Political Advertising and Election Results*

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Abstract

We study the persuasive effects of political advertising. Our empirical strategy exploits FCC regulations that result in plausibly exogenous variation in the number of impressions across the borders of neighboring counties. Applying this approach to uniquely detailed data on television advertisement broadcasts and viewership patterns during the 2004–12 presidential campaigns, our results indicate that total political advertising has almost no impact on aggregate turnout. By contrast, we find a positive and economically meaningful effect of advertising on candidates' vote shares. Taken at face value, our estimates imply that a standard deviation increase in the partisan difference in advertising raises the partisan difference in vote shares by about 0.5 percentage points. Evidence from a regression discontinuity design suggests that advertising affects election results by altering the partisan composition of the electorate.

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1. Introduction

The advent of television has had a profound impact on how politicians communicate with their constituents. While Harry S. Truman traveled over thirty thousand miles and shook over half a million hands during the 1948 presidential campaign, only four years later, Dwight D. Eisenhower leveraged the power of TV advertisements to reach a far greater audience at substantially lower cost. Today, political advertising is the primary method by which candidates reach out to voters in the United States. Leading up to the presidential election in 2012, candidates and their supporters aired more than 1.1 million TV ads (Wesleyan Media Project 2012; Washington Post 2012). Even during the preceding off-year congressional election, TV advertising accounted for between 40% and 50% of campaigns' budgets (Ridout et al. 2012).

Social scientists have long been interested in the consequences of political mass communication. Fearing that voters may be easily manipulated by self-interested agents, some equate persuasion with propaganda (e.g., Herman and Chomsky 1988; Lippmann 1922). Others note that even self-serving messages may further the democratic process by providing citizens with potentially valuable information about candidates and their competitors (see, for example, Bernays 1928 and Downs 1957). Despite the longstanding scholarly interest and the ubiquity of political advertising in modern democracies, our understanding of its effects remains incomplete.

A small well-identified literature documents large electoral effects in consolidating democracies (Da Silveira and De Mello 2011; Durante and Gutierrez 2014; Larreguy et al. 2017). As pointed out by Larreguy et al. (2017), however, political advertising in mature democracies is typically thought to have only a negligible impact. Commercials appear to be ineffective at engaging the electorate (Ashworth and Clinton 2007; Krasno and Green 2008), and their impact on individuals' opinions is extremely short-lived (Gerber et al. 2011). Taken at face value, these findings contradict campaigns' choices. Why allocate close to half of all available funds to a mode of campaigning that promises only minimal results?

In this paper, we reexamine the impact of political advertising on elections in the United States. Our findings are at odds with the conventional wisdom of minimal effects. While we do confirm previous null results with respect to turnout, we present evidence of a positive and economically meaningful impact of advertising on candidates' vote shares.

To estimate the causal effect of political advertising, we build on recent work by Shapiro (2016). Specifically, we exploit Federal Communications Commission (FCC) regulations that result in plausibly exogenous variation in the number of advertisements across county borders. The FCC grants media companies local broadcast rights for a set of counties called a demographic market area (DMA) or media market. Candidates, in turn, determine television-advertising strategies at the DMA level. By comparing *neighboring* counties that are in the same state but assigned to different media markets, our approach relies on thousands of regulation-induced discontinuities in the advertising exposure of constituents.[1]

In the political domain, nearly all ads are purchased at the DMA level (Goldstein and Freedman 2002). Yet, on average, a set of border counties constitutes only about 5% of markets' combined population. Since ad prices as well as campaigns' strategies are likely determined by aggregate, market-level factors, one would expect that a particular border county exerts only minimal influence on the decision of how much air time to buy in a given DMA. If correct, then differences in advertising intensity across neighboring counties that are assigned to different DMAs should be as good as random, especially after conditioning on counties' time-invariant features. As a partial test of this assumption, we show that observables explain only a trivial amount of the variation in advertising intensity across neighboring border counties. An F-test fails to reject the null hypothesis that electorates' observable characteristics are jointly uncorrelated with different measures of political advertising, with p-values ranging from 0.464 to 0.995.

We apply our identification strategy to uniquely detailed data for the 2004, 2008, and 2012 presidential elections. Instead of imputing viewership from self-reported media consumption or noisy cost estimates, we derive measures of how often each political ad was actually seen by using information on ad broadcasts combined with spot-level viewership data provided by The Nielsen Company. To evaluate existing claims about political advertising's impact on voter engagement, we study aggregate turnout as well as vote shares. While naïve estimates suggest that advertising plays an important role in mobilizing the electorate, our within-border pair results imply that the positive correlation between the number of advertisements and overall turnout is spurious. Our results are robust with respect to an array of different specifications, including alternative measures of advertising intensity and different time windows before the election.

After demonstrating that our empirical approach has the potential to detect spurious relationships in the raw data, we explore the impact of political advertising on actual votes. In stark contrast to the results with respect to aggregate turnout, we find that advertising has a nontrivial impact on candidates' vote shares. According to our estimates, a standard deviation increase in the partisan difference in advertising, i.e., the average citizen seeing about twenty-one more ads promoting one candidate rather than the other, increases the partisan difference in vote shares by about 0.5 percentage points.

[1] Our empirical approach is also closely related to previous work by Ansolabehere et al. (2006) and Snyder and Strömberg (2010), who exploit media market definitions to explore the effect of news-media coverage on the incumbency advantage and on political accountability, respectively.

We study the mechanisms behind this effect in a supplemental analysis relying on official turnout histories for millions of registered U.S. voters. To gauge the contribution of compositional changes of the electorate (i.e., the extensive margin) relative to effects on individuals' preferences and opinions (i.e., the intensive margin), we implement a regression discontinuity (RD) design that compares partisans who live nearby but on opposite sides of media market borders. Our RD evidence suggests that partisan differences in turnout depend on partisan differences in political advertising. The size of the RD estimates implies that compositional changes can explain much of the effect of advertising on vote shares. Although political advertising does not appear to lead to universally higher voter engagement, it alters the partisan composition of voters, which, in turn, affects election results.

2. Related Literature

Our paper contributes to a large body of work on the consequences of political mass communication (see, e.g., Zaller 1992). While the minimal effects thesis of Klapper (1960) dominated the literature until the late 1980s, more recent scholarship often reaches different, contradictory conclusions. Some, for instance, argue that political advertising enlarges the electorate by informing and engaging citizens (e.g., Freedman et al. 2004). Others, however, contend that the increasing use of negative advertisements hurts the democratic process, as it turns voters *away* from the polls (Ansolabehere and Iyengar 1995; Ansolabehere et al. 1999). Iyengar and Simon (2000) and Geys (2006) provide reviews of this literature, "which for the most part lacks compelling strategies for identifying causal effects" (DellaVigna and Gentzkow 2010, p. 650).

There are a handful of exceptions. The first is a large, randomized controlled trial by Gerber et al. (2011). Eleven months before the 2006 gubernatorial election in Texas, the authors randomly assigned the timing of an ad campaign across 18 media markets. Relying on a panel of opinion surveys, the evidence indicates a sizeable but fleeting impact on constituents' attitudes. Within one to two weeks, the campaign's effect had all but vanished.

Ultimately, our research design and results complement those of Gerber et al. (2011). Although we lack true randomization, we are able to study real-world election outcomes as opposed to self-declared attitudes and opinions. Moreover, we explore the effects of campaign advertising in a competitive environment, where average spending per media market is more than an order of magnitude higher than in the experiment of Gerber et al. (2011). Importantly, the results in this paper suggest that much of advertising's impact on vote shares is due to changes in the partisan composition of the electorate. This may explain why campaigns advertise so much—often months before the election—despite short-lived effects on individuals' opinions.

Another exception is a recent field experiment by Kendall et al. (2015), who collaborated with an Italian mayor to send voters randomized messages. Relative to the control group, voters who received campaign messages about the mayor's valence updated their beliefs and increased their support by about 4.1 percentage points. The effect was smaller when the message was delivered via mass mailings rather than by phone, or when it contained information about the mayor's ideology instead. Like Kendall et al. (2015), we study actual vote shares. Motivated by the U.S. experience, however, we focus on television commercials and their quantity rather than on how voters update beliefs when presented with different information.[2]

In addition, we contribute to rapidly growing literatures on the political economy of mass media and persuasion (see Prat and Strömberg 2013 and DellaVigna and Gentzkow 2010 for reviews). DellaVigna and Kaplan (2007) demonstrate that the addition of Fox News to local cable networks increased Republican presidential vote shares by about half a percentage point, implying a persuasion rate of $f = 11.6$.[3] In a similar vein, Enikolopov et al. (2011) estimate that Russian voters with access to an independent TV station were significantly more likely to vote for opposition parties ($f = 7.7$). In the U.S. context, Gentzkow (2006) shows that the introduction of television itself reduced voter turnout in congressional elections by about 2 percentage points per decade ($f = 4.4$), while Gentzkow et al. (2011) find that, historically, availability of at least one newspaper per county increased turnout by 1 percentage point ($f = 12.8$).[4]

With persuasion rates between 0.01 and 1.1, our estimates of advertising's effectiveness are only a fraction of those in existing work. This is not surprising. Seeing a few dozen thirty-second political ads constitutes an arguably less intense treatment than having year-round access to newspapers or an additional TV station. Moreover, from a theoretical perspective the effect of partisan advertising ought to be smaller than that of (slanted) news, at least if journalists are less biased than campaigns (Gentzkow and Shapiro 2006; Knight and Chiang 2011). Beyond estimating the effects of political advertising on electoral outcomes, we contribute to this literature by shedding light on the channels through which the persuasive effects of the media operate.

[2] Another strand of the literature uses structural techniques to estimate the impact of political advertising. Gordon and Hartmann (2013) argue that advertising increases aggregate turnout as well as the respective candidate's vote share. Martin (2014) concludes that these effects operate primarily by "persuading" rather than "informing" constituents.

[3] The persuasion rate should be interpreted as the percentage of individuals who change their behavior in response to receiving a particular message (DellaVigna and Kaplan 2007).

[4] Other important contributions include Groseclose and Milyo (2005) and Gentzkow and Shapiro (2010) on measuring media bias, Durante and Knight (2012) on partisan control of the media, Strömberg (2004) on radio's impact on public spending, Oberholzer-Gee and Waldfogel (2009) on media and Hispanic-voter turnout, and Martin and Yurukoglu (2017) on media bias and polarization.

Regarding political advertisements as signals sent by a biased source, the results in this paper also speak to the question of whether such messages can persuade receivers, or whether they will necessarily be perceived as cheap talk (see, e.g., Kamenica and Gentzkow 2011; Knight and Chiang 2011). Prat 2002 shows that a ban on campaign advertising may improve welfare, even if voters are perfectly rational. Gentzkow and Kamenica (2016) consider the impact of competition on information provision. They demonstrate that competition between different senders may increase or decrease the amount of information that is revealed in equilibrium. Our findings indicate that voters do react to biased messages from different senders. Moreover, in an appendix we provide suggestive evidence of approximately constant returns to scale in the number of messages that voters receive.

3. Media Markets and Political Advertising in the United States

When Dwight D. Eisenhower advertised in the 1952 presidential election, almost all viewers received the broadcast signal through over-the-air antennae.[5] Whether an advertisement reached a particular household depended on the strength of the station's signal, the local terrain, and the quality of the household's antenna. The increasing popularity of cable television over the next three decades removed these technological barriers and gave viewers access to the content of any station offered by their cable provider. In response to cable companies' increasing market power, U.S. Congress and the FCC implemented a series of policies to protect local TV stations. In particular, the 1992 Cable Act included a "must-carry" provision that required cable providers to include local broadcast stations.

In order to implement the regulation and to determine which local stations corresponded to a particular cable subscriber, the FCC adopted Nielsen's definition of media markets. According to Nielsen's classification system, U.S. counties are uniquely assigned to a DMA based on historical viewing patterns.[6] DMAs are usually centered around the largest metropolitan area in the region. For example, the Philadelphia DMA includes eight surrounding counties in Pennsylvania, eight counties in New Jersey, and two in Delaware. Any cable provider serving a customer in one of these eighteen counties is required to include local Philadelphia broadcast stations in the customer's cable package.

Similar provisions apply to satellite TV providers. If a satellite provider chooses to offer any of an area's local stations, such as an affiliate of the major TV networks, then the Satellite Home Viewer Act of 1998 requires it to carry all of them. By 2010, more than 90% of households subscribed to either cable or satellite TV (Nielsen 2011).

Importantly for our purposes, local broadcast television is the primary method that po-

[5]A mere seventy communities had access to cable television in 1950 (FCC 2012).
[6]Only seven counties are assigned to multiple DMAs. These counties are excluded from the analysis.

litical candidates use to reach voters. Out of a total of \$2.6 billion in political advertising expenditures leading up to the 2008 general election, approximately \$2 billion was directed at broadcast television, compared to only \$200 million for national cable networks, about \$400 million for radio, and less than \$25 million for digital media (Borrell Associates 2015; New York Times 2008). Even in 2012, when, according to Zac Moffat, Digital Director of Mitt Romney for President, voter engagement via platforms like Twitter, Facebook, and YouTube constituted the biggest change relative to prior years, online advertising accounted for less than 15% of the paid media budget of the presidential campaigns (Scola 2012; Wall Street Journal 2015). TV ads placed through local broadcast networks attract the lion's share of funds because they reach a large number of potential voters in key geographic areas. The coarseness of Nielsen's DMA definitions, however, limits candidates' ability to engage in further location-based targeting. As a consequence, campaigns typically determine their TV advertising strategies at the DMA level (Goldstein and Freedman 2002; Ridout 2007).

Journalistic accounts suggest that political campaigning has undergone an analytics revolution over the last few election cycles. Its most profound impact, however, has been on campaigns' ground operations and digital outreach. Based on the description of one Obama-campaign insider, prior to 2012, TV ad buys were decided by "guys sitting in a back room smoking cigars, saying 'We always buy 60 Minutes'" (Scherer 2012). Only in 2012 did campaigns start to use big data to better target their TV advertisements (Fowler et al. 2016; Issenberg 2013). Internal estimates of the Obama campaign suggest that these optimization efforts resulted in efficiency gains of about 14% relative to 2008 (Scherer 2012). If correct, then improvements in targeting did yield nontrivial cost savings. Yet, relative to the uncertainty inherent in statistical estimates of advertising's effectiveness, an improvement of 14% appears to be rather modest—about the same size as the standard errors on our main result.[7]

Nonetheless, targeted campaign activities that are correlated with ad buys on local broadcast TV pose a threat to our identification strategy. Below, we address this potential issue in two complementary ways. First, we disaggregate our results by election year. Because the targeting of advertisements was still in its infancy in 2004 and 2008, we can assess whether

[7] To understand why improvements in targeting are likely of minor consequence for estimates of advertising's effectiveness, let f denote the persuasion rate and let p be the share of persuadable viewers, i.e., the share of viewers who may change their behavior in response to seeing a particular spot. Advertising's impact is given by $\Delta y = fp + 0(1-p) = fp$. According to Scherer (2012), big data enabled the Obama 2012 campaign to select programs viewed by a greater number of persuadable voters, e.g., "Miami-Dade women under 35." If the campaign reached, on average, 14% more persuadable voters per ad, then, absent simultaneous improvements in f, Δy also increased by 14% relative to 2008—a modest improvement compared to the standard errors below. We are unaware of anecdotal evidence to suggest that campaigns have also become better at producing more persuasive spots.

our estimates are sensitive to this change in technology. Second, we present results that differentiate between battleground and non-battleground states. This is useful because resource constraints force presidential candidates to focus their efforts on swing states. The fact that our estimates for non-battleground states, where campaigns have no meaningful ground game, line up with those for competitive states suggests that unobserved, targeted voter-mobilization activities are not a first-order concern.

