Political Advertising and Election Outcomes*

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Abstract

We propose a new approach for estimating 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 and 2008 presidential campaigns, our results indicate that total political advertising has virtually no impact on aggregate turnout. The point estimates are precise enough to rule out even moderately sized effects. By contrast, we find a positive and economically meaningful effect of advertising on candidates' vote shares. Evidence from a regression discontinuity design with millions of observations shows that advertising's impact on elections is largely due to compositional changes 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. During the most recent presidential election in 2012, both campaigns 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, however, note that even self-serving messages may further the democratic process by providing citizens with potentially valuable information about candidates and their competitors (see Bernays 1928; Downs 1957, among others). Despite the longstanding scholarly interest and the ubiquity of political advertising in modern democracies, our understanding of its effects remains incomplete.

Some of the best available empirical evidence suggests that political advertising is ineffective at engaging the electorate (see, e.g., Ashworth and Clinton 2007; Krasno and Green 2008), and that it has only extremely short-lived effects on individuals' opinions (Gerber et al. 2011). Taken at face value, these conclusions contradict campaigns' choices. Why allocate close to half of all available funds to a mode of campaigning that promises only minimal results?

Besides resolving this apparent puzzle, understanding whether political advertising affects election outcomes is of first order importance for public policy (see, e.g., Prat 2002; Prat and Strömberg 2013). If advertisements "persuade" rather than inform constituents, then special interest groups may be able to "buy" political influence by directly targeting voters (Baron 1989, 1994; Grossman and Helpman 1996, 2001). Concerns about big outside spenders working hand in glove with candidates are especially pertinent, as the Supreme Court's recent decisions in *Citizens United* and *SpeechNow.org v FEC* paved the way for so-called Super PACs. These political action committees may accept unlimited donations from individuals, corporations, and unions in order to overtly advocate for or against particular candidates (see, e.g., Lee et al. 2014). The vast majority of Super PACs' spending is directly related to TV advertising (ProPublica 2012).

In this paper, we propose a new approach to estimating the impact of political advertising on election outcomes. Our empirical strategy exploits Federal Communications Commission (FCC) regulations that result in plausibly exogenous variation in the number of impressions across county borders. More precisely, 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, and thus ameliorates many of the most common endogeneity concerns.[1]

For instance, identification strategies that rely on cross-state variation are typically forced to assume that advertising intensity is uncorrelated with all other state-level determinants of individuals' voting decisions, such as expected winning margins or a state's effect on the Electoral College (e.g., Ashworth and Clinton 2007).[2] Approaches that use only variation across media markets *within* the same state are implicitly assuming that campaigns do not tailor their advertising to different markets (e.g., Huber and Arceneaux 2007; Krasno and Green 2008). Such assumptions are difficult to reconcile with the observation that there is substantial variation in advertising intensity, even within nonbattleground states.[3]

By contrast, the identification strategy in this paper is almost ideally suited to study political advertising. 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 the respective markets' 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 practically uncorrelated with the characteristics of the respective electorates—especially when we restrict attention to counties that make up less than 2% of DMAs' populations.

[1] Our empirical approach builds on previous work by Ansolabehere et al. (2006) and Snyder and Strömberg (2010), who use media market definitions to explore the effect of news media coverage on the incumbency advantage and on political accountability, respectively. Our approach is also closely related to a large literature in labor economics that leverages geographic policy discontinuities to gain identification (see, e.g., Black 1999; Dube et al. 2010; Lavy 2010).

[2] Strömberg (2008), however, shows that campaigns' resource allocations depend greatly on states' votes in the Electoral College as well as on forecasted state-election outcomes.

[3] Krasno and Green (2008) and Huber and Arceneaux (2007) argue that most variation in political advertising within nonbattleground states comes from "natural experiments" in which some residents see more political ads because they live in a DMA that also encompasses counties in battleground states. Toniatti (2014), however, shows that as much as 42% of the identifying variation in Krasno and Green (2008) cannot be attributable to such natural experiments.

As a partial test of our identifying assumption, we turn to observable characteristics and verify that border counties which receive more advertising are statistically indistinguishable from their neighbors that receive less. Moreover, we show that observable characteristics explain less than 1% of the variation in political advertising across neighboring border counties.

We apply our approach to uniquely detailed data for the 2004 and 2008 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 turnout as well as vote shares. While the raw data suggest that advertising plays an important role in mobilizing the electorate, our within-border pair estimates imply that the positive correlation between the number of advertisements and turnout is entirely spurious. Critically, the precision of the point estimates allows us to rule out even moderately large positive or negative effects.

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. We also find no evidence that negative and positive ads exert differential effects.

After demonstrating that our 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 non-trivial impact on candidates' vote shares. Depending on the specification, we estimate that a standard deviation increase in the partisan difference in advertising, i.e., the average viewer seeing about twenty more ads promoting one candidate rather than the other, increases the partisan difference in vote shares by 0.6 to 1.8 percentage points.

In order to speak to the mechanism behind this effect, we 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). Relying on official turnout histories for about 125 million registered U.S. voters, we implement a regression discontinuity (RD) design that compares partisans who live nearby but on opposite sides of media market borders. Our RD evidence shows that registered Democrats (Republicans) are significantly more (less) likely to turn out to vote when the Democratic candidate advertises more than his Republican competitor. The size of the RD estimates implies that changes in the partisan composition of the electorate explain most, if not all, of the effect of advertising on vote shares.

The result that political advertising affects election outcomes has potentially important policy implications—especially for campaign finance regulation. In line with existing evidence on the impact of total campaign spending (e.g., Levitt 1994), our estimates imply that advertising has nontrivial aggregate effects. For instance, eliminating partisan differences in advertising during the 2008 presidential campaign would have narrowed Barack Obama's lead over John McCain by about 600,000 votes. While a shift of 600,000 votes would not have changed the outcome of this particular race, a similar sized effect might very well decide an election that is close. In 2000, the average Florida resident saw about 161 ads supporting George W. Bush, compared to only 82 favoring Al Gore (Shaw 2007). Extrapolating from our estimates, this difference increased Bush's vote share by almost 2.6 percentage points and, thus, won him the presidency.[4]

The remainder of the paper unfolds as follows. The next section contains a brief literature review. Section 3 provides background information on the structure of media markets and political advertising in the U.S., while Section 4 describes the data and explains our econometric approach. Section 5 presents the main empirical findings. In Section 6, we implement a regression discontinuity design to disentangle the mechanisms by which political advertising affects vote shares. The last two sections conclude. Appendices with ancillary results as well as the precise definitions of all variables used throughout the analysis are provided on the authors' websites.

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 reaches often different, sometimes 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).[5] 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 at least two important exceptions. The first one 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 extremely short-lived impact on constituents' attitudes. Within one to two weeks, the campaign's effect had

[4]In 2000, Bush won 50.005% of the two-party vote share in Florida (FEC 2001).

[5]In 2012, the Obama campaign allocated 82% of its advertising expenditures to negative ads. The corresponding number for the Romney campaign is 91% (Washington Post 2012).

all but vanished.

Ultimately, our research design and results complement those of Gerber et al. (2011). While we lack true randomization, we are able to study real-world election outcomes as opposed to self-declared attitudes and intentions. 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). Most importantly, the results in this paper show that much of advertising's impact on vote shares is due to changes in the composition of the electorate. This helps reconcile why campaigns advertise so much—often months before the election—despite small effects on individuals' opinions about candidates.

The second exception is a recent field experiment by Kendall et al. (2015), who collaborate 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 is smaller when the message was delivered via mass mailings rather than by phone, or when it contained information about the mayor's ideology instead. As Kendall et al. (2015), we study actual vote shares. Motivated by the U.S. experience, however, our focus is on television ads and their quantity rather than on how voters update beliefs when presented with different information.

We also 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), for instance, 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 $f = 11.6$.[6] 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 ($f = 4.4$). Gentzkow et al. (2011) find that, historically, availability of at least one newspaper per county increased turnout by 1 percentage point ($f = 12.8$).[7]

With persuasion rates between 0.01 and 0.03, our estimates of advertising's impact on vote shares are about two orders of magnitude smaller than those in existing work. This is not

[6]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).

[7]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 (2014) on media bias and polarization.

surprising. After all, seeing a thirty-second political ad constitutes a far 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 one believes that journalists are more likely to truthfully reveal information than campaigns (Gentzkow and Shapiro 2006; Knight and Chiang 2011). Beyond estimating the effects of political advertising on electoral outcomes, we add to this literature by shedding light on the mechanisms through which the persuasive effects of the media operate.

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.[8] 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 allowed 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 Federal Communications Commission (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 correspond to a particular cable subscriber, the FCC adopted Nielsen's definition of media markets. According to Nielsen's classification system, counties in the U.S. are uniquely assigned to a demographic market area (DMA) based on historical viewing patterns.[9] 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 counties 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 all households in the U.S. subscribed to either cable or satellite TV (Nielsen 2011).

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

[8]A mere seventy communities had access to cable television in 1950 (FCC 2012).

[9]Only four counties are assigned to multiple DMAs. These counties are excluded from the analysis.

dates use to reach voters. Out of $2.6 billion in political advertising expenditures during the 2008 presidential campaign, approximately $2 billion was directed at broadcast television, compared to only $200 million for national cable networks and about $400 million for radio (New York Times 2008). Advertisements placed through local broadcast networks allow candidates to reach a large number of potential voters in key geographic areas. Candidates' ability to geographically target their advertising, however, is limited by the coarseness of Nielsen's DMA definitions. As a consequence, campaigns typically determine their strategies at the DMA level (Goldstein and Freedman 2002; Ridout 2007).

4. Data and Econometric Strategy

4.1. *Econometric Approach*

We exploit this fact to propose a new approach to estimating the persuasive effects of political advertising. At its core, our empirical strategy builds on a large literature in labor economics, which uses spatial policy discontinuities to estimate the economic effects of statewide minimum wages (see Card and Krueger 1994; Dube et al. 2010), right-to-work laws (Holmes 1998), or school-zoning regulations (e.g., Black 1999; Lavy 2010). 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).[10]

To illustrate the mechanics 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.

An example of such a pair is La Salle and Livingston County (highlighted in Figure 1). Both are quite rural. As of the 2000 Census, La Salle County had roughly 110,000 inhabitants and a median household income of $40,300. Livingston County had about 40,000 residents with a median household income of $41,300. Importantly for our purposes, they straddle a media market border. La Salle County is located at the very southwestern tip of the Chicago DMA, whereas Livingston County makes up the northeastern part of the Peoria-Bloomington market. In terms of the total population in their DMAs, La Salle and Livingston County comprise 1.2% and 6.1%, respectively.

[10] 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.

On average, border counties account for only about 5% of DMAs' 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, then differences in the intensity of political advertising should be practically uncorrelated with differences in the characteristics of voters on either side of the DMA border. Loosely speaking, our estimation strategy assumes that voters in La Salle and Livingston County are indistinguishable, except that the former see more political ads on TV because they are assigned to the Chicago media market. Consequently, identification comes from thousands of local discontinuities created by FCC regulations. In total, our data contain 3,299 of these county-level natural experiments.[11]

If differences in advertising between border-pair counties are truly as good as random and if there is no heterogeneity in effect size, then inferring the causal impact of political advertising on outcome y is conceptually straightforward. Consider, for instance, the econometric model

$$\Delta_{p,t} y = \phi \Delta_{p,t} Ads + \Delta_{p,t} X' \gamma + \Delta_{p,t} \varepsilon, \tag{1}$$

where $\Delta_{p,t}$ denotes the year t difference (in the variable following it) between the counties in border pair p, Ads measures the intensity of political advertising, and X is a comprehensive vector of county-level controls. Estimating equation (1) on the set of border pairs that straddle media market borders within the same state produces a consistent estimate of ϕ, the coefficient of interest.

In the specification above each observation corresponds to one natural experiment. Although conceptually simple, estimating ϕ from the differences model in equation (1) comes at the cost of hindering comparisons with other approaches. We, therefore, prefer the following fixed effects specification:

$$y_{c,t} = \mu_{p,t} + \phi Ads_{c,t} + X'_{c,t} \gamma + \varepsilon_{c,t}, \tag{2}$$

where c indexes counties, and $\mu_{p,t}$ marks a year-specific fixed effect for border-county pair p. As long as every border county has exactly one within-state neighbor, the point estimates from specifications (1) and (2) are algebraically identical (cf. Wooldridge 2002, ch. 10), but by omitting $\mu_{p,t}$ we can easily assess whether more naïve strategies deliver substantively different results than our border-pair estimator.

Complications arise when border counties have multiple neighbors that are located in other DMAs. La Salle County in Figure 1, for instance, forms a within-state border-county pair

[11] There are 2,429 natural experiments in our data for 2008 and 870 in that for 2004. As explained in Section 4.2, this difference arises because the 2004 data cover only the 100 largest media markets.

not only with Livingston County, but also with (from north to south) Lee, Bureau, Putnam, Marshall, and Woodford Counties. As a consequence, the total number of border-county pairs exceeds the number of border counties, which precludes us from directly estimating the fixed effects in specification (2). 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. Stacking observations allows us to demean all variables within each border pair-year group and to treat $\mu_{p,t}$ as a nuisance parameter (see Dube et al. 2010). Again, the point estimates from specifications (1) and (2) are algebraically equivalent, but the fixed effects model facilitates comparisons with alternative identification strategies.[12]

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

4.2. *Data Sources*

We apply our estimation strategy to uniquely detailed data on the intensity of political advertising during the 2004 and 2008 presidential campaigns. Information on the broadcast of political advertisements is available through the Campaign Media Analysis Group (CMAG) and the Wisconsin Advertising Project (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.[13] In 2004, the 100 largest media markets, or about 86% of the U.S. population, are covered. For 2008, 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 that it supported, as well as more detailed, human-coded information on its content. The data also contain a cost estimate for every spot.[14]

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 poor measure of advertising intensity, i.e., the number of ads that people actually saw. Most previous work uses either self-reported media consumption to measure advertising exposure, or it relies

[12] 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 very similar results.

[13] 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) found that less than 2% of ads were missing from the CMAG sample.

[14] As of this writing, CMAG data for the 2012 presidential election have not yet been released.

on the cost estimate in the CMAG data to impute viewership.[15] In order to get a sense of the accuracy of the latter approach, we obtained a small number of contracts between TV stations and campaigns and spot-checked the CMAG estimate against actual cost paid. Since estimated and true cost are often very different—sometimes by as much as an order of magnitude—and since campaigns, political action committees, etc. are being charged different prices (see also Moshary 2014), we eschew this method of imputing viewership.

Instead, to more directly gauge constituents' exposure to political advertising we use detailed information on the viewership of *each* spot provided by The Nielsen Company. Nielsen is the market leader in television audience measurement. At the heart of Nielsen's efforts is a proprietary metering technology that monitors the media consumption of a representative cross-section of households. These data then form the basis of the so-called Nielsen ratings, which are available for each DMA.

Our primary measure of advertising exposure is total gross rating points (GRPs). GRPs are denoted in percentage points and correspond to the share of the TV audience that viewed a particular spot. According to Nielsen (2009), about 98% of U.S. households own a TV and are, thus, considered to be part of the potential audience. Since CMAG and Nielsen time stamps do not perfectly match, we average Nielsen ratings 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 the election and, for each market, sum GRPs over all broadcasts of all presidential ads including those sponsored by the national parties and other interest groups. In symbols, aggregate presidential advertising in media market d during year t is defined as $Ads_{d,t} \equiv \sum_k \sum_{s=1}^{S_{k,d,t}} GRP_{s,k}$, where k indexes candidates, and $S_{k,d,t}$ denotes the total number of spots in support of candidate k which aired in that market within 60 days before the election. In 2008, for instance, the average DMA recorded approximately 6,400 GRPs. By construction, this is equivalent to the average viewer watching 64 ads during the two months before the election.

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 GRP 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 measure of advertising intensity is likely to contain measurement error. At the same

[15]Gordon and Hartmann (2013, 2015), for instance, derive exposures per capita by dividing the CMAG cost estimate by a "cost-per-rating-point" forecast. A notable exception is Shaw (1999), who relies in part on information obtained directly from the Republican campaign's media buyers.

time, we believe that the Nielsen data constitute the best available source of information on how large the TV audience happened to be when a particular spot actually aired.[16]

County-level information on the total number of voters, votes for each presidential candidate, write-ins, etc. come from the *Atlas of U.S. Presidential Elections* (Leip 2014). 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 individuals that are ineligible to vote (e.g., felons and non-U.S. citizens), it has the advantage of being robust to endogenous voter registration.[17]

To obtain information on the observable characteristics of counties' residents, we, again, turn the Census Bureau and the Bureau of Labor Statistics. Lastly, Daron Shaw generously shared with us his data on candidate appearances by media market (Huang and Shaw 2009; Shaw 2007). For additional, more detailed information on the data as well as precise definitions of all variables used throughout the analysis, see the Data Appendix.

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

Combining all different sources, Table 1 displays descriptive statistics for our county-level data set, by year and border-pair status. There is considerable variation in advertising intensity. The average county in our data records about 6,550 GRPs. Some, however, receive as much as 31,360 GRPs, whereas other counties have virtually no presidential ads on TV. Variation with respect to turnout and vote shares is almost equally large.

Table 1 also shows that border counties are not perfectly representative of the U.S. as a whole. Although turnout is broadly comparable across border and non-border counties, the former are less populous, somewhat poorer, and lean slightly more Republican.

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 in this paper 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, the assumption that differences in advertising intensity are uncorrelated with differences in electorates' unobserved characteristics 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 observable

[16] Shaw (2007) presents GRP figures for 2000 and 2004, which have been obtained directly from the Bush-Cheney campaign. The correlation between his numbers and our GRP measure for 2004 is 0.96. We have also experimented with other measures of advertising intensity and different time windows, obtaining quantitatively and qualitatively very similar results. Section 5 presents these robustness checks.

[17] Our results change very little when we use county-level counts of registered voters instead.

characteristics would raise concern about a similar correlation with unobservables.

Table 2, however, provides no evidence that this concern is warranted. The results therein are based on the differences specification in equation (1), in which the outcome has been replaced with various measures of $\Delta_{p,t}Ads$. 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. Odd-numbered columns assess the explanatory power of demographic variables, while even-numbered ones also include proxies for economic conditions.

