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Advertising Effects in Presidential Elections

Brett R. Gordon
Columbia Business School, Columbia University, New York, New York 10027,
brgordon@columbia.edu

Wesley R. Hartmann
Stanford Graduate School of Business, Stanford University, Stanford, California 94305,
hartmann_wes@gsb.stanford.edu

Presidential elections provide both an important context in which to study advertising and a setting that mitigates the challenges of dynamics and endogeneity. We use the 2000 and 2004 general elections to analyze the effect of market-level advertising on county-level vote shares. The results indicate significant positive effects of advertising exposures. Both instrumental variables and fixed effects alter the ad coefficient. Advertising elasticities are smaller than are typical for branded goods yet significant enough to shift election outcomes. For example, if advertising were set to zero and all other factors held constant, three states' electoral votes would have changed parties in 2000. Given the narrow margin of victory in 2000, this shift would have resulted in a different president.

Key words: advertising; politics; instrumental variables; presidential elections
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1. Introduction

Advertising is ubiquitous. In 2008, firms spent roughly $65 billion on television advertising for a range of products.[1] The prevalence of advertising suggests that it must be influential. Consequently, the study of advertising often turns to understanding what it affects and why: economists and marketers debate whether advertising is informative or persuasive; marketers assess its effects on intermediate measures such as brand recall; and political scientists wonder if negative advertisements depress voter turnout.[2] Nevertheless, conclusive evidence on the efficacy of advertising is still quite elusive. Many papers lack any source of exogenous variation, and those studies with experimental variation have trouble detecting robust effects.[3]

We examine advertising effectiveness in presidential elections, potentially one of the most important contexts in which to study advertising. Advertising studies typically focus on brands seeking to influence individual consumers. In contrast, an election aggregates the decisions of many into a single outcome with far-reaching consequences. The growing volume of political advertising, and its possible effects on voters' choices, has contributed to a growing debate about campaign fundraising and spending limits in elections (Soberman and Sadoulet 2007, Centre for Law and Democracy 2012, *Economist* 2012). Despite these concerns, researchers still debate the evidence on advertising effects in elections (see Goldstein and Ridout 2004, Gordon et al. 2012 for reviews).

Two common challenges in estimating the effects of advertising are econometric endogeneity and disentangling the effects of past and present advertising. First, as with most empirical questions, a correlation between unobservables and advertising creates an endogeneity problem in isolating causal effect.[4] Potential instruments are variables that enter

[1] See *AdWeek* (2008).

[2] For work analyzing whether advertising is informative or persuasive, see, for example, Nelson (1974), Ackerberg (2001), Narayanan and Manchanda (2009), Clark et al. (2009), and Anand and Shachar (2011). For persuasiveness in political contexts, see Huber and Arceneaux (2007) and Lovett and Peress (2010). Draganska and Klapper (2011) incorporate the effects of brand recall through advertising on demand, and Kanetkar et al. (1992) and Mela et al. (1997) measure the effects of advertising on price sensitivity. Shachar (2009) suggests that political advertising only affects turnout. Ansolabehere et al. (1999), Wattenberg and Brians (1999), and Freedman and Goldstein (1999) investigate the effects of negative advertisements on voter turnout; see Lau et al. (2007) for a meta-analysis on negative advertising effects.

[3] Lodish et al. (1995) conduct a meta-analysis of split-cable television experiments and do not find conclusive positive effects of advertising. Eastlack and Rao (1989), reporting on the results of field experiments in the 1970s by the Campbell Soup Company, find advertising budget levels had little effect on sales of established brands. In the context of Internet advertising, the experimental variation alone in Lewis and Reiley (2011) was unable to find significant positive advertising effects.

[4] Villas-Boas and Winer (1999) provide a detailed discussion of endogeneity problems in discrete choice models commonly used in marketing applications.

the decision process of advertisers, but not that of the targets to be influenced. Dubé and Manchanda (2005) and Doganoglu and Klapper (2006) use the current price of advertising, which is excluded from demand just as marginal costs are excluded when instrumenting for a product's price. A problem with contemporaneous advertising prices is that advertisers may not be price takers; large advertisers could influence the market-clearing price of advertising, violating the exogeneity requirement for an instrument. For example, Procter & Gamble's multibillion-dollar advertising budget gives the company leverage to negotiate favorable advertising rates (*Guardian* 2001). Similarly, reports in the popular press indicate that candidates' demand for political advertisements, whether in presidential or midterm elections, causes advertising rates to increase, with the effects being more pronounced in competitive markets (*Washington Times* 2010, *Cincinnati Enquirer* 2004, *San Francisco Chronicle* 2012). To address this issue, we take advantage of the fact that no elections are held during odd years, and we use the prior year's advertising prices as cost instruments that are free of political campaign effects.

The second challenge is that advertising effects are typically spread over long horizons and multiple choice occasions. This fact may help explain why the few studies that causally identify advertising effects more often find positive effects for new products (e.g., Ackerberg 2001, Lodish et al. 1995). Much of this literature, motivated in part by Nerlove and Arrow (1962), incorporates latent advertising stock variables that depreciate and are reinvested over long horizons (Naik et al. 1998, Dubé et al. 2005, Rutz and Bucklin 2011). Fortunately, the political context concentrates both the choices and the spending. Choices are fully concentrated on Election Day. Spending in presidential general elections is concentrated in the short post-primary period leading up to Election Day, thereby creating a setting where advertising can reasonably be aggregated into a single variable.[5]

Thus presidential elections provide a unique setting that is well suited for identifying the causal effects of advertising. We use advertising data from the 2000 and 2004 presidential elections to measure the effect of advertising on county-level voting decisions using an aggregate discrete choice model. The Electoral College system distorts advertising incentives across geographic areas so that advertising varies from zero in some markets (e.g., New York and Texas) to significant per-capita levels in battleground states (e.g., Ohio and Florida), providing rich variation for estimating advertising's efficacy. To measure the advertising effect as cleanly as possible, we include an extensive set of fixed effects at the market-party level. Focusing on within-market variation removes the worry that unobservables in the candidate choice equation might be cross-sectionally correlated with the advertising price instrument. Such a correlation could exist because major metropolitan areas have higher advertising prices and tend to lean Democrat. The fixed effects shift inference to how within-market changes in advertising prices between two elections indirectly affect within-market changes in advertising levels and vote shares. Furthermore, by pooling candidate-share observations across counties and in two elections, we observe 9,576 advertising exposures and resulting vote shares.

The estimates show a robust positive advertising effect across a number of specifications, including numerous exogenous control variables and their interactions with political party dummies. Advertising elasticities are approximately 0.03, which is smaller than estimates typically found in consumer packaged goods and lower than roughly comparable estimates in Huber and Arceneaux (2007).

To provide a better metric for the role and importance of advertising on state-level outcomes, we consider two counterfactuals that eliminate all advertising. One allows turnout and candidate shares to freely adjust with zero advertising, and the second fixes turnout at observed levels to isolate the persuasive effects of advertising implied by our estimates. In the first zero-advertising counterfactual, we find that three states switched sides in 2000 (two to Gore and one to Bush) and one switched in 2004 (to Bush). The shift in 2000 would have been sufficient for Gore to overtake Bush in electoral votes. The counterfactual that holds turnout fixed produces a similar outcome but without Bush gaining any states in 2000. We note that these results should not be interpreted as strict predictions, because the counterfactuals require all other factors to be held fixed. The goal of this exercise is merely to highlight that advertising's causal effects are great enough to shift the election outcome and that they can be asymmetric across candidates.

Our paper contributes in two ways to the literature on measuring the effects of political advertising. First, and most critically, our particular identification strategy allows us to address the endogeneity of advertising. Political scientists have long recognized the endogeneity of a candidate's choice variables (Green and Krasno 1988, Gerber 1998). In part because of the difficulty of identifying reasonable instruments, recent work employs field or natural experiments to estimate advertising's causal effect (Gerber et al. 2011). Second, our model combines a

[5] We have done several robustness checks with respect to this aggregation and do not find evidence that disaggregating the ads has any impact on voters' decisions.

voter's decision to turn out to vote and the decision regarding the candidate for whom to vote, whereas most prior work considers these decisions separately (Ashworth and Clinton 2006, Huber and Arceneaux 2007). Shachar (2009) also analyzes the effects of candidates' marketing-mix variables but restricts advertising to affect turnout and does not account for unobservable shocks.

