.00168)	(0.00433)	(0.00243)
Effect on Republican Voters	0.00268*	0.00635	0.00419
	(0.00105)	(0.00399)	(0.00244)
Effect on Independents	-0.00079	0.000198	0.00067
	(0.00093)	(0.00145)	(0.00156)
Ads from other Federal Races			
Effect on Democratic Voters	-0.00308	-0.00121	-0.0016
	(0.00251)	(0.00150)	(0.00248)
Effect on Republican Voters	0.00214	0.00228	0.000892
	(0.00179)	(0.00127)	(0.00160)
Effect on Independents	0.000703	-0.0007	-0.00121
	(0.00097)	(0.00067)	(0.00117)
Observations	715,494	715,494	715,494
R-squared	0.113	0.069	0.076

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** p<0.01, * p<0.05. See note at beginning of appendix for interpretation

Table A5: Ad Effects on Turnout by Ad Type

Dependent Variable: Binary Validated Turnout		
Total Ads * Democratic Viewer		0.000380**
		(0.00006)
Total Ads * Republican Viewer		0.000426**
		(0.00006)
Total Ads * Independent		0.000119
		(0.00009)
Positive Ads * Democratic Viewer	0.00208**	
	(0.00039)	
Positive Ads * Republican Viewer	0.00178**	
	(0.00051)	
Positive Ads * Independent Viewer	0.000787*	
	(0.00040)	
Negative Ads * Democratic Viewer	8.40E-05	
	(0.00013)	
Negative Ads * Republican Viewer	0.00024	
	(0.00013)	
Negative Ads * Independent Viewer	3.71E-05	
	(0.00016)	
Observations	670,152	670,152
R-squared	0.024	0.022

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses ** p<0.01, * p<0.05

Table A6: Ad Effects on Turnout by Sponsoring Party

Dependent Variable: Binary Validated Turnout 0,1	
Republican Positive Ads	
* Democratic Viewer	0.00116
	(0.00096)
* Republican Viewer	0.00188
	(0.00106)
* Independent Viewer	0.000195
	(0.00094)
Republican Negative Ads	
* Democratic Viewer	-0.00065
	(0.00062)
* Republican Viewer	-0.00036
	(0.00037)
* Independent Viewer	-0.00063
	(0.00061)
Democratic Positive Ads	
* Democratic Viewer	0.00264**
	(0.00042)
* Republican Viewer	0.00184**
	(0.00060)
* Independent Viewer	0.00129*
	(0.00056)
Democratic Negative Ads	
* Democratic Viewer	0.000831
	(0.00064)
* Republican Viewer	0.000752
	(0.00039)
* Independent Viewer	0.000663
	(0.00050)
Observations	670,152
R-squared	0.025
Tests of differential effectiveness.	
Republican Positive Ads	0.371
Republican Negative Ads	0.627
Democratic Positive Ads	0.043
Democratic Negative Ads	0.880

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses ** $p<0.01$, * $p<0.05$. The last four rows contain tests of whether a particular source and type of ad has differential effects on turnout that could affect the partisan composition of the electorate. The only one that is significant is that for democratic positive ads, indicating that they do have a larger mobilization effect on Democratic-leaning viewers than they have a backlash effect of raising turnout among Republican-leaning viewers.

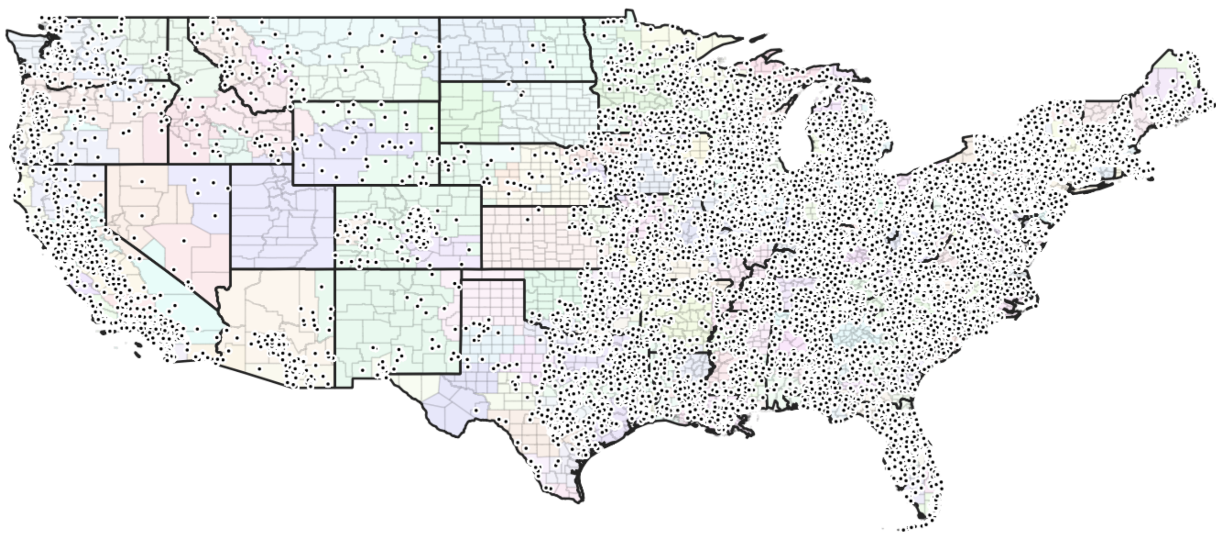

Figure A.1: The full sample of pairs: border county method

Each dot is the location of one CCES respondent who has been selected for a pair. Lines are not drawn connecting the two members of a pair so as not to obscure the locations.