No matter whether we consider the total number of presidential GRPs, ads for the Democratic or Republican candidates separately, the difference between the two, or total non-presidential political advertising, few, if any, of the point estimates in Table 2 are of economically meaningful size and none are 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 0.33 to 0.84. Moreover, observable county characteristics explain less than 1% of the variation in the respective dependent variable.

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

5. Political Advertising and Election Outcomes: Empirical Evidence

5.1. *Political Advertising and Turnout*

We now explore the effect of total political advertising on voter turnout. Pooling over the 2004 and 2008 presidential elections, Table 3 presents the results. The first three columns are based on the entire sample of U.S. counties and show naïve estimates of advertising's impact on turnout. Using all cross- and within-state variation, one would conclude that an increase of a thousand GRPs—i.e., the average TV viewer seeing an additional ten spots—raises voter turnout by 0.3 percentage points. Put differently, a standard deviation increase in presidential advertising increases turnout by about 2.5 percentage points. The estimate is not only statistically, but also economically, highly significant.

Controlling for demographics, economic conditions, lagged turnout, and all other, non-presidential political advertising reduces the point estimate by two thirds. Yet, it remains statistically significant and economically sizeable. Based on these correlations it would appear that political advertising leads to a nontrivial increase in voter engagement.

In order to demonstrate that the results that follow are not due to different samples, columns (4), (5) and (6) replicate the previous exercise, but restrict attention to our sample of stacked border-pair counties.[18] Again, the correlation between presidential advertising and turnout is economically large and statistically significant. In fact, the estimates in columns (1)–(3) and (4)–(6) are not only qualitatively but also quantitatively similar.

Column (7) implements our cross-border pair identification strategy by adding year-specific border-pair fixed effects. This alone reduces the point estimate to zero. Comparing neighboring counties that straddle media market borders, it appears that the correlation between turnout and political advertising is entirely spurious. That is, campaigns advertise more in counties where citizens are more likely to vote, but TV advertising itself does not affect overall voter engagement.

Controlling for lagged turnout, candidate visits as a proxy for campaign's ground operations, non-presidential advertising, as well as all covariates shown in Table 2 does little to change the coefficient. It does, however, increase precision. In our preferred specification in column (9), the 95%-confidence intervals range from -0.021 to 0.053. Our estimation strategy thus affords us enough statistical power to rule out even moderately sized positive or negative effects.

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 effect of "positive" and "negative" advertising.[19] Although estimates that allow for the effect to vary by tone are less precise, they are almost equally close to zero. Some even have the "wrong" sign. All in all, there is little to no evidence to conclude that positive ads engage the electorate or that negative messages depress turnout.

Table 4 probes the robustness of our results with respect to the weighting scheme, different measures of advertising intensity, and various time windows before the election. It also investigates how the estimates vary by year, and by battleground status of a particular state. All point estimates are based on our border-pair strategy, using the full set of controls. The coefficients in Table 4 are generally close to zero and statistically insignificant. In particular, we obtain almost identical results when we reweight each county-year observation by the inverse number of times that it appears in our sample of stacked border-county pairs. Perhaps most importantly, our results remain qualitatively unchanged when we restrict attention to border-pair counties that contain less than 5%, or even 2%, of the respective DMAs' population. We find this reassuring, as our identifying assumption is most plausible in cases where

[18] The number of observations in the border-county 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). Sensitivity analyses in which individual observations are reweighted based on the number of times the county-year appears in the data demonstrate qualitatively and quantitatively similar results (cf. Table 4).

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

border counties are highly unlikely to affect campaigns' strategies.

A remaining concern with our approach is measurement error in advertising intensity. Although our measure of 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 TV 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 significant number of households watch 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 mild assumptions, however, it is possible get a handle on the resulting bias.

Specifically, suppose that a fraction of q randomly chosen households receive their TV 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 equals

$$\phi^* = \frac{1}{1-2q}\widehat{\phi}, \tag{3}$$

where $\widehat{\phi}$ denotes the original estimate.[20]

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 TV 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 originate 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 an order of magnitude smaller than the variables' correlation in the raw data. We, therefore, conclude that advertising has little to no impact on aggregate turnout.

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

[20] To derive (3), let turnout in border counties A and B be denoted by y_A and y_B, respectively, and let measured advertising be given by GRP_A and GRP_B. Abstracting from differences in covariates, the estimated effect of advertising equals $\widehat{\phi} = (y_A - y_B)\,/\,(GRP_A - GRP_B)$. The actual amount of advertising seen by the constituents in A and B, however, is $GRP_A^* = (1-q)GRP_A + qGRP_B$ and $GRP_B^* = (1-q)GRP_B + qGRP_A$. It follows that $\phi^* = (y_A - y_B)\,/\,(GRP_A^* - GRP_B^*) = \widehat{\phi}/(1-2q)$.

on vote shares.

Since advertising by one candidate may affect his own support *and* that of his competitor, Table 5 focuses on the impact of partisan differences in advertising on differences in vote shares. Both variables have been normalized such that positive values indicate an advantage of the Democratic candidate over his Republican opponent. Similar to the preceding analysis, columns (1) and (4) show a strong, positive raw correlation between dependent and independent variable. If anything, this correlation is somewhat weaker in our sample of stacked border counties, making it unlikely that the results that follow are due to an unrepresentative sample.

The next two sets of columns add controls for demographics and economic conditions, candidate visits, non-presidential advertising, as well as the partisan difference in vote shares during the previous election. Accounting for covariates decreases the estimated correlations substantially, but does not render them economically meaningless. Based on these partial correlations one would conclude that a standard deviation increase in the partisan difference in presidential advertising—the equivalent of viewers seeing an additional 20 spots for the Democratic candidate rather than the Republican one—increases the difference in candidates' vote shares by almost 2 percentage points.

Columns (7)–(9) implement our border-county pair identification strategy. This leads to a further reduction in the coefficients. At the same time, the estimated impact of political advertising remains statistically significant and economically sizeable. According to our preferred specification in column (9), which includes the full set of controls and thus yields the most precise point estimate, a standard deviation increase in the partisan imbalance in advertising increases the gap in vote shares by about 0.6 percentage points. Without controls, the effect increases to 1.8 percentage points, but is also less precisely estimated.[21] It, therefore, appears that political advertising has a nonnegligible impact on vote shares, 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 probe the sensitivity of our results with regard to turnout. By and large, the point estimates are very similar to the baseline value in column (9) of the previous table. In particular, it is reassuring that, if anything, the estimated impact of political advertising increases when we restrict attention to counties whose populations comprise less than 2% of the respective media markets, i.e., counties for which we believe our identification strategy to be especially credible.

Next, we investigate heterogeneity in the effect of Democratic and Republican advertising.

[21] Ancillary results (available from the authors upon request) demonstrate that the change in the point estimate is due to controlling for the lagged dependent variable, while accounting for non-presidential political advertising makes essentially no difference.

In order to do so, Table 7 presents results for vote shares that are defined relative to the entire voting-aged population. This has the benefit that we do not have to adjust for changes in turnout when we calculate persuasion rates in Section 7. More importantly, using population-based vote shares as dependent variables allows for the possibility that one candidate's advertising has no effect on the (absolute) support for his opponent.

Empirically, however, there is no evidence that advertising only affects the candidate's own vote share. In fact, the estimated coefficients are in many ways surprisingly "symmetric." First, although Republican ads seem to have had a lower impact than Democratic ones, the sign pattern suggests that own advertising increases support for the respective candidate, while a rival's spots are detrimental to it.[22] Second, ads that support the Democratic (Republican) candidate increase his vote share by nearly the same amount by which they reduce that of his Republican (Democratic) counterpart. Of course, the latter finding would hold mechanically true 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 *a priori* reason for the apparent symmetry in the point 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 contender might mobilize core Democratic supporters all the while deterring Republican ones. In the aggregate such compositional effects might offset each other, which would explain why there is no change in overall turnout. Naturally, with aggregate data there is no way to credibly distinguish between these competing explanations.

6. Decomposing the Effect of Political Advertising on Vote Shares

In order to be able to speak to the channel through which political advertising affects election outcomes, 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 each state maintain a single, computerized voter registration list that is regularly updated by removing the deceased, voters who become ineligible, as well as duplicate entries in accordance with the National Voter Registration Act of 1993. The resulting lists include each voter's residential addresses, date of registration, and turnout history. With the exception of

[22] While individual point estimates are often statistically insignificant, it *is* possible to reject the null hypothesis that the coefficients on Democratic and Republican advertising are jointly equal to zero.

New Hampshire, all states' data contain information on turnout in the 2008 general election. For most states, we also have information on individuals' party affiliation, date of birth, and gender. In total, our data contain more than 180 million records.[23]

We geocode all addresses and leverage the information on voters' precise locations relative to DMA borders in order to implement a regression discontinuity (RD) design (Lee and Lemieux 2010; Thistlethwaite and Campbell 1960). Restricting attention to the subset of individuals whose registration predates the 2008 presidential election, Table 8 presents basic, descriptive statistics.[24] For the average individual in our sample the straight-line distance between her residence and the nearest media market border is about 68 kilometers. Eighteen percent, however, live within 25km of a DMA border; and about 2% reside within 5km.

6.1. *Conceptual Framework*

To see why these data are useful in speaking to the mechanism by which political advertising affects vote shares—despite the lack of information on actual votes—note that we can determine party affiliation for about 56% of records. All states' voter registration files have either a dedicated "party" field, or they indicate in which party's primary (if any) a given individual participated. We say that someone is a "registered Democrat" or "registered Republican" if the state lists her as such, or if she voted exclusively in the respective party's primary.[25] Under the assumption that registered partisans are more likely to vote for their own party's candidate than for his competitor, we can gauge how much of advertising's effect on vote shares can be explained by changes in the partisan composition of the electorate.

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

$$v_D - v_R = [\pi t_D s_D + (1-\pi)\, t_R s_R + \omega t_O s_O] - [(1-\pi)\, t_D s_D + \pi t_R s_R + (1-\omega)\, t_O s_O]\,. \tag{4}$$

[23]For additional information on our voter registration data see the Data Appendix.

[24]We discard about 0.2% of observations because the information supplied in states' voter registration lists is insufficient to derive geocodes. Approximately 10% of observations are not used in the analysis because a handful of states do not contain a *within*-state media market border. The remaining observations are lost because a voter either registered for the first time or updated her registration *after* the 2008 election. Given that our data are current as of the end of 2013/14 and that older lists are generally unavailable, we have no way of determining where these individuals lived in November of 2008. Ancillary robustness checks, however, show that voters on either side of DMA borders have, on average, been registered at their current addresses for an equal amount of time (cf. Appendix Table A.5), which makes it unlikely that our results are driven by selective attrition.

[25]Individuals whose vote history indicates that they had participated in different parties' primaries are classified as "other."

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

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

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

In order to assess how much of advertising's effect on vote shares can be explained by changes in the partisan composition of the electorate (i.e., holding preferences and turnout among "others" fixed), we estimate $\Delta(t_D - t_R)$ by comparing turnout among registered Democrats and Republicans who live just on opposite sides of media market borders.

Mimicking our identification strategy in the previous section, we use voters' location relative to media market borders as a source of quasi-exogenous variation in exposure to political advertising. As a matter of definition, we 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.[26] Formally, let m_i denote individual i's distance to the nearest media market border, with negative values assigned to voters who live on the "left."[27] Interpreting our RD setup through the standard instrumental variables framework (Hahn et al. 2001), we then calculate the impact of partisan differences in political advertising on partisan differences in turnout by forming the Wald estimator:

$$(6) \qquad \frac{\lim_{m_i \to 0^+} \mathbb{E}\left[\mathbb{E}\left[t_i | i = D\right] - \mathbb{E}\left[t_i | i = R\right] | m_i\right] - \lim_{m_i \to 0^-} \mathbb{E}\left[\mathbb{E}\left[t_i | i = D\right] - \mathbb{E}\left[t_i | i = R\right] | m_i\right]}{\lim_{m_i \to 0^+} \mathbb{E}\left[GRP_D - GRP_R | m_i\right] - \lim_{m_i \to 0^-} \mathbb{E}\left[GRP_D - GRP_R | m_i\right]},$$

where GRP_D and GRP_R denote the number of GRPs supporting the Democratic and Republican candidate, respectively.

While our voter registration data is ideally suited to estimate the numerator of the above

[26]As in the previous section, partisan differences are normalized such that positive values indicate an advantage of the Democratic candidate over his Republican opponent.

[27]Multiplying distance by negative one for all voters to the "left" of a media market border is innocuous, as all of our empirical specifications allow for different functional relationships between the dependent variable and distance on either side of the border.

equation, our GRP measure varies only at the DMA level and is, therefore, likely to *over*state the true partisan difference in advertising among voters in the vicinity of media market borders. This is because individuals using terrestrial antennae who reside close to the border may be especially prone to watching TV stations from the "wrong" DMA. If true, then our Wald estimates are biased towards zero, which, in turn, makes it more difficult to pick up any effect.

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. If heterogeneity in effect size is quantitatively important, then 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 important advantages. First, constituents' exposure to radio advertising or campaigns' ground operations is unlikely to exhibit a sharp discontinuity at media market borders, and, therefore, should not bias our results. Second, identification in our setting actually comes from *differences in discontinuities*.[28] Thus, unlike traditional RD designs, our estimation strategy allows for other discontinuities across media market borders as long as these other variables do not *differentially* affect turnout among Republicans and Democrats (for a more general discussion of identification in the "differences in discontinuities" approach see Grembi et al. 2014).

6.2. *Regression Discontinuity Estimates*

Focusing on the 2008 presidential election and pooling over all within-state media market border segments in the lower forty-eight states, Figures 2 and 3 provide graphical depictions of our 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 (6). Figure 3 does so for the numerator, the partisan difference in turnout.

By construction, media market borders feature a large discontinuity in partisan advertising. On average, the size of the gap is slightly more than 2,400 GRPs, so voters to the "right" of the border see about 24 additional ads favoring the Democratic candidate rather than the Republican one.[29]

Interestingly, partisan differences in turnout also exhibit a "jump." Registered Democrats

[28] To see this, note that the numerator of equation (6) can be rearranged 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.

[29] The fact that average GRPs vary across bins on either side of the border is due to the fact that the spatial distribution of voters differs across DMAs.

living just to the "left" of the border are approximately 3.7 percentage points less likely to go to the polls than their Republicans counterparts, but the gap narrows to 2.2 percentage points among those living in the adjacent market. Limiting the sample to voters who reside within 25 kilometers of a DMA border and using a simple, linear parameterization to estimate the size of the discontinuity yields a point estimate of approximately 1.5 percentage points (with a standard error of 0.7). The graphical analysis, therefore, suggests that partisan differences in political advertising induce changes in the partisan composition of the electorate.

Of course, there is no *a priori* reason to believe that the true functional relationship between the running variable and differences in turnout is linear, especially in close proximity to a DMA border. Also, the graphical analysis pools over many different natural experiments and may suffer from the effects of unobserved spatial heterogeneity. Since unobserved heterogeneity and misspecification of the functional form are likely to generate biased estimates, we probe the previous result by using nonparametric techniques (Hahn et al. 2001). More specifically, we leverage the sheer size of our data set to estimate local polynomial fixed effects regressions within *very* narrow bandwidths around media market borders.

Table 9 presents the results. The estimates in the upper panel refer the numerator of the Wald estimator and are based on the following "differences in discontinuities" specification:

$$\begin{aligned} y_{i,p,s} &= \alpha_{p,s} + \tau \mathbf{1}\left[p = D\right] \times \mathbf{1}\left[m > 0\right] + \delta \mathbf{1}\left[m > 0\right] \\ &\quad + g_l\left(m\right) \times \mathbf{1}\left[m < 0\right] + g_r\left(m\right) \times \mathbf{1}\left[m > 0\right] + \varepsilon_{i,p,s}, \end{aligned} \tag{7}$$

where $y_{i,p,s}$ is an indicator variable for whether voter i, who is a registered supporter of party $p \in \{D, R\}$ and lives close to border segment s, went to the polls in the 2008 general election. $g_l(\cdot)$ and $g_r(\cdot)$ are flexibly specified polynomials of distance, which are allowed to differ on either side of the threshold. We control for unobserved spatial heterogeneity by including $\alpha_{p,s}$, a party-specific border segment fixed effect. The parameter of interest is τ. It indicates whether partisan *differences* in turnout vary discontinuously at media market borders.

All estimates use a rectangular kernel with the respective bandwidth indicated at the top of each column. Going from the left to the 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 $g_l(\cdot)$ and $g_r(\cdot)$. To allow for realistic patterns of spatial correlation in the residuals, we cluster standard errors at the state level. Our nonparametric estimates of τ range from 1.0 to 4.1 percentage points. All are statistically significant at either the 5%- or 10%-level. In general, estimates based on a bandwidth of 1,000 meters or less are quite stable and similar in size, while those estimated using all observations within 5 kilometers of the DMA border increase with the order of the local polynomial. As we control more flexibly for distance to the border, the point estimates stabilize between 3 and 4 percentage points, regardless of bandwidth. (For comparison, cross-validation generally suggests bandwidths less than 1,000 meters.) Appendix Table A.4 decomposes the estimates into changes in turnout among registered Democrats and Republicans. Broadly summarizing, although the coefficients are often statistically insignificant, the sign pattern suggests that registered Democrats are more likely to vote—even in absolute terms—when the Democratic candidate advertises more than the Republican one. The opposite appears to be true for registered Republicans. The reduced form evidence, therefore, shows that partisan differences in presidential advertising alter the partisan composition of the electorate. The middle panel of Table 9 displays the "first stage," i.e., the denominator of the Wald estimator. Specifically, the estimates refer to δ in the following econometric model:

$$(8) \qquad (GRP_D - GRP_R)_{i,s} = \alpha_s + \delta \mathbf{1}\left[m > 0\right] + g_l\left(m\right) \times \mathbf{1}\left[m < 0\right] + g_r\left(m\right) \times \mathbf{1}\left[m > 0\right] + \varepsilon_{i,p,s}.$$

Given the use of DMA-level averages as proxies for voters' exposure to political advertising, the coefficients are fairly stable, hovering between 2,000 and 2,600 GRPs. As mentioned before, however, there is reason to suspect that this number overstates the true size of the discontinuity. The lower panel of Table 9 uses two-stage least squares to implement the Wald estimator. Individual point estimates range from 0.004 to 0.019. Most are statistically significant. Again, as we control more flexibly for distance to the border, the estimates stabilize around 0.015. Taking this coefficient at face value, a standard deviation increase in the partisan gap in advertising raises turnout among registered Democrats by 3 percentage points relative to their Republican counterparts. Table 10 replicates the previous analysis, restricting attention to voters who are *not* affiliated with any of the two major parties. In sharp contrast to the results above, partisan differences in political advertising appear to have no measurable impact on turnout among "independents." The finding that there is no effect on unaffiliated voters helps to ameliorate concerns about other, unobserved variables that may also vary discontinuously across media market borders *and* affect turnout. In sum, the evidence from our "differences in discontinuities" design suggests that political advertising tilts the composition of the electorate in favor of the candidate that advertises more.

