

*Quarterly Journal of Political Science*, 2006, x: 01–15

## Does Advertising Exposure Affect Turnout?\*

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### ABSTRACT

We identify an exogenous source of variation in exposure to campaign advertising in the 2000 presidential election, based on residence in battleground states. If exposure to campaign advertising makes a potential voter significantly more likely to vote, then we should see significantly greater turnout in battleground states. We do not. This result is robust to several specifications and evident in a natural experiment consisting of New Jersey residents. Conditional on existing campaign targeting strategies, campaigns do not affect the turnout decisions of the voters we study.

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Turnout in the United States is higher in presidential elections than in non-presidential elections. To explain this variation, the theory of surge and decline posits that peripheral voters participate only when stimulated, and that this occurs more frequently in presidential elections than midterm congressional elections (A. Campbell 1966, J. Campbell 1986, 1991). There are many possible sources of possible stimulation – for example, greater media coverage or perceptions of major consequences in presidential elections might lead more citizens to vote. An important possibility is that actions taken by candidates themselves are the stimulation for the surge.<sup>1</sup> Evaluating this candidate-based explanation is important because the answer addresses the extent to which candidates can affect election outcomes by changing the size (and presumably the composition) of the electorate.

One of the most important mechanisms that candidates use to appeal to voters (see, for example, Johnston, Hagen, and Jamieson 2004, West 2005) is advertising. Despite the resources candidates spend on advertising, a consensus has yet to emerge regarding its impact; some argue that advertising mobilizes voters (e.g. Freedman, Franz, and

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\* We thank Larry Bartels, Markus Prior, an anonymous referee, and the editors for helpful comments.

<sup>1</sup> Candidate behavior certainly affects and generates media coverage, but we restrict our attention to the first-order effect of candidate activity.

Goldstein 2004, Goldstein and Freedman 2002) while others argue that the impact is ambiguous or even negative (e.g., Ansolabehere, Iyengar, and Simon 1999, Lau et al. 1999). We reexamine this question in the context of the 2000 presidential campaign.

Most studies of the impact of advertising on voter participation use regression-type methods to estimate models in which turnout is a function of advertising and control variables. Although it is tempting to interpret the resulting estimates as the causal effect of campaigns on turnout, this conclusion is premature; campaigns could target people likely to vote in the absence of campaign activity. In other words, such regression-based work is susceptible to the objection that the results are spurious, contaminated by campaigns targeting voters whose behavior is independent of the campaign.<sup>2</sup>

For example, campaigns might target voters who are relatively responsive to new information. But voters responsive to new information may also have a greater personal demand for information and might seek out information on their own (by reading about the race in the newspaper or watching coverage of the race) even were they not targeted. The voters who have the most incentive to gather information and update their beliefs independent of campaign activity may be precisely the voters whom campaigns have the greatest incentive to target.<sup>3</sup> Alternatively, the peripheral voters responsible for the “surge” may vote more frequently in presidential election years because they are more willing to believe that the stakes are higher even without advertising. In either case, if campaigns target voters likely to change their vote intention independent of candidate activity, simply comparing the voting behavior of citizens exposed to advertisements to those who are not will overstate the impact of campaign advertising.

Another possible source of endogeneity is correlation between turnout and the recall of advertising (see, for example, Ansolabehere, Iyengar, and Simon 1999 and Vavreck 2005). If the same characteristics that make respondent indicates that they are likely to vote also result in respondents indicating that they recall seeing an advertisement, simple comparisons of turnout for exposed and non-exposed citizens based on these measures will overstate the true impact of advertising.

Given these possibilities, a developing literature analyzes campaign spending and advertising while addressing potential endogeneity issues (e.g., Gerber 1998, Gerber and Green 2000, Green and Krasno 1988, Hillygus 2005 and Lau and Pomper 2002). We contribute to this important project by using the exogenous variation in campaign exposure based on state of residence to identify the individual-level effects of advertising. Our identification strategy relies on the fact that, although battleground states are more likely to receive presidential campaign advertisements, and therefore have higher reports of exposure, there should be no direct effect of residence on turnout holding other covariates fixed. Controlling for individual-level characteristics such as political interest,

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<sup>2</sup> Note that work utilizing experimental methods (e.g., Ansolabehere and Iyengar 1994, Clinton and Lapinski 2005) avoids this criticism but faces external validity concerns.

<sup>3</sup> The front-loading of presidential primaries is consistent with this interpretation. The largest return to candidates is in the first primaries because voters are very uncertain about their positions and therefore susceptible to advertising (see, for example, Bartels 1988 and Ridout 2004). However, because citizens also have the largest return from conducting independent research early in the campaign, they are also more likely to change their beliefs independent of campaign activity.

strength of partisanship and aspects of the political environment such as the existence of prominent statewide elections, South Dakotans (for example) should not be more likely to vote than North Dakotans. Geographic location, by itself, should have no independent effect on turnout.