Perhaps the two papers closest to ours are Che et al. (2007) and Rekkas (2007). The former estimates an individual-level nested logit model using a combination of voter surveys and the number of ads run in each market. Rekkas (2007) studies the effects of overall campaign spending on parliamentary elections in Canada using a model by Berry et al. (1995, hereafter BLP). Both papers consider only a single election year, such that identifying advertising's effects rests on cross-market variation that might be confounded with market-party unobservables. Our within-market identification strategy alleviates such a concern about our analysis.

The remainder of this paper is structured as follows. The next section describes the advertising and election outcome data. Section 3 describes the aggregate discrete choice demand model and our identification strategy. Section 4 presents the estimates, elasticities, and the zero-advertising analysis. Section 5 concludes.

2. Data

This section details our data sources and approach to constructing our instruments. The data vary in the geographic unit at which they are measured. Electoral votes are measured at the state level, but candidates set advertising quantities at the media-market level, which can span multiple states. We measure voting outcomes at the county level, which, in all but a few cases, only include one media market.[6]

2.1. Advertising

The advertising data come from the Campaign Media Analysis Group (CMAG) for the 2000 and 2004 presidential elections, made available through the University of Wisconsin Advertising Project. CMAG monitors political advertising activity on all national television and cable networks and assigns each advertisement to support the proper candidate. The data provide a complete record of every advertisement broadcast in each of the country's top designated market areas (DMAs), representing 78% of the country's population. Television ads are the largest component of media spending for political campaigns according to *AdWeek* (2010). See Freedman and Goldstein (1999) for more details on the creation of the CMAG data set.

The data contain a large number of individual presidential ads: 247,643 for the 75 largest DMAs in 2000 and 807,296 for the 100 largest DMAs in 2004. Because our identification strategy focuses on cross-election changes in outcomes, we restrict all subsequent analysis to the 75 largest DMAs. For each ad, we observe all the dates and times at which it aired, the length in seconds, the candidate supported (e.g., Democrat, Republican, Independent), and the sponsoring group (e.g., the candidate, the national party, independent groups, or "hybrid/coordinated").[7] We include all ads regardless of the sponsoring group and only those ads for which we can identify the target candidate. We further concentrate on those ads airing after Labor Day, which marks the beginning of earnest competition in the general election. The data show that total spent on television advertising by all presidential candidates was \$168 million in 2000 and \$564 million in 2004.

Our key advertising variable is expressed in gross rating points (GRPs), because it measures the number of exposures per capita. One alternative measure is the number (or total length) of ads aired in a market. However, the number of ads is an inaccurate measure of quantity because it treats one ad seen by many people and another ad seen by a few people as the same. GRPs capture the actual "quantity" of advertising relevant for estimating effectiveness because it accounts for variation in exposure rates.

Although we do not directly observe GRPs in our data, we reconstruct the GRPs based on an advertisement's cost and the price per GRP, which is commonly referred to as the cost per point (CPP). The CMAG data include an advertisement's estimated cost. We obtain quarterly forecasts of CPP by market, population subcategory, and time slot (daypart) from SQAD, a market research firm that specializes in estimating media costs. We use CPPs from the third quarter to align with the timing of ad purchases in our data, and we focus on the 18-and-over population demographic to align with voting age. We then match each advertisement with the corresponding CPP according to the market, population subcategory, and daypart. Aggregating over the ads a and dayparts d, we obtain the total GRPs at the election-market-party level:

$$\mathrm{GRP}_{tmj} = \sum_{d} \frac{\sum_a \mathrm{Expenditure}_{tmjda}}{\mathrm{CPP}_{tmd}},$$

[6] Of the 1,596 counties in our data, only five belong to multiple designated marketing areas. We use zip code-level population data to weight the advertising proportionally according to the share of the population in a given DMA.

[7] Among other entities, the independent sponsor groups include the 527 organizations that attracted significant attention during the 2004 election cycle (e.g., American Solutions for Winning the Future, EMILY's List, Swift Boat Veterans for Truth).

where t is the election year, m the media market, and j the party. The equation above first calculates the GRP estimate within each daypart by dividing expenditures by CPP and then aggregates over dayparts to get the aggregate GRPs for a candidate in a given market and election year. We use this measure of GRPs (in thousands) as the advertising variable in the analysis.

Our GRP estimate contains two potential sources of measurement error. First, the actual price a candidate paid could differ from our CPP data as a result of quantity discounts for purchases of large advertising blocks or unobserved variation in advertising prices within the quarter. This concern may not be an issue, because CMAG reconstructs its advertising cost estimates from actual GRPs and CMAG's own estimates of the CPP, so the costs we observe do not include such candidate-specific CPPs. Second, the CPP we observe is a forecast made by SQAD. Although the true CPP probably differs from our data, this particular measurement error is purely random and will be absorbed into the unobservable shocks we include in the model. Measurement error may cause an attenuation bias, but the instrumental variables we describe remove such biases because inference is focused on the variation in our GRP measure that is attributable to variation in the instruments.[8]

Table 1 displays summary statistics for the major party candidates' advertising in 2000 and 2004. Two important points are worth noting. First, we observe significant variation in advertising across markets within a given election. The support of the advertising distribution ranges from zero to about six million dollars in 2000 and from zero to about nine million dollars in 2004. The Republicans chose not to advertise in 20 markets in 2000 and 32 markets in 2004. For Democrats, the numbers are 28 in 2000 and 25 in 2004.[9]

Table 1 Market-Level Advertising by Candidate and Election Year

	No. of obs.	Mean	Std. dev.	Min	Max
2000 election					
GRPs Bush	75	5.82	5.60	0	15.89
GRPs Gore	75	4.78	5.68	0	17.94
GRPs other	75	0.08	0.12	0	0.42
Expenditures Bush ($)	75	879.84	1,218.73	0	6,185.45
Expenditures Gore ($)	75	681.53	1,072.94	0	5,941.61
Expenditures other ($)	75	20.01	44.68	0	322.88
2004 election					
GRPs Bush	75	7.81	10.44	0	35.98
GRPs Kerry	75	9.73	12.81	0	46.22
GRPs other	75	0.003	0.003	0	0.012
Expenditures Bush ($)	75	1,123.76	1,863.43	0	8,386.41
Expenditures Kerry ($)	75	1,349.32	2,207.18	0	9,856.52
Expenditures other ($)	75	1.29	2.27	0	14.17

Note. All units are reported in thousands.

Second, given that our estimation strategy focuses on within-DMA variation, we are fortunate to observe rich variation in total advertising expenditures and GRPs between 2000 and 2004. Table 1 shows that both total ad quantities and total ad expenditures significantly increased from 2000 to 2004, consistent with the growing importance of advertising in political elections. Dividing the total expenditures by the GRPs, the average price for Republicans dropped from $151 to $144 per point in 2004 and for Democrats from $143 to $139. However, the advertising prices for the most common daypart (early news) increased by about 5%. Figure 1 plots the change in GRPs from 2000 to 2004 for the Republican and Democratic candidates. As expected, the changes in GRPs across candidates are highly correlated, indicating that the candidates tended to increase and decrease spending in the same DMAs. The figure also exhibits significant variation both between elections and across DMAs.

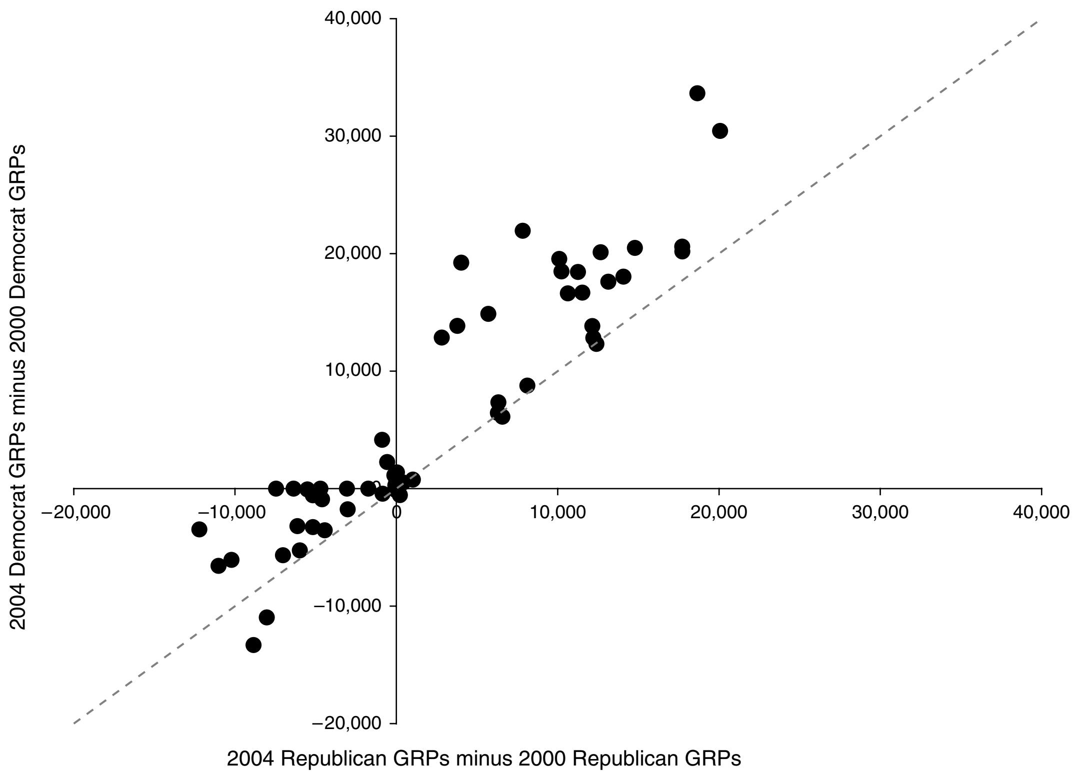
2.2. Instruments

A central issue in our empirical application is the endogeneity of candidate advertising. We naturally