6.3. *Assessing the Importance of Compositional Changes*

Yet, how important are these compositional shifts? To answer this question consider the following back-of-the-envelope calculations. 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

$$\Delta\left(v_D - v_R\right) \approx \left(2\pi - 1\right) s\Delta\left(t_D - t_R\right). \tag{9}$$

The estimates in the lower panel of Table 9 suggest that increasing the partisan difference in advertising by 1,000 GRPs raises $\Delta\left(t_D - t_R\right)$ by 1 to 2 percentage points. Using this range and assuming that $s = 0.28$ (cf. Table 8), Figure 4 plots the right-hand side of equation (9) as a function of π. Naturally, as the fraction of partisans who vote for the candidate of their own party goes up, the partisan difference in vote shares widens. Importantly, Figure 4 shows that for values as low as $\pi = 0.7$, the estimated shift in the partisan composition of the electorate increases the partisan difference in vote shares by 0.112 to 0.224 percentage points. In other words, if, conditional on going to the polls, seven out of ten partisans vote for their own party's candidate, then compositional shifts explain between 41% and 82% of the effect size implied by the sum of the coefficients in column (9) of Table 7.

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.[30] Although self-reported votes are notoriously unreliable indicators of actual choices, the available evidence suggests that π may even exceed 0.7. As a consequence, changes in turnout among partisans alone can explain most, if not all, of the impact of political 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.5–A.15 and in Figures A.1–A.3.

Briefly, to check for irregularities in the running variable, we look at population density in the vicinity of DMA borders. Based on the results in Figure A.1, there is no 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 systematic differences in how long voters on either side of the border have been registered at their current address (cf. Table A.5),

[30] Restricting attention to "strong" partisans results in estimates that are a few percentage points larger.

which helps to ameliorate concerns about selective attrition from our sample.

Tables A.6–A.8 test for discontinuities in the share of registered Democrats, voters' age, and gender. The point estimates in these tables are economically small, and in the vast majority of specifications statistically insignificant. In the same vein, Table A.9 shows that, on average, partisan differences in non-presidential political advertising do not systematically vary across media market borders. In particular, the sign of the estimated discontinuity does not only change from one specification to the other, but its size is often more than an order of magnitude smaller than the respective coefficient in Table 9. Even if there was a discontinuity in non-presidential political advertising that we, for whatever reason, fail to pick up, our reduced form results in the upper panel of Table 9 would still be capturing the effects of political advertising as a whole, of which presidential advertising is the most important component.[31]

The evidence in Table A.10 indicates that our RD estimates are robust to controlling for voters' observable characteristics as well as 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 our geocoding procedure is able to locate the exact street address (cf. Table A.11). In Table A.12, we show that one would draw similar conclusions if one were to discard all DMA border segments with less than a 500-GRP jump in the partisan difference in presidential advertising.

In order to gauge 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 U.S. In particular, it released a list of counties where out-of-market broadcast stations have a nontrivial viewership. In Table A.13 we restrict our sample to voters who live in counties where not a single out-of-market station appears on the FCC's list. Consistent with the idea that measurement error in advertising introduces attenuation bias, the resulting Wald estimates range from 0.007 to 0.071. While these estimates are generally larger than their counterparts in Table 9, we note that the smaller sample size leads to standard errors that make any quantitative comparisons highly speculative.

Lastly, Table A.14 demonstrates that our previous finding of a null effect of political advertising on overall turnout is not an artifact of using aggregate data. The coefficients in this table are based on a slight modification of the estimator in equation (6). Instead of constructing our instrument by assigning a particular voter to either side of the DMA

[31] When using our border-pair identification strategy the coefficients on non-presidential political advertising in Tables 3, 5, and 7 are always close to zero and statistically insignificant, which suggests that non-presidential advertising exerts little to no independent effect.

border according to the partisan difference in presidential advertising, we now do so based on whether *total* presidential advertising in her DMA exceeds that in the neighboring one. Reassuringly, the Wald estimates of political advertising's effect on aggregate turnout are very close to and statistically indistinguishable from zero (see Appendix Figures A.2 and A.3 for a graphical depiction). This finding continues to hold when we focus on voters in communities that do not appear on the FCC's Significantly Viewed List (cf. Table A.15). We, therefore, conclude that political advertising induces changes in the partisan composition of the electorate, which tend to offset in the aggregate.

7. Discussion

To put the estimates in this paper into perspective we follow DellaVigna and Kaplan (2007) and calculate persuasion rates, i.e., the percentage of individuals who changed their behavior in response to seeing a political ad. Formally, the persuasion rate is defined as

$$f_p = \frac{1}{1-\widetilde{y}_p}\frac{\Delta y_p}{\Delta Ads_p}, \tag{10}$$

where $\Delta y_p/\Delta Ads_p$ approximates the change in the outcome of interest induced by seeing one additional advertisement by candidate p, and $1-\widetilde{y}_p$ is the fraction of individuals who may be swayed by the respective candidate's message.[32]

We take the outcome of interest to be the partisan difference in vote shares defined as percentage of the entire voting-aged population. Defining y in this way has two advantages: (i) It is not necessary to adjust for changes in turnout, and (ii) we capture advertising's positive effect on own vote shares as well as the negative impact on the support for political rivals. The latter point is important. If the Democratic candidate, for instance, is purely office motivated, then he should be indifferent between one more vote for himself or 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. To proxy for $\widetilde{y}_p$ we use the results in columns (3) and (6) of Table 7, and predict vote shares in the absence of advertising by the respective candidate. To proxy for $\Delta y_p/\Delta Ads_p$ we rely on the point estimates in column (9) of the same table, divided by ten to account for the fact that the coefficients refer to the impact of an additional 1,000 GRPs—the equivalent of the average TV viewer seeing ten more ads.

With the above-mentioned approximations in hand, the persuasion rate of Democratic spots is given by $f_D = (1/0.702)\,(0.0018)\,/10 \approx 0.03\%$, while that for Republican advertising equals $f_R = (1/0.742)(0.001)/10 \approx 0.01\%$.

[32] Appendix A derives equation (10) formally.

These numbers are at least one, often two orders of magnitude smaller than the persuasion rates reported in the literature (cf. DellaVigna and Gentzkow 2010). This should not come as a surprise. Arguably, watching a thirty-second political ad constitutes a far less intense treatment than having year-round access to additional TV stations or newspapers. From a theoretical perspective the effect of partisan advertising ought to be smaller than that of slanted news, at least as long as journalists are more likely to truthfully reveal information than campaigns (Knight and Chiang 2011). However, given the sheer scale of political advertising and the ongoing debate about campaign finance regulation, the perhaps more important question is: Does advertising have economically meaningful aggregate effects?

Within 60 days leading up to the 2008 presidential election, the average media market registered approximately 4,000 GRPs in support of Barack Obama and about 2,600 GRPs favoring John McCain. According the U.S. Census Bureau about 206 million Americans were eligible to vote that year (File and Crissey 2012). Given the persuasion rates above, political advertising impacted a total of 2.1 million voting decisions.[33] 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 GRPs in favor of Barack Obama to the same level as those for John McCain would have cost the former about 600,000 votes.[34] 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.

As a further way of putting the impact of political advertising into perspective, we note that a shift of 600,000 votes is about three times as large as the nationwide "FOX News effect" (DellaVigna and Kaplan 2007).[35] While DellaVigna and Kaplan (2007) report *much* higher persuasion rates than we do, the enormous scale of political advertising results in a nonnegligible aggregate impact.

8. Concluding Remarks

This paper studies the persuasive effects of political advertising. In doing so, we propose a new approach to estimating the impact of 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 political advertising has

[33]Democratic advertising affected $(206\times10^6)\,(0.702)\,(0.0003\times40) \approx 1.7\times10^6$ votes, while the corresponding number for Republican spots is given by $(206 \times 10^6)\,(0.742)\,(0.0001 \times 28) \approx 0.4 \times 10^6$.

[34]In symbols, $\Delta votes_D = (206 \times 10^6)\,(0.702)\,(0.0003 \times 14) \approx 0.6 \times 10^6$.

[35]DellaVigna and Kaplan (2007) estimate that the introduction of FOX News to local cable networks increased the number votes for George W. Bush during the 2000 presidential election by about 200,000.

virtually no impact on overall turnout. At the same time, our results show that advertising alters the partisan composition of the electorate. Since registered partisans are much more likely to vote for their own party's candidate than his competitor, these compositional changes result in nontrivial effects on actual vote shares.

Our findings help to reconcile an important puzzle. Why do modern campaigns advertise so much, despite only small effects on overall voter engagement and individuals' opinions about candidates (Gerber et al. 2011; Krasno and Green 2008)? Our answer is that while political advertising may not be able to affect individuals' preferences, it brings the "right" set of voters to the polls.

More generally, our work sheds light on the channels through which the persuasive effects of the media operate. At least for the case of political ads, the evidence indicates that much of their impact comes from the extensive margin (i.e., who turns out to vote) rather than from changes at the intensive one (i.e., shifts in preferences and beliefs). Whether these findings generalize to other settings remains an important question for future research.

Finally, the results in this paper have implications for public policy, especially for campaign finance regulation. Given the size of the estimates above, partisan imbalances in political advertising have the potential to decide close elections. While advertising may be welfare-improving if it provided voters with new information about candidates, the finding that most of its impact comes from a partisan reshuffling of the electorate casts doubt on this argument.[36] In addition, our results suggest that candidates' advertising efforts partially offset each other. A benevolent social planner would, therefore, curb at least some of the associated spending.

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[36] Any model in which political advertising supplies citizens with valuable information would have to rationalize why there is no effect on independent voters, all the while partisans, whose beliefs about the relative quality of candidates are presumably more settled, react to it. By contrast, models in which advertising mobilizes core supporters by affecting the perceived benefit of voting *are* consistent with the most salient patterns in the data (see, e.g., Shachar and Nalebuff 1999).

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APPENDIX MATERIALS

A. Derivation of Equation (10)

Here, we formally derive the expression for the persuasion rate in Section 7. The main difference between our setup and existing derivations, i.e. DellaVigna and Kaplan (2007) and Enikolopov et al. (2011), is that we consider the outcome of interest to be the *difference* in candidates' vote shares, which are defined with respect to the entire voting-eligible population. The expressions below show that this leaves the basic structure of the formula unchanged.

Formally, let y_D and y_R denote the observed vote shares of the Democratic and Republican candidates, respectively. Further, let f_p denote the persuasion rate of the spots sent by candiate $p \in \{R, D\}$ with Ads_p indicating their quantity. Hypothetical vote shares in the absence of advertising by candidate p are given by $\widetilde{y}^p_D$ and $\widetilde{y}^p_R$. Assuming that f_p is constant and does not vary by audience, the partisan difference in vote shares equals

$$y_p - y_{\neg p} = \left(\widetilde{y}^p_p - \widetilde{y}^p_{\neg p}\right) + \underbrace{f_p\left(1 - \widetilde{y}^p_p - \widetilde{y}^p_{\neg p}\right) Ads_p}_{\text{effect on ``abstainers''}} + \underbrace{f_p\left(\widetilde{y}^p_{\neg p}\right) Ads_p}_{\text{effect on supporters of rival}} .$$

Collecting terms and rearranging gives

$$f_p = \frac{1}{1 - \widetilde{y}^p_p} \frac{(y_p - y_{\neg p}) - \left(\widetilde{y}^p_p - \widetilde{y}^p_{\neg p}\right)}{Ads_p}.$$

Setting $\Delta y = (y_p - y_{\neg p}) - \left(\widetilde{y}^p_p - \widetilde{y}^p_{\neg p}\right)$ and $\Delta Ads_p = Ads_p$ produces equation (10), as desired.

B. Data Appendix

This appendix provides a description of all data used in the paper, as well as precise definitions together with the sources of all variables. We first describe each source of data. We then explain the construction of our samples, and how we calculate every variable.

B.1. *Data Sources*

B.1.1. *Campaign Media Analysis Group*

Data on political advertising during the 2004 and 2008 general elections come from Kantar Media and the Campaign Media Analysis Group (CMAG). We obtained these data through the Wisconsin Advertising Project (http://wiscadproject.wisc.edu/). The 2004 sample includes political advertising spots aired in the 100 largest DMAs. In 2008, CMAG expanded coverage to all 210 DMAs.

B.1.2. *Nielsen Media Data*

Data on advertisement ratings come from the Nielsen Media Data file. The data include the estimated number of viewers of each recorded advertisement in any of the 210 DMAs.[37] Because Nielsen does not include estimates for every single political advertisement in the CMAG sample, we assign a viewership estimate to each political advertisement based on the average viewership of all advertisements recorded in the Nielsen data that air within the same 30 minute window (i.e., 0–29 minutes and 30–59 minutes) as the political advertisement. Ninety-five percent of political advertisements are successfully matched through this process.[38] When no viewership data are available within the same 30-minute window as the political advertisement, the number of viewers is imputed based CMAG's estimated cost for the spot. The imputed value is calculated from a linear regression of the estimated number of viewers on a quadratic function of CMAG's estimated cost for other spots in the same market and year that were successfully matched to Nielsen data.

To construct Gross Ratings Points (GRPs), we divide the number of estimated viewers by the Market Size Universe Estimates provided by Nielsen.

Nielsen data also include a crosswalk that maps each county to demographic market areas. All but four counties are uniquely assigned to a DMA. Apache County (AZ), El Dorado County (CA), Imperial County (CA), and Nevada County (CA) are assigned to more than one DMA. These four counties are excluded from the analysis.[39]

B.1.3. *Election Results*

County-level information on the total number of voters, votes for each presidential candidate, write-ins, etc. come from the *Atlas of U.S. Presidential Elections* (Leip 2014). These data are compiled from state-level election offices, and include results from the 2000, 2004, 2008 elections. Counties in Alaska are excluded from the analysis because the election results are not available at the county-level. The data also do not include election results from Kalawao County (HI).

B.1.4. *U.S. Census Data*

Information on the racial composition of each county come from the U.S. Census *Intercensal Estimates of the Resident Population by Sex, Race, and Hispanic Origin for Counties*, and are based on an official estimate as of July 1 for each year.[40]

[37] Nielsen includes data on three types of advertisement spots: Spot TV, Network Clearance Spot TV, and Syndicated Clearance Spot TV.

[38] This statistic is based all political advertisements within 60 days of the U.S. Presidential election, the main specification in the paper.

[39] In the raw data, Holmes County (FL) is incorrectly assigned to the Dothan DMA. We reassign it to the Panama City DMA.

[40] The U.S. Census aggregates information from Clifton Forge (VA) and Alleghany County (VA). As a result, we exclude these two counties from the analysis. Broomfield County (CO) separated from Boulder County (CO) in 2001. Because the 2000 U.S. Census and 2000 election results are not available for these counties individually, we also exclude them from the analysis.

Information on the number of individuals that are at least 18 years old come from two sources. First, the number of adults 20 years or older come from the U.S. Census *Intercensal Estimates of the Resident Population by Five-Year Age Groups, Sex, Race, and Hispanic Origin for Counties.* These data are based on an official estimate as of July 1 for each year. The number of 18 and 19 year olds in each year are imputed based on a linear interpolation of the number of 18 and 19 year olds using the 2000 and 2010 *Decennial Censuses of Population and Housing.*

Data on poverty and income come from the U.S. Census *Small Area Income & Poverty Estimates* (SAIPE).

Information on counties' spatial position come from 2010 U.S. Census TIGER/Line Shapefiles. The distance between two counties is the minimum distance between the county borders, measured as an equirectangular projection of the longitude and latitude using every tenth geocoded point. Any counties less than 10 kilometers apart by this measure are characterized as neighboring counties.

B.1.5. *Candidate Visits*

Information on the number of candidate visits to each media market was generously provided to us by Daron Shaw (Huang and Shaw 2009; Shaw 2007). The data for the 2004 election cover the period from September 3 to November 1, and that for the 2008 election encompass visits between June 11 and November 4. Only public appearances by candidates are counted. Private fund-raising events, vacation days, and working days in Washington, D.C. are excluded.

B.1.6. *Voter Registration Data*

Information on turnout of individual voters come from official voter registration lists that are maintained by the states. Instead of approaching each state's board of elections individually, we purchased these data in bulk from emerges.com, a non-partisan data vendor that specializes in compiling and standardizing public records, including registered voter lists. The data we obtained are current as of 2013/14 (depending on the state) and contain voters' residential addresses and turnout histories. The latter include turnout in the 2008 presidential election for voters in all states but New Hampshire. For most states, the data also include information on individuals' date of birth, gender, and party affiliation. To make use of the spatial information in the data we rely on the Census Bureau's TIGER/Line shapefiles in order to geocode voters' residential addresses. In total these data include records for more than 180 million registered U.S. voters.

B.2. *Samples*

Our county-level data include all counties with information on political advertising spots aired during the 2004 and 2008 U.S. Presidential campaigns, Nielsen ratings for the respective DMAs, and county-level votes for the 2000, 2004, and 2008 U.S. Presidential elections. Our analysis of the sample of "border-pair counties" is further restricted to all counties that share a border with at least one other county that is assigned to a different demographic market area within the same state. In what follows, we give precise definitions for each sample.

All Counties is a data set that includes all counties with information on political advertising spots aired during the 2004 and 2008 U.S. Presidential campaigns, Nielsen ratings for the advertising spots aired in demographic market area of the county, and county-level votes for the 2000, 2004, and 2008 U.S. Presidential elections. The unit of observation in this data set is a county-year. The data encompass a total of 5,117 observations and 3,084 unique counties.