4. Data and Econometric Strategy

4.1. *Econometric Approach*

As explained above, we exploit the coarseness of Nielsen's DMA classifications to estimate the electoral impact of political advertising. At its core, the empirical strategy in this paper builds on a large literature in labor economics, which uses spatial policy discontinuities to estimate the economic effects of state-wide minimum wages (see Card and Krueger 1994; Dube et al. 2010), right-to-work laws (Holmes 1998), and school-zoning regulations (e.g., Black 1999). Our approach is also closely related to several papers that rely on media market definitions to explore the importance of mass media for the political economy (see Ansolabehere et al. 2006; Campbell et al. 1984; Niemi et al. 1986; Snyder and Strömberg 2010).[8] Shapiro (2016) uses essentially the same identification strategy to estimate a structural model of demand spillovers from pharmaceutical advertising.

For an intuitive illustration of our approach, consider Figure 1, which displays counties and DMAs in the state of Illinois. Illinois has 102 counties served by 10 media markets. We define a "border-county pair" as two neighboring counties that are assigned to different DMAs. In order to ensure that our results are not contaminated by comparisons across potentially very different state-level electoral environments (say, due to states' varying competitiveness), we restrict attention to border-county pairs in which both counties belong to the same state.

For example, we examine Fayette and Shelby Counties (highlighted in Figure 1). Both are quite rural. As of the 2010 Census, Fayette County had roughly 22,100 inhabitants and a median household income of $41,300. Shelby County had about 22,400 residents with a median household income of $44,600. Importantly for our purposes, they straddle a media market border. Fayette County is located at the far east of the St. Louis market, whereas Shelby County is part of the Champaign-Springfield-Decatur DMA. Being assigned to the former rather than the latter media market has significant consequences for voters' exposure to political advertising. Within sixty days leading up to the 2008 election, local broadcast

[8] Ansolabehere et al. (2006), for instance, compare incumbent vote margins in markets where content originates in the same state as voters with margins in markets where content originates out of state. Snyder and Strömberg (2010) use congruency between newspaper markets and congressional districts to study the impact of press coverage on political accountability.

stations in the St. Louis DMA aired more than 13,600 presidential ads, while the Champaign-Springfield-Decatur market registered less than 20.

On average, border counties account for only about 5% of DMAs' combined population. Since almost all political advertising is purchased at the DMA level, one would expect that prices as well as campaigns' strategies are determined by aggregate, market-level factors, on which individual border counties have only a small influence. If correct and if every border county has exactly one within-state neighbor, then inferring the causal impact of political advertising on outcome y is conceptually straightforward. Consider, for instance, the econometric model

$$(1) \qquad y_{c,t} = \alpha_c + \mu_{p,t} + \phi Ads_{c,t} + X'_{c,t}\gamma + \varepsilon_{c,t},$$

where $Ads_{c,t}$ measures the intensity of political advertising in county c during election year t, α_c denotes a county fixed effect, $\mu_{p,t}$ marks a year-specific fixed effect for border-county pair p, and $X_{c,t}$ is a comprehensive vector of time-varying controls. The coefficient of interest is ϕ, which is identified by comparing *deviations* from the mean in one county to *deviations* from the respective mean in the neighboring county. Intuitively, identification in our approach comes from thousands of local discontinuities created by FCC regulations. In total, our data contain 5,924 of these county-level natural experiments.[9]

Complications arise when border counties have multiple neighbors that are located in other DMAs. For instance, as shown in Figure 1 Fayette County forms a within-state border-county pair not only with Shelby, but also with Effingham County. As a consequence, the total number of border-county pairs exceeds the number of border counties, which precludes us from directly estimating the border-pair fixed effect. We resolve this issue by stacking observations so that a particular county appears in our sample exactly as many times as it can be paired with a within-state across-DMA neighbor. This allows us to treat $\mu_{p,t}$ as a nuisance parameter (see Dube et al. 2010). Stacking does not affect the intuition for how ϕ is identified.[10]

To allow for arbitrary patterns of serial correlation and for correlation in the residuals of counties that are geographically close, we cluster standard errors at the state level. Clustering also corrects for the correlation that is introduced by stacking.[11]

[9]There are 2,529 natural experiments in our data for 2008 and 2012, but only 866 in that for 2004. As explained in Section 4.2, this difference arises because the 2004 data cover only the 100 largest media markets.

[10]Another possibility that does not involve stacking would be to replace the border-county pair fixed effect with one for the DMA-border segment, i.e., the entire border between two media markets. This, however, comes at the cost of comparing counties that are further apart from each other, and thus likely less similar on unobservables. Nevertheless, both approaches yield qualitatively similar results (see Spenkuch and Toniatti 2016).

[11]By clustering at the state level, we choose to err on the side of caution. Clustering by DMA or border-pair

4.2. *Data Sources*

We apply this estimation strategy to uniquely detailed data on the intensity of political advertising during the 2004, 2008, and 2012 presidential campaigns. Information on the broadcast of political advertisements is available through a cooperation between the Campaign Media Analysis Group (CMAG) and the Wesleyan Media and Wisconsin Advertising Projects (Fowler et al. 2015; Goldstein et al. 2011; Goldstein and Rivlin 2007). According to CMAG, the data form a complete record of all political ads that aired on any of the national television or cable networks.[12] In 2004, the 100 largest media markets, or about 86% of the U.S. population, are covered. For 2008-12, coverage was expanded to all 210 DMAs. The CMAG data include timestamps for each ad, the sponsoring group (i.e., a candidate's campaign, the national party, independent interest groups, such as PACs, etc.), the candidate it supported, as well as more detailed, human-coded information on its content.

As political advertisements air at all times of the day and during different programs, the total number of ads that are broadcast in a particular market makes for a questionable measure of advertising intensity, i.e., the number of ads that people actually saw. To directly gauge constituents' exposure to political advertising, we use detailed information on the viewership of spots collected by The Nielsen Company. Nielsen is the market leader in television-audience measurement. At the heart of Nielsen's efforts is a proprietary technology that tracks the media consumption of a representative cross section of households. Relying on metering devices installed in about 30,000 households, Nielsen monitors which channel is being watched at any particular point in time on all TVs in the home. In addition, each year the company collects approximately 2 million week-long TV diaries. These data then form the basis of the so-called Nielsen ratings, which are available by gender and age group for each DMA.

We measure advertising intensity in impressions per capita among voting-aged adults. An impression is defined as one viewer being exposed to one commercial. Our metric of advertising intensity thus corresponds to the number of ads seen by the average adult in a particular DMA. Given that CMAG and Nielsen time stamps do not perfectly match, we average the Nielsen-reported number of impressions (among all viewers age 18 and older) over thirty-minute intervals, and assign the corresponding value to the particular instance in which an ad aired. To assess aggregate presidential advertising, we focus on a 60-day time window leading up to the election and, for each market, sum impressions over all local broadcasts of all presidential ads, including those sponsored by the national parties and other

would in many cases yield somewhat smaller standard errors.

[12]Small-sample audits have found that the CMAG data are highly correlated with invoice data from television stations. For example, in an audit of Philadelphia stations, Hagen and Kolodny (2008) report that less than 2% of ads were missing from the CMAG sample.

interest groups. In symbols, aggregate presidential advertising in media market d during year t is defined as $Ads_{d,t} \equiv \sum_k \sum_{s=1}^{S_{k,d,t}} Imps_{s,k}^{18+}/Pop_{d,t}^{18+}$, where k indexes candidates, and $S_{k,d,t}$ denotes the total number of spots in support of candidate k that aired in that market within 60 days before the election.[13]

We measure partisan advertising in the same way, except that we sum only over ads that support a particular candidate—either through positive messaging related to the candidate or through negative messaging directed at his opponent. Since Nielsen ratings are only available at the DMA level, we assign the same advertising measures to all counties within a given market. If viewing habits in border counties differ from those in the remainder of the media market, then our advertising variable is likely to contain measurement error, which would bias our estimates towards zero. Yet, we believe that the Nielsen data constitute the best available source of information on how many potential voters viewed a particular spot.[14]

County-level information on the total number of voters, votes for each presidential candidate, write-ins, etc., come from the *CQ Voting and Elections Collection* (Congressional Quarterly 2015). To calculate voter turnout, we combine these data with population estimates from the U.S. Census Bureau. All individuals age 18 and older are considered potential voters. While this broad categorization includes some who are ineligible to vote (e.g., felons and non-U.S. citizens), it has the advantage of being robust to endogenous voter registration.

To obtain information on the observable characteristics of counties' residents, we turn to the Census Bureau and the Bureau of Labor Statistics. To measure election coverage by the local press, we count the number of election-related articles in the Factiva database, weighted by the respective newspapers' circulation in a particular county. In addition, we use the slant index of Gentzkow and Shapiro (2010) to proxy for newspapers' political leanings. Data on candidate appearances by media market come from Shaw (2007), Huang and Shaw (2009), and FairVote.org. For more detailed information on the data as well as precise definitions of all variables used throughout the analysis, see the Data Appendix.

[13]According to CMAG, approximately 0.5% (0.1%) of spots in 2008 (2012) aired on cable channels. Since the Nielsen data do not contain information on the viewership of cable channels disaggregated by DMA, we exclude these ads from our calculations. Similarly, our baseline measure of advertising intensity does not account for spots that aired nationally on any of the major networks because the CMAG data contain no information on national ads in 2004. In 2008 (2012), less than 0.2% (0.9%) of ads aired nationally. The robustness checks in Tables 4 and 6 demonstrate that including national ads has virtually no effect on the estimated coefficients for the respective elections.

[14]We have also experimented with other measures of advertising intensity and different time windows, obtaining qualitatively very similar results. Tables 4 and 6 present these robustness checks.

4.3. *Descriptive Statistics & Tests of the Identifying Assumption*

Combining all different sources, Table 1 displays summary statistics for our county-level data set, by border-pair status. There is considerable variation in advertising intensity. The average county in our data records 71 impressions per capita. In some areas, however, voting-aged adults see over 450 spots, whereas other counties have virtually no presidential ads on TV. Variation with respect to turnout and vote shares is also quite large.

Table 1 further shows that border counties are not perfectly representative of the United States as a whole. Although turnout is broadly comparable across border and non-border counties, the former are less populous and have lower median incomes. More important for our purposes is whether, conditional on constituent characteristics, advertising intensity is truly as good as random across media market borders. If so, then the estimates below recover a local treatment effect. That is, we estimate the impact of political advertising on voters who viewed a given number of ads only because they lived on either side of a DMA border.

Unfortunately, our identifying assumption that differences in advertising intensity are uncorrelated with differences in time-varying unobservables is fundamentally untestable. One may be willing to judge its plausibility, however, by asking whether differences in observables predict differences in advertising. A correlation between political advertising and border counties' time-varying observable characteristics would raise concern about a similar correlation with unobservables.

Table 2 provides no evidence that this concern is warranted. The results therein are based on the estimator in equation (1), using different measures of political advertising as outcomes. For ease of interpretation, all variables have been standardized, so that the coefficients refer to the standard deviation change in advertising resulting from a standard deviation increase in the regressor. Within each set of regressions, the first column assesses the explanatory power of demographic variables, while the second one also includes proxies for economic conditions. The third column additionally controls for the (potentially endogenous) political bias and election coverage by the local the press.

Regardless of which advertising measure we consider, few of the point estimates in Table 2 are economically large or statistically significant. In fact, for each specification a joint F-test is unable to reject the null hypothesis that all coefficients are exactly equal to zero, with p-values ranging from 0.464 to 0.995. Remarkably, observable characteristics explain less than 1% of the within-variation in the respective measure of advertising intensity.[15]

For a subset of thirty-one media markets we have also been able to obtain transcripts of

[15] In addition, we have used our border-pair estimator to regress each outcome on each covariate separately. Two out of fifty coefficients are statistically significant at the 5%-confidence level, and the distribution of p-values is statistically indistinguishable from a uniform distribution (cf. Appendix Figure A.5).

televised news shows ahead of the 2008 election. In Appendix B, we rely on these transcripts to measure election coverage by local TV stations. Reassuringly, differences in news coverage do not predict differences in political advertising. Moreover, Appendix D presents placebo tests asking whether future advertising "affects" current election results. It does not. Of the 162 point estimates in Appendix Tables A.6 and A.7, only eight are statistically significant at the 10%-confidence level or below, most of which have the "wrong" sign.

Our interpretation of these results is that differences in political advertising between border counties are essentially random. We hasten to add, however, that it is impossible to definitively prove the validity of our identifying assumption.

5. Political Advertising and Election Results: Empirical Evidence

5.1. *Political Advertising and Turnout*

We now explore the effect of total political advertising on voter turnout. Pooling over the 2004–12 presidential elections, Table 3 presents the results. The first three columns rely on the entire sample of U.S. counties. Based on the simple OLS estimate in column (1), one would conclude that an additional ten impressions per capita raise voter turnout by about 0.25 percentage points. Put differently, a standard deviation increase in presidential advertising is associated with an increase in turnout of almost 2.7 percentage points. Adding county fixed effects to control for time-invarinat unobservables reduces the point estimate by about forty percent. Yet, it remains statistically significant and economically sizeable.

Columns (4)–(6) replicate the previous exercise, but restrict attention to the set of stacked border-pair counties. The number of observations in this sample is larger because a county-year may appear multiple times in the data if it shares a DMA border with more than one other county (see Section 4.1). Again, the correlation between presidential advertising and turnout is economically large. In fact, the estimates in columns (1)–(3) and (4)–(6) are not only qualitatively but also quantitatively similar. Based on the evidence presented so far, it would appear that political advertising leads to a nontrivial increase in voter engagement.

Column (7) implements our cross-border pair estimator in equation (1). Remarkably, comparing only neighboring counties reduces the coefficient on total advertising to near zero. The drop in the point estimate between columns (5) and (7) suggests that campaigns advertise more in areas and years in which citizens are more likely to vote anyway. Since counties that are geographically close tend to experience similar shocks, our border-pair estimator is able to account for this confound, while more naïve approaches cannot.

Column (8) additionally controls for all time-varying covariates shown in Table 2, all non-presidential political advertising as well as candidate visits as a proxy for campaigns' ground operations. The last column in Table 3 controls for the lagged dependent variable in lieu of county fixed effects. This specification may be more appropriate if campaigns directly base their advertising decisions on the outcome of the last election. Moreover, the model in column (9) lets us split the data by year in order to estimate the impact of political advertising separately for each election (see below).

The results in the lower panel of Table 3 are based on the same specifications as those in the upper one, but allow for heterogeneity in the impact of "positive" and "negative" advertising.[16] Although estimates that allow for the effect to vary by tone are less precise, they are almost equally close to zero. All in all, there is little to no evidence to conclude that political advertising has a meaningful impact on aggregate turnout.