[8] Our specification treats advertising GRPs across dayparts as perfectly substitutable. Some dayparts may be more relevant for certain candidates because of variation in a daypart's audience demographics and the audience's likely political preferences. This assumption raises two potential concerns: First, such variation could be interpreted as either heterogeneity across candidates in the effectiveness of advertising or as candidate-specific measurement error in the relevant GRPs for a fixed advertising coefficient. Daypart distinctions in the GRP definition would imply that daypart distinctions in our instruments (defined in §2.2) are also relevant. This possibility could prevent the instruments from removing such a source of measurement error. Second, variation in CPPs across programs and dayparts may be systematically related to voters' preferences. CPPs vary across programs because the attractiveness of an audience to advertisers varies across programs. Two shows with identically sized audiences may have different CPPs because of variation in the characteristics of those audiences. For example, the CPP of one show may be higher if its audience has higher consumer spending levels and is more likely to purchase the advertiser's product. In a sense, audience members of this show "count more" to advertisers than audience members of a less attractive show. Voters, however, whether high- or low-spending ones, all count the same when it comes to obtaining a vote. Audiences of different TV programs likely differ in their responsiveness to political advertising, but the critical question is whether the propensity to respond to a political advertisement is also correlated with how attractive these individuals are as potential customers for an advertiser. Although we have no reason to believe such a systematic correlation exists, our data are not sufficiently rich to allow us to address this point.

[9] The structure of the Electoral College creates particular incentives that drive much of the observed pattern of advertising. In particular, so-called battleground states receive a disproportionate share of a candidate's advertising as a result of the expected narrow margin of victory. Conversely, non-battleground states, where a given party expects to win by a handsome margin, receive little to no advertising from either candidate because any such intervention would not be expected to alter the outcome of the state's election. Candidates' advertising allocation decisions are explored in detail in Gordon and Hartmann (2012).

Figure 1 Within-DMA Changes in GRPs: 2000 to 2004



expect the advertising variation depicted in Figure 1 to reflect some knowledge that the candidates observe but that we do not. In standard differentiated product choice contexts (e.g., BLP), ignoring price endogeneity leads to an underestimation of price sensitivity, because the unobservables are positively correlated with prices. However, in the context of political candidate choice, the direction of the bias is ambiguous. Candidates are both unlikely to advertise in markets where they have little chance of winning and unlikely to advertise in markets where they strongly expect to win. Therefore, whether unobserved demand shocks are substantially higher or lower in the presence of more advertising is unclear, making assigning the direction of the endogeneity bias difficult.

We consider a candidate's decision process to find suitable instruments. Although we do not model the candidates' decisions here, one obvious variable that affects advertising allocations but is unlikely to affect voters' preferences is the price of advertising. Two potential concerns arise from such an instrument choice. First, candidates might purchase enough advertising in a market to affect the equilibrium price of advertising in that market, such that they would no longer act as price takers. This issue would invalidate the instrument, because the price would not be exogenous to candidates' advertising decisions. To avoid this concern, we use the prior year's advertising price (1999 for 2000 and 2003 for 2004) when market advertising prices were free of political factors.

Second, measurement errors in the lagged CPP estimates arising from SQAD's methodology could be systematically related to current CPP estimates. We do not expect such a bias to exist, because SQAD updates its advertising price predictions each quarter to account for realized prices in the past quarters. If the measurement errors were correlated, SQAD would be making a systematic mistake in the same direction, which seems unlikely, given the nature of the firm's business.

To convert the instrument to a per-capita basis, we use the cost-per-thousand impressions (CPM) instead of per point. Our motivation for using CPM is similar to our decision to use GRPs as our endogenous advertising variable: CPMs more accurately account for exposures per capita. As with the CPP, the CPM in a market varies over the dayparts because the cost of reaching a thousand viewers varies over the day. We use the CPM in each of the eight dayparts as our primary lagged advertising price instrument.[10]

[10] CPM and CPP are directly related through the population: CPP = (Population × CPM)/100, where Population is in thousands. When analyzing data on a per-capita basis (e.g., market shares), CPM is relevant because it reflects the cost to reach one person in the relevant population. We therefore use CPM when instrumenting for ad exposures, whereas the CPP is relevant for calculating GRPs because it is defined as GRP = (Ad Expenditures)/CPP.

We observe significant variation across candidates in when they choose to advertise during the day and in the price of advertising across elections. Figures 2 and 3 show how each candidate spread his GRPs across dayparts in 10 DMAs in 2000. The early news and daytime slots are the most common across DMAs, yet Gore, for instance, bought fewer GRPs in Kansas City during the early news than in prime access or late fringe. Although 30% of Bush's GRPs in Spokane were in early news, less than 15% in Milwaukee were in early news. Given this mix, each daypart CPM is potentially relevant for advertising decisions, and the importance of each daypart CPM varies across DMAs.

Table 2 provides summary statistics of the change in the CPM between 1999 and 2003 in each daypart and demonstrates substantial variation in the daypart CPMs over time. Most CPMs increased over this period, with only a few markets experiencing declines. Daypart CPMs are correlated—though not perfectly—with one another. For example, the smallest correlation of 0.55 is between daytime and prime time, whereas the largest is 0.93 between late fringe and prime time. Figure 4 illustrates how the early news CPM varied within DMAs between 1999 and 2003.

Considering why advertising prices varied within markets over time is important to ensure that this variation is not correlated with preferences for political candidates. One source of within-market variation in ad prices is local demand shocks for major advertisers, which we expect (and must assume) are uncorrelated with changes in voter preferences. Another could be changes in local economic conditions or demographics, both of which could relate to changes in political preferences. We therefore detail in §2.4 a set of economic and demographic variables that we include in the analysis to address this concern.

Given the motivation for our particular instrumental variables, we now compare our approach to the extant literature. Work in political science uses both instrumental variables and field/natural experiments to deal with the endogeneity of candidate choice variables. First, instrumental variable techniques gained early traction in work that measures the effects of aggregate candidate campaign spending on voting outcomes (Jacobson 1978). These studies typically examine congressional races to take advantage of more independent observations, although campaign spending levels are much lower than in presidential campaigns. Green and Krasno (1988) use lagged incumbent spending in Senate elections to instrument for current incumbent spending, and they must assume challenger spending is exogenous. Recognizing this issue, Gerber (1998) uses a combination of instruments, including a measure of the challenger's personal wealth and the state's voting age population. A wealthier candidate should be able to spend more on advertising, although a concern might be that candidate wealth is not excluded from voters' decisions if it signals a candidate's quality. A large population provides the candidate with more citizens from whom to raise funds. Yet more voters need to be reached in more populous places, so such an argument may only apply to advertising under large fixed costs, as opposed to affecting advertising levels at the margin. This argument also does not transfer to a presidential election setting because funds can be raised nationwide. Ansolabehere et al. (1999) consider the effects of negative ads on voter turnout using GRPs as instruments, but the GRPs are a choice variable the candidate potentially determines in response to an econometric unobservable in the choice equation. Levitt (1994) addresses candidate unobservables by examining congressional races in which two opposing candidates face each other in multiple elections. Differencing eliminates any fixed candidate or local influences, and the results suggest congressional campaign spending has little effect on voting outcomes.