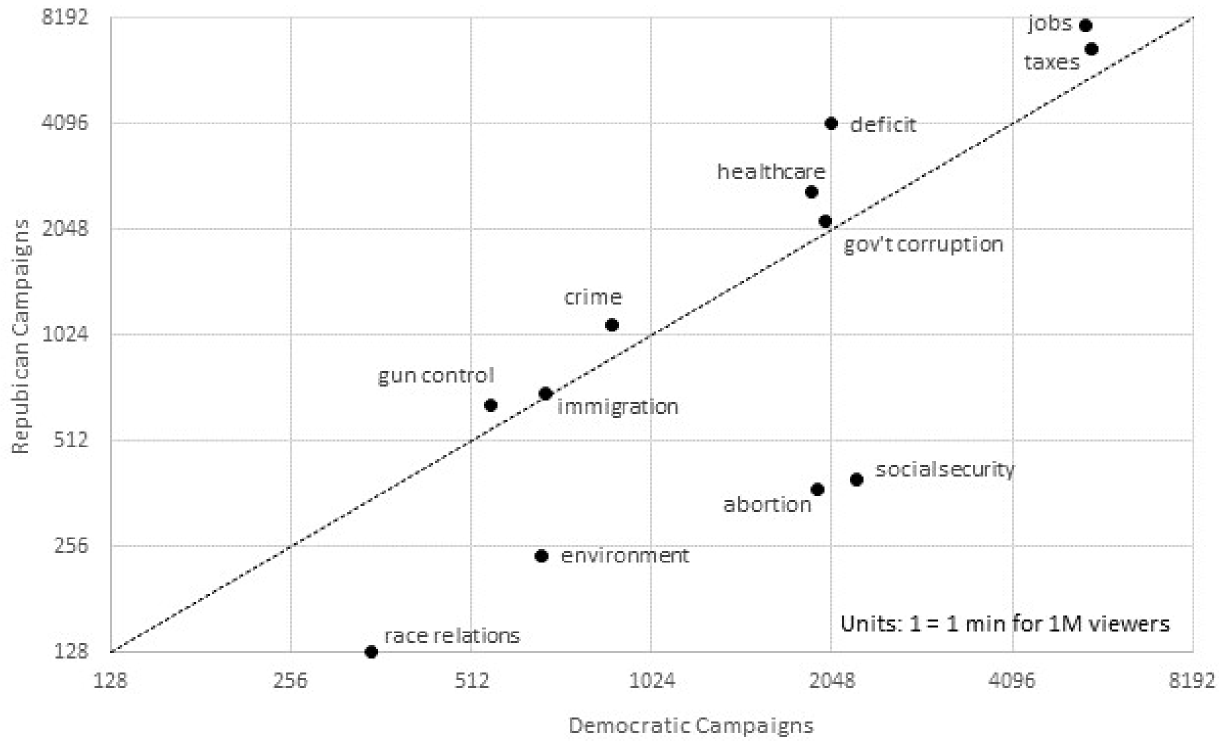

Figure A.2: Advertising by issue and party

I tabulate, across all campaigns, during the 4 weeks before the election, the total minutes of advertising from each party on each issue, as coded by WMP. I weight each minute by the number of viewers as estimated by Nielsen ratings. Ads that are coded as mentioning multiple issues are counted multiple times. Issues falling above (below) the dotted 45-degree line are those to which Republican (Democratic) campaigns devoted more attention. The axes are log-scale and the units are millions of viewer-minutes.