Table 4 probes the robustness of our findings with respect to the weighting scheme, different measures of advertising intensity, and various time windows before the election. It also investigates how the results vary across years. All point estimates are based on our border-pair strategy, using the same specification as in column (9) of the previous table. The coefficients in Table 4 are generally close to zero and, in most cases, statistically insignificant. In particular, we obtain almost identical results when we reweight border-pair counties by the inverse number of times that they appear in our stacked data set. Our results are also qualitatively robust to restricting attention to border-pairs that contain less than 5%, or even 2%, of the respective DMAs' combined population. We find this reassuring, as our identifying assumption is most plausible in cases where border counties are highly unlikely to affect campaigns' decisions.

Further, Table 4 shows that the point estimates remain nearly unaffected when we focus on county pairs that are (almost) entirely contained within the same congressional districts. This finding is important because some border-pair counties also straddle the boundaries of congressional districts, and voters might be differentially mobilized based on the characteristics of other salient races. The fact that we obtain quantitatively very similar results from border pairs in which both counties belong to the same congressional district helps to ameliorate this concern.

Lastly, we find similar effect sizes in battleground and non-battleground states. In the former, campaigns' unobserved ground operations may pose a serious threat to our identification strategy. The latter set of states, however, remains typically untreated, as finite resources force campaigns to focus their mobilization efforts. Observing qualitatively similar effects in both sets of states suggests that campaigns' ground operations are likely not a significant confounder.

A remaining worry is measurement error in advertising intensity. Although our measure of

[16] All evaluations of advertisements' tone are due to human coders of the Wesleyan Media and Wisconsin Advertising Projects. See Freedman and Goldstein (1999) for a detailed description of the coding process.

advertising is likely more precise than any in the literature, we cannot rule out that viewing habits in border counties differ from the respective market average, or that a nontrivial number of border-county households receive their television signal from the "wrong" DMA. In 2010, for instance, about 9.5% of U.S. households relied on terrestrial antennae for their television programming (Nielsen 2011). If a nontrivial number of households watches TV stations from a neighboring DMA, then our advertising measure overstates the true difference in treatment intensity, leading to estimates that are biased toward zero.

Under some assumptions, however, it is possible to gauge the severity of the bias. Suppose that a fraction of q randomly chosen households receive their television signal from the neighboring DMA. If these households were to exclusively watch programs originating in the "wrong" market, then the actual, unattenuated effect of political advertising would equal

$$(2) \qquad \phi^* = \frac{1}{1-2q}\widehat{\phi},$$

where $\widehat{\phi}$ denotes the original estimate.[17] To get a sense of reasonable values for q, consider the case in which border-county households have the same propensity to rely on antenna TV as the national average, and further assume that one in two antenna households obtain their television signal from the "wrong" DMA. In such a case, $q \approx 0.05$ and $\phi^* \approx 1.1\widehat{\phi}$. Even if households in border counties were twice as likely as the national average to watch antenna TV, and if every single antenna household watched only programs that originated in the neighboring DMA, i.e., even if $q \approx 0.2$ and $\phi^* \approx 1.67\widehat{\phi}$, the true effect of political advertising on voter turnout would still be only a fraction of the variables' correlation in the raw data. We, therefore, conclude that political advertising has at best a small impact on aggregate turnout.

This result is well aligned with Ashworth and Clinton (2007) and Krasno and Green (2008), who argue that advertising is ineffective at engaging the electorate. The main difference between our estimates and theirs is that ours are precise enough to rule out moderately large effect sizes. In our preferred specification in column (9) of Table 3, the 95%-confidence interval ranges from -0.004 to 0.036 percentage points. By contrast, Ashworth and Clinton (2007) estimate that having seen "many" campaign advertisements increased survey respondents' intent to vote by 0.7 percentage points, with a 95%-confidence interval of $[-15.7, 17.1]$. Krasno and Green (2008) use gross ratings points (GRPs) to measure advertising intensity.

[17] To derive (2), let turnout in border counties A and B be denoted by y_A and y_B, respectively, and let measured advertising be given by Ads_A and Ads_B. Abstracting from differences in covariates, the estimated effect of advertising equals $\widehat{\phi} = (y_A - y_B)\,/\,(Ads_A - Ads_B)$. The actual amount of advertising seen by the constituents in A and B, however, is $Ads^*_A = (1-q)Ads_A + qAds_B$ and $Ads^*_B = (1-q)Ads_B + qAds_A$. It follows that $\phi^* \equiv (y_A - y_B)\,/\,(Ads^*_A - Ads^*_B) = \widehat{\phi}/(1-2q)$.

Controlling for the lagged dependent variable and state fixed effects in a cross-section of 128 DMAs, they find that the average TV viewer seeing ten additional ads increases turnout by 0.05 percentage points. The 95%-confidence interval on their coefficient ranges from -0.06 to 0.16, which comes close to but excludes the naïve OLS estimate.

5.2. *Political Advertising and Vote Shares*

The evidence above suggests that our empirical approach is capable of distinguishing between true effects and relationships that are spurious. We now use it to study advertising's impact on vote shares.

Table 5 focuses on the impact of partisan differences in advertising on differences in vote shares. We define both variables so that positive values indicate an advantage of the Democratic candidate over his Republican opponent, i.e., $\Delta Ads \equiv Ads_D - Ads_R$ and $\Delta v \equiv v_D - v_R$. As in the preceding analysis, columns (1) and (4) show a strong, positive raw correlation between dependent and independent variable. The next two sets of columns add county fixed effects as well as controls for demographics, economic conditions, candidate visits, newspaper reporting, and non-presidential advertising. This decreases the estimated correlations substantially, but does not render them economically meaningless.

Columns (7)–(9) implement our border-county pair identification strategy. Comparing only neighboring counties leads to a further reduction in the coefficients. At the same time, the estimated coefficients become much more precise. Taking the point estimates in columns (7)–(9) at face value, a standard deviation increase in the partisan difference in presidential advertising—the equivalent of potential voters seeing an additional twenty-one spots for the Democratic candidate rather than the Republican one—increases the Democratic candidate's vote share by about 0.48 to 0.65 percentage points relative to that of his Republican opponent. It, therefore, appears that political advertising has a nonnegligible impact on election results, especially if one suspects that measurement error in advertising intensity attenuates the coefficients.

Table 6 performs the same set of robustness checks that we used to check the sensitivity of our results with regard to aggregate turnout. Due to the smaller number of observations, the point estimates for the 2004 election are far less precise than those for 2008 or 2012. Yet, the baseline estimates for each year are statistically indistinguishable from each other ($p = 0.214$). This is noteworthy because 2004 and 2008 predate the analytics revolution in electioneering, after which narrowly targeted campaign activities may pose a problem for our identification strategy. It is also reassuring that the estimated effect of political advertising on vote shares remains qualitatively the same when we limit the sample to counties whose populations comprise less than 2% of the respective media markets, i.e., counties for which we believe our approach to be the most credible.[18]

Interestingly, there is no evidence to suggest that advertising was differentially effective in battleground and non-battleground states, or across states with clear partisan leanings. We do, however, find evidence that political ads exerted greater effects on less educated populations. Splitting our sample into counties above and below the median share of college graduates yields point estimates (standard errors) of 0.238 (0.065) and 0.434 (0.079), which are statistically distinguishable at the 5%-confidence level.

5.3. *Instrumental Variables Estimates*

As a further robustness check, Tables 7 and 8 present results from an instrumental variables strategy in the spirit of Krasno and Green (2008) and Huber and Arceneaux (2007). Given that campaigns tend to focus their resources on states in which the race is likely to be close, these authors observe that some voters are exposed to more political ads than others simply because they happen to live in a DMA that partially overlaps with a battleground state. Going back to the example in Figure 1, Illinois voters living in the St. Louis media market saw more political ads than those in the Champaign-Springfield-Decatur DMA at least in part because the former market also serves voters in Missouri, where the 2008 election was highly competitive. We build on this insight and combine it with the border-pair approach.

Comparing neighboring counties within the same state, Table 7 demonstrates that the share of a media market's population that is contained in a battleground state is, indeed, a strong predictor of advertising intensity.[19] Voters in noncompetitive states see more presidential ads on TV when their own DMA overlaps to a greater extent with a battleground state than the neighboring market. Conversely, individuals in competitive states see fewer political ads when the share of *non*-battleground voters who reside in the same DMA is larger.[20] Importantly, advertising in support of the Democratic candidate is more responsive to DMAs' "battleground population share" than that supporting the Republican one. Leading up to the 2008 election, Barack Obama and John McCain pursued different strategies (see, e.g., Franz and Ridout 2010). Not only did the former campaign advertise more than the latter, but it also put greater emphasis on highly competitive states, such as Florida, North Carolina, Virginia, and Nevada. As a consequence, our instrument is not only predictive of total presidential advertising but also of partisan differences therein.

[18] Appendix D shows results from several additional robustness checks, including a matching estimator, specifications that interact the lagged outcome with state indicators, and covariate selection via the LASSO.

[19] Because our IV strategy examines two different outcomes, we refrain from including the lagged dependent variable in the first stage regressions. Ancillary results (available from the authors upon request) show that adding lags of either or both outcomes has only a minimal effect on the point estimates reported in Table 7.

[20] As explained in the Data Appendix, we define battleground states according to the classification by RealClearPolitics.com six to eight weeks prior to the election.

Table 8 displays reduced form as well as two-stage least squares estimates of the impact of political advertising on turnout and vote shares. Intuitively, the identifying assumption is that differences in the extent to which neighboring DMAs overlap with battleground states are uncorrelated with time-varying differences in unobserved determinants of individuals' voting decisions. If this exclusion restriction is, indeed, satisfied, then the IV estimates have a causal interpretation.

Relative to their counterparts in Tables 3 and 5, three out of the four two-stage least-squares coefficients are larger in magnitude but also less precisely estimated. With p-values 0.076 and 0.039 the estimated effect on vote shares remains marginally significant. Based on the IV results, we continue to conclude that political advertising has no appreciable effect on overall turnout, but skews the outcome of the election in favor of whichever candidate advertises more.

Notwithstanding the imprecision of the estimates, the results in Table 8 are useful for at least two reasons: *(i)* they correct for attenuation bias due to measurement error in advertising intensity, and *(ii)* they help to address an array of potential confounds. Shapiro (2016), for instance, documents cross-media market differences in advertising for antidepressants, and there may well be other, unobserved variables that also vary across DMA borders. For some unobserved factor to bias our IV estimates, it would not only have to affect the election result in the respective county, but it would also have to be systematically correlated with the extent to which the remainder of the media market overlaps with battleground states. Given that DMA borders were drawn by The Nielsen Company based on historical viewing patterns, most unobserved determinants of election outcomes seem *a priori* unlikely to be correlated with whether or not *other* counties in the same DMA belong to a competitive state.

5.4. *Partisan Effects*

Next, we return to our workhorse empirical model and investigate heterogeneity in the effect of Democratic and Republican advertising. Table 9 presents results for vote shares that are defined relative to the entire voting-aged population. This frees us from having to adjust for turnout when we calculate persuasion rates in Section 7. More importantly, using population-based vote shares as dependent variables allows for the theoretical possibility that one candidate's advertising has no effect on the (absolute) support for his opponent.

Although some of the estimates in Table 9 are small and lack statistical significance, as a whole the evidence suggests that own advertising increases support for the respective candidate, while a rival's spots are detrimental to it. Of course, this pattern would emerge automatically had we used regular two-party vote shares as outcomes. With vote shares defined relative to the entire voting-eligible population, however, there is no mechanical reason for the apparent symmetry in the estimates.

One plausible explanation—especially in light of our null result with respect to aggregate turnout—is that the persuasive effects of political advertising operate primarily on the intensive margin. That is, advertising might convince those who would have gone to the polls anyway to vote for one candidate rather than the other. Another possible rationalization is that political advertising works on the extensive margin by affecting *who* turns out to vote. For instance, advertising by the Democratic candidate might mobilize core Democratic supporters all the while deterring Republican ones. In the aggregate such compositional effects might happen to offset each other, which would explain why there appears to be no meaningful impact on overall turnout.

6. Political Advertising and the Partisan Composition of the Electorate

Using only aggregate data there is little hope to credibly distinguish between these two explanations. In order to shed at least some light on the mechanism behind our main result, we have acquired individual-level voter-registration data for the lower forty-eight states and the District of Columbia. The Help America Vote Act of 2002 requires that all states maintain a single, computerized voter-registration list that is regularly updated by removing individuals who are deceased or ineligible, as well as duplicate entries in accordance with the National Voter Registration Act of 1993. The resulting lists include voters' residential address, date of registration, and turnout history.

For a subset of individuals, we also have information on date of birth, gender, and party affiliation. In particular, thirty-nine states' voter-registration files have either a dedicated "party" field, or they contain enough information to determine in which party's primary (if any) a given individual participated. We identify individuals as a "registered Democrat" or "registered Republican" if the state lists them as such, or if they voted in the respective party's primary. Voters who are not officially affiliated with any of the two major parties and did not participate in one of their primaries are classified as "other."

6.1. *Empirical Approach*

To assess whether political advertising leads to changes in the partisan composition of the electorate, we geocode all addresses and use the information on voters' precise locations relative to DMA borders in a regression discontinuity (RD) design (Lee and Lemieux 2010; Thistlethwaite and Campbell 1960). That is, we compare turnout among registered Democrats and Republicans who live on opposite sides of media market borders. Specifically, we are interested in whether the partisan difference in turnout varies discontinuously at the border. In Section 6.3, we show that advertising's impact on the partisan difference in turnout is a key parameter in assessing the importance of the compositional channel.

As above, we define the partisan difference in advertising as the number of impressions per capita in support of the Democratic candidate minus that for his Republican opponent. We then say that a particular voter lives "left" ("right") of the border if partisan differences in presidential advertising are smaller (larger) in the DMA in which she resides than in the neighboring one.

Interpreting our RD setup through the standard instrumental variables framework (Hahn et al. 2001), we calculate the impact of partisan differences in political advertising on partisan differences in turnout by forming the Wald estimator:

(3)
$$\Delta(t_D - t_R) = \frac{\lim\limits_{m_i \to 0^+} \left(\mathbb{E}\left[t_i | i = D, m_i\right] - \mathbb{E}\left[t_i | i = R, m_i\right]\right) - \lim\limits_{m_i \to 0^-} \left(\mathbb{E}\left[t_i | i = D, m_i\right] - \mathbb{E}\left[t_i | i = R, m_i\right]\right)}{\lim\limits_{m_i \to 0^+} \mathbb{E}\left[Ads_D - Ads_R | m_i\right] - \lim\limits_{m_i \to 0^-} \mathbb{E}\left[Ads_D - Ads_R | m_i\right]}.$$

Here, t_i is an indicator for whether individual i turned out to vote, and m_i denotes her distance to the nearest media market border, with negative values assigned to voters who live on the "left." Ads_D and Ads_R are the number of Democratic and Republican impressions per capita, respectively.

While our voter-registration data are well suited to estimate the numerator of equation (3), our advertising measure varies only at the DMA level and is, therefore, likely to overstate the true difference in the advertising exposure of voters in the vicinity of media market borders. This is because individuals who reside close to the border may be more likely to use terrestrial antennae to watch TV stations from the "wrong" DMA. If true, then our Wald estimates are biased towards zero.

Even in the absence of this issue, it bears emphasizing that RD methods can only identify local average treatment effects (Imbens and Angrist 1994). That is, we estimate the impact of political advertising on the set of voters who live close to media market borders. Since identification comes from only a small percentage of the electorate, the results below may not generalize to the U.S. population as a whole.