A second approach is to exploit natural experiments or to conduct field experiments to generate exogenous variation. Huber and Arceneaux (2007) take advantage of the fact that some media markets overlap battleground and non-battleground states, exposing voters in the latter to higher advertising levels than the candidate intended. The authors link advertising levels to data from the National Annenberg Election Surveys (NAES) on an individual's campaign interests and voting intentions, and they find evidence that advertising influences voters' candidate choices but not whether to turn out to vote. Gerber et al. (2011) use a field experiment in the 2006 gubernatorial election in Texas to examine the effect of advertising on voters' stated attitudes and intentions (collected via telephone surveys), and they find televised ads have strong but short-lived effects on voting preferences.

2.3. Votes

The county-level vote data are available from http://www.polidata.org. For each of the 1,596 counties, we observe the number of votes cast for all possible candidates and the size of the voting age population (VAP). The VAP estimates serve as our market size parameters and allow us to calculate a measure of voter turnout at the county level. The voting-eligible population (VEP), a more accurate measure for calculating turnout that removes non citizens and criminals, is only available at the state level.[11]

[11] See the Web page maintained by Michael McDonald at http://elections.gmu.edu/voter_turnout.htm (accessed October 19, 2012) for more information on measures of voter turnout.

Figure 2 **Daypart Mix for Democrats in 2000: GRPs in 10 DMAs**

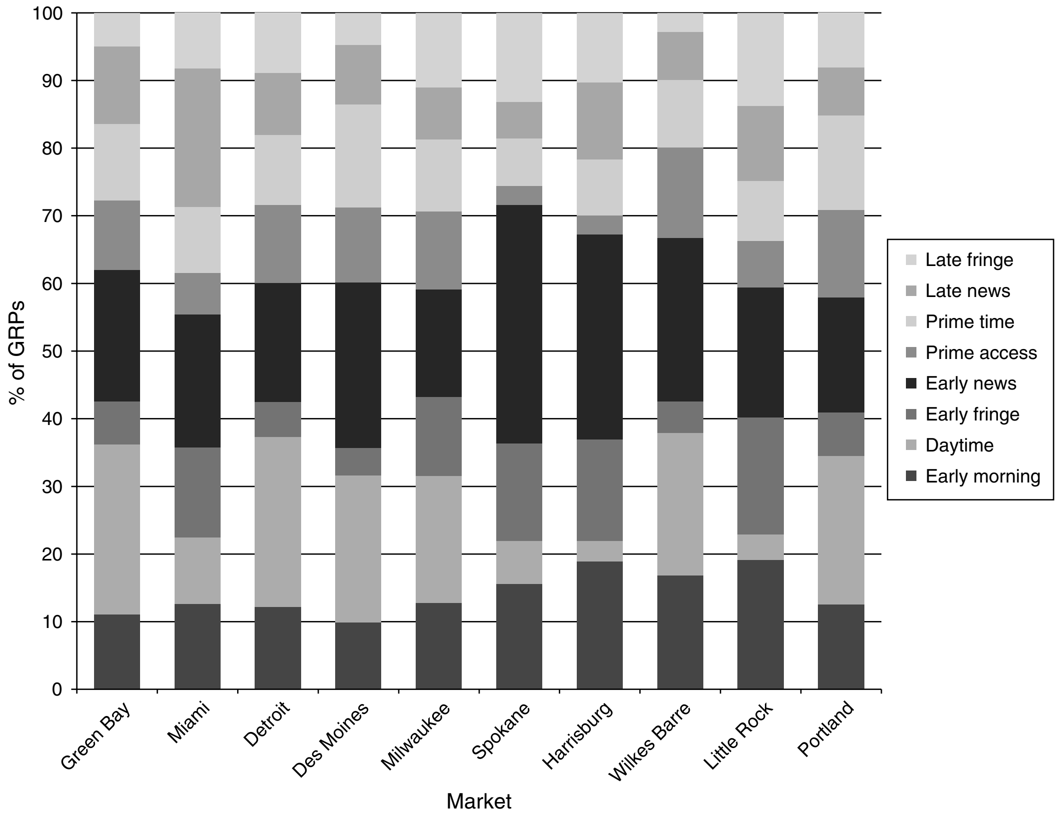


Figure 3 **Daypart Mix for Republicans in 2000: GRPs in 10 DMAs**

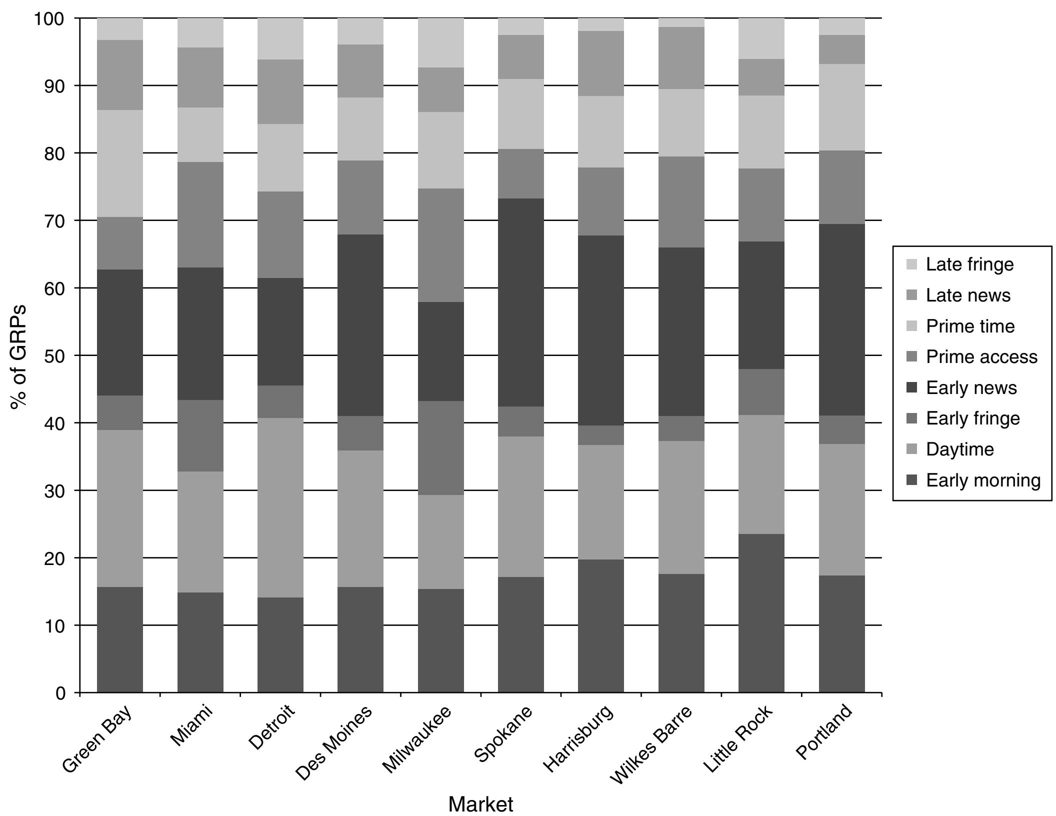


Table 2 **Summary Statistics of the Change in the Lag CPMs from 1999 to 2003**

Daypart	No. of obs.	Mean change	Std. dev.	Min	Max
Early morning	75	1.21	1.30	−1.24	4.85
Daytime	75	0.02	0.88	−2.05	2.38
Early fringe	75	0.65	1.18	−1.98	2.74
Early news	75	1.50	1.56	−2.26	5.40
Prime access	75	3.10	1.88	−1.11	10.06
Prime time	75	3.82	2.80	−2.48	11.75
Late news	75	3.72	2.07	−1.75	9.06
Late fringe	75	1.04	1.98	−3.47	6.21

Figure 4 **Early News CPM by DMA: 1999 vs. 2003**

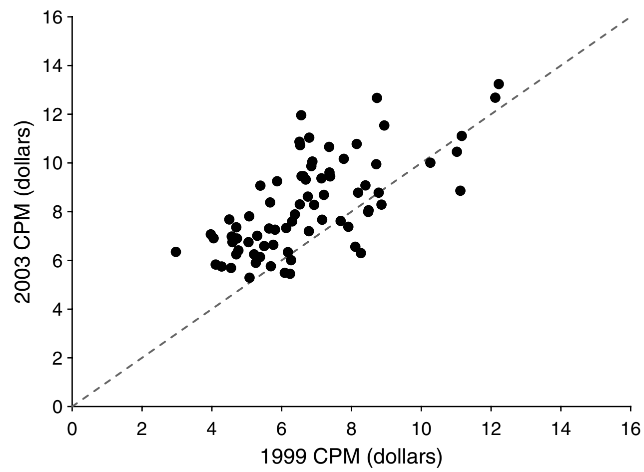


Table 3 summarizes the total votes and vote shares of each candidate by county and election year. The Democrats had a higher number of average votes per county in both years than did Republicans. However, the Republicans had higher average shares per county. Together, these voting outcomes reveal that Democrats tend to do better in larger counties. By focusing on counties within the top 75 DMAs, our data omit a greater number of Republican votes. Whether excluding such Republican-leaning counties would bias our parameter estimates is unclear. In computing our counterfactual in which we set advertising to zero, we include the voting outcomes in counties outside the top 75 DMAs and hold their levels fixed.