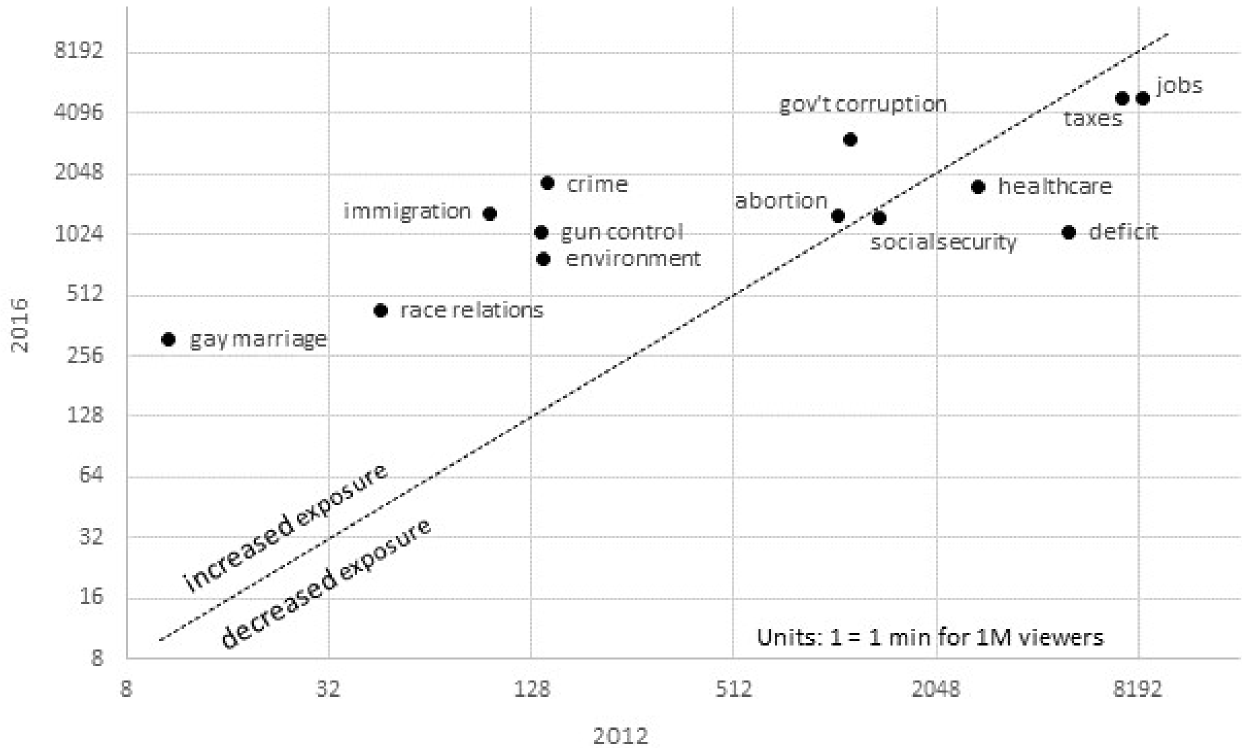

Figure A.3: Advertising by issue change over time

I tabulate, across all campaigns, during the 4 weeks before the election, the total minutes of advertising on each issue, as coded by WMP. I weight each minute by the number of viewers as estimated by Nielsen ratings. Ads that are coded as mentioning multiple issues are counted multiple times. Issues falling above (below) the dotted 45-degree line are those to which 2016 (2012) campaigns devoted more attention. The axes are log-scale and the units are millions of viewer-minutes.

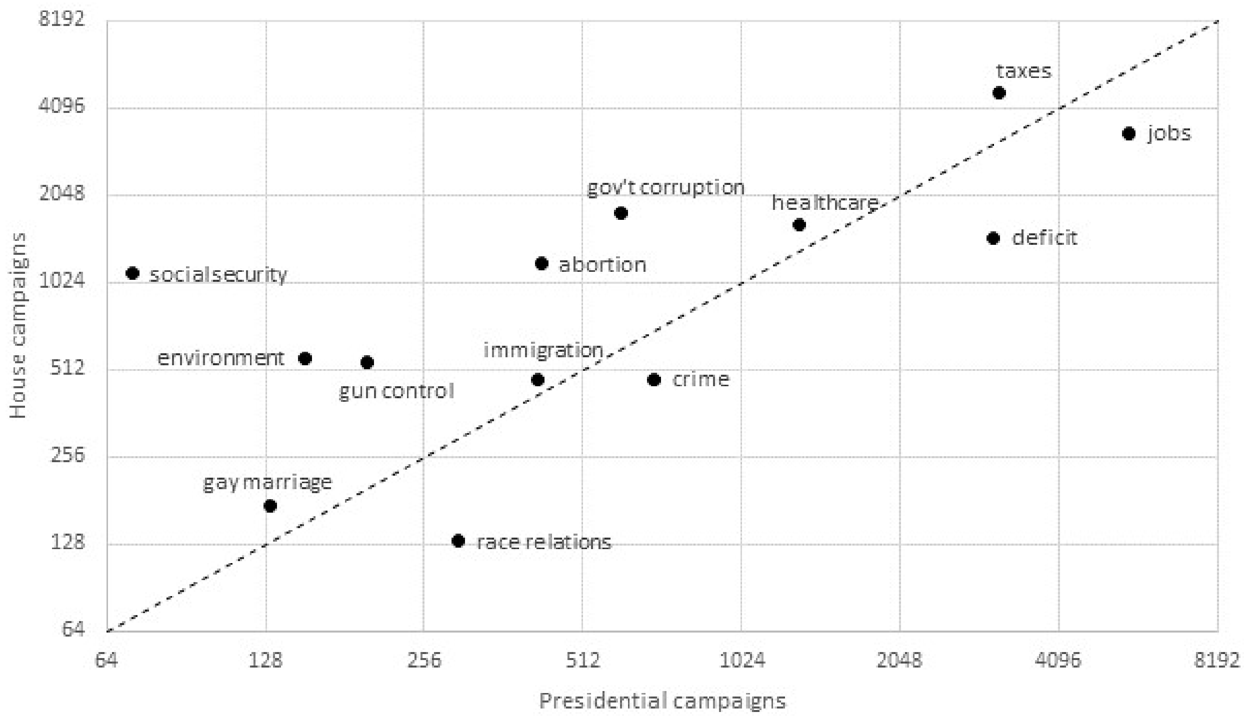

Figure A.4: Advertising by issue and office

I tabulate, across all campaigns for a specific office, during the 4 weeks before the election, the total minutes of advertising on each issue, as coded by WMP. I weight each minute by the number of viewers as estimated by Nielsen ratings. Ads that are coded as mentioning multiple issues are counted multiple times. Issues falling above (below) the dotted 45-degree line are those to which House (Presidential) campaigns devoted more attention. The axes are log-scale and the units are millions of viewer-minutes.