At the same time, our RD strategy has at least two advantages. First, constituents' exposure to radio advertising or campaigns' ground operations is unlikely to exhibit a sharp discontinuity at within-state media market borders and, therefore, should not bias the RD estimates. Second, identification in our setting actually comes from *differences in discon-*

tinuities.[21] Thus, unlike traditional RD designs, our estimation strategy allows for other variables to vary discontinuously across media market borders, as long as these variables do not differentially affect turnout among Republicans and Democrats (see Grembi et al. 2016 for a discussion of identification in the DRD design).

In the appendix, we present evidence consistent with the more demanding assumption that there are no discontinuities in other, predetermined variables. Briefly, to check for irregularities in the running variable, we look at population density in the vicinity of DMA borders. Based on the evidence in Figures A.11 and A.12, there is little reason to suspect that individuals in our sample are more likely to settle on one side of the border than on the other. Similarly, we find no evidence of economically meaningful differences in how long voters on either side of the border have been registered at their current address (cf. Table A.9), which helps to ameliorate concerns about selective attrition.

We also test for discontinuities in voters' age, gender, party affiliation, and turnout in other elections (cf. Tables A.10–A.13). The point estimates are economically small, and often of varying sign. In the same vein, Table A.14 shows that partisan differences in non-presidential political advertising do not systematically vary "left" and "right" of the DMA border. In particular, the sign of the estimated discontinuity is an order of magnitude smaller than that in Figure 2 below. Appendix Tables A.15–A.17 test for systematic differences in newspaper circulation, local-school expenditures, and property values, none of which appear to exhibit a discontinuity.[22]

6.2. *RD Estimates*

Table 10 presents basic, descriptive statistics for our voter-registration data. Given that most states did not create statewide, digital voter-registration databases until after 2004, turnout histories for earlier years tend to be incomplete.[23] We, therefore, focus on the 2008 and 2012 presidential elections. Another important limitation of our data is that we only observe the current address at which someone is registered. Since we cannot retrospectively ascertain

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[21] To see this, rearrange the numerator of equation (3) to $\left(\lim_{m_i \to 0^+} \mathbb{E}\left[t_i | i = D, m_i\right] - \lim_{m_i \to 0^-} \mathbb{E}\left[t_i | i = D, m_i\right]\right) - \left(\lim_{m_i \to 0^+} \mathbb{E}\left[t_i | i = R, m_i\right] - \lim_{m_i \to 0^-} \mathbb{E}\left[t_i | i = R, m_i\right]\right)$. The first term denotes the discontinuity in turnout among Democrats, while the second one gives the discontinuity in turnout among Republicans.

[22] Data on these outcomes come from the Alliance for Audited Media, the National Center for Education Statistics, and CoreLogic/DataQuick. See the Data Appendix for details.

[23] In Wisconsin, for instance, voter registration and participation lists were maintained by municipal clerks, and municipalities with populations of 5,000 or less were exempt from such record keeping. While the state of Wisconsin does not include turnout information prior to 2006 with its official voter registration data, other states do. These lists provide an accurate picture of turnout in earlier election cycles only where locally maintained records were thoroughly integrated into the statewide database. In our data, turnout numbers for 2004 are often substantially lower than what would be expected based on official statistics.

individuals' place of residence, we restrict attention to cases in which a voter's registration predates the respective election. For the average individual in our sample, the straight-line distance between her residence and the nearest media market border is about 71 kilometers. Eighteen percent, however, live within 25km of a DMA border; and about two percent reside within 5km.

Pooling over all partisans living within 25km of a media market border, Figures 2 and 3 depict our main RD results. Figure 2 shows raw averages for the partisan difference in advertising within 2.5 kilometer intervals on either side of the border, i.e., the denominator in equation (3). Figure 3 does so for the numerator, the partisan difference in turnout.

By construction, media market borders feature a large discontinuity in partisan advertising.[24] On average, the size of the gap is a bit more than twenty impressions per capita. That is, voting-aged adults to the "right" of the border see about twenty additional ads favoring the Democratic candidate. Partisan differences in turnout also exhibit a discontinuity. Registered Democrats living just to the "left" of the border are between five and six percentage points less likely to go to the polls than their Republicans counterparts, but the gap narrows by almost two percentage points among those living just on the other side. The evidence in Figures 2 and 3, therefore, suggests that partisan differences in political advertising induce changes in the partisan composition of the electorate.

Based on the graphical analysis, one would conclude that an increase in the partisan difference in advertising by ten impressions per capita raises turnout of registered Democrats by approximately one percentage point relative to their Republican counterparts. Of course, this simple analysis is subject to a number of limitations. First, there is no *a priori* reason for why the true functional relationship between the running variable and differences in turnout would need to be linear. Second, Figures 2 and 3 pool over different natural experiments and may, therefore, be affected by unobserved spatial heterogeneity. In what follows, we probe the results of the graphical analysis by using nonparametric techniques (Hahn et al. 2001; Porter 2003).

Table 11 presents the results. The estimates in the upper panel refer to the numerator of the Wald estimator and are based on the following "differences in discontinuities" specification:

$$\begin{aligned} (4) \qquad y_{i,p,s,e} = {} & \alpha_{p,s,e} + \tau \mathbf{1}\left[p = D\right] \times \mathbf{1}\left[m_i > 0\right] + \delta \mathbf{1}\left[m_i > 0\right] \\ & + g_p^l\left(m_i\right) \times \mathbf{1}\left[m_i < 0\right] + g_p^r\left(m_i\right) \times \mathbf{1}\left[m_i > 0\right] + \xi_{i,p,s,e}, \end{aligned}$$

where $y_{i,p,s,e}$ is an indicator variable for whether voter i, who is a registered supporter of

[24]The fact that the average number of impressions varies across bins on either side of the border is due to differences in the spatial distribution of voters across DMAs.

party $p \in \{D, R\}$ and lives close to border segment s, went to the polls in election e. $g_p^l(\cdot)$ and $g_p^r(\cdot)$ are flexibly specified, party-specific polynomials of distance, which are allowed to differ on either side of the threshold. To control for unobserved spatial heterogeneity, we divide every DMA border into segments of up to 10km length and include $\alpha_{p,s,e}$, a party- and election-specific fixed effect for each of them. The parameter of interest is τ.

All estimates use a rectangular kernel with the respective bandwidth indicated at the top of each column. Going from left to right, the bandwidth increases from 500 meters to 5 kilometers, with the last column relying on 10-fold cross-validation for bandwidth selection (Ludwig and Miller 2005). Successive rows use higher-order polynomials to approximate $g_p^l(\cdot)$ and $g_p^r(\cdot)$.

Our nonparametric estimates of τ range from 1.1 to 2.9 percentage points, which is roughly inline with the graphical analysis. Eleven out of the sixteen estimates are statistically significant. Appendix Table A.18 decomposes the point estimates into changes in turnout among registered Democrats and Republicans. The sign pattern suggests that registered Democrats are more likely to vote—even in absolute terms—the more the Democratic candidate advertises relative to the Republican one. For registered Republicans we tend to observe the opposite effect, though the coefficients are more variable from one specification to the next.

One, admittedly speculative, explanation for why political advertising may also have demobilizing effects is that a substantial share of ads are negative. As in the experiments of Ansolabehere and Iyengar (1995), attack advertising may diminish the psychological benefits of turning out to support a particular candidate. Unfortunately, RD estimates that attempt to disentangle the effects of positive and negative advertising are too imprecise to draw any conclusions. As a whole, however, the reduced form evidence suggests that partisan differences in presidential advertising alter the partisan composition of the electorate.

The lower panel of Table 11 uses two-stage least squares to implement the Wald estimator.[25] To facilitate comparisons with the results in the remainder of the paper, we scale the coefficients so that they refer to the impact of 10 impressions per capita. The resulting Wald estimates range from 0.5 to 1.5 percentage points. The median coefficient equals 0.9 percentage points.[26] Based on this evidence, we conclude that political advertising has a detectable impact on the partisan composition of the electorate.

[25] For completeness, Appendix Table A.20 presents the "first stage" estimates, i.e., the denominator of the Wald estimator.

[26] Replacing the border segment fixed effect in equation (4) with one for every individual and thus exploiting only the time series variation in our data yields Wald estimates between 0.4 and 0.6 percentage points.

6.3. *Assessing the Importance of Compositional Changes*

How important are these compositional shifts? Under the assumption that registered partisans are more likely to vote for their own party's candidate than for his competitor, we can assess how much of the estimated effect of political advertising on vote shares can be explained by changes in the partisan composition of the electorate alone.

Formally, let candidates' vote shares be given by v_D and v_R, and assume that, conditional on going to the polls, registered partisans vote for the candidate of their own party with probability $\pi > 0.5$. With v_D and v_R defined relative to the entire voting-eligible population, the following accounting identity must always hold:

$$v_D - v_R = \left[\pi t_D s_D + (1-\pi)\, t_R s_R + \omega t_O s_O\right] - \left[(1-\pi)\, t_D s_D + \pi t_R s_R + (1-\omega)\, t_O s_O\right].$$

Here, t_p denotes turnout among supporters of party p, s_p is their population share, and ω stands for the likelihood that "others" will vote for the Democratic candidate. Noting that $s_D \approx s_R$ among voters close to media market borders, we can decompose changes in the partisan difference in vote shares into

$$\begin{aligned} \Delta\left(v_D - v_R\right) \approx{} & (2\pi - 1)\, s\Delta\left(t_D - t_R\right) + (2\omega - 1)\,(1 - 2s)\,\Delta t_O \\ & + 2s\left(t_D - t_R\right)\Delta\pi + 2\,(1 - 2s)\, t_O \Delta\omega \\ & + 2s\Delta\left(t_D - t_R\right)\Delta\pi + 2\,(1 - 2s)\,\Delta t_O \Delta\omega. \end{aligned} \tag{5}$$

The first term on the right-hand side of equation (5) denotes the contribution of changes in turnout among partisans, while the second one refers to turnout of unaffiliated individuals. The terms in the row beneath constitute the effect of changing preferences (i.e., changes in the probability of voting for a particular party, conditional on going to the polls), while the ones in the third row refer to the interaction between shifts in both preferences and turnout.

To assess the importance of the compositional channel, suppose that political advertising has no effect on preferences and beliefs, and that it leads to no changes in turnout among unaffiliated voters. Equation (5) then simplifies to

$$\Delta\left(v_D - v_R\right) \approx (2\pi - 1)\, s\Delta\left(t_D - t_R\right). \tag{6}$$

Assuming that Democrats and Republicans each represent one-third of the population (i.e., $s = 0.33$), and relying on the range of the Wald estimates for plausible values of $\Delta\left(t_D - t_R\right)$, Figure 4 plots the right-hand side of equation (6) as a function of π. For comparison, in Table 9 we estimated that political advertising raises the partisan difference in vote shares by 0.167 percentage points (horizontal line).

Naturally, as the fraction of partisans who vote for the candidate of their own party increases, differences in turnout explain a greater proportion of the difference in vote shares. To get a sense of plausible values for π, we turn to the American National Election Survey (ANES). Among other questions, the 2008–2009 ANES Panel Study elicited respondents' vote choice in the 2008 presidential election as well as their self-declared party affiliation *prior* to election day. Respondents could identify as "strong Republican/Democrat," "not very strong Republican/Democrat," "independent Republican/Democrat," or as truly "independent." Almost 86% of those who self-identified as "strong" or "not very strong" Democrats later indicated that they also voted for Barack Obama. Conversely, about 92% of self-declared Republicans supported John McCain. Although self-reported votes are notoriously unreliable indicators of actual choices, the available evidence suggests that π may well exceed 0.7 or even 0.8. If correct, then changes in turnout among partisans can explain most, if not all, of the estimated impact of advertising on vote shares.

6.4. *Sensitivity and Robustness Checks*

We have conducted an extensive set of sensitivity and robustness checks. To conserve on space, the corresponding results are presented in Appendix Tables A.21–A.25. The evidence in these tables indicates that our RD estimates are robust to controlling for voters' observable characteristics and advertising related to non-presidential races. We also obtain qualitatively and quantitatively similar results when we restrict attention to the set of voters for whom our geocodes are the most precise, i.e., those for whom the geocoding procedure is able to locate the exact street address, or when we only compare voters on opposite sides of media market borders *within* the same congressional district.

To assess the impact of measurement error in advertising intensity, we turn to the FCC's Significantly Viewed List (FCC 2005). In 2005, the FCC issued an updated, comprehensive assessment of all media markets in the United States. In particular, it released a list of counties where out-of-market broadcast stations have a nontrivial viewership. In Appendix Table A.25, we restrict our sample to voters who live in counties where no out-of-market station appears on the FCC's list. Consistent with the idea that measurement error introduces attenuation bias, the majority of the resulting Wald estimates are larger than their counterparts in Table 11. At the same time, we note that the smaller sample size leads to standard errors that make any quantitative comparisons highly speculative.

6.5. *Total Presidential Advertising and Turnout*

So far we have restricted attention to registered partisans, as this has allowed us to assess the importance of the compositional channel. We now provide a partial test of the hypothesis that political advertising also transmits useful information.

Theoretical work that relates the quality of voters' information to turnout typically concludes that, in a given election, informed individuals are more likely to vote than the uninformed (see Feddersen and Pesendorfer 1996, 1999). The intuition behind this result is that informed voters are less likely to make a mistake by choosing the *ex post* worse candidate. If information does, indeed, increase turnout, and if political advertising does contain new information, then we would expect that independent voters who see more political ads on TV are more likely to turn out than those who see fewer spots.

We test this hypothesis using a slight modification of our RD design. Instead of assigning a particular voter to either side of the DMA border according to the partisan difference in presidential advertising, we do so based on whether *total* presidential advertising in her DMA exceeds that in the neighboring one. Despite a large discontinuity in total advertising (cf. Figure 5), the Wald estimates in the upper panel of Table 12 suggest that *unaffiliated* voters who are exposed to more political ads are no more likely to turn out.

For completeness, Figure 6 and the lower panel of Table 12 present results for turnout among all voters. Again, there is no evidence of a positive discontinuity at the DMA border, which implies that our previous finding of a minimal effect of presidential advertising on overall turnout is not an artifact of using aggregate data. While we find no evidence in favor of the idea that political advertising endows voters with useful information, we readily acknowledge that a better test would also consider actual votes.

In sum, our RD results suggest that political advertising induces changes in the partisan composition of the electorate, which offset in the aggregate. To be clear, we do not claim that changes in the partisan composition of the electorate will always exactly cancel out. However, in an environment with two candidates who have roughly the same number of supporters, and if ads have both mobilizing and demobilizing effects, one would expect a considerably smaller net impact—especially when both campaigns advertise in similar proportions.

7. Discussion

To put the estimated effect of political advertising on vote shares into perspective, we follow DellaVigna and Kaplan (2007) and calculate persuasion rates. Intuitively, the persuasion rate measures the percentage of individuals who changed their behavior in response to being exposed to a particular message. Given that different studies use different left- and right-hand side variables, and in light of the fact that the share of individuals who are susceptible to being persuaded varies from one setting to the next, it is useful to rescale effect sizes in this way in order to make them more comparable. Formally, the persuasion rate is defined as

$$f_p = \frac{1}{1-\widetilde{y}_p}\frac{\Delta y_p}{\Delta Ads_p}, \tag{7}$$

where $\Delta y_p/\Delta Ads_p$ approximates the change in the outcome of interest induced by seeing additional advertisements in support of candidate p, and $1-\widetilde{y}_p$ is the fraction of individuals who may be swayed by the respective candidate's message.[27]

We take the outcome of interest to be the partisan difference in vote shares defined as a percentage of the entire voting-aged population. Defining y in this way has two advantages. First, it is not necessary to adjust for turnout. Second, we capture advertising's positive effect on own vote shares as well as any negative impact on the support for political rivals (cf. Table 9). If the Democratic candidate, for instance, is purely office motivated, then he should be indifferent between one more vote for himself and one less for his Republican competitor. As a consequence, $1-\widetilde{y}_D$, the target audience for his ads, includes everybody who does not already vote for him, i.e., everybody who would either abstain or vote for his opponent.