For estimation, we group all candidates outside of the two major parties into a single third-party candidate option, summing the votes and GRPs across these candidates. Estimating the model without aggregating the smaller candidates into a single option is possible. However, the majority of voters were probably unaware of many of these candidates.[12] With the exception of Ralph Nader in the 2000 election, the other nonmajor party candidates' vote shares were very small (below 0.5%), and many spent little on advertising. Thus we prefer to aggregate them so we can focus on measuring the effectiveness of advertising for the Republican, Democrat, and collective third-party candidates.

2.4. Additional Control Variables

By focusing on within-market variation, fixed effects absorb the systematic variation across geographies, so that we can estimate the advertising effect as cleanly as possible. Nevertheless, accounting for some within-market changes is useful. We include four categories of variables that absorb some of the remaining within-market variation: (1) variables that measure local political preferences (market-level party affiliation), (2) variables that affect voter turnout but not candidate choice (the occurrence of a Senate election and local weather conditions), (3) demographic and economic variables (population age demographics, local unemployment and wages), and (4) candidate-specific local variables (distance to the candidate's home state, whether there was a same-party incumbent governor, and several candidate–intercept interactions). Summary statistics for all these variables can be found in Table 4.

A county's average political preferences may shift to the left or right depending on their similarity to those of the incumbent party or the local political climate. We use the NAES to include a measure of the percentage of voters in a media market who identify with a political party. In each year, we merged the six national cross-sectional surveys into a single data set, resulting in 58,373 observations for 2000 and 81,422 observations for 2004. Between 2000 and

[12] The only third-party candidate to run in both elections who had any significant public visibility was Ralph Nader, who ran on the Green Party ticket in 2000 and as an independent in 2004. In 2000, Nader received 2.74% of the popular vote, but only 0.38% in 2004, and he did not win any electoral votes either time. In 2000, some of the other candidates included Harry Browne (Libertarian Party), Howard Phillips (Constitution Party), and John Hagelin (Natural Law Party). In 2004, the other candidates were Michael Badnarik (Libertarian Party), Michael Peroutka (Constitution Party), and David Cobb (Green Party).

Table 3 **Summary Statistics of County-Level Voting by Candidate and Election Year**

	No. of obs.	Mean	Std. dev.	Min	Max
2000 election					
Votes Bush	1,596	23,569	51,889	210	871,930
Votes Gore	1,596	25,520	78,142	77	1,710,505
Share Bush	1,596	0.297	0.086	0.039	0.630
Share Gore	1,596	0.214	0.064	0.056	0.472
2004 election					
Votes Bush	1,596	29,168	63,315	216	1,076,225
Votes Kerry	1,596	29,698	89,285	95	1,907,736
Share Bush	1,596	0.341	0.089	0.047	0.666
Share Kerry	1,596	0.229	0.081	0.057	0.569

Table 4 Summary Statistics for Control Variables

	Obs.	Mean	Std. dev.	Min	Max
2000 election					
Senate election	1,596	0.666	0.472	0.000	1.000
Rain (inches)	1,596	0.198	0.262	0.000	1.633
Snow (inches)	1,596	0.080	0.398	0.000	6.108
%25 ≤ *Age* < 44	1,596	0.378	0.050	0.156	0.581
%45 ≤ *Age* < 64	1,596	0.316	0.032	0.160	0.509
%65 ≤ *Age*	1,596	0.190	0.052	0.040	0.408
%*Unemployment*	1,596	4.091	1.559	1.300	15.600
Average salary	1,596	24.962	6.644	11.546	76.820
%*Identified Republican*	75	0.290	0.056	0.133	0.415
%*Identified Democrat*	75	0.302	0.053	0.178	0.445
Gub. Incumb. Same					
Republican	1,596	0.617	0.486	0.000	1.000
Democrat	1,596	0.342	0.475	0.000	1.000
Distance					
Republican	1,596	9.484	4.104	0.000	18.060
Democrat	1,596	6.173	4.586	0.000	19.940
2004 election					
Senate election	1,596	0.679	0.467	0.000	1.000
Rain (inches)	1,596	0.279	0.507	0.000	3.919
Snow (inches)	1,596	0.020	0.116	0.000	1.388
%25 ≤ *Age* < 44	1,596	0.342	0.053	0.145	0.559
%45 ≤ *Age* < 64	1,596	0.334	0.032	0.172	0.551
%65 ≤ *Age*	1,596	0.186	0.049	0.044	0.391
%*Unemployment*	1,596	5.622	1.664	2.400	16.100
Average salary	1,596	27.885	6.947	12.063	80.013
%*Identified Republican*	75	0.314	0.065	0.141	0.456
%*Identified Democrat*	75	0.315	0.053	0.176	0.438
Gub. Incumb. Same					
Republican	1,596	0.468	0.499	0.000	1.000
Democrat	1,596	0.532	0.499	0.000	1.000
Distance					
Republican	1,596	9.484	4.104	0.000	18.060
Democrat	1,596	10.836	6.263	0.000	26.310

Note. Average salary is in thousands of dollars, and distance is in hundreds of miles.

2004, the percentage of Republicans increased about 2.4 percentage points and the percentage of registered Democrats increased 1.3 points on average across all DMAs. Republican shares varied between 10 positive and negative percentage points at the extremes. Democratic shares of registered voters dropped by at most 5.5 percentage points in a DMA, whereas the greatest increase was 7 points.

The party affiliation variables above are designed to capture variation in preferences across parties, and hence candidates, within a market. We also want to include variables that primarily affect a voter's decision to turn out for the election. First, we include separate variables to indicate whether a Senate election occurred in the same year.[13] Although presidential elections are much more likely to drive turnout, a hotly contested Senate seat could generate some spillover effects. Second, we include county-level estimates of rainfall and snowfall on Election Day from the National Climatic Data Center's "Summary of the Day" database (obtained through EarthInfo). Gomez et al. (2007) show that weather can affect voter turnout in presidential elections.

We include demographic and economic variables that exhibit variation between the two elections. We use Census data on the percentage of the county's population between the ages of 25 and 44, 45 and 64, and older than 65. Because of the lingering effects of the baby boom and migration patterns, these percentages change even within the four-year time span we consider. To capture variation in the local economic conditions, we obtain percentage unemployment data at the county level from the Bureau of Labor Statistics. To account for variation in economic conditions among employed persons not controlled for by unemployment, we calculated the average salary using the total annual wages paid by firms, divided by the total number of employees in a county as reported in the County Business Patterns.

Finally, we include some variables that differ across candidates within the local markets in which we conduct our analysis. We create an indicator for whether the candidate has a same-party incumbent governor in the state, the distance (in miles) between a given DMA and the candidate's home state, and interactions between the major party candidate intercepts and the demographic and economic variables. These interactions, in particular, allow for changes in these local conditions to exert asymmetric effects on voters' preferences for a given candidate.

3. Modeling Voter Preferences

We specify a static aggregate discrete choice model of demand for political candidates. The model reflects voter utility for the candidate and is relatively agnostic about the precise mechanism through which advertising affects candidate choice. In this sense, the model is not a fully structural representation of the decision to vote. We do not specify a functional form to distinguish between informative or prestige effects of advertising, and we do not distinguish between positive or negative ads.[14] We also abstract from several aspects of voter choice found in more formal models of political economy, such that voters do not act strategically based on their expectations of being the pivotal voter to decide the election outcome.[15]

[13] We cannot include an indicator for gubernatorial elections because they occur every four years. Because the same set of states held gubernatorial elections in 2000 and 2004, these indicators would largely be absorbed into the DMA–party fixed effects.

[14] Advertising negativity, for instance, is likely driven entirely by voters' preferences, making it particularly challenging to identify a valid instrument for inference.

[15] The lack of strategic voting is consistent with Feddersen and Pesendorfer (1996); however, Coate et al. (2008) show that voters' expectations of being pivotal can play a role in small elections.