Border-Pair Counties is a data set that includes all county-year observations in the All Counties data set that share a border with another county that is assigned to a different demographic market area within the same state. The unit of observation is a county-year-pair. As explained in the main text, county-year may appear in the data set more than once if it shares a border with more than one county in the same state but in a different DMA. There are a total of 6,598 observations in this data set, 3,663 unique year-counties, and 2,250 unique counties.

Border-Pair Differences is a data set of the differences between paired counties in the Border-Pair Counties data set. The order of differencing is normalized such that the difference in the variable Presidential GRPs is weakly positive. Paired counties with the same value of Presidential GRPs are differenced randomly. There are 3,299 observations in this data set.

Our RD sample is based on the voter registration files of the lower forty-eight states and the District of Columbia. We restrict attention to voters for whom we can derive geocodes either based on their exact street address, zip code, or city (together about 98.8% of all records; see Table 8 for numbers split by the quality of "match"), and who live in a state with at least one within-state DMA border segment (c. 90% of records). The remainder of observations is excluded because a voter either registered for the first time after the 2008 general election, or because she updated her registration thereafter. The later restriction is necessary because individuals who updated their registration are likely to have moved, meaning that we do not have reliable information on where they lived in November of 2008. In total, our RD sample includes 124,887,857 observations.

B.3. *Variable Definitions*

B.3.1. *Advertising Measures*

Presidential GRPs (in 1,000s) is the total amount of advertising—measured in Gross Ratings Points divided by 1,000—by candidates, parties, and interest groups related to the U.S. Presidential Democratic and Republican candidates within 60 days of the election.

Positive Presidential GRPs (in 1,000s) is the amount of advertising—measured in Gross Ratings Points divided by 1,000—by candidates, parties, and interest groups related to the U.S. Presidential Democratic and Republican candidates within 60 days of the election that promotes any candidate. The tone of the advertisement is determined by human coders of the University of Wisconsin Advertising Project.

Negative Presidential GRPs (in 1,000s) is the amount of advertising—measured in Gross Ratings Points divided by 1,000—by candidates, parties, and interest groups related to the U.S. Presidential Democratic and Republican candidates within 60 days of the election that attacks or contrasts candidates. The tone of the advertisement is determined by human coders of the University of Wisconsin Advertising Project.

GRPs within 180 Days Before Election (in 1,000s) is the total amount of advertising—measured in Gross Ratings Points divided by 1,000—by candidates, parties, and interest groups related to the U.S. Presidential Democratic and Republican candidates within 180 days of the election.

GRPs within 120 Days Before Election (in 1,000s) is the total amount of advertising—measured in Gross Ratings Points divided by 1,000—by candidates, parties, and interest groups related to the U.S. Presidential Democratic and Republican candidates within 120 days of the election.

GRPs within 30 Days Before Election (in 1,000s) is the total amount of advertising—measured in Gross Ratings Points divided by 1,000—by candidates, parties, and interest groups related to the U.S. Presidential Democratic and Republican candidates within 30 days of the election.

Partisan Difference in GRPs (in 1,000s) is the difference in the total amount of advertising—measured in Gross Ratings Points divided by 1,000—between Democratic and Republican candidates, parties, and interest groups related to the U.S. Presidential Democratic and Republican candidates within 60 days of the election. A positive number indicates relatively more Democratic advertising.

Impressions per Capita is the total amount of advertising—measured in viewers divided by the U.S. Census population in the market (in 1,000s)—by candidates, parties, and interest groups related to the U.S. Presidential Democratic and Republican candidates within 60 days of the election.

Number of Political Ads (in 1,000s) is the total amount of advertising—measured as the total number of seconds of advertising divided by 30—by candidates, parties, and interest groups related to the U.S. Presidential Democratic and Republican candidates within 60 days of the election.

Non-Presidential Political Advertising (in 1,000 GRPs) is the total amount of political advertising—measured in Gross Ratings Points divided by 1,000—by candidates, parties, and interest groups related to Congressional, Gubernatorial, and other non-U.S. Presidential elections within 60 days of the election.

Republican Non-Presidential Political Advertising (in 1,000 GRPs) is the total amount of political advertising—measured in Gross Ratings Points divided by 1,000—by candidates, parties, and interest groups related to Congressional, Gubernatorial, and Republican other non-U.S. Presidential elections within 60 days of the election.

Democratic Non-Presidential Political Advertising (in 1,000 GRPs) is the total amount of political advertising—measured in Gross Ratings Points divided by 1,000—by candidates, parties, and interest groups related to Congressional, Gubernatorial, and Democratic other non-U.S. Presidential elections within 60 days of the election.

Partisan Difference in Non-Presidential Political Advertising (in 1,000 GRPs) is defined as Democratic Non-Presidential Political Advertising (1,000 GRPs) minus Republican Non-Presidential Political Advertising (1,000 GRPs).

Candidate Visits are the total number of visits by Presidential and Vice-Presidential candidates of the two major parties aggregated to the DMA level.

Republican Candidate Visits are the total number of visits by Republican Presidential and Vice-Presidential candidates aggregated to the DMA level.

Democratic Candidate Visits are the total number of visits by Democratic Presidential and Vice-Presidential candidates aggregated to the DMA level.

Partisan Difference in Candidate Visits is defined as Democratic Candidate Visits minus Republican Candidate Visits.

B.3.2. *Election Results*

Turnout (%) is the total number of votes for all U.S. Presidential candidates divided by the population age 18 and older.

Lagged Turnout (%) is the total number of votes for all U.S. Presidential candidates in the previous U.S. Presidential election divided by the population age 18 and older.

Democratic Two-Party Vote Share (%) is the number of votes for the Democratic U.S. Presidential candidate divided by the total number of votes for the U.S. Presidential Democratic and Republican candidates.

Republican Two-Party Vote Share (%) is the number of votes for the Republican U.S. Presidential candidate divided by the total number of votes for the U.S. Presidential Democratic and Republican candidates.

Partisan Difference in Two-Party Vote Share (%) is the number of votes for the Democratic U.S. Presidential candidate minus the number of votes for the Republican U.S. Presidential candidate divided by the total number of votes for the U.S. Presidential Democratic and Republican candidates.

Democratic Vote Share (as Percentage of the Voting-Aged Population) is the number of votes for the Democratic U.S. Presidential candidate divided by the number of adults in the county

18 years old and older.

Republican Vote Share (as Percentage of the Voting-Aged Population) is the number of votes for the Republican U.S. Presidential candidate divided by the number of adults in the county 18 years old and older.

Partisan Difference in Vote Shares (as Percentage of the Voting-Aged Population) is the number of votes for the Democratic U.S. Presidential candidate minus the number of votes for the Republican U.S. Presidential candidate divided by the number of adults in the county 18 years old and older.

Battleground State is the list of states characterized as a "toss up" by RealClearPolitics.com approximately sixto eight weeks prior to the election. The 2004 definition is based on the earliest polling data posted on the site on September 21, 2004. The 2008 definition is based on polling data from August 24, 2008.

Nonbattleground State is the list of states not characterized as a "toss up" by RealClearPolitics.com approximately six to eight weeks prior to the election. The 2004 definition is based on the earliest polling data posted on the site on September 21, 2004. The 2008 definition is based on polling data from August 24, 2008.

B.3.3. *Demographic & Economic Measures*

Percent White (%) is the percentage of population that is characterized as White alone as measured by the U.S. Census *Intercensal Estimates of the Resident Population by Sex, Race, and Hispanic Origin for Counties.*

Percent Black (%) is the percentage of population that is characterized as Black or African American alone as measured by the U.S. Census *Intercensal Estimates of the Resident Population by Sex, Race, and Hispanic Origin for Counties.*

Percent Hispanic (%) is the percentage of population that is characterized as Hispanic alone as measured by the U.S. Census *Intercensal Estimates of the Resident Population by Sex, Race, and Hispanic Origin for Counties.*

Percent Asian (%) is the percentage of population that is characterized as Asian alone as measured by the U.S. Census *Intercensal Estimates of the Resident Population by Sex, Race, and Hispanic Origin for Counties.*

Percent Other Race (%) is the percentage of population that is characterized as American Indian, Native Hawaii, Other Pacific Islander, or two more races as measured by the U.S. Census *Intercensal Estimates of the Resident Population by Sex, Race, and Hispanic Origin for Counties.*

Percent Minority (%) is the percentage of population that is not characterized as White alone as measured by the U.S. Census *Intercensal Estimates of the Resident Population by Sex, Race, and Hispanic Origin for Counties.*

Percent High School Dropouts (%) is based on a linear interpolation of the percentage of the population with less than a high school education, as identified in the GeoLytics Estimates Premium data for 2000 and 2010. GeoLytics takes these numbers from the *Decennial Censuses of Population and Housing.*

Percent High School Educated (%) is based on a linear interpolation of the percentage of the population with a high school education only, as identified in the GeoLytics Estimates Premium data for 2000 and 2010. GeoLytics takes these numbers from the *Decennial Censuses of Population and Housing.*

Percent College Educated (%) is based on a linear interpolation of the percentage of the population with at least an associate's degree, as identified in the GeoLytics Estimates Premium data for 2000 and 2010. GeoLytics takes these numbers from the *Decennial Censuses of Population and Housing.*

Median Household Income (in $1,000) is a measure of median household income in the county from the *Small Area Income & Poverty Estimates* (SAIPE) provided by the U.S. Census.

Percent in Poverty (%) is measure of percentage of residents in poverty in the county based on the U.S. Census *Small Area Income & Poverty Estimates* (SAIPE).

Unemployment Rate (%) is a county-level, annual measure of unemployment based on *Local Area Unemployment Statistics* from the Bureau of Labor Statistics.

Total Population (in 1,000s) come from the *Intercensal Estimates of the Resident Population by Five-Year Age Groups, Sex, Race, and Hispanic Origin for Counties* and are based on an official estimate of July 1 for each year.

Population Share of Media Market is defined as the population of a particular county divided by the population of all counties assigned to the same DMA as the county.

B.3.4. *Individual-Level Variables*

Turnout in 2008 is an indicator variable equal to one if an individual's turnout history indicates that she voted in the 2008 general election, and zero otherwise.

Distance to Nearest Media Market Border (in meters) is defined as the straight-line distance from an individual's official residence to the nearest point on any DMA border within the state of registration. As explained in the main text, we multiply this number by negative one for all voters who live in a media market where the partisan difference in presidential GRPs (as defined above) is smaller than in the neighboring one.

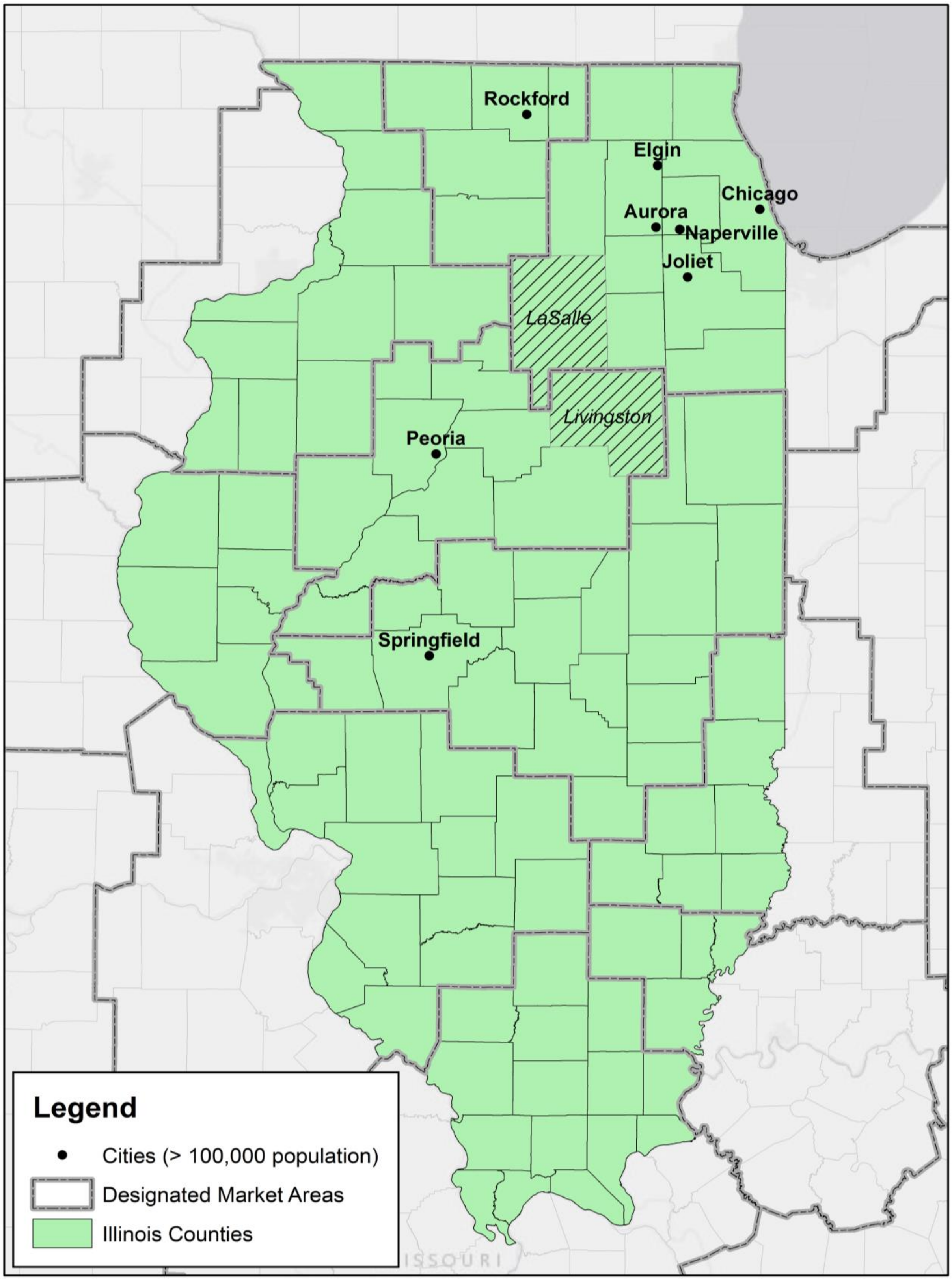
Party Affiliation is a set of indicator variables derived from the party field in states' voter registration lists in conjunction with individuals' turnout histories. We classify an individual voter as "registered Republican" or "registered Democrat" if the state lists her as such. We amend this classification by taking into account in which party's primaries, if any, a voter participated. Individuals who are not registered as partisans with the respective states and whose turnout history indicates that they have not participated in any of the major two parties' primaries are grouped in the residual category: "unaffiliated/independent." The same is true for individuals who participated in different parties' primaries.

Years Registered at Current Address is defined as the number of years, as of 2008, since a voter registered at the address in our data, based on the date of registration in states' files.

Age is defined as an individuals' age, as of 2008. This variable is missing for voters in states that do not supply information on date of birth in their voter registration files.

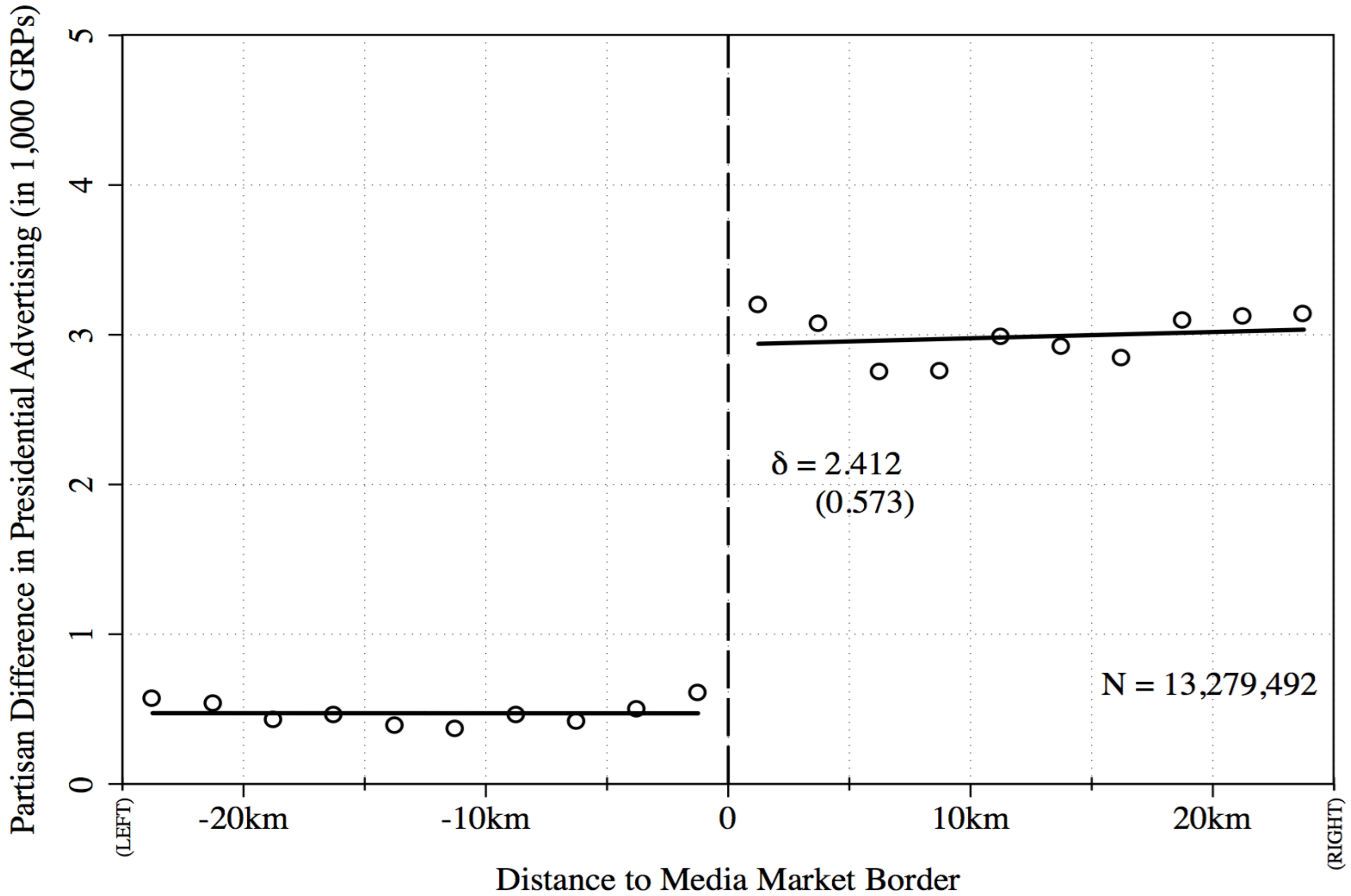
Female is an indicator variable equal to one if the individual is listed as female on the state's list, and zero if the record indicates a male. This variable is missing for voters in states that do not supply information on gender in their voter registration files.