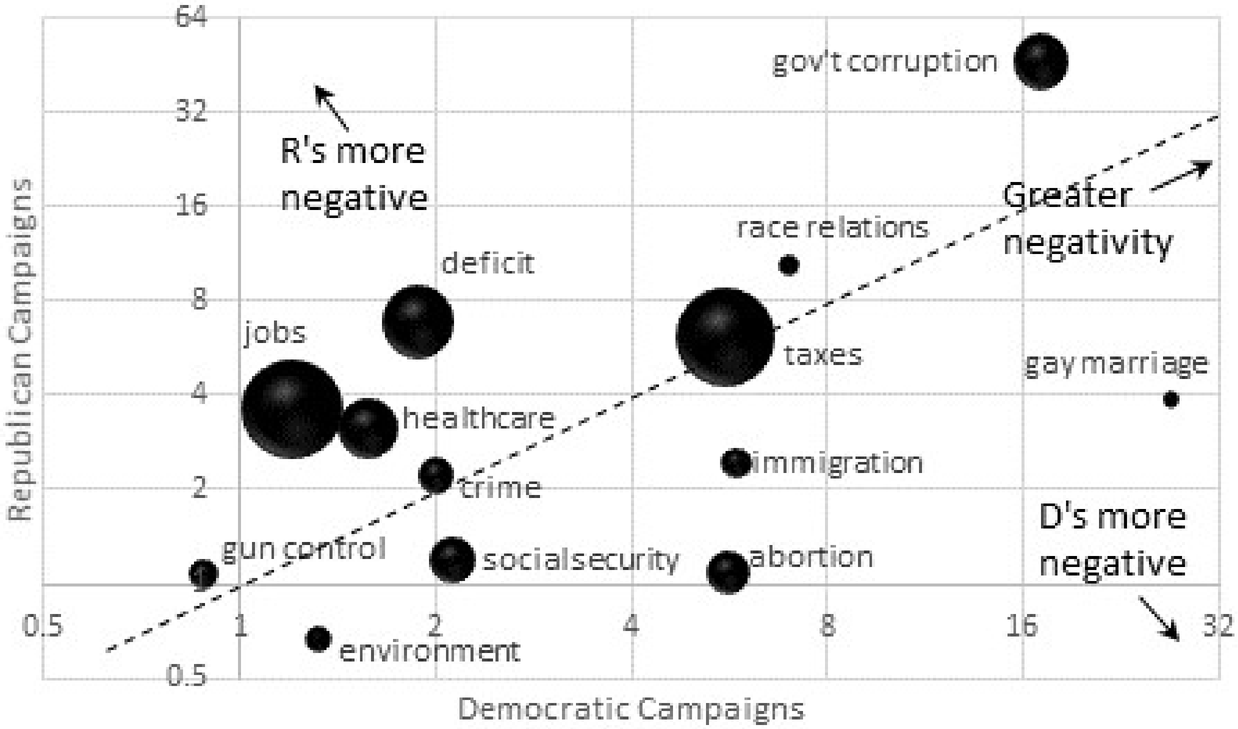

Figure A.5: Advertising tone by issue and party

I tabulate, across all campaigns, during the 4 weeks before the election, the total minutes of advertising on each issue, as coded by WMP. Ads that are coded as mentioning multiple issues are counted multiple times. I weight each minute by the number of viewers as estimated by Nielsen ratings. The size of a bubble is proportional to the number of viewer-minutes on that issue. I calculate, for each party, the ratio of positive to negative ads. Negative ads predominate for both parties across virtually all dimensions. These ratios are plotted on the x- and y-axes. Thus, issues falling above (below) the dotted 45-degree line are those to which Republican (Democratic) campaigns were more negative. The axes are log-scale and the units are millions of viewer-minutes.

Table A7: Robustness to use of Ordered Logit

Vote Choice (post)						
Dep Var:	**Net Democratic vote {-1, 0, 1}**					
Estimator	**OLS (multi-way FE)**			**Ordered Logit (1-way FE)**		
Race:	**Pres**	**Senate**	**House**	**Pres**	**Senate**	**House**
Effect of Total Ads (D+R)						
Ads from own race						
Dem Voters	0.00222**	0.00145**	0.00191**	0.00711**	0.00549**	0.00628**
	(0.00027)	(0.00052)	(0.00045)	(0.00067)	(0.00075)	(0.00081)
Rep Voters	-0.00139**	-0.00197**	-0.00252**	-0.00399**	-0.00726**	-0.00841**
	(0.00033)	(0.00054)	(0.00026)	(0.00049)	(0.00072)	(0.00063)
Independents	0.00051	-0.00051	-0.00016	0.00169*	-0.00214**	-0.00044
	(0.00026)	(0.00026)	(0.00029)	(0.00082)	(0.00080)	(0.00082)
Ads from other Federal Races						
Dem Voters	0.00212**	0.00164**	0.00179**	0.00671**	0.00619**	0.00596**
	(0.00032)	(0.00031)	(0.00020)	(0.00045)	(0.00052)	(0.00041)
Rep Voters	-0.00222**	-0.00140**	-0.00148**	-0.00696**	-0.00528**	-0.00463**
	(0.00028)	(0.00028)	(0.00015)	(0.00038)	(0.00049)	(0.00031)
Independents	-3.1E-05	0.000318*	0.000142	-9.4E-05	0.00121*	4.94E-04
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Effect of Democratic Ad Advantage (D-R)						
Ads from own race						
Dem Voters	0.00109	0.000216	0.00281	0.00338	-0.00059	0.00934**
	(0.00106)	(0.00123)	(0.00158)	(0.00193)	(0.00254)	(0.00278)
Rep Voters	-0.00038	8.39E-05	-0.00134	-0.00089	0.000646	-0.00464
	(0.00073)	(0.00126)	(0.00169)	(0.00140)	(0.00225)	(0.00293)
Independents	-0.00215	0.00165*	0.000319	-0.00694**	0.00722*	0.000674
	(0.00118)	(0.00080)	(0.00127)	(0.00247)	(0.00315)	(0.00359)
Ads from other Federal Races						
Dem Voters	0.00162	0.000905	0.00128	0.00518**	0.00368*	0.00392*
	(0.00108)	(0.00083)	(0.00092)	(0.00174)	(0.00184)	(0.00185)
Rep Voters	-0.00227	-0.00089	-0.00118	-0.00674**	-0.00314*	-0.00352*
	(0.00126)	(0.00086)	(0.00098)	(0.00133)	(0.00140)	(0.00149)
Independents	-0.00039	-0.00132	-0.00136*	-0.00124	-0.00521*	-0.00446*
	(0.00079)	(0.00070)	(0.00064)	(0.00214)	(0.00241)	(0.00201)
Observations	670,164	670,164	670,164	670,736	670,736	670,736
R-squared	0.146	0.106	0.119			