First, we calculate the persuasion rate of one additional spot, given the observed overall level of advertising and candidates' equilibrium vote shares. To proxy for $\Delta y_p/\Delta Ads_p$, we rely on the point estimates in column (8) of Table 9, divided by ten to account for the fact that the coefficients refer to the impact of ten impressions per capita. With the respective numbers in hand, the persuasion rate (and its standard error) is $f_D \approx 0.03\%$ (0.004%) for Democratic spots, and that for Republican advertising equals $f_R \approx 0.01\%$ (0.005%).[28]

Next, we consider the share of potential voters who changed their behavior in response to a candidates' *total* advertising. To proxy for $\widetilde{y}_p$ in the absence of advertising by the respective candidate, we use the results in columns (2) and (5) of Table 8 and predict counterfactual vote shares. $\Delta y_p/\Delta Ads_p$ is, again, given by the appropriately scaled coefficients in column (8) of the same table. The resulting persuasion rates are $f_D \approx 1.1\%$ (0.19%) and $f_R \approx 0.4\%$ (0.14%).[29]

Regardless of whether we calculate the persuasion rate for a single spot or for all of a candidates' combined advertising, the respective numbers are only a fraction of the persuasion rates reported in the literature (cf. DellaVigna and Gentzkow 2010). Our findings are,

[27] Appendix E derives equation (7) formally.

[28] To account for clustering at the state level, standard errors are calculated using the block bootstrap.

[29] It is important to note that these numbers are based on Nielsen ratings data, which do not correct for viewer inattention during commercial breaks. Persuasion rates adjusted for inattention may well be higher.

therefore, consistent with the theoretical prediction that, as long as journalists are less likely to be biased than campaigns, the effect of partisan advertising ought to be smaller than that of slanted news (e.g., Knight and Chiang 2011). Assuming constant returns to scale, for political advertising to be similarly persuasive as being exposed to FOX News ($f = 11.6$), a viewer would need to see about 500 spots. Within two months before the 2012 elections, only one media market registered a similarly high number of impressions per voting-aged adult. In 2004 and 2008 none did.

However, in light of the large number of viewers who see at least some political ads on TV, a more relevant metric might be advertising's aggregate impact. Within 60 days leading up to the 2008 presidential election, the average voting-aged citizen saw about 45 spots in support of Barack Obama and almost 30 ads favoring John McCain. According to the U.S. Census Bureau, about 206 million Americans were eligible to vote that year (File and Crissey 2012), which, together with the numbers above, implies that political advertising affected about 2.2 million voting decisions. Naturally, the effects of Democratic and Republican ads will partially offset each other, resulting in a smaller net impact. Still, simply eliminating the partisan difference in advertising by reducing the number of impressions in favor of Barack Obama to the same level as those for John McCain would have narrowed the difference in votes by more than half a million. While this would not have made much of a difference in 2008, in years in which the election is close a similar sized shift might well decide the overall outcome of the race.

To put the effectiveness of political advertising into perspective, we note that, leading up to the general election, Obama and McCain were estimated to have spent a combined $366 million on TV ads (New York Times 2008), which implies a cost per persuaded voter of about $170. Experimental evidence from get-out-the-vote studies suggest that direct mail or high-quality commercial phone banks generate an additional vote at a cost of about $100 to $200 (see, e.g., Green and Gerber 2015). Phone banks staffed with volunteers or door-to-door canvasing campaigns mobilize supporters at substantially lower cost—about $30 to $50 per vote—but are inherently limited in scale. Based on these back-of-the-envelope calculations, political advertising appears to be roughly as effective as other scalable modes of electioneering.

8. Concluding Remarks

In this paper we study the impact of political advertising on electoral outcomes. Our empirical strategy exploits FCC regulations that result in plausibly exogenous variation in the number of impressions across media market borders. Using aggregate county-level data as well as individual turnout histories for millions of U.S. voters, we find that advertising affects elections by altering the partisan composition of the electorate. Because registered partisans are more likely to vote for their own party's candidate than his competitor, these compositional changes give rise to nontrivial effects on actual election results.

We find no evidence, however, that advertising has an impact on overal turnout. In the aggregate, the mobilizing and demobilizing effects of political ads tend to cancel out. This may help to explain why a large number of previous studies have detected only minimal or even no effects. More generally, our findings help to explain why modern campaigns advertise so much, despite negligible changes in overall voter engagement and individuals' opinions about candidates. Even if political advertising does not have a lasting impact on preferences or beliefs, the evidence in this paper suggests that it increases the respective candidate's vote share by bringing the "right" set of voters to the polls. Given the size of our estimates, partisan imbalances in political advertising have the potential to decide close elections.

The findings above have potentially important implications for public policy, especially for campaign-finance regulation in the post-*Citizens United* era. Ever since the Supreme Court's landmark decision, so-called Super PACs may accept unlimited donations from individuals, corporations, and unions in order to overtly advocate for or against particular candidates. As much of Super PACs' spending directly relates to campaign advertising, our results reinforce existing concerns about the ability of deep-pocketed donors to influence democratic outcomes.

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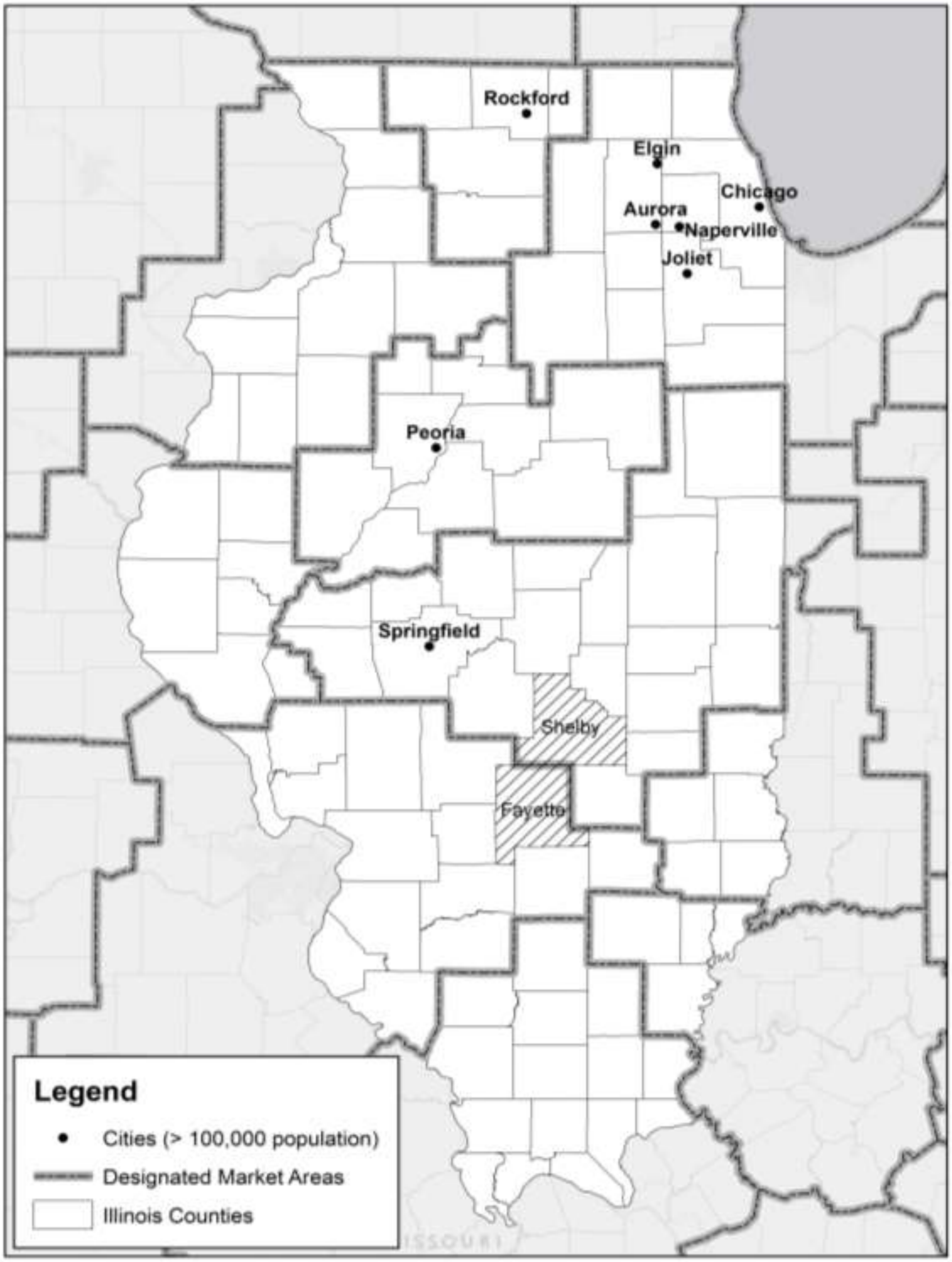
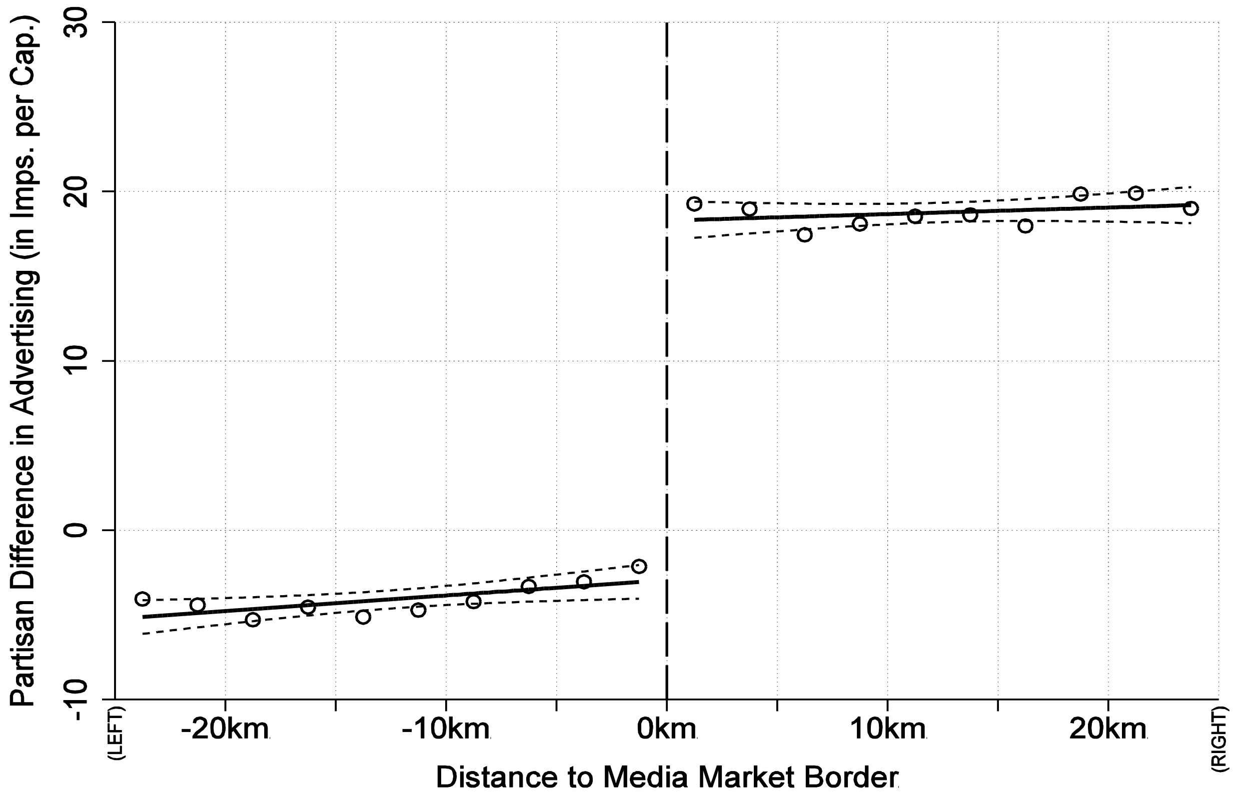
Figure 1: Counties and Media Markets in the State of Illinois



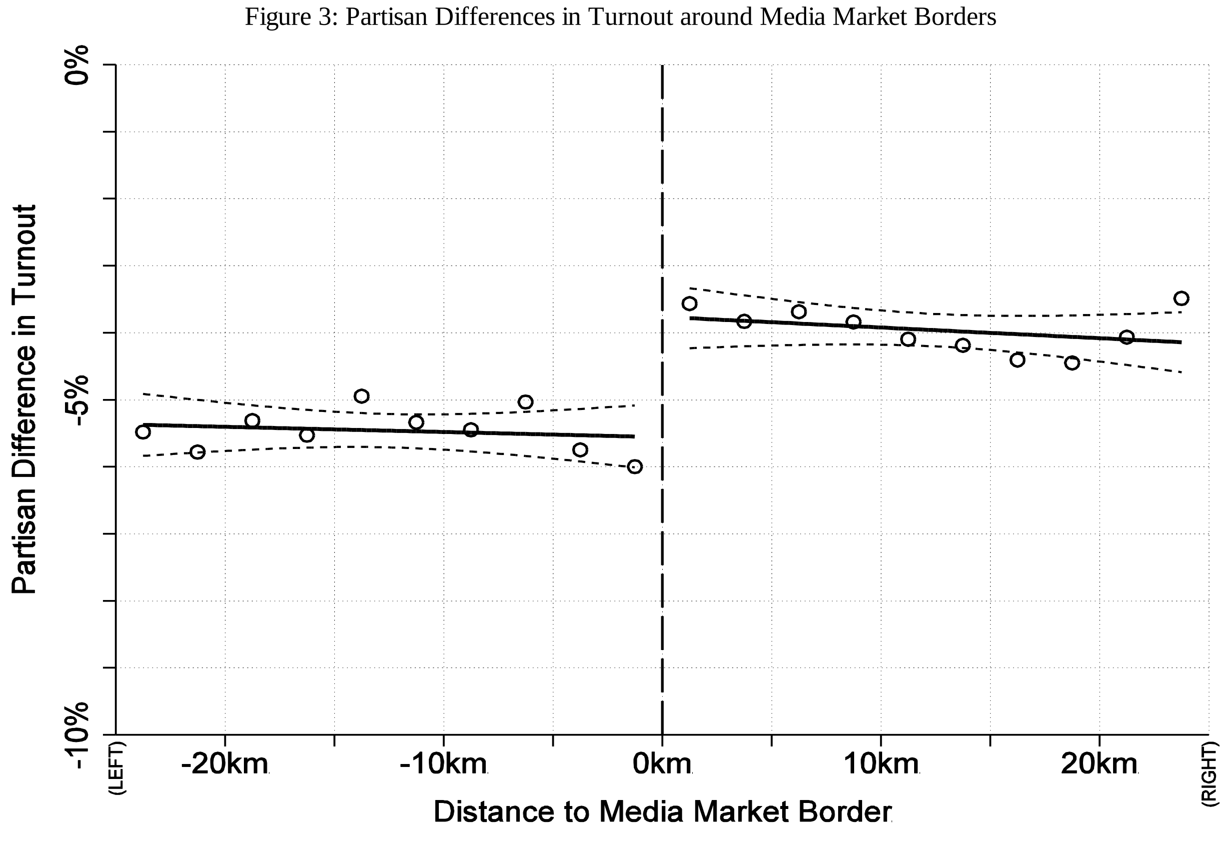
Notes: Figure displays counties, media markets, and cities with a population of more than 100,000 in the state of Illinois.