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



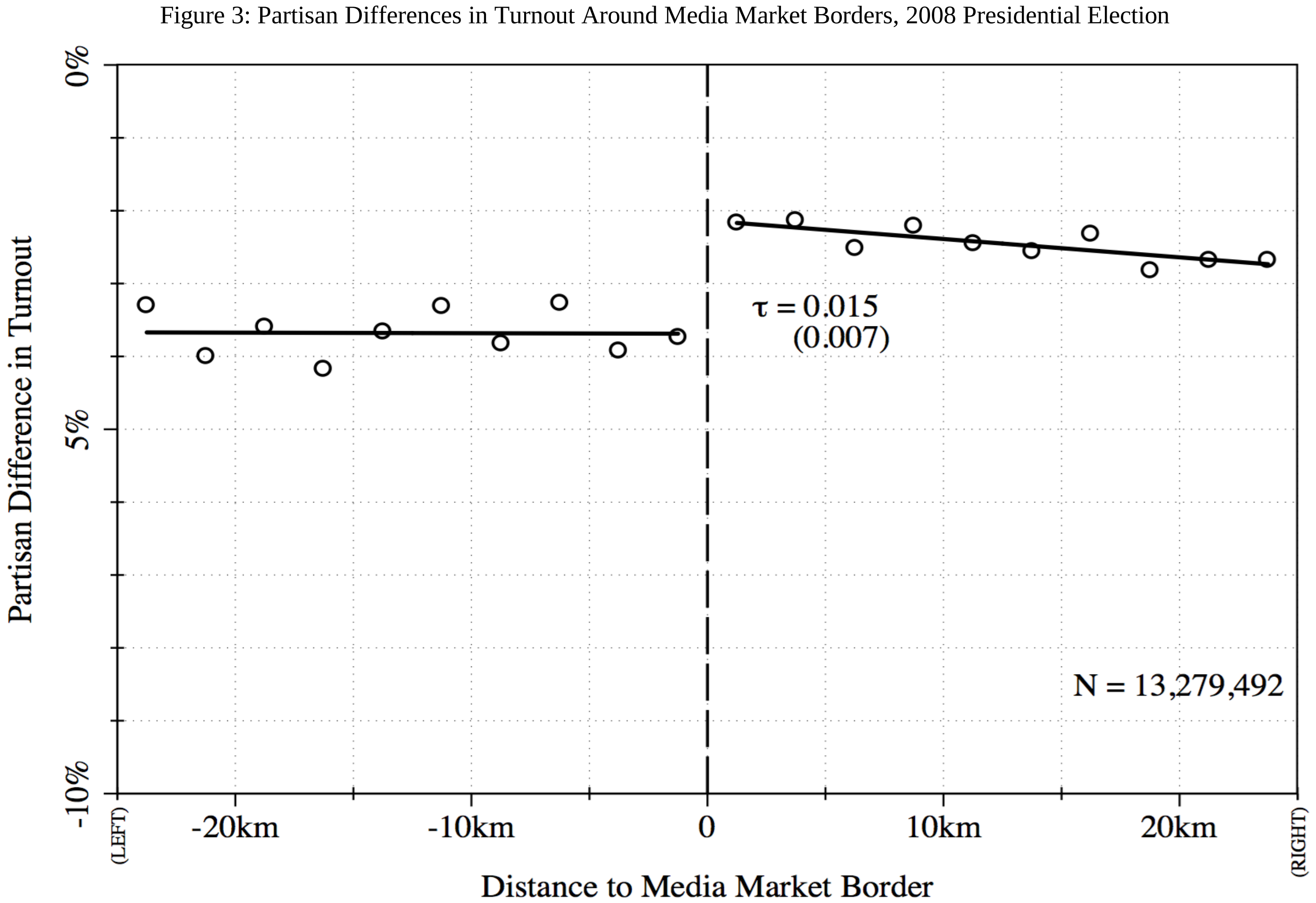
Notes: Figure displays counties, media markets, and cities with more than 100,000 population in the state of Illinois.

Figure 2: Average Partisan Difference in Political Advertising in the RD Setup, 2008 Presidential Election



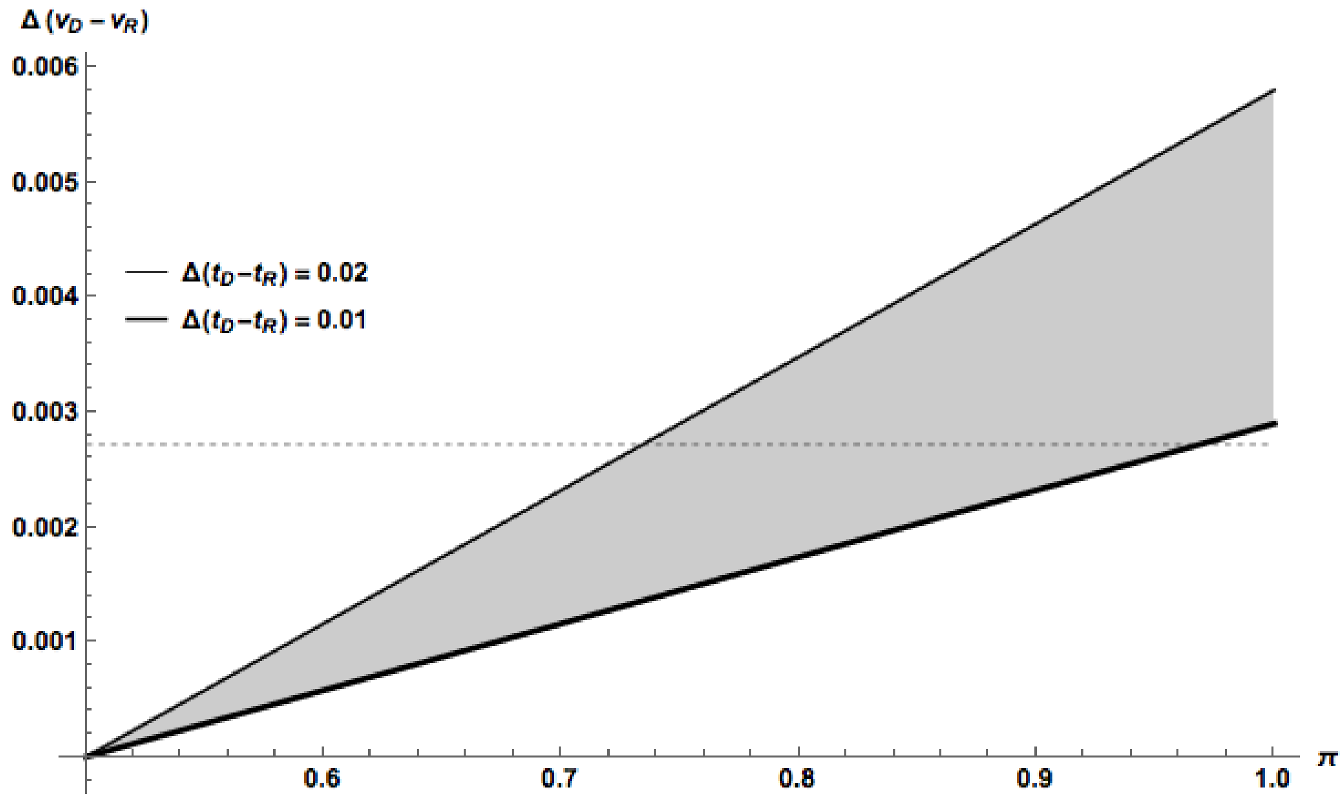
Notes: Figure plots the mean partisan difference in advertising during the 2008 presidential election within 2.5km-wide bins around media market borders. Larger values indicate more advertising in support of the Democratic candidate than for his Republican competitor. The sample consists of all registered Democrats and Republicans for whom our voter registration data contains a valid address as of the 2008 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, 2008 Presidential Election



Notes: Figure plots the mean partisan difference in turnout during the 2008 presidential election 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 contains a valid address as of the 2008 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 $\Delta(v_D - v_R)$, as defined in equation (9), as a function of π and for a range of plausible values of $\Delta(t_D - t_R)$. Substantively, $\Delta(v_D - v_R)$ measures the change in the partisan difference in vote shares due to changes in the partisan composition of the electorate. The dashed line indicates the effect size implied by the sum of the coefficients in column (9) of Table 7. See the main text for details.

Table 1: Descriptive Statistics, Pooling Across 2004 & 2008

	All Counties				Border-Pair Counties			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Media Market Measures:								
Presidential GRPs (in 1,000s)	6.55	8.55	0.00	31.36	6.81	8.68	0.00	31.36
Partisan Difference in GRPs (in 1,000s)	1.07	2.01	-4.66	8.41	1.10	2.03	-4.66	8.41
Impressions per Capita	6.04	7.94	0.00	29.94	6.29	8.07	0.00	29.94
Number of Political Ads (in 1,000s)	3.10	4.20	0.00	17.34	3.20	4.23	0.00	17.34
Non-Presidential Political Advertising (in 1,000 GRPs)	13.09	11.51	0.00	51.40	12.95	11.39	0.00	51.40
Candidate Visits	2.21	3.41	0.00	18.00	2.23	3.40	0.00	18.00
County-Level Variables:								
Turnout (%)	58.50	9.24	19.24	100.00	58.05	9.13	24.93	100.00
Lagged Turnout (%)	56.29	9.71	15.16	100.00	55.97	9.52	26.29	100.00
Democratic Two-Party Vote Share (%)	41.48	13.44	5.03	93.40	40.90	12.79	5.03	89.98
Republican Two-Party Vote Share (%)	58.52	13.44	6.60	94.97	59.10	12.79	10.02	94.97
Percent White (%)	79.84	19.04	2.78	99.12	80.47	18.75	2.78	99.12
Percent Minority (%)	20.16	19.04	0.88	97.22	19.53	18.75	0.88	97.22
Percent High School Dropouts (%)	22.80	8.72	3.24	65.46	23.76	8.78	3.24	65.46
Percent High School Educated (%)	54.74	6.65	23.55	74.30	55.19	6.40	25.82	74.30
Percent College Educated (%)	22.46	9.00	6.45	67.63	21.06	8.13	6.45	62.24
Median Household Income (in $1,000)	42.33	11.24	18.38	111.58	40.84	10.38	18.38	102.25
Percent in Poverty (%)	13.73	5.51	2.25	49.38	14.26	5.58	3.05	49.38
Unemployment Rate (%)	5.74	1.91	1.30	22.40	5.92	1.93	1.70	22.40
Total Population (in 1,000s)	112	346	0	10,084	84	265	0	5,396
Number of Unique Counties	3,084				2,250			
Number of County-Year Observations	5,117				3,663			

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 Advertsing and Border-County Characteristics

Independent Variables (in Standard Deviation Units)	Δ Presidential Advertising (in Standard Deviation Units): Both Candiates		Democratic Candidate		Republican Candidate		Partisan Difference		Δ Total Non-Presidential Political Advertising	
Δ Total Population	-0.002	0.000	0.009	0.011	-0.017	-0.016	0.034	0.038	-0.005	0.003
	(0.035)	(0.036)	(0.043)	(0.044)	(0.023)	(0.023)	(0.046)	(0.048)	(0.017)	(0.019)
Δ Percent Black	0.000	0.007	0.007	0.011	-0.009	0.002	0.021	0.015	0.018	-0.005
	(0.029)	(0.037)	(0.028)	(0.036)	(0.029)	(0.036)	(0.022)	(0.027)	(0.039)	(0.042)
Δ Percent Hispanic	0.015	0.016	0.021	0.022	0.007	0.005	0.025	0.029	-0.018	-0.010
	(0.026)	(0.026)	(0.027)	(0.026)	(0.026)	(0.025)	(0.025)	(0.025)	(0.029)	(0.029)
Δ Percent Asian	0.004	0.006	-0.004	-0.003	0.015	0.017	-0.026	-0.025	-0.003	-0.004
	(0.023)	(0.022)	(0.026)	(0.025)	(0.019)	(0.019)	(0.030)	(0.028)	(0.017)	(0.018)
Δ Percent Other Race	-0.065	-0.057	-0.073	-0.068	-0.046	-0.036	-0.061	-0.064	0.016	0.000
	(0.036)	(0.038)	(0.036)	(0.037)	(0.035)	(0.036)	(0.033)	(0.031)	(0.031)	(0.034)
Δ Percent High School Dropouts	-0.002	0.000	0.009	0.011	-0.017	-0.016	0.034	0.038	-0.005	0.003
	(0.035)	(0.036)	(0.043)	(0.044)	(0.023)	(0.023)	(0.046)	(0.048)	(0.017)	(0.019)
Δ Percent College Educated	-0.020	-0.006	-0.002	0.014	-0.042	-0.030	0.049	0.060	-0.044	-0.032
	(0.036)	(0.045)	(0.037)	(0.047)	(0.036)	(0.044)	(0.043)	(0.051)	(0.050)	(0.048)
Δ Median Household Income		-0.025		-0.023		-0.025		-0.006		0.016
		(0.055)		(0.055)		(0.056)		(0.061)		(0.029)
Δ Percent in Poverty		-0.053		-0.049		-0.052		-0.014		0.023
		(0.065)		(0.061)		(0.066)		(0.044)		(0.040)
Δ Unemployment Rate		0.013		0.023		-0.002		0.038		0.086
		(0.038)		(0.041)		(0.034)		(0.041)		(0.032)
H_0: All Coefficients = 0										
F-Statistic	0.707	0.635	0.703	0.557	0.976	0.855	0.829	0.754	0.525	1.180
p -value	0.666	0.776	0.669	0.839	0.460	0.580	0.569	0.671	0.811	0.329
R-Squared	0.003	0.004	0.004	0.005	0.003	0.004	0.004	0.006	0.002	0.008
Number of Observations	3,299	3,299	3,299	3,299	3,299	3,299	3,299	3,299	3,299	3,299

Notes: Entries are coefficients and standard errors from regressing differences in political advertising between border-pair counties on differences in observables, 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.

Table 3: Estimating the Impact of Political Advertising on Voter Turnout, 2004 & 2008 Presidential Elections

A. All Presidential Advertising

	Percent Voter Turnout								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All Presidential GRPs (in 1,000s)	0.297	0.140	0.100	0.238	0.077	0.075	0.000	-0.008	0.016
	(0.075)	(0.049)	(0.030)	(0.081)	(0.056)	(0.031)	(0.055)	(0.047)	(0.019)
H_0: Coefficient on GRPs = 0									
t-Statistic	3.980	2.882	3.343	2.950	1.377	2.377	0.008	0.163	0.863
p -value	0.000	0.006	0.002	0.005	0.175	0.022	0.994	0.872	0.393
Fixed Effects:									
Year	No	Yes	Yes	No	Yes	Yes	No	No	No
Border Pair × Year	No	No	No	No	No	No	Yes	Yes	Yes
Controls:									
Demographic & Economic Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Candidate Visits & Non-Presidential GRPs	No	No	Yes	No	No	Yes	No	No	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-Squared	0.076	0.440	0.891	0.053	0.402	0.881	0.745	0.830	0.965
Number of Observations	5,117	5,117	5,117	6,598	6,598	6,598	6,598	6,598	6,598

B. Positive vs. Negative Presidential Advertsing

	Percent Voter Turnout								
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Positive Presidential GRPs (in 1,000s)	0.333	-0.019	0.160	0.941	0.296	0.131	0.092	-0.108	-0.011
	(0.333)	(0.342)	(0.145)	(0.301)	(0.306)	(0.171)	(0.344)	(0.278)	(0.110)
Negative Presidential GRPs (in 1,000s)	0.286	0.193	0.077	-0.002	0.002	0.053	-0.033	0.029	0.027
	(0.150)	(0.138)	(0.051)	(0.145)	(0.139)	(0.062)	(0.112)	(0.097)	(0.044)
H_0: Both Coefficients = 0									
F-Statistic	8.581	4.135	5.831	9.200	1.772	2.907	0.045	0.076	0.420
p -value	0.001	0.022	0.005	0.000	0.182	0.065	0.956	0.927	0.660
Fixed Effects:									
Year	No	Yes	Yes	No	Yes	Yes	No	No	No
Border Pair × Year	No	No	No	No	No	No	Yes	Yes	Yes
Controls:									
Demographic & Economic Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Candidate Visits & Non-Presidential GRPs	No	No	Yes	No	No	Yes	No	No	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-Squared	0.076	0.440	0.891	0.059	0.403	0.881	0.745	0.830	0.965
Number of Observations	5,117	5,117	5,117	6,598	6,598	6,598	6,598	6,598	6,598

Notes: Entries are coefficients and standard errors from estimating ϕ in equation (2) 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 positve 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 for the 2004 and 2008 presidential elections. 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. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For precise definitions and the sources of all variables, see the Data Appendix.