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** p<0.01, * p<0.05

Table A8: Robustness to Binary Vote Choice

Vote Choice (post)						
Dep Var:	**Democratic vote {-1, 1}**					
Estimator:	**OLS (two-way FE)**			**Logit (one-way FE)**		
Race:	**Pres**	**Senate**	**House**	**Pres**	**Senate**	**House**
Effect of Total Ads (D+R)						
Ads from own race						
Dem Voters	0.00242**	0.00142*	0.00196**	0.00779**	0.00583**	0.00662**
	(0.00029)	(0.00056)	(0.00050)	(0.00072)	(0.00079)	(0.00088)
Rep Voters	-0.00133**	-0.00190**	-0.00273**	-0.00380**	-0.00746**	-0.00920**
	(0.00040)	(0.00062)	(0.00029)	(0.00052)	(0.00085)	(0.00068)
Independents	0.000546	-0.000551*	-0.00029	0.00182*	-0.00272**	-0.00098
	(0.00033)	(0.00027)	(0.00032)	(0.00091)	(0.00095)	(0.00095)
Ads from other Federal Races						
Dem Voters	0.00222**	0.00156**	0.00188**	0.00703**	0.00656**	0.00641**
	(0.00032)	(0.00035)	(0.00021)	(0.00045)	(0.00053)	(0.00044)
Rep Voters	-0.00231**	-0.00132**	-0.00145**	-0.00727**	-0.00555**	-0.00462**
	(0.00030)	(0.00033)	(0.00018)	(0.00043)	(0.00055)	(0.00035)
Independents	-7.4E-05	0.000342*	0.000135	-0.00027	0.00160*	4.94E-04
	(0.00022)	(0.00015)	(0.00014)	(0.00056)	(0.00068)	(0.00049)
Effect of Democratic Ad Advantage (D-R)						
Ads from own race						
Dem Voters	0.00134	-0.00039	0.00331	0.00417*	-0.00253	0.0116**
	(0.00115)	(0.00119)	(0.00182)	(0.00201)	(0.00256)	(0.00305)
Rep Voters	-0.00028	5.07E-05	-0.00126	-0.00065	-1.2E-05	-0.00443
	(0.00079)	(0.00138)	(0.00198)	(0.00147)	(0.00272)	(0.00356)
Independents	-0.00216	0.00142	0.00102	-0.00703*	0.00688	0.00361
	(0.00134)	(0.00083)	(0.00149)	(0.00282)	(0.00392)	(0.00436)
Ads from other Federal Races						
Dem Voters	0.00153	0.00155	0.00176	0.00498**	0.00707**	0.00581**
	(0.00111)	(0.00091)	(0.00105)	(0.00179)	(0.00194)	(0.00201)
Rep Voters	-0.00271	-0.00102	-0.00138	-0.00825**	-0.00435**	-0.00454*
	(0.00143)	(0.00097)	(0.00119)	(0.00146)	(0.00160)	(0.00179)
Independents	-0.00073	-0.00158*	-0.00171*	-0.00252	-0.00769*	-0.00614*
	(0.00089)	(0.00075)	(0.00073)	(0.00238)	(0.00299)	(0.00241)
Observations	419,219	489,383	408,696	419,570	489,744	409,042
R-squared	0.149	0.101	0.124			

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** p<0.01, * p<0.05

Table A9: Robustness to Window Length

Vote Choice (post)						
Dep Var:	**Net Democratic vote {-1, 0, 1}**					
Horizon		**2 week**			**6 week**	
Race:	**Pres**	**Senate**	**House**	**Pres**	**Senate**	**House**
Effect of Total Ads (D+R)						
Ads from own race						
Dem Voters	0.00364**	0.00260**	0.00345**	0.00159**	0.00102*	0.00149**
	(0.00047)	(0.00091)	(0.00066)	(0.00023)	(0.00042)	(0.00037)
Rep Voters	-0.00231**	-0.00328**	-0.00433**	-0.000979**	-0.00144**	-0.00197**
	(0.00051)	(0.00089)	(0.00044)	(0.00027)	(0.00045)	(0.00021)
Independents	0.000909*	-0.000863*	-5.4E-05	0.000367	-0.00037	-0.00013
	(0.00043)	(0.00042)	(0.00043)	(0.00020)	(0.00021)	(0.00024)
Ads from other Federal Races						
Dem Voters	0.00365**	0.00263**	0.00295**	0.00163**	0.00130**	0.00129**
	(0.00051)	(0.00048)	(0.00034)	(0.00027)	(0.00021)	(0.00013)
Rep Voters	-0.00376**	-0.00231**	-0.00237**	-0.00171**	-0.00105**	-0.00105**
	(0.00043)	(0.00046)	(0.00025)	(0.00023)	(0.00020)	(0.00012)
Independents	6.22E-05	0.000617**	0.000251	-3.3E-05	0.000261*	1.20E-04
	(0.00029)	(0.00021)	(0.00020)	(0.00017)	(0.00012)	(0.00009)
Effect of Democratic Ad Advantage (D-R)						
Ads from own race						
Dem Voters	0.00349*	0.00141	0.00163	0.00108	0.000298	0.000992
	(0.00140)	(0.00147)	(0.00110)	(0.00114)	(0.00071)	(0.00095)
Rep Voters	-0.00389**	-0.00088	-0.00167	-0.00168	-0.0004	-0.00088
	(0.00144)	(0.00158)	(0.00097)	(0.00130)	(0.00063)	(0.00094)
Independents	-0.00145	-0.00219*	-0.00229**	-0.00014	-0.00091	-0.00104*
	(0.00105)	(0.00092)	(0.00081)	(0.00068)	(0.00055)	(0.00051)
Ads from other Federal Races						
Dem Voters	0.000928	0.00217	0.00528*	0.000494	8.67E-05	0.00168
	(0.00142)	(0.00188)	(0.00205)	(0.00097)	(0.00124)	(0.00151)
Rep Voters	-0.00022	-0.00089	-0.00284	0.000182	0.000263	-0.00053
	(0.00095)	(0.00196)	(0.00195)	(0.00064)	(0.00127)	(0.00142)
Independents	-0.00372**	0.00256*	0.000368	-0.00144	0.00160*	3.42E-05
	(0.00130)	(0.00129)	(0.00185)	(0.00101)	(0.00072)	(0.00103)
Observations	670,164	670,164	670,164	670,164	670,164	670,164
R-squared	0.156	0.114	0.127	0.14	0.101	0.113