Figure 2: Average Partisan Difference in Political Advertising in the RD Setup



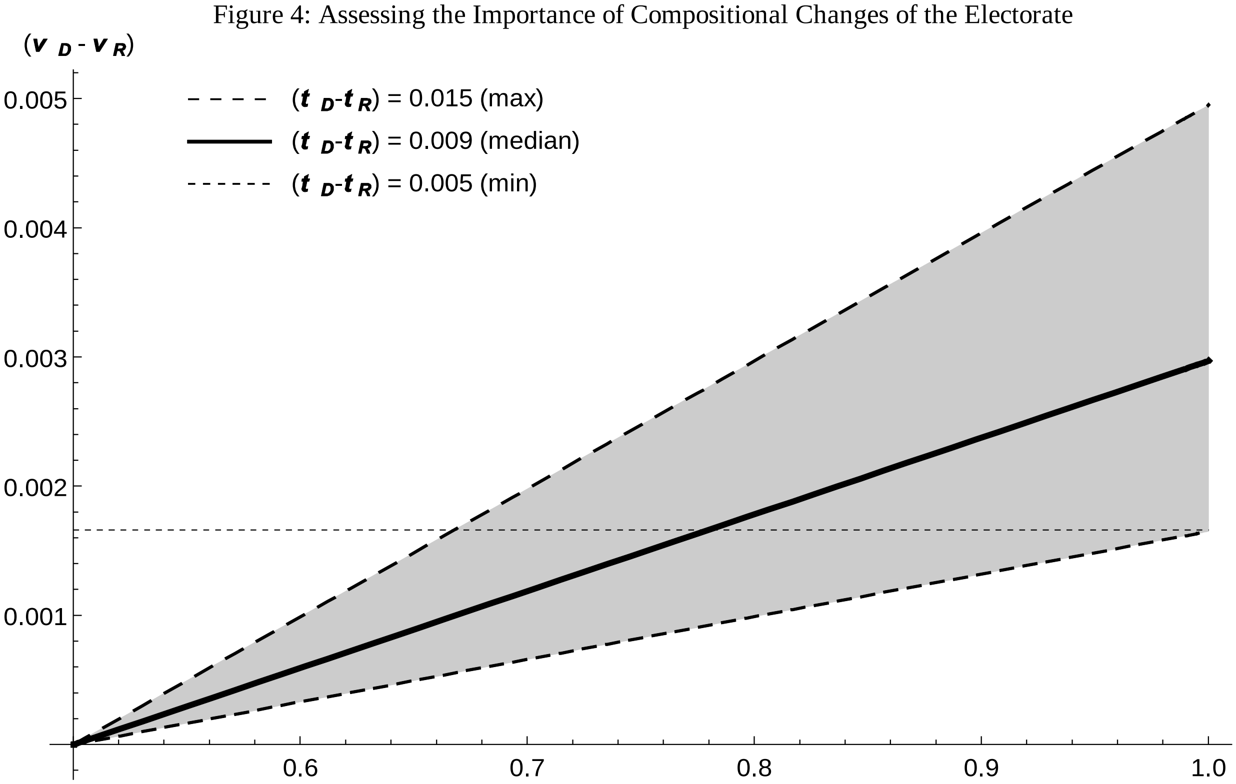
Notes: Figure plots the mean partisan difference in advertising within 2.5km-wide bins around media market borders. Larger values indicate more advertising in support of the Democratic candidate than for his Republican competitor. The sample consists of all registered Democrats and Republicans for whom our voter-registration data contain a valid address as of the respective election. As explained in the main text, we use voters' residential addresses to calculate distance to the nearest within-state media market border, with negative values assigned to individuals who live in a media market in which the partisan differential in presidential advertising is lower than in the neighboring one. For precise definitions and the sources of all variables, see the Data Appendix.

Figure 3: Partisan Differences in Turnout around Media Market Borders



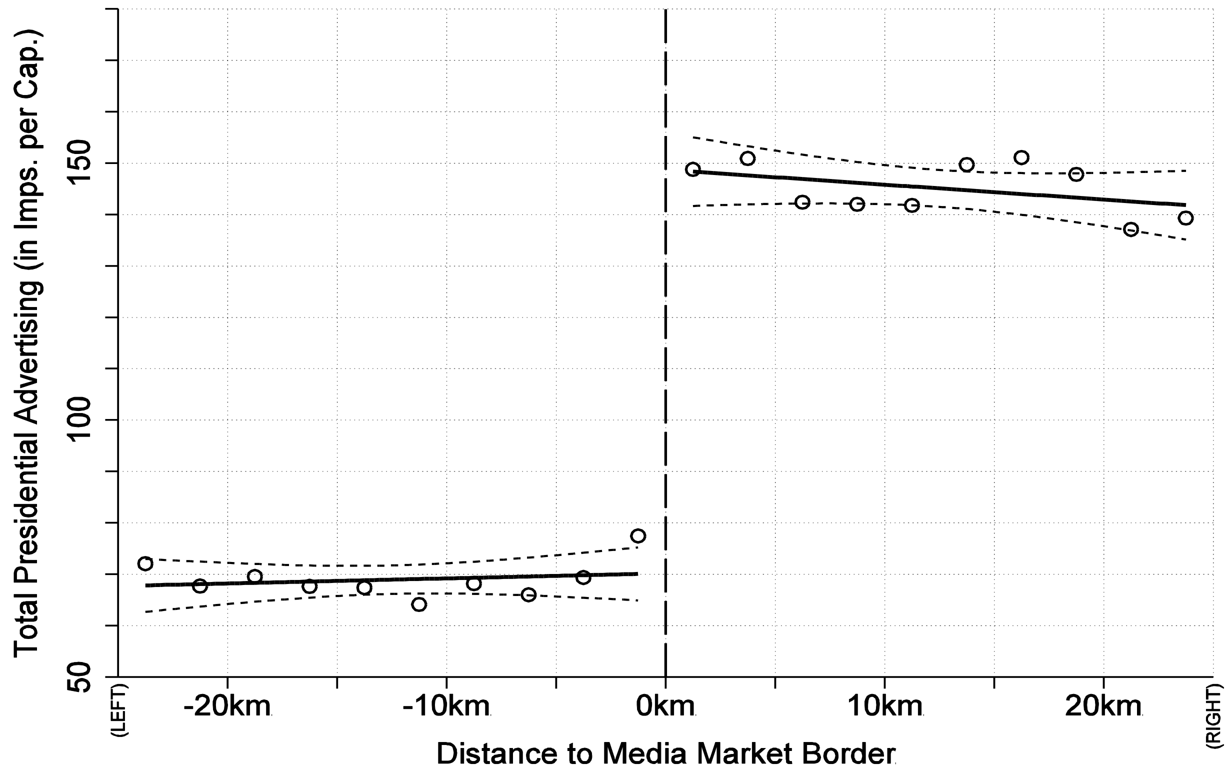
Notes: Figure plots the mean partisan difference in turnout within 2.5km-wide bins around media market borders. Larger values indicate higher turnout among registered Democrats relative to their Republican counterparts. The sample consists of registered Democrats and Republicans for whom our voter-registration data contain a valid address as of the respective election. As explained in the main text, we use voters' residential addresses to calculate distance to the nearest within-state media market border, with negative values assigned to individuals who live in a media market in which the partisan differential in presidential advertising is lower than in the neighboring one. For precise definitions and the sources of all variables, see the Data Appendix.

Figure 4: Assessing the Importance of Compositional Changes of the Electorate



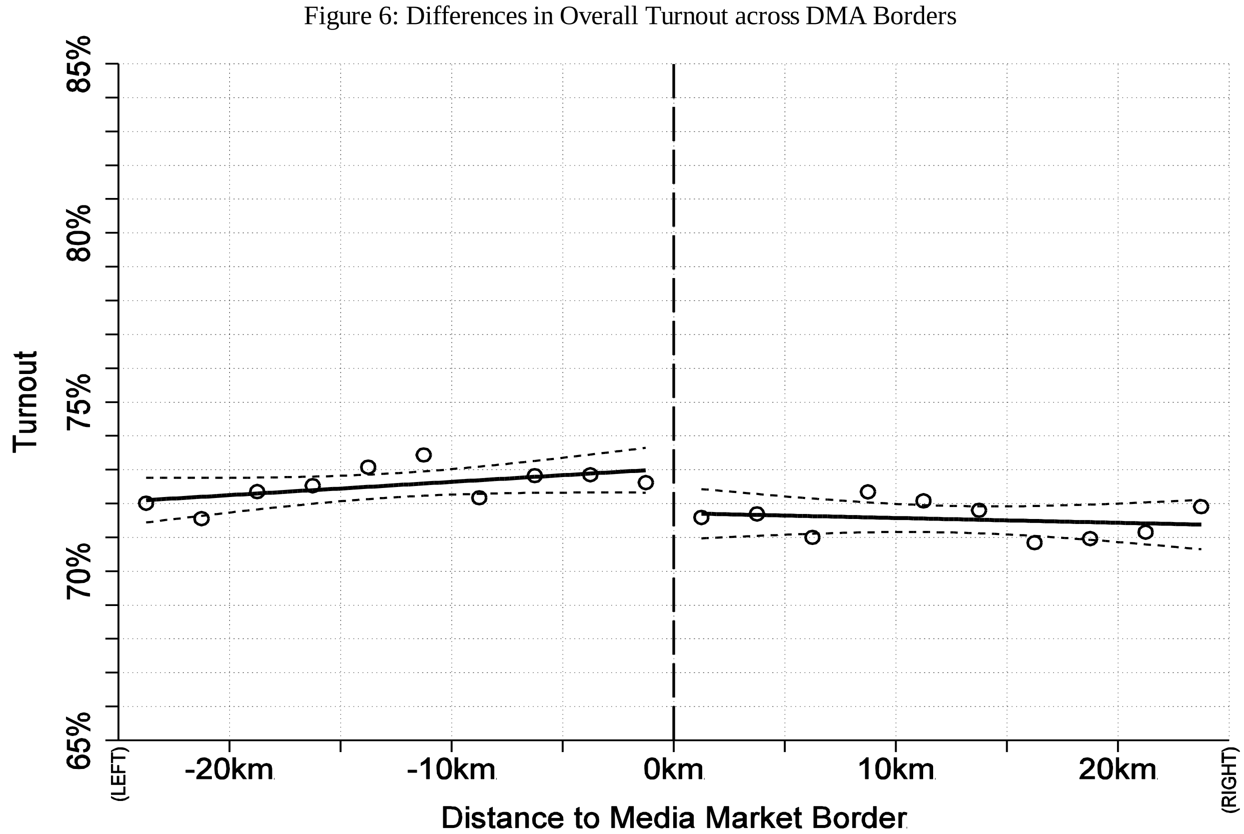
Notes: Figure plots the change in the partisan difference in vote shares due to changes in the partisan composition of the electorate ($\Delta(v_D - v_R)$) as a function of the probability that a registered partisan votes for her party (π). We use the range of the Wald estimates in the lower panel of Table 11 as plausible values for $\Delta(t_D - t_R)$, the impact of partisan differences in advertising on partisan differences in vote shares. The horizontal line indicates the effect size implied by the coefficient in column (9) of Table 9. For details, see the main text.

Figure 5: Differences in Total Presidential Advertising across DMA Borders



Notes: Figure plots average total presidential advertising within 2.5km-wide bins around media market borders. We use voters' residential addresses to calculate distance to the nearest within-state media market border, with negative values assigned to individuals who live in a media market in which *total* presidential advertising is lower than in the neighboring one. For precise definitions and the sources of all variables, see the Data Appendix.

Figure 6: Differences in Overall Turnout across DMA Borders



Notes: Figure plots average turnout among all voters within 2.5km-wide bins around media market borders. We use voters' residential addresses to calculate distance to the nearest within-state media market border, with negative values assigned to individuals who live in a media market in which *total* presidential advertising is lower than in the neighboring one. For precise definitions and the sources of all variables, see the Data Appendix.

Table 1: Descriptive Statistics, Pooling across 2004–2012

	All Counties				Border-Pair Counties			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Media Market Measures:								
Presidential Impressions per Capita (in 10s)	7.12	10.54	0.00	46.62	7.37	10.67	0.00	46.62
Partisan Difference in Imps. per Cap. (in 10s)	0.56	2.11	-5.81	9.35	0.58	2.14	-5.81	9.35
Number of Political Ads (in 1,000s)	6.62	10.78	0.00	56.88	6.73	10.68	0.00	56.88
Non-Presidential Political Imps. per Cap. (in 10s)	16.81	14.33	0.00	60.26	16.57	14.32	0.00	60.26
Candidate Visits	2.01	3.52	0.00	19.00	2.00	3.49	0.00	19.00
County-Level Variables:								
Turnout (%)	57.58	9.57	15.49	100.00	57.02	9.55	24.93	100.00
Lagged Turnout (%)	57.21	9.65	15.16	100.00	56.67	9.54	24.93	100.00
Democratic Two-Party Vote Share (%)	40.65	14.11	3.47	93.98	40.15	13.15	3.47	90.69
Republican Two-Party Vote Share (%)	59.35	14.11	6.02	96.53	59.85	13.15	9.31	96.53
Percent White (%)	79.30	19.28	1.32	100.00	79.75	19.04	1.32	100.00
Percent Minority (%)	20.70	19.28	0.00	98.68	20.25	19.04	0.00	98.68
Percent High School Dropouts (%)	20.17	8.79	2.45	65.46	21.18	8.99	2.45	65.46
Percent High School Educated (%)	55.54	6.75	17.81	74.30	56.13	6.48	25.82	74.30
Percent College Educated (%)	24.29	9.44	6.45	75.16	22.70	8.68	6.45	65.27
Percent Foreign Born (%)	4.27	5.39	0.00	51.20	3.80	4.95	0.00	51.20
Median Household Income (in $1,000)	43.54	11.54	18.38	122.84	41.82	10.51	18.38	107.82
Percent in Poverty (%)	14.71	6.01	0.00	49.45	15.40	6.04	0.00	49.38
Employment to Population Ratio (%)	93.45	2.48	72.65	98.88	93.16	2.51	72.65	98.37
Total Voting-Aged Population (in 1,000s)	81	249	0	7,622	56	168	0	3,082
Newspaper Slant	0.46	0.04	0.36	0.59	0.46	0.04	0.36	0.59
Newspaper Election Coverage (in 100s)	1.69	3.28	0.01	29.09	1.62	3.23	0.01	29.09
Number of Unique Counties	3,111				2,012			
Number of County-Year Observations	8,260				5,252			

Notes: Entries are descriptive statistics for the most important variables in our county-level data set. For precise definitions and the sources of all variables, see the Data Appendix.