Finally, we aggregate advertising across time during the election for two reasons. First, aggregating advertising provides a more easily defined variable of interest than a specification that indexes advertising by time. Second, and more importantly, instruments are not available at a higher frequency (e.g., weekly) to exogenously shift candidates' preferences for advertising, preventing us from properly estimating such a model. Thus, we are unable to estimate a dynamic advertising model in this context and cannot rule out the possibility that dynamic advertising effects may exist.[16]

3.1. Voter Utility

A voter's utility for candidate j in election t is

$$u_{itcj} = \beta_{tj} + \boldsymbol{\alpha} A_{tmj} + \phi' \mathbf{X}_{tc} + \gamma_{mj} + \xi_{tcj} + \varepsilon_{itcj}, \qquad (1)$$

where β_{tj} is the taste for a candidate from party j in election t, A_{tmj} is advertising by the candidate, α captures the marginal utility of advertising, γ_{mj} represents market–party fixed effects that fit the mean utility for a party in a market, and ε_{itcj} captures idiosyncratic variation in utility across voters, candidates, and periods; ξ_{tcj} is a time–county–party demand shock that is perfectly observable to voters when casting their votes but is unobservable to the researcher. Candidates have beliefs about the demand shocks ξ_{tcj} that induce endogeneity in candidates' advertising strategies. Because advertising effects likely exhibit diminishing marginal returns, we operationalize the advertising variable using $A_{tmj} = \log(1 + \text{GRP}_{tmj})$. If a voter does not turn out for the election, he or she selects the outside good and receives a (normalized) utility of $u_{itc0} = \varepsilon_{itc0}$.

$\mathbf{X}_{tc}$ is a vector of observables containing the variables described in §2.4. We do not index this by j because only a subset of these variables is specific to the party in a county/market and election. These variables affect a voter's decision to turn out for the election (e.g., county-level Senate election dummies, county-level rain and snow) or a voter's decision to vote for a particular candidate (e.g., market-level interactions between candidate intercepts and party identification variables).

Assuming the $\{\varepsilon_{itcj}\}_j$ are independent and identically distributed Type I extreme value, we can integrate over these idiosyncratic shocks to obtain vote shares:

$$s_{tcj}(\mathbf{A}_{tm}, \mathbf{X}_{tc}, \xi_{tc}; \theta) = \frac{\exp\{\beta_{tj} + \alpha A_{tmj} + \phi' \mathbf{X}_{tc} + \gamma_{mj} + \xi_{tcj}\}}{1 + \sum_{k \in \{1,\ldots,J\}} \exp\{\beta_{tk} + \alpha A_{tmk} + \phi' \mathbf{X}_{tc} + \gamma_{mk} + \xi_{tck}\}}. \qquad (2)$$

This model is an aggregate market share model with homogeneous preferences, equivalent to the logit specification in Berry (1994). As a robustness check, we considered a more flexible model with unobserved heterogeneity in the candidate intercepts β_{tj} and advertising coefficient α. We were unable to find evidence of unobserved heterogeneity in any version of the model that included the market–party fixed effects. Section 4 documents that the market–party fixed effects are critical to resolve the endogeneity concerns, and so we retain the model with homogeneous preferences.

We consider a second robustness check that relaxes the inference of cross-advertising elasticities arising from the homogeneous logit. This specification allows the advertising of the Republican and Democratic candidates to separately enter voter utility for each candidate choice. Thus, $\alpha = [\alpha_{\text{own}}, \alpha_{\text{opp}}]$ becomes a vector with own and opponent advertising effects. Although this specification abstracts from common structural elements, it has the benefit of allowing us to infer cross elasticities directly, as opposed to through the unobserved heterogeneity of BLP.

3.2. Estimation and Identification

We use two-stage least squares (2SLS) to estimate our model specifications.[17] Identification of the parameters follows from standard arguments when estimating demand using aggregate market share data. We observe variation in vote shares, advertising levels, demand-side covariates, and instruments across time and many markets. The specification in Equation (1) involves a single endogenous variable (advertising), and the exclusion of the price of advertising from utility forms the basis for lagged advertising prices to serve as the excluded exogenous variable. There are three distinct factors to discuss about our identification strategy.

First, whereas most aggregate demand models use cross-market variation for identification, we use market–party fixed effects to absorb cross-sectional additive unobservables that could be correlated with both candidate shares and instruments. These fixed

[16] Our model implicitly assumes that advertising is perfectly substitutable across weeks of the election such that all advertising can be aggregated over time. Although we lack weekly advertising prices to properly test this restriction, we estimated a number of robustness checks and descriptive regressions that allowed individual weeks or groups of weeks to separately influence votes. We are unable to find any significant explanatory power attributable to including disaggregated advertising across weeks during the election. The results are available from the authors upon request.

[17] To estimate a BLP version of the model as a robustness check, we use the approach in Dubé et al. (2012) and direct the interested reader to that paper for more details.

effects narrow the identification to explaining within-market variation in a party's performance based on within-market variation in explanatory variables and instruments. We do not account for market-specific unobservables in the slope coefficient on advertising. Such unobservables could arise, for instance, from cross-market variation in the fraction of swing voters if swing voters differ in their advertising sensitivity compared with non-swing voters. Wooldridge (1997) shows 2SLS yields consistent estimates of the coefficient on the endogenous variable despite the presence of such unobservables.[18] The intercepts may, however, be biased, suggesting caution in interpreting any results that rely on the intercepts when such unobservables are likely present. We note our elasticity estimates remain consistent, because their calculation only relies on the advertising coefficient, observed advertising levels, and vote shares, but the zero advertising counterfactuals could potentially be biased.

Second, temporal variation in unobservable factors, not captured by our fixed effects γ_{mj}, could influence instruments and voting outcomes. Although part of this unobservable variation is captured in the temporal variation in the advertising prices, some remaining variation could be a result of unobserved changes in demographics or economic conditions. Changes in local conditions might also affect voter preferences. We attempt to address these unobservables through the observed temporal variation in $\mathbf{X}_{tc}$, our demographic and economic variables, and we use interactions between $\mathbf{X}_{tc}$ and candidate intercepts to allow changes in these local conditions to affect voter choice. However, some unobserved temporal variation within a market could still induce correlation between the instruments and voting outcomes.

Third, we use the one-year lagged advertising prices described in §2.2. A single instrument is sufficient to identify the model that excludes opponent advertising from voter utility in Equation (1), because the second stage in 2SLS is essentially a seemingly unrelated regression across candidates with own-candidate advertising as the sole endogenous variable.

The alternative specification with the opponent's advertising introduces an additional endogenous variable. This extra endogenous variable requires another instrument that varies over candidates. We obtain additional instruments by interacting the excluded advertising prices with the candidate dummies and covariates $\mathbf{X}_{tc}$, which includes variables that are either common or specific to the candidate. These interactions must be excluded from voter utility because advertising prices themselves are excluded. Such interactions may theoretically arise from nonlinearities in a supply-side model in which candidates set advertising levels across markets (see Gordon and Hartmann 2012). Because these nonlinearities between advertising choices and covariates exist under numerous functional form assumptions, identification here is fairly general. Moreover, the interactions are testable in a first-stage estimation, and even though a nonlinearity motivates the interactions, a nonlinearity is not required to be imposed in estimation.

For all specifications only with own-candidate advertising, we form instruments using interactions between the lagged advertising prices (for each daypart) and party/year dummies. We only include the party and covariate interactions when estimating the model with the opponent's advertising, because 2SLS is known to exhibit finite sample bias in the presence of too many redundant instruments (Hansen et al. 2008). We report tests for overidentification and weak instruments below.

4. Results

This section begins by presenting parameter estimates from a variety of specifications. Then we report advertising elasticities and the predicted election outcomes from two counterfactuals in which we set all advertising to zero.

4.1. Parameter Estimates

Table 5 contains the estimates from eight specifications. We cluster standard errors at the DMA–party level to account for the fact that advertising and lagged price instruments are constant across counties within a DMA. The first two specifications include party and party–year fixed effects, and the remaining specifications add DMA–party fixed effects. For columns (1)–(7), the instruments are the lagged advertising prices and interactions between them and election and political party dummies. In column (8), we include interactions between the lagged advertising price variables and the demographic and economic variables, as discussed in §3.2.[19] To test whether our instruments are sufficiently strong, we also report the first-stage F-statistic of excluded regressors and Hansen's J-statistic of overidentifying restrictions.

We begin with an ordinary least squares (OLS) regression in column (1), in which the dependent variable is the difference in the log shares of a candidate and the outside no-vote option. The advertising coefficient is 0.111 and significant at the 1% level, although

[18] Several papers in marketing consider approaches for dealing with this "slope endogeneity," such as Manchanda et al. (2004) and Luan and Sudhir (2010).