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** p<0.01, * p<0.05

Table A10: Robustness to Method of Generating Pairs

Vote Choice (post)						
Dep Var:	**Net Democratic vote {-1, 0, 1}**					
Sample	**Pairs within 100km**			**Pairs within 50km**		
Race:	**Pres**	**Senate**	**House**	**Pres**	**Senate**	**House**
Effect of Total Ads (D+R)						
Ads from own race						
Dem Voters	0.00146*	0.00345**	0.000661	0.0017	0.00501**	-0.00141
	(0.00070)	(0.00051)	(0.00096)	(0.00108)	(0.00035)	(0.00123)
Rep Voters	7.81E-05	-0.00499**	0.000762	0.00476**	-0.00525**	0.00176
	(0.00089)	(0.00060)	(0.00131)	(0.00135)	(0.00102)	(0.00224)
Independents	-0.00016	0.000115	-0.00025	0.00198*	0.000533	-0.00175*
	(0.00074)	(0.00049)	(0.00057)	(0.00081)	(0.00070)	(0.00083)
Ads from other Federal Races						
Dem Voters	0.00179**	-0.00109*	0.00174**	0.00219**	-0.00199**	0.00300**
	(0.00025)	(0.00051)	(0.00041)	(0.00027)	(0.00031)	(0.00046)
Rep Voters	-0.00282**	0.00204**	-0.00240**	-0.00372**	0.00401**	-0.00215**
	(0.00043)	(0.00052)	(0.00045)	(0.00054)	(0.00107)	(0.00078)
Independents	-8.9E-05	-0.00061	0.000128	-0.000593**	0.000171	0.00148**
	(0.00020)	(0.00048)	(0.00035)	(0.00019)	(0.00063)	(0.00039)
Effect of Democratic Ad Advantage (D-R)						
Ads from own race						
Dem Voters	-0.00633**	0.00501**	0.00743*	-0.00512*	0.0031	0.0065
	(0.00222)	(0.00129)	(0.00296)	(0.00235)	(0.00186)	(0.00488)
Rep Voters	0.00652**	-0.00508**	-0.0120*	0.00640**	-0.00249	-0.0182
	(0.00187)	(0.00130)	(0.00513)	(0.00154)	(0.00305)	(0.00907)
Independents	-0.00292**	-4.5E-06	-0.00256	-0.00360**	0.00155	-0.00759
	(0.00110)	(0.00091)	(0.00387)	(0.00126)	(0.00187)	(0.00395)
Ads from other Federal Races						
Dem Voters	0.00378**	0.000957	0.000411	0.00122	0.00214	0.000434
	(0.00070)	(0.00134)	(0.00157)	(0.00195)	(0.00246)	(0.00117)
Rep Voters	-0.00749**	0.000226	-0.00268	-0.00962**	-0.00485*	-0.00264
	(0.00091)	(0.00155)	(0.00223)	(0.00128)	(0.00224)	(0.00325)
Independents	-0.00197*	0.00117	-0.00180*	-0.00234	0.000569	-0.00189
	(0.00090)	(0.00153)	(0.00073)	(0.00140)	(0.00184)	(0.00095)
Observations	642,293	642,293	642,293	66,173	66,173	66,173
R-squared	0.156	0.114	0.131	0.159	0.115	0.128