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

	Pooled		2004 Election		2008 Election	
	Coefficient	*p* -value	Coefficient	*p* -value	Coefficient	*p* -value
Baseline	0.016	0.393	0.000	0.995	0.024	0.219
	(0.019)		(0.032)		(0.019)	
Downweighting Stacked Observations	0.016	0.199	-0.002	0.915	0.024	0.067
	(0.012)		(0.021)		(0.013)	
By Population Share of Media Market:						
< 15%	0.016	0.415	-0.002	0.942	0.024	0.223
	(0.019)		(0.032)		(0.019)	
< 10%	0.016	0.406	-0.002	0.946	0.024	0.219
	(0.019)		(0.033)		(0.020)	
< 5%	0.020	0.316	0.001	0.988	0.029	0.112
	(0.020)		(0.034)		(0.018)	
< 2%	0.027	0.315	0.006	0.872	0.035	0.205
	(0.026)		(0.038)		(0.027)	
Alternative Advertising Measures:						
Number of Ads per 1,000 Population	0.000	0.551	0.000	0.958	0.000	0.499
	(0.000)		(0.001)		(0.000)	
Impressions per Capita (÷10)	0.017	0.381	-0.004	0.876	0.028	0.205
	(0.019)		(0.027)		(0.022)	
GRPs within 180 Days Before Election (in 1,000s)	0.009	0.473	0.002	0.930	0.013	0.352
	(0.013)		(0.022)		(0.013)	
GRPs within 120 Days Before Election (in 1,000s)	0.010	0.488	0.001	0.960	0.013	0.352
	(0.014)		(0.025)		(0.014)	
GRPs within 30 Days Before Election (in 1,000s)	0.018	0.491	-0.003	0.952	0.029	0.271
	(0.026)		(0.045)		(0.026)	
By Battleground Status:						
Battleground State	0.018	0.519	-0.016	0.204	0.045	0.139
	(0.027)		(0.010)		(0.028)	
Nonbattleground State	0.016	0.494	0.025	0.434	0.015	0.535
	(0.023)		(0.032)		(0.024)	

Notes: Entries are coefficients and standard errors on ϕ in equation (2), 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 advertsing (in 1,000 GRPs) 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 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 & 2008 Presidential Elections

	Partisan Difference in Presidential Two-Party Vote Shares								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Difference Between All Democratic and Republican Presidential GRPs (in 1,000s)	2.682	2.490	0.998	1.875	1.784	0.964	0.892	0.773	0.324
	(0.706)	(0.645)	(0.263)	(0.725)	(0.707)	(0.306)	(0.457)	(0.293)	(0.089)
H_0: Coefficient on Difference in GRPs = 0									
t-Statistic	3.797	3.864	3.797	2.587	2.522	3.150	1.952	2.643	3.652
p -value	0.000	0.000	0.000	0.013	0.015	0.003	0.057	0.011	0.001
Fixed Effects:									
Year	No	Yes	Yes	No	Yes	Yes	No	No	No
Border Pair × Year	No	No	No	No	No	No	Yes	Yes	Yes
Controls:									
Demographic & Economic Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Candidate Visits & Non-Presidential GRPs	No	No	Yes	No	No	Yes	No	No	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-Squared	0.040	0.409	0.920	0.025	0.371	0.897	0.788	0.895	0.986
Number of Observations	5,117	5,117	5,117	6,598	6,598	6,598	6,598	6,598	6,598

Notes: Entries are coefficients and standard errors from estimating ϕ in equation (2) 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 1,000 GRPs), 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 for the 2004 and 2008 presidential elections. 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. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For precise definitions and the sources of all variables, see the Data Appendix.

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

	Pooled		2004 Election		2008 Election	
	Coefficient	*p* -value	Coefficient	*p* -value	Coefficient	*p* -value
Baseline	0.324	0.001	0.200	0.268	0.341	0.003
	(0.089)		(0.178)		(0.109)	
Downweighting Stacked Observations	0.305	0.000	0.237	0.107	0.312	0.000
	(0.067)		(0.143)		(0.081)	
By Population Share of Media Market:						
< 15%	0.333	0.001	0.198	0.273	0.350	0.003
	(0.089)		(0.178)		(0.110)	
< 10%	0.351	0.000	0.231	0.210	0.358	0.003
	(0.090)		(0.181)		(0.113)	
< 5%	0.375	0.000	0.271	0.153	0.396	0.002
	(0.097)		(0.186)		(0.122)	
< 2%	0.422	0.003	0.422	0.138	0.433	0.017
	(0.132)		(0.278)		(0.175)	
Alternative Advertising Measures:						
Number of Ads per 1,000 Population	0.006	0.001	0.006	0.276	0.006	0.005
	(0.002)		(0.005)		(0.002)	
Impressions per Capita (÷10)	0.340	0.001	0.228	0.253	0.356	0.003
	(0.097)		(0.196)		(0.115)	
GRPs within 180 Days Before Election (in 1,000s)	0.254	0.002	0.186	0.063	0.250	0.026
	(0.076)		(0.097)		(0.109)	
GRPs within 120 Days Before Election (in 1,000s)	0.290	0.001	0.195	0.176	0.291	0.009
	(0.085)		(0.141)		(0.106)	
GRPs within 30 Days Before Election (in 1,000s)	0.272	0.091	0.198	0.450	0.281	0.165
	(0.158)		(0.260)		(0.199)	
By Battleground Status:						
Battleground State	0.379	0.026	0.178	0.410	0.408	0.047
	(0.150)		(0.186)		(0.177)	
Nonbattleground State	0.273	0.013	0.382	0.124	0.270	0.051
	(0.106)		(0.242)		(0.134)	

Notes: Entries are coefficients and standard errors on ϕ in equation (2), 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 advertsing (in 1,000 GRPs) 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 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: Estimating the Impact of Partisan Advertising on Eligible Voters, 2004 & 2008 Presidential Elections

	Democratic Vote Share (as Percentage of the Voting-Aged Popultation)			Republican Vote Share (as Percentage of the Voting-Aged Popultation)			Partisan Difference in Vote Shares (as Percentage of the Voting-Aged Popultation)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All Democratic Presidential GRPs (in 1,000s)	0.257	0.182	0.070	-0.218	-0.205	-0.108	0.475	0.387	0.176
	(0.168)	(0.119)	(0.041)	(0.134)	(0.099)	(0.052)	(0.256)	(0.165)	(0.071)
All Republican Presidential GRPs (in 1,000s)	-0.156	-0.008	-0.003	0.108	0.027	0.091	-0.264	-0.035	-0.096
	(0.202)	(0.149)	(0.063)	(0.182)	(0.146)	(0.075)	(0.342)	(0.239)	(0.112)
H_0: Both Coefficients = 0									
F-Statistic	1.575	2.269	2.461	1.353	2.191	1.859	1.697	2.796	2.886
p -value	0.090	0.008	0.003	0.167	0.011	0.036	0.062	0.001	0.001
Fixed Effects:									
Year	No	No	No	No	No	No	No	No	No
Border Pair × Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:									
Demographic & Economic Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Candidate Visits & Non-Presidential GRPs	No	No	Yes	No	No	Yes	No	No	Yes
Lagged Dependent Variable	No	No	Yes	No	No	Yes	No	No	Yes
Sample	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties	Border Counties
R-Squared	0.801	0.879	0.981	0.707	0.853	0.977	0.761	0.882	0.984
Number of Observations	6,598	6,598	6,598	6,598	6,598	6,598	6,598	6,598	6,598

Notes: Entries are coefficients and standard errors from estimating ϕ in equation (2) 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 set of included covariates varies across columns. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For precise definitions and the sources of all variables, see the Data Appendix.

Table 8: Summary Statistics for Voter Registration Data, 2008 Presidential Election

	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	48.1	17.1	49.2	17.1	49.7	16.8
Years Registered at Current Address	11.7	11.4	12.5	11.9	12.4	11.8
Turnout & Political Affiliation:						
Voted in 2008 Presidential Election	0.74	0.44	0.74	0.44	0.74	0.44
Registered Democrat	0.30	0.46	0.31	0.46	0.30	0.46
Registered Republican	0.25	0.43	0.27	0.44	0.28	0.45
"Other" or No Party Information	0.44	0.50	0.42	0.49	0.42	0.49
GIS Measures:						
Distance to Nearest DMA Border (in kilometers)	68.0	65.6	14.4	6.86	2.70	1.40
Street Address Level Match	0.86	0.35	0.83	0.37	0.86	0.35
Zip Code Level Match	0.14	0.35	0.17	0.37	0.14	0.35
City Level Match	0.00	0.01	0.00	0.01	0.00	0.01
Number of Observations	124,887,857		22,948,549		2,793,748	

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. Throughout the analysis we restrict attention to individuals who live in a state that contains at least one media market border segment and for whom we have a valid residential address as of the 2008 general election. For precise definitions and the sources of all variables, see the Data Appendix.

Table 9: Regression Discontinuity Estimates of the Effect of Partisan Differences in Political Advertising on the Partisan Composition of the Electorate, 2008 Presidential Election

A. Partisan Difference in Turnout Across DMA Borders

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.037	0.025	0.010	0.025
	(0.014)	(0.011)	(0.004)	(0.012)
Quadratic	0.032	0.033	0.021	0.035
	(0.017)	(0.013)	(0.006)	(0.013)
Cubic	0.041	0.039	0.030	0.038
	(0.019)	(0.016)	(0.009)	(0.017)
Quartic	0.040	0.032	0.033	0.032
	(0.017)	(0.018)	(0.012)	(0.018)
Number of Observations	109,178	229,437	1,608,650	--

B. Partisan Difference in Political Advertising Across DMA Borders

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	2.257	2.333	2.764	2.320
	(0.437)	(0.455)	(0.535)	(0.456)
Quadratic	2.160	2.230	2.598	2.237
	(0.427)	(0.442)	(0.481)	(0.441)
Cubic	2.063	2.225	2.523	2.215
	(0.451)	(0.425)	(0.449)	(0.425)
Quartic	2.007	2.121	2.302	2.107
	(0.422)	(0.438)	(0.433)	(0.444)
Number of Observations	109,178	229,437	1,608,650	--

C. Wald Estimator

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.017	0.011	0.004	0.011
	(0.008)	(0.005)	(0.002)	(0.006)
Quadratic	0.015	0.015	0.008	0.016
	(0.009)	(0.007)	(0.002)	(0.007)
Cubic	0.019	0.018	0.012	0.018
	(0.012)	(0.009)	(0.004)	(0.009)
Quartic	0.019	0.015	0.014	0.015
	(0.010)	(0.010)	(0.006)	(0.010)
Number of Observations	109,178	229,437	1,608,650	--

Notes: Entries in the upper panel are nonparametric estimates of the discontinuity in the partisan difference in turnout across media market borders, i.e. τ in equation (7). Larger values indicate an increase in turnout of registered Democrats relative to registered Republicans. The sample is limited to registered Democrats and Republicans for whom we have a valid residential address as of the 2008 presidential election. Estimates in the middle panel refer to the discontinuity in partisan differences in presidential advertising, i.e. δ in equation (8). The lower panel displays Wald estimates of the impact of partisan differences in political advertising on partisan differences in turnout (cf. equation (6)). 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 the 27,114 observations that lie within 100m of a media market border. Following Imbens and Lemieux (2008), we use the optimally chosen bandwidth for the outcome equation in all three panels. Every specification includes party-specific border segment fixed effects in order to account for unobserved spatial heterogeneity. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For further information on our RD setup see the Data Appendix, or the main text.

Table 10: Regression Discontinuity Estimates of the Effect of Partisan Differences in Political Advertising on Turnout Among Independents, 2008 Presidential Election

A. Turnout Among Unaffiliated Voters

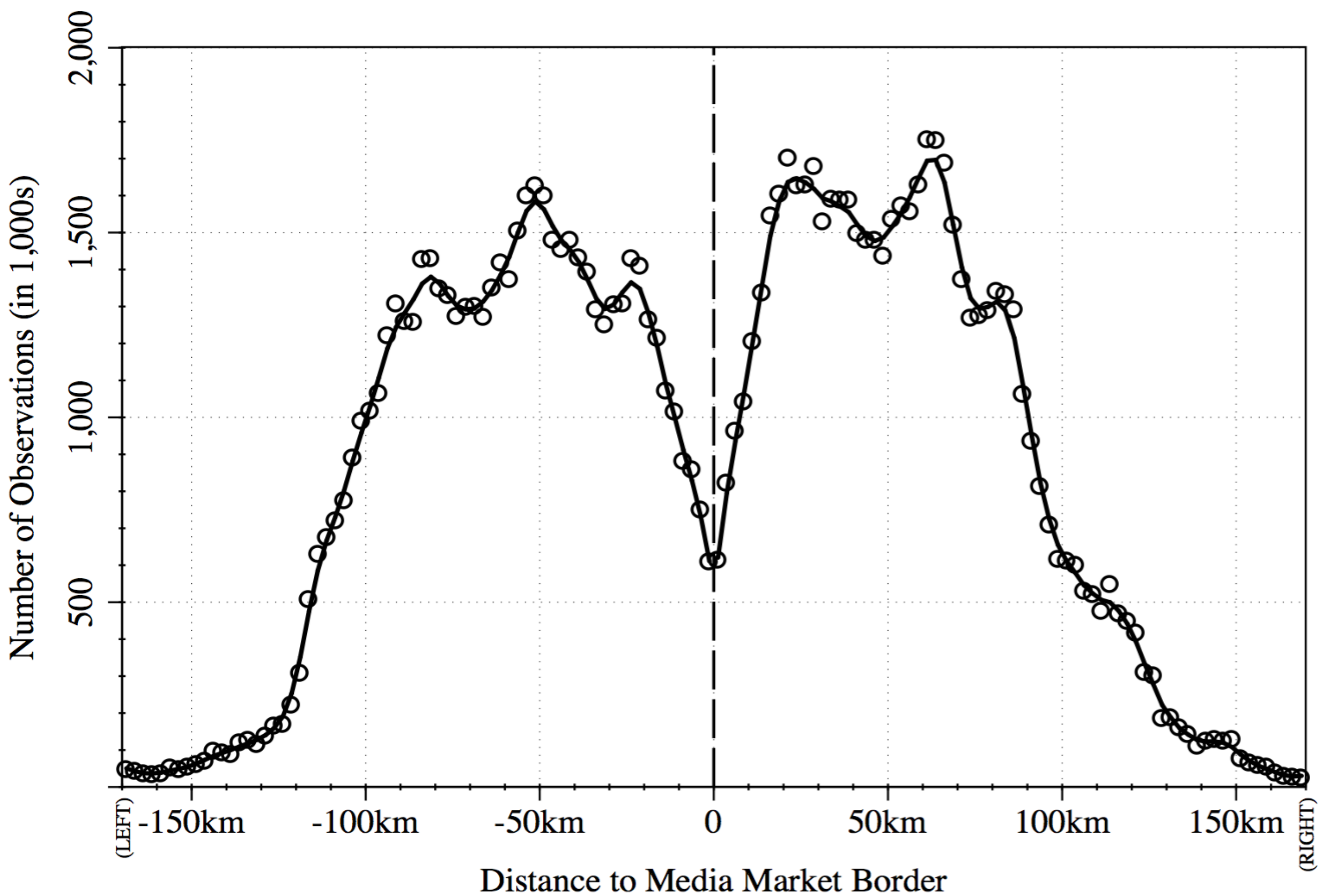
	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	-0.003	-0.005	-0.006	-0.001
	(0.006)	(0.006)	(0.007)	(0.006)
Quadratic	0.005	-0.004	-0.003	0.004
	(0.007)	(0.006)	(0.008)	(0.006)
Cubic	0.006	0.004	-0.008	0.007
	(0.007)	(0.006)	(0.006)	(0.007)
Quartic	0.008	0.007	0.000	0.007
	(0.007)	(0.007)	(0.005)	(0.007)
Number of Observations	90,568	180,673	1,185,098	--

B. Wald Estimator

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	-0.002	-0.003	-0.003	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Quadratic	0.003	-0.002	-0.000	0.002
	(0.004)	(0.003)	(0.001)	(0.003)
Cubic	0.003	0.002	-0.004	0.004
	(0.004)	(0.003)	(0.003)	(0.004)
Quartic	0.005	0.004	0.000	0.004
	(0.004)	(0.004)	(0.002)	(0.004)
Number of Observations	90,568	180,673	1,185,098	--

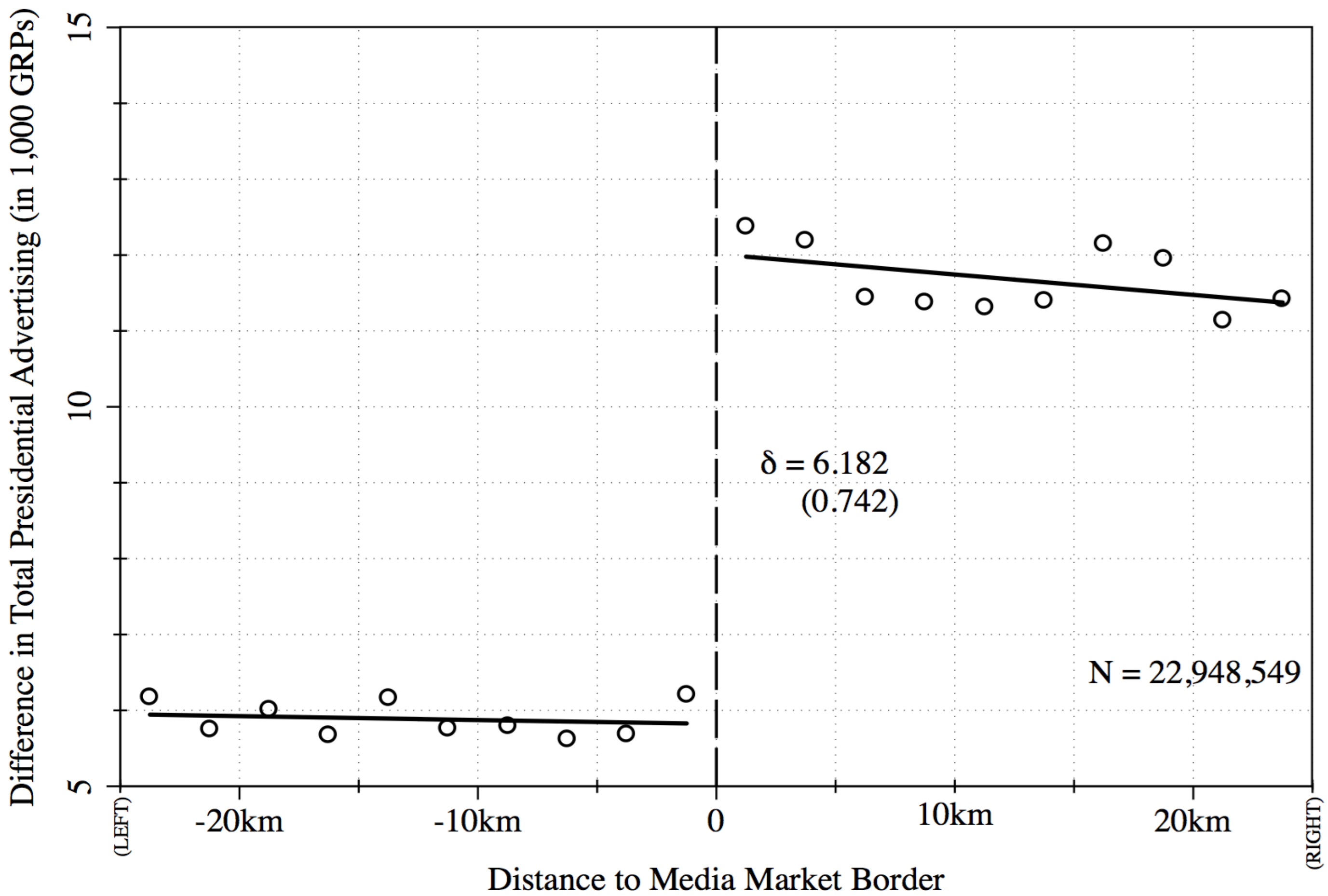
Notes: Entries in the upper panel are nonparametric estimates of the across-media market border discontinuity in turnout among "independent" voters, i.e. δ in equation (8) with the dependent variable replaced by an indicator for turnout in the 2008 general election. The sample is limited to registered voters who are not affiliated with any of the two major parties and for whom we have a valid residential address as of November 2008. The lower panel displays Wald estimates of the impact of partisan differences in political advertising on turnout among independents. 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 the 31,726 observations that lie within 100m of a media market border. Following Imbens and Lemieux (2008), we use the optimally chosen bandwidth for the outcome equation in both panels. Every specification includes border segment fixed effects in order to account for unobserved spatial heterogeneity. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For further information on our RD setup see the Data Appendix, or the main text.