Table 2: Political Advertising and Border-County Characteristics

Independent Variables (in Standard Deviation Units)	Presidential Advertising (in Standard Deviation Units)												Non-Presidential Political Advertising		
	Both Candidates			Democratic Candidate			Republican Candidate			Partisan Difference					
Total Voting-Aged Population	0.039	0.051	0.048	-0.007	0.012	0.008	0.089	0.093	0.092	-0.231	-0.191	-0.198	0.106	0.097	0.107
	(0.102)	(0.108)	(0.109)	(0.132)	(0.140)	(0.140)	(0.105)	(0.105)	(0.105)	(0.344)	(0.340)	(0.339)	(0.159)	(0.166)	(0.170)
Percent Minority	-0.073	-0.069	-0.058	-0.052	-0.055	-0.051	-0.094	-0.082	-0.063	0.088	0.048	0.012	-0.056	-0.040	-0.000
	(0.182)	(0.182)	(0.180)	(0.193)	(0.192)	(0.191)	(0.191)	(0.191)	(0.188)	(0.362)	(0.355)	(0.352)	(0.228)	(0.227)	(0.228)
Percent Foreign Born	-0.055	-0.052	-0.048	-0.039	-0.038	-0.032	-0.070	-0.066	-0.064	0.063	0.055	0.067	-0.115	-0.112	-0.129
	(0.082)	(0.084)	(0.085)	(0.099)	(0.099)	(0.100)	(0.074)	(0.078)	(0.078)	(0.190)	(0.189)	(0.187)	(0.128)	(0.129)	(0.131)
Percent High School Dropouts	0.012	0.019	0.023	0.031	0.034	0.038	-0.011	0.001	0.005	0.110	0.090	0.088	-0.115	-0.106	-0.105
	(0.059)	(0.056)	(0.055)	(0.060)	(0.058)	(0.059)	(0.068)	(0.064)	(0.062)	(0.138)	(0.138)	(0.138)	(0.093)	(0.096)	(0.098)
Percent College Educated	-0.002	0.003	0.007	-0.006	0.005	0.008	0.002	0.002	0.006	-0.021	0.009	0.006	-0.009	-0.017	-0.015
	(0.062)	(0.062)	(0.062)	(0.065)	(0.064)	(0.064)	(0.062)	(0.063)	(0.063)	(0.099)	(0.099)	(0.099)	(0.097)	(0.099)	(0.098)
Median Household Income		-0.094	-0.096		-0.131	-0.132		-0.050	-0.051		-0.231	-0.231		0.041	0.041
		(0.059)	(0.059)		(0.070)	(0.070)		(0.052)	(0.052)		(0.122)	(0.121)		(0.089)	(0.088)
Percent in Poverty		-0.074	-0.078		-0.064	-0.067		-0.083	-0.086		0.027	0.027		-0.039	-0.038
		(0.042)	(0.042)		(0.041)	(0.041)		(0.048)	(0.048)		(0.082)	(0.082)		(0.048)	(0.049)
Employment to Population Ratio		0.010	0.011		0.012	0.013		0.007	0.008		0.015	0.014		0.000	0.001
		(0.041)	(0.040)		(0.042)	(0.042)		(0.043)	(0.042)		(0.070)	(0.070)		(0.069)	(0.069)
Newspaper Slant			-0.002			0.021			-0.029			0.127			-0.158*
			(0.042)			(0.052)			(0.036)			(0.096)			(0.065)
Newspaper Election Coverage			-0.086			-0.079			-0.092			0.009			0.027
			(0.058)			(0.058)			(0.057)			(0.044)			(0.043)
H_0: All Coefficients = 0															
F-Statistic	0.194	0.822	0.745	0.082	0.930	0.891	0.405	0.735	0.750	0.360	0.859	0.852	0.466	0.637	0.992
p -value	0.963	0.588	0.679	0.995	0.502	0.549	0.843	0.660	0.675	0.873	0.558	0.582	0.799	0.743	0.464
Within-R^2	0.000	0.004	0.007	0.000	0.004	0.006	0.001	0.004	0.008	0.001	0.005	0.007	0.002	0.003	0.008
Number of Observations	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848

Notes: Entries are coefficients and standard errors estimating estimating equation (1) with different measures of political advertising as outcome variable, as explained in the main text. The respective measure of advertising is given at the top of each column. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. All variables have been standardized, so that the coefficients refer to the standard deviation change in the outcome due to a standard deviation change in the respective covariate. For precise definitions and the sources of all variables, see the Data Appendix. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table 3: Estimating the Impact of Political Advertising on Voter Turnout, 2004–2012 Presidential Elections

A. All Presidential Advertising

	Percent Voter Turnout								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Presidential Impressions per Capita (÷10)	0.253**	0.147**	0.164**	0.208**	0.131**	0.140**	0.015	0.013	0.016
	(0.057)	(0.032)	(0.039)	(0.061)	(0.031)	(0.038)	(0.015)	(0.017)	(0.010)
Fixed Effects:									
County	No	Yes	Yes	No	Yes	Yes	Yes	Yes	No
Year	No	Yes	Yes	No	Yes	Yes	No	No	No
Border Pair × Year	No	No	No	No	No	No	Yes	Yes	Yes
Controls:									
Baseline Controls	No	Yes	Yes	No	No	Yes	No	Yes	Yes
Lagged Dependent Variable	No	No	No	No	No	No	No	No	Yes
Sample	All Counties	All Counties	All Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties
R^2	0.078	0.956	0.959	0.056	0.960	0.962	0.988	0.988	0.960
Number of Observations	8,260	8,260	8,260	11,848	11,848	11,848	11,848	11,848	11,848

B. Positive vs. Negative Presidential Advertising

	Percent Voter Turnout								
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Positive Impressions per Capita (÷10)	0.371**	0.116*	0.107*	0.352**	0.112*	0.095	-0.012	-0.002	0.012
	(0.097)	(0.047)	(0.048)	(0.102)	(0.049)	(0.048)	(0.035)	(0.034)	(0.030)
Negative Impressions per Capita (÷10)	0.211**	0.154**	0.190**	0.155*	0.135**	0.160**	0.021	0.019	0.018
	(0.052)	(0.033)	(0.039)	(0.061)	(0.034)	(0.039)	(0.015)	(0.017)	(0.015)
H_0: Both Coefficients = 0									
F-Statistic	9.910	11.087	11.981	6.639	8.669	8.383	1.192	0.674	1.335
p -value	0.000	0.000	0.000	0.003	0.001	0.001	0.313	0.515	0.273
Fixed Effects:									
County	No	Yes	Yes	No	Yes	Yes	Yes	Yes	No
Year	No	Yes	Yes	No	Yes	Yes	No	No	No
Border Pair × Year	No	No	No	No	No	No	Yes	Yes	Yes
Controls:									
Baseline Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Lagged Dependent Variable	No	No	Yes	No	No	Yes	No	No	Yes
Sample	All Counties	All Counties	All Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties
R^2	0.080	0.956	0.959	0.059	0.960	0.962	0.988	0.988	0.960
Number of Observations	8,260	8,260	8,260	11,848	11,848	11,848	11,848	11,848	11,848

Notes: Entries are coefficients and standard errors from estimating ϕ in equation (1) by ordinary least squares. The outcome variable in all specifications is voter turnout as percentage of counties' voting-aged population. The upper panel estimates the impact of all presidential advertising, while the lower panel distinguishes between positive and negative ads. Estimates in the first three columns within each panel are based on the sample of all U.S. counties with available advertising measures. Estimates in the remaining six columns rely on our sample of stacked border-pair counties instead, as explained in the main text. The set of included controls and fixed effects varies across columns. Our set set of baseline controls includes all covariates shown in Table 2, as well as candidate visits, and total nonpresidential advertising. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table 4: Sensitivity Analysis for the Impact of Political Advertising on Aggregate Turnout

	Pooled		2004 Election		2008 Election		2012 Election	
	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Baseline	0.016	0.116	0.010	0.670	0.018	0.225	0.025	0.047
	(0.010)		(0.023)		(0.015)		(0.012)	
Downweighting Stacked Observations	0.013	0.163	0.005	0.823	0.017	0.227	0.019	0.114
	(0.009)		(0.021)		(0.014)		(0.012)	
County Pairs in Same Congressional District:								
> 80% of Each County's Area	0.022	0.141	0.001	0.973	0.019	0.347	0.029	0.079
	(0.015)		(0.025)		(0.020)		(0.016)	
> 90% of Each County's Area	0.024	0.125	-0.003	0.904	0.021	0.320	0.032	0.065
	(0.015)		(0.026)		(0.021)		(0.017)	
> 95% of Each County's Area	0.024	0.128	-0.003	0.901	0.022	0.303	0.033	0.058
	(0.016)		(0.026)		(0.021)		(0.017)	
> 99% of Each County's Area	0.025	0.129	-0.002	0.927	0.022	0.313	0.035	0.062
	(0.016)		(0.027)		(0.022)		(0.018)	
By Population Share of Media Market:								
< 15%	0.016	0.131	0.008	0.720	0.018	0.226	0.024	0.053
	(0.010)		(0.023)		(0.014)		(0.012)	
< 10%	0.016	0.137	0.009	0.700	0.017	0.264	0.024	0.057
	(0.011)		(0.023)		(0.015)		(0.012)	
< 5%	0.019	0.086	0.012	0.643	0.018	0.175	0.031	0.034
	(0.011)		(0.025)		(0.013)		(0.014)	
< 2%	0.019	0.170	0.013	0.603	0.017	0.325	0.030	0.068
	(0.014)		(0.025)		(0.017)		(0.016)	
Alternative Advertising Measures:								
Total Number of Ads (in 1,000s)	0.011	0.269	0.014	0.675	0.006	0.686	0.019	0.220
	(0.010)		(0.034)		(0.015)		(0.015)	
Number of Unique Ads	0.002	0.274	0.001	0.863	0.003	0.365	0.005	0.105
	(0.002)		(0.005)		(0.003)		(0.003)	
Gross Rating Points (÷1,000)	0.017	0.089	0.010	0.653	0.019	0.162	0.025	0.034
	(0.010)		(0.022)		(0.013)		(0.011)	
Imps. per Cap. within 180 Days Before Election (÷10)	0.009	0.057	0.007	0.678	0.009	0.371	0.011	0.058
	(0.005)		(0.017)		(0.010)		(0.006)	
Imps. per Cap. within 120 Days Before Election (÷10)	0.010	0.078	0.007	0.693	0.009	0.356	0.015	0.043
	(0.006)		(0.017)		(0.010)		(0.007)	
Imps. per Cap. within 30 Days Before Election (÷10)	0.019	0.243	0.012	0.698	0.020	0.362	0.041	0.049
	(0.016)		(0.031)		(0.022)		(0.020)	
Imps. per Cap. for Viewers Age 2 and Older (÷10)	0.021	0.104	0.012	0.662	0.023	0.219	0.033	0.040
	(0.013)		(0.028)		(0.018)		(0.015)	
Imps. per Cap. incl. Nat'l Ads (÷10, 2008 & 2012 only)	--	--	--	--	0.020	0.165	0.025	0.045
	--		--		(0.014)		(0.012)	
By Battleground Status:								
Battleground State	0.014	0.163	-0.014	0.531	0.046	0.048	0.012	0.337
	(0.010)		(0.022)		(0.022)		(0.012)	
Non-battleground State	0.023	0.234	0.066	0.028	-0.005	0.826	0.059	0.090
	(0.019)		(0.028)		(0.021)		(0.033)	
By State Partisan Leanings:								
Democratic	0.005	0.725	-0.012	0.758	-0.012	0.593	0.041	0.156
	(0.014)		(0.037)		(0.021)		(0.028)	
Neither	0.016	0.170	-0.017	0.339	0.042	0.131	0.023	0.127
	(0.011)		(0.016)		(0.026)		(0.013)	
Republican	0.028	0.200	0.060	0.013	0.021	0.530	0.057	0.144
	(0.021)		(0.022)		(0.034)		(0.037)	
By Racial Composition:								
Below Median Share of Minorities	0.016	0.304	0.020	0.472	0.035	0.110	0.022	0.174
	(0.015)		(0.027)		(0.021)		(0.016)	
Above Median Share of Minorities	0.037	0.035	0.007	0.805	0.046	0.171	0.051	0.045
	(0.017)		(0.027)		(0.033)		(0.025)	
By Educational Attainment:								
Below Median Share of College Graduates	0.036	0.201	0.038	0.272	0.037	0.231	0.052	0.200
	(0.027)		(0.034)		(0.031)		(0.040)	
Above Median Share of College Graduates	0.010	0.443	-0.014	0.588	0.009	0.706	0.008	0.596
	(0.012)		(0.025)		(0.022)		(0.014)	

Notes: Entries are coefficients and standard errors on ϕ in equation (1), estimated on various subsamples of the data. The outcome variable in each specification is voter turnout as percentage of counties' voting-aged population, while total presidential advertising is the independent variable of interest. All estimates are based on our sample of stacked border-pair counties, controlling for year-specific border-pair fixed effects, the lagged dependent variable, and the full set of controls, as in column (9) of Table 3. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. *p*-values for the null hypothesis of no effect of presidential advertising on turnout are reported next to each coefficient. As noted in the main text, when we downweigh stacked observations, we weigh each county-year observation by the inverse of the number of times that it appears in our sample of stacked border-county pairs. For precise definitions and the sources of all variables, see the Data Appendix.