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** p<0.01, * p<0.05

Table A11: Robustness to Definition of Partisan Affiliation

Vote Choice (post)						
Dep Var:	**Net Democratic vote {-1, 0, 1}**					
Partisan Affil:	**Independent includes Leans**			**Independent excludes Leans**		
Race:	**Pres**	**Senate**	**House**	**Pres**	**Senate**	**House**
Effect of Total Ads (D+R)						
Ads from own race						
Dem Voters	0.00222**	0.00145**	0.00191**	0.00217**	0.00138**	0.00185**
	(0.00027)	(0.00052)	(0.00045)	(0.00024)	(0.00051)	(0.00044)
Rep Voters	-0.00139**	-0.00197**	-0.00252**	0.000368	-0.00045	-0.00041
	(0.00033)	(0.00054)	(0.00026)	(0.00034)	(0.00032)	(0.00044)
Independents	0.00051	-0.00051	-0.00016	-0.00125**	-0.00201**	-0.00259**
	(0.00026)	(0.00026)	(0.00029)	(0.00037)	(0.00053)	(0.00029)
Ads from other Federal Races						
Dem Voters	0.00212**	0.00164**	0.00179**	0.00223**	0.00162**	0.00183**
	(0.00032)	(0.00031)	(0.00020)	(0.00029)	(0.00026)	(0.00018)
Rep Voters	-0.00222**	-0.00140**	-0.00148**	(0.000)	0.000	(0.000)
	(0.00028)	(0.00028)	(0.00015)	(0.00027)	(0.00023)	(0.00020)
Independents	-3.1E-05	0.000318*	0.000142	-0.00235**	-0.00129**	-0.00143**
	(0.00020)	(0.00015)	(0.00012)	(0.00032)	(0.00031)	(0.00016)
Effect of Democratic Ad Advantage (D-R)						
Ads from own race						
Dem Voters	0.00109	0.000216	0.00281	0.00118	0.00102	0.00309
	(0.00106)	(0.00123)	(0.00158)	(0.00119)	(0.00114)	(0.00157)
Rep Voters	-0.00038	8.39E-05	-0.00134	-0.00188	0.00194	1.42E-05
	(0.00073)	(0.00126)	(0.00169)	(0.00132)	(0.00105)	(0.00120)
Independents	-0.00215	0.00165*	0.000319	-0.00022	0.000315	-0.00108
	(0.00118)	(0.00080)	(0.00127)	(0.00076)	(0.00119)	(0.00183)
Ads from other Federal Races						
Dem Voters	0.00162	0.000905	0.00128	0.00206*	0.00119	0.00135
	(0.00108)	(0.00083)	(0.00092)	(0.00092)	(0.00080)	(0.00091)
Rep Voters	-0.00227	-0.00089	-0.00118	-0.00018	-0.00073	-0.00033
	(0.00126)	(0.00086)	(0.00098)	(0.00076)	(0.00068)	(0.00095)
Independents	-0.00039	-0.00132	-0.00136*	-0.00214	-0.00098	-0.00125
	(0.00079)	(0.00070)	(0.00064)	(0.00127)	(0.00087)	(0.00109)
Observations	670,152	670,152	670,152	665,137	665,137	665,137
R-squared	0.146	0.106	0.119	0.183	0.128	0.147

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** p<0.01, * p<0.05

Table A12: Robustness to (non)Weighting of Ads

Vote Choice (post)						
Dep Var: Net Democratic vote {-1, 0, 1}						
Race:	**Pres**	**Senate**	**House**	**Pres**	**Senate**	**House**
Effect of Total Ads (D+R)						
Ads from own race						
Dem Voters				0.00119**	0.00101**	0.00105**
				(0.00013)	(0.00035)	(0.00033)
Rep Voters				-0.000798**	-0.00136**	-0.00163**
				(0.00018)	(0.00032)	(0.00020)
Independents				0.000499**	-0.000243	-0.000279
				(0.00014)	(0.00016)	(0.00021)
Ads from other Federal Races						
Dem Voters				0.00124**	0.000988**	0.00105**
				(0.00016)	(0.00020)	(0.00012)
Rep Voters				-0.00158**	-0.000838**	-0.000913**
				(0.00017)	(0.00018)	(0.00015)
Independents				-0.000121	0.000331**	0.000189*
				(0.00014)	(0.00013)	(0.00009)
Effect of Democratic Ad Advantage (D-R)						
Ads from own race						
Dem Voters	0.00551**	0.00219**	0.00257	1.44E-05	-0.000547	0.000953
	(0.00049)	(0.00079)	(0.00170)	(0.00046)	(0.00085)	(0.00152)
Rep Voters	-0.00473**	-0.00251**	-0.00488**	0.00082	0.00046	-0.00153
	(0.00087)	(0.00076)	(0.00136)	(0.00079)	(0.00079)	(0.00131)
Independents	-0.00022	0.000236	-0.000329	-0.000865	0.000359	-5.07E-05
	(0.00077)	(0.00026)	(0.00082)	(0.00075)	(0.00036)	(0.00091)
Ads from other Federal Races						
Dem Voters	0.00386**	0.00353**	0.00445**	0.000651	-0.00034	-1.47E-05
	(0.00052)	(0.00060)	(0.00054)	(0.00071)	(0.00065)	(0.00063)
Rep Voters	-0.00451**	-0.00405**	-0.00419**	-0.000862	0.000138	-6.47E-05
	(0.00068)	(0.00035)	(0.00057)	(0.00090)	(0.00064)	(0.00089)
Independents	-0.000229	-0.000564	-0.00042	-0.000251	-0.00101*	-0.000819
	(0.00049)	(0.00044)	(0.00042)	(0.00051)	(0.00048)	(0.00044)
Observations	670,152	670,152	670,152	670,152	670,152	670,152
R-squared	0.106	0.068	0.082	0.147	0.107	0.118

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** $p<0.01$, * $p<0.05$

Table A13: Robustness to Interview Dates (candidate preferences)