[19] We compared the F-statistics reported in Table 4 to the critical values derived by Stock and Yogo (2005), which are calculated in Stata, and conclude that weak instruments are not a problem.

Table 5 Parameter Estimates

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) 2SLS	(6) 2SLS	(7) 2SLS	(8) 2SLS
Candidate's ads	0.111***	0.150***	0.052***	0.060***	0.070**	0.048*	0.069***	0.064*
	(0.020)	(0.050)	(0.011)	(0.013)	(0.030)	(0.028)	(0.024)	(0.035)
Opponent's ads								0.017
								(0.038)
Senate election					−0.032	−0.040	−0.041	−0.042
					(0.045)	(0.044)	(0.044)	(0.045)
Gub. Incumb. Same					−0.002	−0.005	0.001	−0.002
					(0.014)	(0.016)	(0.015)	(0.017)
Distance $*$ 100					0.002	0.002	0.003	0.003
					(0.004)	(0.004)	(0.004)	(0.003)
Rain (2000)					−0.072	−0.062	−0.071	−0.073
					(0.083)	(0.083)	(0.088)	(0.089)
Rain (2004)					0.070	0.082	0.078	0.083
					(0.079)	(0.081)	(0.083)	(0.087)
Snow (2000)					−0.053**	−0.036*	−0.028	−0.026
					(0.021)	(0.021)	(0.021)	(0.022)
Snow (2004)					−0.395**	−0.323***	−0.316***	−0.328***
					(0.135)	(0.121)	(0.106)	(0.103)
$\%25 \le Age < 44$						−1.709***	−3.084***	−3.089***
						(0.271)	(0.634)	(0.633)
$\%45 \le Age < 64$						5.445***	5.471***	5.471***
						(0.588)	(1.153)	(1.151)
$\%65 \le Age$						−0.483	−1.211	−1.211
						(0.522)	(1.191)	(1.189)
%*Unemployment*						−0.066***	−0.108***	−0.109***
						(0.008)	(0.017)	(0.017)
Average salary						0.009***	0.010***	0.010***
						(0.002)	(0.003)	(0.003)
Fixed effects								
Party	Y	Y	Y	Y	Y	Y	Y	Y
Year–party	Y	Y	Y	Y	Y	Y	Y	Y
DMA–party			Y	Y	Y	Y	Y	Y
Interactions								
Party–PartyID							Y	Y
Year/candidate **X**							Y	Y
No. of observations	9,576	9,576	9,576	9,576	9,576	9,576	9,576	9,576
First-stage excluded *F*	—	20.3	—	93.0	95.8	95.2	91.6	55.3
Hansen's *J*	—	58.4[a]	—	51.9	59.4	53.3	52.8	97.6[b]
p-value	—	0.12	—	0.29	0.11	0.24	0.26	0.38

Notes. Robust standard errors clustered by DMA–party are in parentheses. *Gub. Incumb. Same* indicates whether the incumbent governor and the candidate are in the same party, and *Distance* is in miles. Interactions: Party–PartyID is a set of six interactions between the Republican and Democrat intercepts and the percentage of voters who self-identify as Republican, Democrat, or Independent in a market; Year/Candidate **X** indicates interactions between election year and *Republican/Democrat* choice intercepts with the three percent age variables, percent unemployment, and average salary. Estimates for the interactions are omitted because of space limitations.

[a] $\chi^2(47)$ for columns (1) to (7), [b] $\chi^2(98)$ for column (8).

*Significant at 0.1; **significant at 0.05; and ***significant at 0.01.

this estimate does not account for the endogeneity of advertising. The next specification in column (2) uses 2SLS to instrument for the advertising levels. The advertising coefficient increases to 0.150 and remains highly significant.

The remaining columns in Table 5 introduce DMA–party fixed effects. These fixed effects account for potential correlation between the advertising instruments and cross-sectional variation in voter preferences. To illustrate, column (3) estimates an OLS regression including the DMA–party fixed effects to facilitate comparison with column (1). The additional fixed effects reduce the advertising coefficient to 0.052 and it remains significant at the 1% level. Estimation using 2SLS in column (4) increases the advertising coefficient slightly to 0.060 and maintains significance at the 1% level. As previously indicated, the sign of the endogeneity bias is ambiguous because advertising occurs when candidates are close in a market, such that strong positive or negative unobservables both tend toward zero advertising. Thus, controlling for cross-sectional unobservables with the DMA–party fixed effects seems to significantly reduce the advertising coefficient.

Next we introduce a series of additional covariates explained in §2.4. Column (5) starts by including the control variables for senate elections, same-party incumbent governors, rainfall and snowfall by year, and the distance to the candidate's home state. Column (6) adds the age demographics, county-level percent unemployment, and average salaries among the county's labor force. We find that snow had a significant negative effect on turnout in both elections. Lower unemployment and higher average salaries are associated with higher voter turnout. In both specifications, the advertising coefficient remains significant, although the coefficient's significance weakens slightly in column (6).

Column (7) includes additional interactions that increase the advertising coefficient to 0.069 with significance at the 1% level. The first set of interactions, labeled Party–PartyID, interacts the three party intercepts with the percentage of the population that identifies itself as Republican or Democrat. These interactions allow local political preferences to exert an asymmetric influence on each candidate's vote share, whereas the other control variables help shift voters' preferences between turning out for the election or not voting at all. Two of the Party–PartyID variables are significant (omitted for brevity), yet the advertising coefficient barely changes. This specification also interacts the demographic and economic control variables with year dummies and the two major party intercepts. Four of the five base variables remain highly significant, and six of the (omitted) interactions are significant, suggesting that these variables influence voters' decisions differently for each political party.

Our last specification in column (8) presents the robustness check that allows a major opposing candidate's advertising to enter the utility for a given major party candidate. Because the opposing advertising introduces an additional endogenous variable, we include a set of interactions between the lagged advertising price and the county-level percent unemployment as additional instruments in the first-stage regression.[20] The estimated own-candidate advertising coefficient is statistically the same but with a p-value of 0.084. The estimated coefficient on the opposing candidate's advertising is, however, insignificant. This effect is consistent across numerous specifications (including many unreported ones) and persists despite the inclusion of many control variables, fixed effects, and correction for the endogeneity of advertising.

As noted earlier, we also estimate several BLP versions of the model that allow for continuous unobserved heterogeneity in the candidate intercepts and advertising coefficient. Without the DMA–party fixed effects, we find significant parameter heterogeneity: the standard deviations of the heterogeneity distributions are positive and significant. With the DMA–party fixed effects, the standard deviations were insignificant and the mean advertising coefficient was close to the estimated coefficient from the 2SLS specification. The DMA–party fixed effects absorb the cross-sectional variation that otherwise would help identify the unobserved heterogeneity.[21]

4.2. Elasticity Estimates

Table 6 presents the elasticity estimates from our preferred specification in column (7) of Table 5. The estimated elasticities are 0.033 for Republicans, 0.036 for Democrats, and an order of magnitude smaller for the third-party candidate.

These sensitivities are lower compared with the median value of 0.05 reported in Sethuraman et al. (2011), which conducts a meta-analysis of advertising elasticities for consumer goods. However, the effectiveness of advertising is likely to vary significantly depending on the product category. For instance, Kadiyali et al. (1999) estimate an advertising elasticity of about 0.03 using GRP data for a personal care product. In the case of new products, which, similar to elections, experience an intense advertising campaign at launch, Ackerberg (2001) finds an elasticity of 0.15. Although we might at first suspect political ads are of greater influence, seeing that people are more wedded to a political candidate than to a yogurt product is perhaps unsurprising.

Comparing our advertising elasticity estimates to previous work in political science is difficult. Few studies report advertising elasticities, and many use

[20] We tested alternative combinations of instruments formed through interactions between the lagged advertising prices and the various demographic and economic variables. Because the demographic and economic variables are correlated with each other, including additional interactions as instruments beyond the first set leads to redundant variables in the first-stage regression.

[21] We considered three additional robustness checks: a model with observable heterogeneity in advertising effects, a heterogeneous BLP model with opponent advertising, and a nested logit model with only the own candidate's advertising. In the first, we interacted the percentage of self-identified independent voters in a DMA with the advertising variable. The implied mean advertising effect remains unchanged, but both advertising coefficients become statistically insignificant. In the second, the opponent's advertising variable remains insignificant even in the presence of unobserved heterogeneity. In the third, the nested logit, a voter first chooses whether to vote and then, conditional on voting, which candidate to vote for. The estimated nesting parameter was not significantly different from zero, so a nested model does not appear helpful. Both of these last two specifications use variables that are exclusive to the turnout decision because of the presence of variables such as rainfall, snowfall, and the Senate race indicator.