Figure A.1: Number of Registered Voters, by Distance to the Nearest Media Market Border



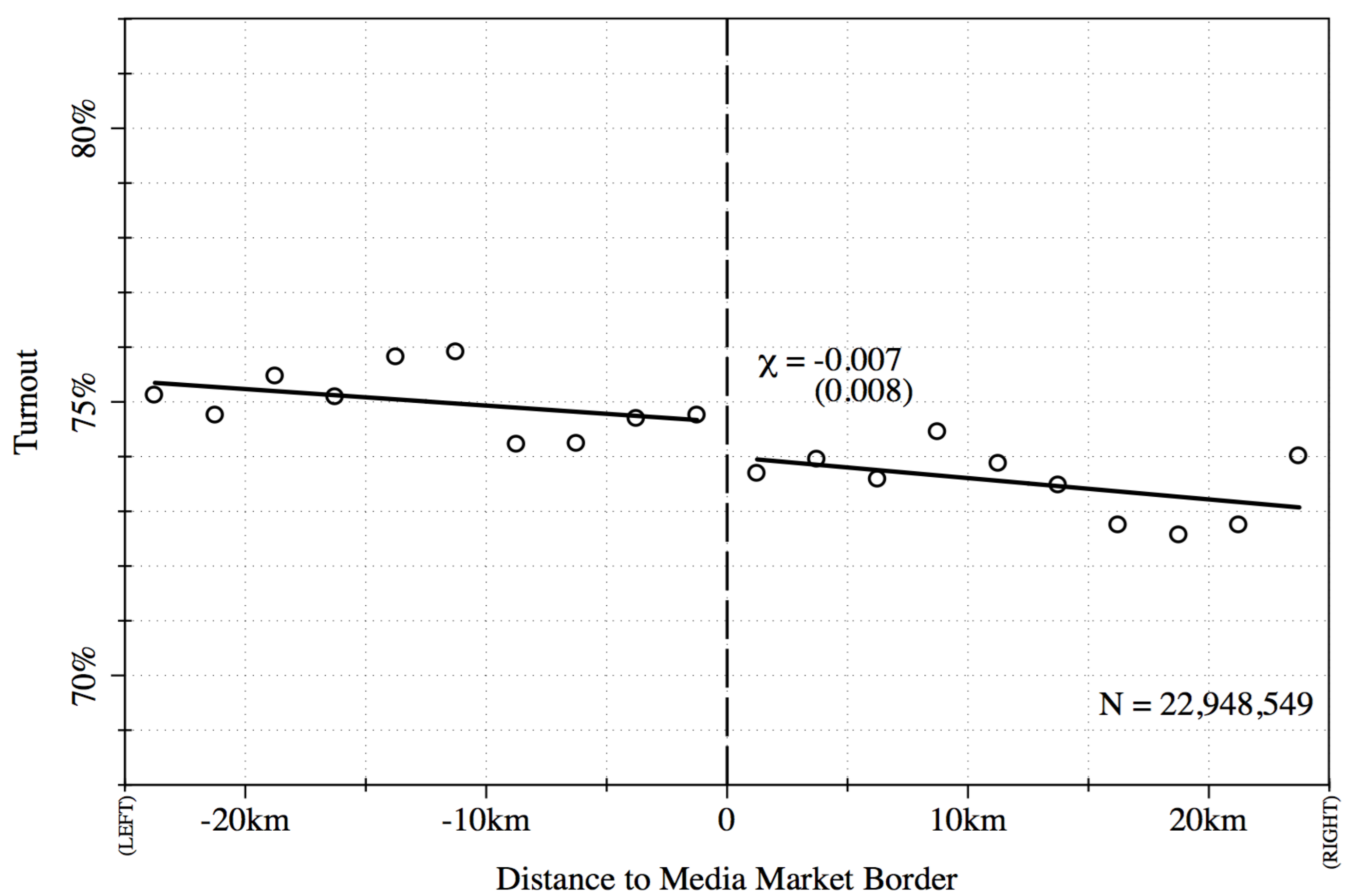
Notes: Figure shows the number of observations in our voter registration data within 2.5km-wide bins around media market borders. The sample consists of all registered voters for whom our voter registration data contains a valid address as of the 2008 election. As explained in the main text, the running variable is voters' distance to the nearest within-state media market border. Estimates of the associated density are based on the procedure of McCrary (2008). For precise definitions and the sources of all variables, see the Data Appendix.

Figure A.2: Differences in Total Presidential Advertising Across DMA Borders, 2008 Presidential Election



Notes: Figure plots average total presidential advertising during the 2008 presidential election within 2.5km-wide bins around media market borders. The sample consists of all registered voters for whom our voter registration data contains a valid address as of the 2008 election. In a deviation from our RD design in the main text, we now 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 A.3: Differences in Overall Turnout Across DMA Borders, 2008 Presidential Election



Notes: Figure plots average turnout during the 2008 presidential election within 2.5km-wide bins around media market borders. The sample consists of all registered voters for whom our voter registration data contains a valid address as of the 2008 election. In a deviation from our RD design in the main text, we now 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 A.1: Descriptive Statistics, by Election Year

	All Counties				Border-Pair Counties			
	Mean	SD	Min	Max	Mean	SD	Min	Max
A. 2004 Presidential Election								
Media Market Measures:								
Presidential GRPs (in 1,000s)	6.71	9.59	0.00	31.36	7.12	9.87	0.00	31.36
Partisan Difference in GRPs (in 1,000s)	0.92	1.71	-2.28	6.32	0.94	1.73	-2.28	6.32
Impressions per Capita	6.38	9.13	0.00	29.94	6.77	9.40	0.00	29.94
Number of Political Ads (in 1,000s)	2.95	4.19	0.00	15.28	3.11	4.28	0.00	15.28
Non-Presidential Political Advertising (in 1,000 GRPs)	12.81	11.37	0.00	44.18	12.86	11.53	0.00	44.18
Candidate Visits	3.15	4.63	0.00	18.00	3.29	4.67	0.00	18.00
County-Level Variables:								
Turnout (%)	58.17	9.09	29.46	100.00	57.77	8.94	31.00	100.00
Lagged Turnout (%)	52.92	8.82	23.70	97.10	52.69	8.74	27.24	97.10
Democratic 2-Party Vote Share (%)	40.31	12.39	9.09	90.52	39.65	11.50	9.09	83.06
Republican 2-Party Vote Share (%)	59.69	12.39	9.48	90.91	60.35	11.50	16.94	90.91
Percent White (%)	81.21	18.14	3.13	98.89	82.19	17.51	3.13	98.89
Percent Black (%)	8.51	13.49	0.00	85.45	8.15	13.43	0.00	85.45
Percent Hispanic (%)	0.32	0.47	0.00	5.91	0.29	0.44	0.00	4.07
Percent Asian (%)	1.15	2.60	0.00	44.58	0.88	1.93	0.00	29.27
Percent Other Race (%)	8.82	12.32	0.80	95.72	8.49	11.97	0.80	95.72
Percent High School Dropouts (%)	22.58	8.68	3.24	58.01	23.61	8.71	3.24	58.01
Percent High School Educated (%)	54.66	6.68	23.78	68.92	55.21	6.37	29.23	68.83
Percent College Educated (%)	22.76	9.38	6.86	67.62	21.18	8.41	6.86	62.24
Median Household Income (in $1,000)	39.70	10.43	18.38	94.66	38.23	9.60	18.38	94.66
Percent in Poverty (%)	12.67	4.97	2.25	38.59	13.07	4.98	3.05	38.59
Unemployment Rate (%)	5.69	1.65	2.40	16.10	5.83	1.67	2.40	16.10
Total Population (in 1,000s)	128	380	0	10,084	96	293	0	5,396
Number of Counties		2,025				1,413		
Number of Media Markets		100				72		
B. 2008 Presidential Election								
Media Market Measures:								
Presidential GRPs (in 1,000s)	6.44	7.80	0.00	24.53	6.62	7.84	0.00	24.53
Partisan Difference in GRPs (in 1,000s)	1.17	2.18	-4.66	8.41	1.21	2.20	-4.66	8.41
Impressions per Capita	5.82	7.04	0.00	21.90	5.99	7.09	0.00	21.90
Number of Political Ads (in 1,000s)	3.20	4.20	0.00	17.34	3.25	4.20	0.00	17.34
Non-Presidential Political Advertising (in 1,000 GRPs)	13.27	11.60	0.00	51.40	13.01	11.30	0.00	51.40
Candidate Visits	1.59	2.05	0.00	11.67	1.56	1.99	0.00	11.67
County-Level Variables:								
Turnout (%)	58.72	9.34	19.24	100.00	58.23	9.24	24.93	100.00
Lagged Turnout (%)	58.50	9.63	15.16	100.00	58.03	9.41	26.29	100.00
Democratic 2-Party Vote Share (%)	42.24	14.03	5.03	93.40	41.69	13.48	5.03	89.98
Republican 2-Party Vote Share (%)	57.76	14.03	6.60	94.97	58.31	13.48	10.02	94.97
Percent White (%)	78.95	19.56	2.78	99.12	79.39	19.41	2.78	99.12
Percent Black (%)	8.93	14.56	0.00	85.92	9.03	14.92	0.00	85.92
Percent Hispanic (%)	0.49	0.62	0.00	6.56	0.47	0.60	0.00	5.89
Percent Asian (%)	1.08	2.30	0.00	43.68	0.86	1.77	0.00	31.36
Percent Other Race (%)	10.56	14.22	0.58	96.14	10.25	14.13	0.66	96.14
Percent High School Dropouts (%)	22.94	8.76	3.44	65.46	23.85	8.83	3.44	65.46
Percent High School Educated (%)	54.80	6.63	23.55	74.30	55.17	6.42	25.82	74.30
Percent College Educated (%)	22.26	8.73	6.45	67.63	20.98	7.96	6.45	62.14
Median Household Income (in $1,000)	44.05	11.41	19.18	111.58	42.48	10.51	19.83	102.25
Percent in Poverty (%)	14.43	5.73	2.79	49.38	15.01	5.81	3.05	49.38
Unemployment Rate (%)	5.77	2.07	1.30	22.40	5.97	2.08	1.70	22.40
Total Population (in 1,000s)	102	322	0	10,039	77	246	0	5,304
Number of Counties		3,084				2250		
Number of Media Markets		197				158		

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

Table A.2: Alternative Estimates of the Impact of Political Advertising on Voter Turnout

A. All Presidential Advertising

	Δ Percent Voter Turnout			
	(1)	(2)	(3)	(4)
Δ All Presidential GRPs (in 1,000s)	0.016	0.027	0.019	0.031
	(0.013)	(0.021)	(0.018)	(0.023)
H_0: Coefficient on GRPs = 0				
t-Statistic	1.221	1.238	1.033	1.360
p -value	0.229	0.222	0.307	0.180
Fixed Effects:				
State	No	Yes	No	No
Year	No	No	Yes	No
State × Year	No	No	No	Yes
Controls:				
Demographics	Yes	Yes	Yes	Yes
Candidate Visits & Non-Presidential GRPs	Yes	Yes	Yes	Yes
Lagged Turnout	Yes	Yes	Yes	Yes
Sample	Border-Pair Differences	Border-Pair Differences	Border-Pair Differences	Border-Pair Differences
R-Squared	0.862	0.865	0.862	0.868
Number of Observations	3,299	3,299	3,299	3,299

B. Positive vs. Negative Presidential Advertsing

	Δ Percent Voter Turnout			
	(5)	(6)	(7)	(8)
Δ Positive Presidential GRPs (in 1,000s)	-0.011	-0.074	-0.010	-0.116
	(0.078)	(0.093)	(0.087)	(0.100)
Δ Negative Presidential GRPs (in 1,000s)	0.027	0.063	0.030	0.083
	(0.031)	(0.038)	(0.035)	(0.040)
H_0: Both Coefficients = 0				
F-Statistic	0.840	1.518	0.632	2.200
p -value	0.438	0.230	0.536	0.123
Fixed Effects:				
State	No	Yes	No	No
Year	No	No	Yes	No
State × Year	No	No	No	Yes
Controls:				
Demographics	Yes	Yes	Yes	Yes
Candidate Visits & Non-Presidential GRPs	Yes	Yes	Yes	Yes
Lagged Turnout	Yes	Yes	Yes	Yes
Sample	Border-Pair Differences	Border-Pair Differences	Border-Pair Differences	Border-Pair Differences
R-Squared	0.862	0.866	0.862	0.868
Number of Observations	3,299	3,299	3,299	3,299

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 difference in voter turnout (as percentage of the voting-aged population) between neighboring border counties. The upper panel estimates the impact of all presidential advertising, while the lower panel distinguishes between positve and negative ads. The set of included controls and fixed effects also varies across columns. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For precise definitions and the sources of all variables, see the Data Appendix.

Table A.3: Alternative Estimates of the Effect of Partisan Differences in Advertising on Vote Shares

A. All Presidential Advertising

	Δ Partisan Difference in Presidential Vote Shares			
	(1)	(2)	(3)	(4)
Δ Partisan Difference in Presidential GRPs (in 1,000s)	0.324	0.297	0.249	0.313
	(0.063)	(0.079)	(0.069)	(0.093)
H_0: Coefficient on GRPs = 0				
t-Statistic	5.165	3.765	3.612	3.358
p -value	0.000	0.000	0.001	0.002
Fixed Effects:				
State	No	Yes	No	No
Year	No	No	Yes	No
State × Year	No	No	No	Yes
Controls:				
Demographics	Yes	Yes	Yes	Yes
Candidate Visits & Non-Presidential GRPs	Yes	Yes	Yes	Yes
Lagged Turnout	Yes	Yes	Yes	Yes
Sample	Border-Pair Differences	Border-Pair Differences	Border-Pair Differences	Border-Pair Differences
R-Squared	0.934	0.936	0.935	0.938
Number of Observations	3,299	3,299	3,299	3,299

B. Democratic vs. Republican Advertsing

	Δ Partisan Difference in Presidential Vote Shares			
	(5)	(6)	(7)	(8)
Δ Democratic Presidential GRPs (in 1,000s)	0.265	0.303	0.240	0.316
	(0.073)	(0.082)	(0.073)	(0.097)
Δ Republican Presidential GRPs (in 1,000s)	-0.062	-0.158	-0.091	-0.182
	(0.117)	(0.123)	(0.111)	(0.138)
H_0: Both Coefficients = 0				
F-Statistic	23.061	12.624	12.107	10.055
p -value	0.000	0.000	0.000	0.000
Fixed Effects:				
State	No	Yes	No	No
Year	No	No	Yes	No
State × Year	No	No	No	Yes
Controls:				
Demographics	Yes	Yes	Yes	Yes
Candidate Visits & Non-Presidential GRPs	Yes	Yes	Yes	Yes
Lagged Turnout	Yes	Yes	Yes	Yes
Sample	Border-Pair Differences	Border-Pair Differences	Border-Pair Differences	Border-Pair Differences
R-Squared	0.935	0.937	0.935	0.939
Number of Observations	3,299	3,299	3,299	3,299

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 presidential two-party vote shares (in percentage points), with larger values indicating more votes for the Democratic candidate. The independent variable of interest in the upper panel is the partisan difference in presidential advertising (in 1,000 GRPs), defined as the difference between advertising in support of the Democratic candidate and that for his Republican competitor. The lower panel distinguishes between Democratic and Republican ads. The set of included fixed effects varies across columns. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For precise definitions and the sources of all variables, see the Data Appendix.

Table A.4: Discontinuities in Turnout Among Partisans, by Party Affiliation

A. Turnout Among Registered Democrats

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.021	0.010	-0.001	0.011
	(0.011)	(0.009)	(0.007)	(0.009)
Quadratic	0.014	0.017	0.009	0.021
	(0.011)	(0.010)	(0.006)	(0.011)
Cubic	0.012	0.023	0.003	0.019
	(0.009)	(0.012)	(0.007)	(0.011)
Quartic	0.008	0.013	0.011	0.011
	(0.008)	(0.010)	(0.010)	(0.010)
Number of Observations	54,613	115,946	826,387	--

B. Turnout Among Registered Republicans

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	-0.016	-0.015	-0.011	-0.014
	(0.010)	(0.008)	(0.005)	(0.008)
Quadratic	-0.018	-0.016	-0.012	-0.018
	(0.013)	(0.010)	(0.005)	(0.011)
Cubic	-0.029	-0.017	-0.028	-0.014
	(0.015)	(0.012)	(0.006)	(0.013)
Quartic	-0.032	-0.019	-0.022	-0.025
	(0.015)	(0.013)	(0.010)	(0.014)
Number of Observations	54,565	113,491	783,263	--

Notes: Entries are nonparametric estimates of the discontinuity in turnout for a particular group of partisans only, i.e. δ in equation (8) with the dependent variable replaced by an indicator for whether the voter went to the polls in the 2008 general election. The sample is limited to registered partisans for whom we have a valid residential address as of November 2008. 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 all supporters of the respective party who live within 100m of a media market border. Every specification includes border segment fixed effects in order to account for unobserved spatial heterogeneity. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For further information on our RD setup see the Data Appendix, or the main text.