Table 5: Estimating the Impact of Political Advertising on Two-Party Vote Shares, 2004–2012 Presidential Elections

	Partisan Difference in Presidential Two-Party Vote Shares								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Difference between Democratic and	1.670**	0.312	0.317	1.128**	0.378	0.405*	0.227**	0.235**	0.308**
Republican Imps. per Cap. (÷10)	(0.450)	(0.190)	(0.192)	(0.399)	(0.188)	(0.177)	(0.045)	(0.043)	(0.043)
Fixed Effects:									
County	No	Yes	Yes	No	Yes	Yes	Yes	Yes	No
Year	No	Yes	Yes	No	Yes	Yes	No	No	No
Border Pair × Year	No	No	No	No	No	No	Yes	Yes	Yes
Controls:									
Baseline Controls	No	No	Yes	No	No	Yes	No	Yes	Yes
Lagged Dependent Variable	No	No	No	No	No	No	No	No	Yes
Sample	All Counties	All Counties	All Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties
R^2	0.016	0.967	0.970	0.008	0.968	0.971	0.996	0.997	0.990
Number of Observations	8,260	8,260	8,260	11,848	11,848	11,848	11,848	11,848	11,848

Notes: Entries are coefficients and standard errors from estimating ϕ in equation (1) by ordinary least squares. The outcome variable in all specifications is the partisan difference in two-party presidential vote shares (in percentage points), with larger values indicating more votes for the Democratic candidate. The independent variable of interest is the partisan difference in presidential advertising (in impressions per capita), defined as the difference between advertising in support of the Democratic candidate and that for his Republican competitor. Estimates in the first three columns are based on the sample of all U.S. counties with available advertising measures. Estimates in the remaining six columns rely on our sample of stacked border-pair counties instead. The set of included controls and fixed effects varies across columns. Our set set of baseline controls includes all covariates shown in Table 2, as well as partisan differences in candidate visits and nonpresidential advertising. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table 6: Sensitivity Analysis for the Effect of Partisan Differences in Political Advertising on Partisan Differences in Two-Party Vote Shares

	Pooled		2004 Election		2008 Election		2012 Election	
	Coefficient	*p* -value	Coefficient	*p* -value	Coefficient	*p* -value	Coefficient	*p* -value
Baseline	0.308	0.000	0.127	0.297	0.360	0.000	0.274	0.000
	(0.043)		(0.120)		(0.065)		(0.070)	
Downweighting Stacked Observations	0.290	0.000	0.149	0.267	0.332	0.000	0.254	0.001
	(0.049)		(0.132)		(0.072)		(0.072)	
County Pairs in Same Congressional District:								
> 80% of Each County's Area	0.325	0.000	0.154	0.370	0.392	0.000	0.284	0.000
	(0.060)		(0.170)		(0.085)		(0.074)	
> 90% of Each County's Area	0.325	0.000	0.158	0.366	0.381	0.000	0.302	0.000
	(0.060)		(0.172)		(0.088)		(0.077)	
> 95% of Each County's Area	0.329	0.000	0.164	0.347	0.386	0.000	0.302	0.000
	(0.060)		(0.172)		(0.088)		(0.078)	
> 99% of Each County's Area	0.328	0.000	0.159	0.329	0.383	0.000	0.304	0.000
	(0.059)		(0.161)		(0.088)		(0.078)	
By Population Share of Media Market:								
< 15%	0.321	0.000	0.143	0.220	0.362	0.000	0.280	0.000
	(0.040)		(0.115)		(0.064)		(0.068)	
< 10%	0.333	0.000	0.181	0.121	0.367	0.000	0.287	0.000
	(0.040)		(0.114)		(0.065)		(0.070)	
< 5%	0.356	0.000	0.193	0.105	0.411	0.000	0.253	0.001
	(0.048)		(0.116)		(0.068)		(0.071)	
< 2%	0.399	0.000	0.345	0.048	0.440	0.000	0.314	0.003
	(0.059)		(0.169)		(0.088)		(0.099)	
Alternative Advertising Measures:								
Total Number of Ads (in 1,000s)	0.269	0.000	0.152	0.419	0.335	0.000	0.198	0.000
	(0.043)		(0.186)		(0.060)		(0.047)	
Number of Unique Ads	0.059	0.000	0.057	0.013	0.069	0.000	0.040	0.032
	(0.010)		(0.022)		(0.015)		(0.018)	
Gross Rating Points (÷1,000)	0.294	0.000	0.118	0.314	0.342	0.000	0.266	0.000
	(0.042)		(0.116)		(0.064)		(0.070)	
Imps. per Cap. within 180 Days Before Election (÷10)	0.202	0.000	0.141	0.115	0.309	0.000	0.113	0.045
	(0.039)		(0.087)		(0.057)		(0.054)	
Imps. per Cap. within 120 Days Before Election (÷10)	0.219	0.000	0.140	0.129	0.313	0.000	0.127	0.027
	(0.038)		(0.090)		(0.062)		(0.055)	
Imps. per Cap. within 30 Days Before Election (÷10)	0.245	0.003	0.107	0.525	0.314	0.017	0.132	0.387
	(0.077)		(0.167)		(0.127)		(0.152)	
Imps. per Cap. for Viewers Age 2 and Older (÷10)	0.387	0.000	0.154	0.311	0.453	0.000	0.342	0.000
	(0.053)		(0.150)		(0.081)		(0.088)	
Imps. per Cap. incl. Nat'l Ads (÷10, 2008 & 2012 only)	--	--	--	--	0.355	0.000	0.249	0.001
	--		--		(0.065)		(0.067)	
By Battleground Status:								
Battleground State	0.285	0.000	0.147	0.261	0.342	0.000	0.278	0.004
	(0.048)		(0.126)		(0.065)		(0.085)	
Non-battleground State	0.353	0.000	0.226	0.352	0.349	0.008	0.332	0.000
	(0.071)		(0.237)		(0.121)		(0.080)	
By State Partisan Leanings:								
Democratic	0.188	0.063	0.460	0.488	0.133	0.215	0.462	0.156
	(0.095)		(0.635)		(0.102)		(0.309)	
Neither	0.349	0.000	0.187	0.258	0.386	0.002	0.344	0.022
	(0.067)		(0.142)		(0.090)		(0.124)	
Republican	0.317	0.000	0.151	0.304	0.538	0.002	0.242	0.002
	(0.074)		(0.143)		(0.146)		(0.069)	
By Racial Composition:								
Below Median Share of Minorities	0.364	0.000	0.293	0.020	0.444	0.000	0.152	0.155
	(0.060)		(0.120)		(0.090)		(0.105)	
Above Median Share of Minorities	0.235	0.007	0.064	0.852	0.260	0.025	0.288	0.040
	(0.082)		(0.340)		(0.112)		(0.136)	
By Educational Attainment:								
Below Median Share of College Graduates	0.434	0.000	0.374	0.112	0.550	0.000	0.214	0.010
	(0.079)		(0.228)		(0.111)		(0.079)	
Above Median Share of College Graduates	0.238	0.001	0.138	0.311	0.190	0.007	0.340	0.002
	(0.065)		(0.134)		(0.067)		(0.105)	

Notes: Entries are coefficients and standard errors on ϕ in equation (1), estimated on various subsamples of the data. The outcome variable in each specification is the partisan difference in two-party vote shares (in percentage points), while the partisan difference in presidential advertising is the independent variable of interest. All estimates are based on our sample of stacked border-pair counties, controlling for year-specific border-pair fixed effects, the lagged dependent variable, and the full set of controls, as in column (9) of Table 5. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. *p* -values for the null hypothesis of no effect of partisan differences in advertising are reported next to each coefficient. As noted in the main text, when we downweigh stacked observations, we weigh each county-year observation by the inverse of the number of times that it appears in our sample of stacked border-county pairs. For precise definitions and the sources of all variables, see the Data Appendix.

Table 7: First-Stage Regressions

	Democratic Imps. per Cap.		Republican Imps. per Cap.		Total Pres. Imps. per Cap.		Partisan Diff. in Imps. per Cap.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of DMA's Population in Battleground State	34.259**	56.358**	13.239	43.877**	47.498**	100.236**	21.021*	12.481**
	(9.332)	(7.913)	(9.621)	(7.160)	(17.253)	(14.508)	(7.851)	(4.155)
Fixed Effects:								
County	Yes	No	Yes	No	Yes	No	Yes	No
Border Pair × Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties
R^2	0.965	0.915	0.971	0.916	0.972	0.922	0.856	0.743
Number of Observations	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848

Notes: Entries are coefficients and standard errors for the first stage of the instrumental variables strategy in Section 5.3. All estimates are based on our sample of stacked border-pair counties, controlling for the full set of covariates. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table 8: Instrumental Variables Estimates

A. Reduced Form	Turnout		Partisan Diff. in Vote Shares	
	(1)	(2)	(3)	(4)
Share of DMA's Population in Battleground State	0.286 (0.353)	-0.045 (0.273)	1.899 (1.052)	1.181** (0.346)
Fixed Effects:				
County	Yes	No	Yes	No
Border Pair × Year	Yes	Yes	Yes	Yes
Controls:				
Baseline	Yes	Yes	Yes	Yes
Lagged Dependent Variable	No	Yes	No	Yes
Sample	Border Counties	Border Counties	Border Counties	Border Counties
R^2	0.965	0.915	0.971	0.916
Number of Observations	11,848	11,848	11,848	11,848

B. 2SLS	Turnout		Partisan Diff. in Vote Shares	
	(5)	(6)	(7)	(8)
Total Presidential Impressions per Capita (÷10)	0.060 (0.064)	-0.004 (0.027)		
Partisan Difference in Imps. per Cap. (÷10)			0.904 (0.498)	0.811* (0.380)
Fixed Effects:				
County	Yes	No	Yes	No
Border Pair × Year	Yes	Yes	Yes	Yes
Controls:				
Baseline	Yes	Yes	Yes	Yes
Lagged Dependent Variable	No	Yes	No	Yes
Sample	Border Counties	Border Counties	Border Counties	Border Counties
First-Stage F-Statistic	6.83	44.98	8.26	13.21
Number of Observations	11,848	11,848	11,848	11,848

Notes: Entries in the upper panel are reduced from estimates for the instrumental variables strategy in Section 5.3. Entries in the lower panel are two-stage least squares estimates based on the same instrument. All estimates are based on our sample of stacked border-pair counties, controlling for year-specific border-pair fixed effects and the full set of covariates. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table 9: Partisan Impacts

	Democratic Vote Share (as Percentage of the Voting-Aged Population)			Republican Vote Share (as Percentage of the Voting-Aged Population)			Partisan Difference in Vote Shares (as Percentage of the Voting-Aged Population)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Democratic Imps. per Cap. (÷10)	0.088**	0.089**		-0.073*	-0.088**		0.161**	0.176**	
	(0.018)	(0.018)		(0.031)	(0.027)		(0.034)	(0.028)	
Republican Imps. per Cap. (÷10)	-0.027	-0.032		0.036	0.065*		-0.062*	-0.097**	
	(0.024)	(0.024)		(0.025)	(0.032)		(0.027)	(0.035)	
Partisan Difference in Imps. per Cap. (÷10)			0.083**			-0.085**			0.167**
			(0.019)			(0.027)			(0.029)
H_0: Both Coefficients = 0									
F-Statistic	3.468	4.987	--	1.660	2.907	--	3.415	6.364	--
p -value	0.008	0.000	--	0.273	0.028	--	0.009	0.000	--
Fixed Effects:									
County	Yes	No	No	Yes	No	No	Yes	No	No
Border Pair × Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:									
Baseline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Sample	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties
R^2	0.995	0.983	0.983	0.992	0.975	0.975	0.996	0.987	0.987
Number of Observations	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848	11,848

Notes: Entries are coefficients and standard errors from estimating models akin to equation (1) by ordinary least squares. The respective outcome variable is given at the top of each column, with all vote shares defined with respect to counties' entire voting-aged population. All estimates rely on our sample of stacked border-pair counties and control for year specific border-pair fixed effects. The remaining controls vary across columns. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table 10: Summary Statistics for the Voter Registration Data, 2008 & 2012 Presidential Elections

	All Voters		< 25km to DMA Border		< 5km to DMA Border	
	Mean	SD	Mean	SD	Mean	SD
Demographics:						
Female	0.54	0.50	0.54	0.50	0.53	0.50
Age	47.11	17.21	48.18	17.18	48.73	16.83
Years Registered at Current Address	14.27	11.76	14.94	12.22	14.76	12.02
Turnout & Political Affiliation:						
Voted in 2008 General Election	0.74	0.44	0.75	0.43	0.75	0.43
Voted in 2012 General Election	0.67	0.47	0.69	0.46	0.70	0.46
Registered Democrat	0.31	0.46	0.31	0.46	0.29	0.45
Registered Republican	0.24	0.43	0.26	0.44	0.27	0.44
"Other" or No Party Information	0.45	0.50	0.44	0.50	0.44	0.50
GIS Measures:						
Distance to Nearest DMA Border (in km)	71.46	69.62	14.42	6.91	2.70	1.41
Street Address Level Match	0.85	0.36	0.83	0.38	0.86	0.35
Zip Code Level Match	0.15	0.36	0.17	0.38	0.14	0.35
City Level Match	0.00	0.01	0.00	0.01	0.00	0.01
Number of Voters	160,693,127		28,196,107		3,461,874	
Number of Observations	250,634,434		45,072,024		5,623,113	

Notes: Entries are descriptive statistics for the most important variables in our voter registration data set, by distance to the nearest media market border within a voter's state of registration. For precise definitions and the sources of all variables, see the Data Appendix.

Table 11: RD Estimates of the Effect of Partisan Differences in Political Advertising on Partisan Differences in Turnout, 2008 & 2012 Presidential Elections

A. Partisan Difference in Turnout across DMA Borders

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.011	0.016*	0.024**	0.024**
	(0.009)	(0.007)	(0.007)	(0.007)
Quadratic	0.018	0.011	0.023**	0.023**
	(0.011)	(0.009)	(0.007)	(0.007)
Cubic	0.029*	0.013	0.021**	0.020**
	(0.012)	(0.011)	(0.008)	(0.007)
Quartic	0.025*	0.019	0.019**	0.019*
	(0.012)	(0.011)	(0.007)	(0.008)
Number of Observations	212,711	457,252	3,163,747	--

B. Wald Estimator

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.005	0.007	0.009*	0.010**
	(0.005)	(0.004)	(0.004)	(0.004)
Quadratic	0.009	0.005	0.010*	0.010*
	(0.006)	(0.005)	(0.004)	(0.004)
Cubic	0.015*	0.007	0.009*	0.009*
	(0.006)	(0.005)	(0.004)	(0.004)
Quartic	0.014*	0.010	0.009*	0.009*
	(0.006)	(0.006)	(0.004)	(0.004)
Number of Observations	212,711	457,252	3,163,747	--

Notes: Entries in the upper panel are estimates of the discontinuity in the partisan difference in turnout across media market borders, i.e., τ in equation (4). Larger values indicate an increase in turnout of registered Democrats relative to registered Republicans. The lower panel displays Wald estimates of the effect of partisan differences in political advertising on partisan differences in turnout (cf. equation (3)). All Wald estimates have been scaled so that the coefficients refer to the impact of 10 impressions per capita. As explained in the main text, the running variable is voters' distance to the nearest within-state media market border. All estimates are based on local polynomial regressions using a rectangular kernel. The order of the local polynomial is given on the left of each row, while the respective bandwidth is indicated at the top of each column. The rightmost column uses 10-fold cross-validation for bandwidth selection, with the holdout sample consisting of observations that lie within 3km of a media market border. Following Imbens and Lemieux (2008), we use the optimally chosen bandwidth for the outcome equation for the Wald estimator. To account for unobserved spatial heterogeneity, every specification includes party-specific fixed effects for individual border segments of up to 10km length. Heteroskedasticity robust standard errors are clustered by media market border and reported in parentheses. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table 12: Total Political Advertising and Turnout, 2008 & 2012 Presidential Elections

A. Wald Estimates for Unaffiliated Voters

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	-0.001	-0.001	-0.001	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
Quadratic	-0.001	-0.003*	-0.002*	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Cubic	-0.001	-0.002	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Quartic	-0.001	-0.001	-0.001	-0.002
	(0.002)	(0.001)	(0.001)	(0.001)
Number of Observations	190,409	382,401	2,459,366	--

B. Wald Estimates for All Voters

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	-0.001*	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Quadratic	-0.001	-0.002*	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Cubic	-0.001	-0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Quartic	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Number of Observations	403,120	839,653	5,623,113	--

Notes: Entries in the upper panel are Wald estimates of the impact of total presidential advertising on turnout among voters who are not affiliated with either of the two major parties. The lower panel displays Wald estimates of the impact of total presidential advertising on all registered voters. All estimates have been scaled so that the coefficients refer to the impact of 10 impressions per capita. As explained in the text, the running variable is voters' distance to the nearest within-state media market border. All estimates are based on local polynomial regressions using a rectangular kernel. The order of the local polynomial is given on the left of each row, while the respective bandwidth is indicated at the top of each column. The rightmost column uses 10-fold cross-validation for bandwidth selection, with the holdout sample consisting of observations that lie within 3km of a media market border. Following Imbens and Lemieux (2008), we use the optimally chosen bandwidth for the outcome equation of the Wald estimator. To account for unobserved spatial heterogeneity, every specification includes fixed effects for individual border segments of up to 10km length. Heteroskedasticity robust standard errors are clustered by media market border and reported in parentheses. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.