Our main empirical result is easy to state: although (self-reported) advertising exposure is nearly 20% greater for battleground-state residents, post-campaign turnout reports are essentially identical for battleground and non-battleground state residents. This comparison suggests that exposure to the campaign has essentially no effect on turnout. This result is robust to controlling for covariates in a variety of statistical models.

## DATA

We analyze the results of a survey administered by Knowledge Networks. The survey was administered to 4,000 randomly selected panelists over the age of 18 from the Knowledge Networks panel on 27 October 2000. Respondents could complete the survey until 7 November 2000 and 68% of the respondents completed the survey.

Our sample respondents closely approximate a national RDD sample because the panelists we use were randomly selected using list-assisted random digit dialing (RDD) sampling techniques on a quarterly updated sample frame of the United States telephone population living within the Microsoft Web TV network (87% of the United States population). When the survey was conducted, all Knowledge Networks Panelists were given a Microsoft WebTV and a free Internet connection in exchange for taking surveys. No evidence of panel bias was evident (Clinton 2001).

Knowledge Networks administered a survey collecting information relevant for assessing political participation almost immediately after a respondent joined the panel. Because the survey was administered prior to the start of the campaign for most respondents, it provides a baseline to compare responses from later surveys can be compared.

We measure turnout intention using two survey questions. Initial vote intention is measured using responses to the question: “What are your chances of voting in the election for President in November? Definitely WILL vote (57%); Probably WILL vote (14%); Probably WILL NOT vote (11%); Definitely WILL NOT vote (12%); Not Sure (6%).” The post-campaign intention to vote was collected just before the election using responses to: “Do you plan to vote for the President next Tuesday, November 7th? Yes, definitely will vote (63%); Probably will vote (8%); No, Probably won’t vote (8%); Definitely will not vote (15%); Not sure (4%).” We recode responses into an indicator variable equal to 1 if a respondent either definitely or probably intends to vote. Approximately 72% intend to vote according to the recoded indicator in both instances, although 12% change their intention.

We are interested in the relationship between campaign events and turnout intention. Given the extensive literature focused on the impact of campaign advertisements and the amount of campaign resources devoted to their production and distribution, we focus on the impact of advertising. (In our data all measures of contact are highly correlated.) We measure respondent exposure to advertising using: “Have you seen or heard any paid

political advertisements for the presidential candidates on television and the radio? Yes, many political ads (40%); Some here and there (35%); Hardly any political ads (11%); None at all (9%); Not sure (4%).” Responses are recoded to indicate whether the respondent reported seeing many political advertisements or not.<sup>4</sup> Although we acknowledge the limitations of this measure – well documented by Ansolabehere, Iyengar, and Simon 1999; Johnston, Hagen, and Jamieson 2004 and Goldstein and Ridout 2004 – we lack the data required to calculate alternative measures. Theoretical results on instrumental variables estimators with mismeasured binary endogenous variables suggest, however, that this kind of error does not drive our substantive conclusions.

We control for individual-level heterogeneity based on: political interest, gender, strength of partisanship, an indicator for Black respondents, an indicator for Hispanic respondents, and age. We also use indicators for union members and respondents who attend church “once or twice a month” or more to control for possible mobilization efforts undertaken by unions and churches in the 2000 election.<sup>5</sup> We also use an indicator variable for voters who live in one of the 20 states identified by CNN as a battleground state for the 2000 presidential election: Washington, Oregon, Nevada, Arizona, New Mexico, Iowa, Missouri, Arkansas, Louisiana, Wisconsin, Illinois, Tennessee, Michigan, Ohio, West Virginia, Florida, Delaware, Pennsylvania, New Hampshire, Maine.<sup>6</sup> Approximately 39% of our sample resides in a battleground state. Finally, we control for the political context by indicating whether senatorial and gubernatorial elections were held in the state.

## IDENTIFICATION STRATEGY

The simplest way to estimate campaign effects is to compare the behavior of voters who are exposed to the campaign to those who are not. In our sample, turnout was about 65% among respondents who did not recall seeing any advertisements, and was 82% among respondents who recall seeing advertisements. It is tempting to conclude that seeing advertisements raises the probability of voting by about 17 percentage points.