Dep Var:	Net Democratic vote {-1, 0, 1}		
Race:	**Presidential**		
Effect of Total Ads (D+R)			
Ads from own race			
Effect on Democratic Voters	0.00243**	0.00249**	0.00300**
	(0.00038)	(0.00040)	(0.00048)
Effect on Republican Voters	-0.00211**	-0.00213**	-0.00181**
	(0.00045)	(0.00046)	(0.00052)
Effect on Independents	0.000494	0.00026	0.000742
	(0.00042)	(0.00039)	(0.00042)
Ads from other Federal Races			
Effect on Democratic Voters	0.00397**	0.00401**	0.00416**
	(0.00041)	(0.00046)	(0.00057)
Effect on Republican Voters	-0.00439**	-0.00427**	-0.00472**
	(0.00051)	(0.00050)	(0.00059)
Effect on Independents	-0.000472	-0.000377	-0.000602
	(0.00036)	(0.00036)	(0.00047)
Effect of Democratic Ad Advantage (D-R)			
Ads from own race			
Effect on Democratic Voters	-0.000408	0.00013	-0.00121
	(0.00168)	(0.00159)	(0.00171)
Effect on Republican Voters	0.00268*	0.00312**	0.00296*
	(0.00105)	(0.00102)	(0.00118)
Effect on Independents	-0.000786	-5.55E-10	-9.59E-05
	(0.00093)	(0.00081)	(0.00102)
Ads from other Federal Races			
Effect on Democratic Voters	-0.00308	-0.00298	-0.00478
	(0.00251)	(0.00249)	(0.00297)
Effect on Republican Voters	0.00214	0.0023	0.00327
	(0.00179)	(0.00173)	(0.00177)
Effect on Independents	0.000703	0.000647	0.000655
	(0.00097)	(0.00102)	(0.00130)
Control for interviewdate	N	Y	N
Pairs interviewed same week	N	N	Y
Observations	715,494	666,419	210,555
R-squared	0.113	0.115	0.113

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** p<0.01, * p<0.05

Table A14: Robustness to Interview Dates (vote choice)

Dep Var:	**Net Democratic vote {-1, 0, 1}**		
Race:	**Presidential**		
Effect of Total Ads (D+R)			
Ads from own race			
Effect on Democratic Voters	0.00222**	0.00207**	0.00228**
	(0.00027)	(0.00030)	(0.00029)
Effect on Republican Voters	-0.00139**	-0.00146**	-0.00126**
	(0.00033)	(0.00035)	(0.00038)
Effect on Independents	0.00051	0.000397	0.000666*
	(0.00026)	(0.00027)	(0.00032)
Ads from other Federal Races			
Effect on Democratic Voters	0.00212**	0.00237**	0.00218**
	(0.00032)	(0.00031)	(0.00029)
Effect on Republican Voters	-0.00222**	-0.00221**	-0.00233**
	(0.00028)	(0.00029)	(0.00030)
Effect on Independents	-0.0000313	2.58E-05	-1.34E-05
	(0.00020)	(0.00020)	(0.00022)
Effect of Democratic Ad Advantage (D-R)			
Ads from own race			
Effect on Democratic Voters	0.00109	0.00229*	0.00122
	(0.00106)	(0.00104)	(0.00111)
Effect on Republican Voters	-0.000377	-3.79E-06	-3.57E-05
	(0.00073)	(0.00086)	(0.00089)
Effect on Independents	-0.00215	-0.00143	-0.00301**
	(0.00118)	(0.00124)	(0.00116)
Ads from other Federal Races			
Effect on Democratic Voters	0.00162	0.00182	0.00147
	(0.00108)	(0.00116)	(0.00105)
Effect on Republican Voters	-0.00227	-0.0022	-0.00254
	(0.00126)	(0.00122)	(0.00132)
Effect on Independents	-0.000392	-0.000192	-0.000771
	(0.00079)	(0.00077)	(0.00086)
Control for interviewdate	N	Y	N
Pairs interviewed same week	N	N	Y
Observations	670,152	608,495	173,378
R-squared	0.146	0.163	0.141

Unreported controls include age, race, gender, education, and a constant. Robust standard errors in parentheses: ** p<0.01, * p<0.05

Table A15: Sample Restricted to Various Demographically Matched Pairs

Method of pairs:	All (baseline)	Same gender	Same nonwhite	Same 4yr college
Effect of Total Ads (D+R)				
Ads from own race				
Dem Voters	0.00212**	0.00210**	0.00204**	0.00203**
	(0.000319)	(0.000324)	(0.000309)	(0.000323)
Rep Voters	-0.00222**	-0.00223**	-0.00216**	-0.00212**
	(0.000278)	(0.000284)	(0.000269)	(0.000280)
Independents	-3.13E-05	-1.24E-05	-3.64E-05	-0.000154
	(0.000195)	(0.000198)	(0.000186)	(0.000217)
Ads from other Federal Races				
Dem Voters	0.00222**	0.00215**	0.00239**	0.00212**
	(0.000273)	(0.000285)	(0.000305)	(0.000310)
Rep Voters	-0.00139**	-0.00137**	-0.00140**	-0.00150**
	(0.000333)	(0.000337)	(0.000286)	(0.000316)
Independents	5.10E-04	4.60E-04	0.000588*	0.000485
	(0.000262)	(0.000259)	(0.000233)	(0.000247)
Effect of Democratic Ad Advantage (D-R)				
Ads from own race				
Dem Voters	0.00162	0.00139	0.00217	0.00153
	(0.001080)	(0.001030)	(0.001140)	(0.001280)
Rep Voters	-0.00227	-0.00236	-0.00153	-0.00122
	(0.001260)	(0.001230)	(0.001230)	(0.001160)
Independents	-3.92E-04	-2.64E-04	-3.50E-04	-0.000187
	(0.000787)	(0.000791)	(0.000716)	(0.000836)
Ads from other Federal Races				
Dem Voters	0.00109	0.00112	0.00159	0.00129
	(0.001060)	(0.000930)	(0.000982)	(0.001250)
Rep Voters	-0.000377	-0.00036	0.0000123	-0.000053
	(0.000733)	(0.000772)	(0.000739)	(0.000778)
Independents	-2.15E-03	-1.99E-03	-2.15E-03	-0.00206
	(0.001180)	(0.001150)	(0.001250)	(0.001420)
Observations	670,152	339,432	474,390	349,680
R-squared	0.146	0.147	0.128	0.144