Table 6 Elasticity Estimates

	Republican	Democrat	Third party
Republican	0.0333	−0.0144	−0.0144
Democrat	−0.0112	0.0363	−0.0112
Third party	−0.0001	−0.0001	0.0043

Notes. All estimates are significant at the 0.01 level. Results use estimates in column (6) of Table 5. To interpret, for example, a 1% increase in Democrat advertising implies a 0.0112% decrease in the market share of the Republican candidate.

stated preference and intentions data (e.g., "On a seven-point scale, how likely would you be to vote for Gore over Bush on Election Day?") as dependent variables instead of actual voting outcomes. Gerber (2004) compares the estimated effects of campaign spending (as opposed to specifically television advertising) across several prominent studies and shows how their predictions vary by more than an order of magnitude depending on the estimation technique.

The studies by Huber and Arceneaux (2007) and Gerber et al. (2011) permit some comparison because both use GRPs of television advertising and focus on recent elections. Restricting attention to nonbattleground states, Huber and Arceneaux (2007) use cross-sectional survey data and find that increasing Bush's advertising by 1,000 GRPs increases the probability a voter supports Bush by 1.7%, whereas increasing Gore's advertising by the same amount increases his support by 3.8%. Using a regression and actual voting data, the authors estimate that 1,000 GRPs increase Bush's proportion of the two-party vote by 4.0%.[22] Gerber et al. (2011) couple a television field experiment with telephone surveys to gather information on candidate preferences and voting intentions; they found that 1,000 GRPs increase respondents' intentions to vote for a gubernatorial candidate by about 5%.

To make our elasticity estimates comparable with these results, we calculate the percentage change in votes for a candidate given an increase of 1,000 GRPs. We exclude from this calculation those counties that received zero GRPs by the target candidate. An increase of 1,000 GRPs yields on average 1.5% more votes for the Republican candidate and 1.7% more votes for the Democratic candidate. An extra 1,000 GRPs represents a nontrivial amount of advertising dollars (e.g., about $300,000 for a prime daypart in Dallas in 2004), but the predicted response could have a significant effect on a state's voting outcome. Thus our estimates are roughly consistent with—although distinctly lower than—two studies that exploit (quasi) experimental variation to isolate the causal effect of advertising. This comparison comes with some caveats, because each study differs on some dimension. Our approach is distinct in our use of instrumental variables to measure advertising effectiveness with actual voting data. Whether the effectiveness of advertising is the same across elections for different offices is also unclear.

[22] For details, please refer to pages 969 and 975 of Huber and Arceneaux (2007).

4.3. Zero-Advertising Counterfactuals

In this section, we examine the power of advertising to influence overall election outcomes. We consider how the electoral votes would have changed if all advertising were set to zero, holding all other factors fixed. Note that we do not view this exercise as an actual prediction of the election outcome if advertising were banned, because many other variables would endogenously change. For example, without television advertising, candidates might substitute other forms of campaign activities to communicate their stances on policy issues to voters (e.g., canvassing, get-out-the-vote drives). Candidates might even be forced to change their policy stances themselves. Although the counterfactual is unable to address these issues, the exercise still gives us a rough idea of voters' preferences without the influence of advertising.

Table 7 shows how the electoral votes would have changed under this zero-advertising scenario using the estimates from column (7) in Table 5. The column "Switched states" in Table 7 lists the states that switched to the candidate listed in that row. In 2004, removing advertising gives Bush one extra state (Wisconsin), thereby increasing his margin of victory

Table 7 Zero Advertising Counterfactual

Election		Electoral votes		Switched states
Year	Candidate	Observed	Zero ad	(Electoral votes)
2000	Bush	271	249	OR (7)
2000	Gore	266	288	FL (25), NH (4)
2004	Bush	286	296	WI (10)
2004	Kerry	252	242	—

Turnout	% of population		
Year	Observed	Zero ad	% change
2000	0.645	0.626	−2.93
2004	0.706	0.688	−2.53

Notes. Electoral College results from setting advertising to zero in both elections, using estimates from column (7) of Table 5. The "Observed" column presents the actual number of electoral votes each candidate received, and the "Zero ad" column presents the predicted number after setting advertising to zero everywhere. The rightmost column indicates which states switched hands. For example, in the 2000 election, Bush would have lost Florida and New Hampshire to Gore but gained Oregon. The bottom panel provides the change in the percentage of voters who turn out for the election. Note that 538 electoral votes were at stake in each election. However, the results for 2000 sum to 537 votes because one elector in Washington, DC abstained from voting in the Electoral College.

from 34 electoral votes to 54. However, under zero advertising in 2000, Bush would have won Oregon but lost Florida and New Hampshire to Gore. The net loss of 22 electoral votes would have been enough to tip the election in Gore's favor. In fact, because both of the states that switched to Gore were decisive, eliminating the advertising in either of these states would have changed the election outcome. The bottom panel of Table 7 presents in the counterfactual the percentage of voters who turn out. We find that removing the advertising decreases voter turnout by about 2.9% (3.06 million voters) and 2.5% (3.08 million voters) in 2000 and 2004, respectively.

Although advertising shifted three states' preferred candidate in 2000, these outcomes combine changes in voter turnout and differences in candidates' relative vote shares. Assessing the relative importance of advertising's effects on voter turnout and persuasion is difficult because both effects operate simultaneously given the functional form of the logit demand model. To isolate the persuasive effects of advertising, we also conduct a zero-advertising counterfactual that holds turnout fixed at observed levels.[23] Under this scenario, Gore retains Oregon and wins Florida and New Hampshire in 2000, yielding a net gain of 29 electoral votes. Thus our model and estimates imply advertising's persuasive effects drive up Gore's electoral votes, but turnout effects run in favor of Bush by flipping Oregon.

Our results appear to support the dual effect of advertising on voters' decisions. However, squaring them with work in political science is challenging because the literature is mixed, and the same model rarely considers both decisions. One series of studies find support for a positive effect of advertising on turnout (Freedman and Goldstein 1999, Freedman et al. 2004, Goldstein and Freedman 2002, Hillygus 2005), whereas another group of papers finds a null effect of advertising on turnout (Ashworth and Clinton 2007, Krasno and Green 2008), and yet other work points to a negative effect (Ansolabehere et al. 1999). Similarly, Huber and Arceneaux (2007) find that advertising exhibits a strong persuasive effect on voters in the 2000 presidential election, but Gerber et al. (2011) find the persuasive effects of advertising are fleeting in the 2006 gubernatorial election. Assessing the relevant literature is particularly complex because of the varying nature of the methods employed: some papers rely on individual survey data with indirect outcome variables and others use observational data on voting and turnout at the aggregate level. Another complicating factor is that mean advertising effects likely differ across elections for different political offices depending on the self-selected sample of voters who participate.

[23] To hold turnout fixed, within each county, we remove from the choice set the outside option of not voting and then recompute shares, assuming voters choose among the three inside options. Then we scale the resulting vote shares, according to the original level of turnout in the county.

5. Conclusions

This paper documents a robust positive effect of advertising in the case of general elections for the U.S. president. Our analysis indicates that instrumental variables, fixed effects, and observable controls impact the estimate of the advertising coefficient. Because the election setting minimizes any dynamic concerns, this estimation strategy allows us to cleanly identify positive advertising effects, whereas causal studies for branded goods often find no effect. Overall, our findings illustrate that advertising is capable of shifting the electoral votes of multiple states and consequently the outcome of an election.

Our analysis comes with several caveats that might be interesting to pursue as future research. First, we aggregate all of a candidate's advertising into a single variable and do not separately consider the effects of positive or negative advertisements. We made this choice because numerous papers in political science specifically examine positive versus negative advertising (e.g., Ansolabehere et al. 1994), and to analyze them from a causal perspective using nonexperimental data requires an instrument that shifts the relative balance of positive and negative advertisements across DMAs. Second, we do not model voters' expectations about the potential outcome of the election and how forming such expectations could ultimately alter their voting decision. Expectations data have well-known challenges and would require additional structure that we felt would impose parametric restrictions that were too stringent while trying to focus on the causal relationship in the data. Third, we assume the effectiveness of advertising is fixed over time. If the effectiveness of advertising could vary over time and be observed, candidates could use variation as a basis for scheduling advertising during the campaign, generating an additional source of endogeneity.

We hope our illustration of the value of fixed effects and instruments motivated by advertiser objective functions influences future empirical studies of these political applications as well as other questions in advertising more broadly.

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