Table A.5: Testing for Discontinuities in Voters' Observable Characteristics

Dependent Variable: Years Registered at Current Address

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	-0.016	0.131	0.140	-0.022
	(0.271)	(0.221)	(0.163)	(0.229)
Quadratic	0.218	-0.174	0.206	0.166
	(0.327)	(0.261)	(0.226)	(0.341)
Cubic	0.133	0.170	0.178	0.083
	(0.277)	(0.314)	(0.275)	(0.272)
Quartic	-0.024	0.235	0.041	0.061
	(0.277)	(0.347)	(0.238)	(0.273)
Number of Observations	109,178	229,437	1,608,650	--

Notes: Entries are nonparametric estimates of the across-media market border discontinuity in how long voters have been registered at their current address, i.e. δ in equation (8) with the dependent variable replaced by the number of years since a voter registered at her residential address. The sample is limited to registered Democrats and Republicans for whom we have a valid address as of the 2008 presidential election, and who are not missing information on the date of registration. 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 the 27,101 observations that lie within 100m of a media market border. Every specification includes border segment fixed effects in order to account for unobserved spatial heterogeneity. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For further information on our RD setup see the Data Appendix, or the main text.

Table A.6: Testing for Discontinuities in Voters' Observable Characteristics

Dependent Variable: Democrat

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.011	0.003	0.021	-0.004
	(0.011)	(0.010)	(0.013)	(0.014)
Quadratic	0.000	0.007	0.012	0.009
	(0.009)	(0.010)	(0.012)	(0.015)
Cubic	-0.007	0.008	0.003	0.013
	(0.012)	(0.010)	(0.012)	(0.017)
Quartic	0.004	-0.004	0.006	0.021
	(0.014)	(0.011)	(0.012)	(0.018)
Number of Observations	109,178	229,437	1,608,650	--

Notes: Entries are nonparametric estimates of the across-media market border discontinuity in the share of registered Democrats, i.e. δ in equation (8) with the dependent variable replaced by an indicator variable for whether a particular voter is affiliated with the Democratic party. The sample is limited to registered Democrats and Republicans for whom we have a valid residential address as of the 2008 presidential election. 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 the 27,114 observations that lie within 100m of a media market border. Every specification includes border segment fixed effects in order to account for unobserved spatial heterogeneity. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For further information on our RD setup see the Data Appendix, or the main text.

Table A.7: Testing for Discontinuities in Voters' Observable Characteristics

Dependent Variable: Age

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.117	0.014	0.090	-0.092
	(0.318)	(0.417)	(0.167)	(0.217)
Quadratic	0.290	-0.017	-0.094	0.231
	(0.526)	(0.375)	(0.505)	(0.387)
Cubic	-0.264	0.379	-0.642	0.285
	(0.415)	(0.423)	(0.838)	(0.442)
Quartic	-0.276	0.236	-0.485	0.087
	(0.383)	(0.432)	(0.554)	(0.379)
Number of Observations	108,672	228,496	1,601,926	--

Notes: Entries are nonparametric estimates of the across-media market border discontinuity in age, i.e. δ in equation (8) with the dependent variable replaced by voters' age (in years). The sample is limited to registered Democrats and Republicans for whom we have a valid residential address as of the 2008 presidential election, and who are not missing information on their date of birth. 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 the 26,948 observations that lie within 100m of a media market border. Every specification includes border segment fixed effects in order to account for unobserved spatial heterogeneity. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For further information on our RD setup see the Data Appendix, or the main text.

Table A.8: Testing for Discontinuities in Voters' Observable Characteristics

Dependent Variable: Female

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.004	0.000	0.005	0.004
	(0.006)	(0.002)	(0.002)	(0.002)
Quadratic	-0.000	0.004	0.001	0.001
	(0.007)	(0.006)	(0.002)	(0.002)
Cubic	-0.004	0.003	-0.003	-0.004
	(0.007)	(0.007)	(0.002)	(0.002)
Quartic	-0.000	-0.001	-0.004	-0.001
	(0.008)	(0.007)	(0.002)	(0.003)
Number of Observations	84,401	180,195	1,250,580	--

Notes: Entries are nonparametric estimates of the across-media market border discontinuity in the share of women i.e. δ in equation (8) with the dependent variable replaced by an indicator variable for whether a particular voter is female. The sample is limited to registered Democrats and Republicans for whom we have a valid residential address as of the 2008 presidential election, and who are not missing information on gender. 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 the 20,901 observations that lie within 100m of a media market border. Every specification includes border segment fixed effects in order to account for unobserved spatial heterogeneity. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For further information on our RD setup see the Data Appendix, or the main text.

Table A.9: Testing for Discontinuities in Non-Presidential Political Advertising

Dependent Variable: Partisan Difference in Non-Presidential Political Advertising (in 1,000 GRPs)

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	C-V Optimal
Linear	-0.191	-0.339	-0.459	0.345
	(0.833)	(1.025)	(0.967)	(0.487)
Quadratic	0.181	0.014	-0.526	0.018
	(0.592)	(0.746)	(1.076)	(0.514)
Cubic	0.236	0.060	-0.374	0.050
	(0.524)	(0.649)	(1.078)	(0.515)
Quartic	0.278	0.189	-0.201	0.064
	(0.492)	(0.545)	(1.008)	(0.526)
Number of Observations	109,178	229,437	1,608,650	--

Notes: Entries are nonparametric estimates of the across-media market discontinuity in the partisan gap in *non* -presidential advertising, i.e. δ in equation (8). 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 the 21,114 observations that lie within 100m of a media market border. Every specification includes border segment fixed effects in order to account for unobserved spatial heterogeneity. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For further information on our RD setup see the Data Appendix, or the main text.

Table A.10: Replicating the Main RD Estimates, Controlling for Non-Presidential Political Advertising and Voters' Observable Characteristics

A. Partisan Difference in Turnout Across DMA Borders

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.036	0.025	0.009	0.025
	(0.013)	(0.011)	(0.003)	(0.012)
Quadratic	0.027	0.031	0.021	0.033
	(0.015)	(0.012)	(0.006)	(0.012)
Cubic	0.036	0.036	0.032	0.035
	(0.018)	(0.015)	(0.008)	(0.015)
Quartic	0.034	0.027	0.033	0.026
	(0.017)	(0.017)	(0.011)	(0.017)
Number of Observations	109,178	229,437	1,608,650	--

B. Wald Estimator

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.016	0.011	0.003	0.011
	(0.007)	(0.005)	(0.002)	(0.006)
Quadratic	0.012	0.014	0.008	0.015
	(0.008)	(0.007)	(0.002)	(0.007)
Cubic	0.017	0.017	0.012	0.016
	(0.011)	(0.008)	(0.004)	(0.008)
Quartic	0.016	0.012	0.014	0.012
	(0.010)	(0.009)	(0.006)	(0.009)
Number of Observations	109,178	229,437	1,608,650	--

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 (7). Larger values indicate an increase in turnout of registered Democrats relative to registered Republicans. The sample is limited to registered Democrats and Republicans for whom we have a valid residential address as of the 2008 presidential election. The lower panel displays Wald estimates of the impact of partisan differences in political advertising on partisan differences in turnout (cf. equation (6)). 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 the 27,114 observations that lie within 100m of a media market border. Following Imbens and Lemieux (2008), we use the optimally chosen bandwidth for the outcome equation in both panels. Every specification includes party-specific border segment fixed effects in order to account for unobserved spatial heterogeneity. Further, all specifications control for non-presidential political advertising, voters' age, gender, the length of time an individual has been registered at her address, as well as indicator variables for missing values on each covariate. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For additional information on our RD setup see the Data Appendix, or the main text.

Table A.11: Replicating the Main RD Estimates with Street Address Level-Matched Observations Only

A. Partisan Difference in Turnout Across DMA Borders

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.027	0.015	0.007	0.021
	(0.014)	(0.010)	(0.004)	(0.008)
Quadratic	0.027	0.026	0.021	0.017
	(0.018)	(0.013)	(0.006)	(0.011)
Cubic	0.037	0.030	0.024	0.025
	(0.020)	(0.018)	(0.009)	(0.014)
Quartic	0.040	0.026	0.021	0.037
	(0.018)	(0.019)	(0.010)	(0.016)
Number of Observations	99,701	209,109	1,374,271	--

B. Wald Estimator

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.012	0.006	0.003	0.008
	(0.007)	(0.004)	(0.002)	(0.003)
Quadratic	0.012	0.011	0.008	0.007
	(0.010)	(0.007)	(0.003)	(0.005)
Cubic	0.017	0.014	0.009	0.011
	(0.011)	(0.009)	(0.004)	(0.007)
Quartic	0.019	0.012	0.009	0.016
	(0.011)	(0.010)	(0.005)	(0.009)
Number of Observations	99,701	209,109	1,374,271	--

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 (7). Larger values indicate an increase in turnout of registered Democrats relative to registered Republicans. The sample is limited registered Democrats and Republicans for whom we have a valid residential address at the time of the 2008 presidential election, and for whom our geocoding procedure is able to locate the exact street address. The lower panel displays Wald estimates of the impact of partisan differences in political advertising on partisan differences in turnout (cf. equation (6)). 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 the 25,358 observations that lie within 100m of a media market border. Following Imbens and Lemieux (2008), we use the optimally chosen bandwidth for the outcome equation in both panels. Every specification includes party-specific border segment fixed effects in order to account for unobserved spatial heterogeneity. For further information on our RD setup see the Data Appendix, or the main text.

Table A.12: Replicating the Main RD Estimates, Restricting Attention to Media Markets Borders with Sizeable Partisan Differences in Presidential Advertising

A. Partisan Difference in Turnout Across DMA Borders

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.033	0.026	0.009	0.024
	(0.016)	(0.014)	(0.005)	(0.013)
Quadratic	0.023	0.030	0.022	0.031
	(0.015)	(0.015)	(0.006)	(0.016)
Cubic	0.029	0.029	0.032	0.030
	(0.015)	(0.017)	(0.012)	(0.016)
Quartic	0.030	0.020	0.032	0.024
	(0.015)	(0.017)	(0.015)	(0.017)
Number of Observations	73,909	159,842	1,121,940	--

B. Wald Estimator

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.010	0.008	0.002	0.007
	(0.005)	(0.004)	(0.001)	(0.004)
Quadratic	0.007	0.009	0.006	0.009
	(0.005)	(0.005)	(0.002)	(0.005)
Cubic	0.008	0.009	0.009	0.009
	(0.005)	(0.005)	(0.003)	(0.005)
Quartic	0.009	0.006	0.009	0.007
	(0.005)	(0.005)	(0.005)	(0.005)
Number of Observations	73,909	159,842	1,121,940	--

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 (7). Larger values indicate an increase in turnout of registered Democrats relative to registered Republicans. The sample is limited to registered Democrats and Republicans for whom we have a valid residential address as of the 2008 presidential election, and who live near a media market border with at least a 500-GRP gap in the partisian difference in presidential advertising. The lower panel displays Wald estimates of the impact of partisan differences in political advertising on partisan differences in turnout (cf. equation (6)). 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 the 17,516 observations that lie within 100m of a media market border. Following Imbens and Lemieux (2008), we use the optimally chosen bandwidth for the outcome equation in both panels. Every specification includes party-specific border segment fixed effects in order to account for unobserved spatial heterogeneity. For further information on our RD setup see the Data Appendix, or the main text.

Table A.13: Replicating the Main RD Estimates, Restricting Attention to Registered Voters in Counties With No Out-of-Market TV Stations on the FCC's Significantly Viewed List

A. Partisan Difference in Turnout Across DMA Borders

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.058	0.030	0.007	0.015
	(0.033)	(0.021)	(0.011)	(0.008)
Quadratic	0.070	0.033	0.026	0.012
	(0.042)	(0.030)	(0.013)	(0.023)
Cubic	.104	0.051	0.019	0.025
	(0.043)	(0.0.42)	(0.023)	(0.032)
Quartic	0.117	0.092	0.023	0.035
	(0.036)	(0.034)	(0.027)	(0.037)
Number of Observations	26,267	57,503	364,113	--

B. Wald Estimator

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	0.040	0.024	0.009	0.016
	(0.040)	(0.026)	(0.012)	(0.011)
Quadratic	0.043	0.025	0.026	0.011
	(0.043)	(0.033)	(0.018)	(0.021)
Cubic	0.059	0.034	0.020	0.020
	(0.045)	(0.042)	(0.026)	(0.031)
Quartic	0.064	0.057	0.021	0.027
	(0.043)	(0.043)	(0.029)	(0.038)
Number of Observations	26,267	57,503	364,113	--

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 (7). Larger values indicate an increase in turnout of registered Democrats relative to registered Republicans. The sample is limited to registered Democrats and Republicans for whom we have a valid residential address as of the 2008 presidential election, and who live in a county where no out-of-market TV stations appear on the FCC's Significantly Viewed List. The lower panel displays Wald estimates of the impact of partisan differences in political advertising on partisan differences in turnout (cf. equation (6)). 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 the 6,448 observations that lie within 100m of a media market border. Following Imbens and Lemieux (2008), we use the optimally chosen bandwidth for the outcome equation in both panels. Every specification includes party-specific border segment fixed effects in order to account for unobserved spatial heterogeneity. For further information on our RD setup see the Data Appendix, or the main text.

Table A.14: Regression Discontinuity Estimates of the Effect of Total Political Advertising on Aggregate Turnout, 2008 Presidential Election

A. Difference in Aggregate Turnout Across DMA Borders

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	-0.007	-0.002	-0.003	-0.005
	(0.006)	(0.006)	(0.005)	(0.007)
Quadratic	-0.001	-0.006	-0.002	-0.002
	(0.007)	(0.006)	(0.005)	(0.008)
Cubic	0.000	-0.001	0.007	-0.002
	(0.007)	(0.006)	(0.006)	(0.008)
Quartic	-0.004	0.003	-0.000	-0.001
	(0.007)	(0.007)	(0.007)	(0.008)
Number of Observations	199,746	410,110	2,793,748	--

B. Difference in Total Political Advertising Across DMA Borders

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	5.109	5.368	5.877	4.729
	(0.743)	(0.850)	(0.914)	(0.672)
Quadratic	4.874	5.177	5.584	4.580
	(0.665)	(0.764)	(0.861)	(0.704)
Cubic	4.621	4.972	5.750	4.604
	(0.663)	(0.668)	(0.907)	(0.703)
Quartic	4.571	4.723	5.599	4.588
	(0.630)	(0.634)	(0.882)	(0.674)
Number of Observations	199,746	410,110	2,793,748	--

C. Wald Estimator

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	-0.001	-0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Quadratic	-0.000	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.002)
Cubic	0.000	-0.000	0.001	-0.000
	(0.002)	(0.001)	(0.001)	(0.002)
Quartic	-0.001	0.001	-0.000	-0.000
	(0.002)	(0.002)	(0.001)	(0.002)
Number of Observations	199,746	410,110	2,793,748	--

Notes: Entries in the upper panel are nonparametric estimates of the across-media market border discontinuity in turnout, i.e. δ in equation (8) with the dependent variable replaced by an indicator for turnout in the 2008 general election. The sample consists of all voters for whom we have a valid residential address as of November 2008. The lower panel displays Wald estimates of the impact of *total* presidential advertising on turnout. In a deviation from our RD design in the main text, we now 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. 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 the 58,840 observations that lie within 100m of a media market border. Following Imbens and Lemieux (2008), we use the optimally chosen bandwidth for the outcome equation in all three panels. Every specification includes border segment fixed effects in order to account for unobserved spatial heterogeneity. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For further information on our RD setup see the Data Appendix, or the main text.

Table A.15: Replicating Estimates of the Effect of Total Political Advertising on Aggregate Turnout, Restricting Attention to Registered Voters in Counties With No Out-of-Market TV Stations on the FCC's Significantly Viewed List

A. Partisan Difference in Aggregate Turnout Across DMA Borders

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	-0.016	0.001	0.010	-0.011
	(0.010)	(0.017)	(0.020)	(0.009)
Quadratic	-0.010	-0.021	0.011	-0.005
	(0.009)	(0.010)	(0.024)	(0.012)
Cubic	-0.001	-0.012	0.038	-0.006
	(0.013)	(0.009)	(0.024)	(0.014)
Quartic	-0.010	-0.004	0.004	-0.011
	(0.013)	(0.011)	(0.022)	(0.013)
Number of Observations	45,403	96,778	588,540	--

B. Wald Estimator

	Bandwidth (in meters)			
Local Polynomial	500	1,000	5,000	10-fold C-V
Linear	-0.004	0.000	0.003	-0.002
	(0.003)	(0.004)	(0.005)	(0.002)
Quadratic	-0.002	-0.005	0.003	-0.001
	(0.002)	(0.003)	(0.006)	(0.002)
Cubic	-0.000	-0.003	0.010	-0.001
	(0.003)	(0.002)	(0.006)	(0.003)
Quartic	-0.002	-0.001	0.001	-0.002
	(0.003)	(0.002)	(0.005)	(0.003)
Number of Observations	45,403	96,778	588,540	--

Notes: Entries in the upper panel are nonparametric estimates of the across-media market border discontinuity in turnout, i.e. δ in equation (8) with the dependent variable replaced with an indicator for turnout in the 2008 general election. The sample is limited to all voters for whom we have a valid residential address as of the 2008 presidential election, and who live in a county where no out-of-market TV stations appear in the FCC's Significantly Viewed List. Estimates in the middle panel refer to the discontinuity in *total* presidential advertising. The lower panel displays Wald estimates of the impact of total presidential advertising on overall turnout. In a deviation from our RD design in the main text, we now 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. 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 the 12,860 observations that lie within 100m of a media market border. Following Imbens and Lemieux (2008), we use the optimally chosen bandwidth for the outcome equation in both panels. Every specification includes border segment fixed effects in order to account for unobserved spatial heterogeneity. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. For further information on our RD setup see the Data Appendix, or the main text.