Unfortunately, this simple procedure is biased if some of the variation in exposure to campaign advertising is due to factors that also affect turnout. For example, voters who are more interested in the election might seek out campaign information that others avoid (see, for example, work by Prior 2005). If more interested voters are also more likely to vote, the difference in turnout rates incorrectly attributes the difference to

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<sup>4</sup> This coding decision was made for expositional purposes and permits the use of readily available methods of accounting for endogeneity. Keeping the categories distinct requires using more complicated IV estimators due to the ordered nature of the potentially endogenous exposure variable.

<sup>5</sup> Table 3 summarizes the descriptive sample statistics by battleground residence.

<sup>6</sup> CNN indicates that the list is based on “the states most closely watched and highly contested in the final weeks of the campaign.” We also experimented with a complete set of state dummies as instruments, to let the data decide which states were battleground states. This led to weak instrument problems, so we focus on the dichotomous instrument in the text.

advertising. Similarly, if the propensity to recall advertisements is greater for voters than for non-voters, the estimates are inflated.

To avoid this bias we need variation in campaign exposure that is unconfounded with voters’ participation propensities. In this paper we exploit the variation associated with differences in the state of residence. In presidential elections, campaigns vary their treatment of different states to take advantage of the Electoral College system (see, for example, Strömberg 2002). So two otherwise identical voters are exposed to different levels of campaign activity because one lives in Pennsylvania (a battleground state) and the other in New York (a nationally uncompetitive state). Comparing voters living in battleground and non-battleground states therefore offers the potential to reveal the true campaign effect (Ansolabehere, Snowberg, and Snyder 2006 use a similar identification strategy to examine the impact of news on the incumbency advantage).

These considerations suggest a simple instrumental variables strategy: use residence in a battleground state as an instrument for campaign exposure. Although battleground states are more likely to receive campaign advertisements, and therefore higher reports of exposure, we assume that there is no independent causal effect of residence on turnout. Holding individual-level characteristics and electoral contexts fixed, whether a respondent lives a few miles north or south of the New York and Pennsylvania border (for example) should not affect the probability of voting.

Our estimator has a simple intuition. To highlight this intuition we first develop the estimator without control variables. If battleground residence is independent of the propensity to vote, then we can estimate the causal effect of battleground residence on turnout by the simple difference:

$$\Pr(\text{vote} = 1|BG = 1) - \Pr(\text{vote} = 1|BG = 0),$$

where *vote* and *BG* are indicator variables for vote intention and residence in a battleground state. Our exclusion restriction assumes that this difference is due entirely to different levels of advertising exposure in the two types of states. Because not all battleground state citizens are exposed to the increased advertising activity, the estimated effect is diluted – the actual individual level effect of advertisements must be even larger than the effect of battleground residence. The exclusion restriction lets us factor the causal effect of battleground status on turnout into the causal effect of battleground status on advertising exposure and the causal effect of advertising exposure on turnout ( $\beta$ ):

$$\begin{aligned} & \Pr(\text{vote} = 1|BG = 1) - \Pr(\text{vote} = 1|BG = 0) \\ &= \beta(\Pr(\text{ad} = 1|BG = 1) - \Pr(\text{ad} = 1|BG = 0)), \end{aligned}$$

where *ad* is a indicator variable for advertising exposure. Expressing the causal effect of advertising on turnout as the ratio of the two identified causal effects yields:

$$\beta = \frac{\Pr(\text{vote} = 1|BG = 1) - \Pr(\text{vote} = 1|BG = 0)}{\Pr(\text{ad} = 1|BG = 1) - \Pr(\text{ad} = 1|BG = 0)}.$$

Using sample averages on the right-hand side gives the *Wald estimator*, the simplest case of two-stage least squares (2SLS) and the starting point for our analysis. (See Angrist, Imbens, and Rubin 1996 for a more formal derivation in a potential outcomes framework.)

This derivation assumes that the difference in advertising exposure is the sole mechanism by which battleground status affects turnout. This assumption could fail in several different ways. The most obvious is that residents of battleground and non-battleground states might differ in ways known to predict turnout. Because we extensively address this possibility in a subsequent section, we initially focus on alternative causal paths linking residence and turnout even when covariates are held fixed. Two such causal paths seem particularly salient. First, other types of campaign activity, such as canvassing, are greater in battleground states than in non-battleground states. Second, residents of battleground states might perceive a greater probability that they are pivotal in the election. As emphasized by Bartels 1991, such failures of the exclusion restriction lead to biased estimates.

These two possible biases work *against* our conclusions. Proposition 2 of Angrist, Imbens, and Rubin 1996 says that, as long as the impact of advertising and alternative causal paths such as responsiveness to the perceived pivot probability of the state and omitted campaign channels (e.g., canvassing) are in the same direction, the estimates of advertising’s effect will be biased *away* from zero. Intuitively, the Wald estimator will overstate the true effect because it assumes that the only difference in turnout is due to differing levels of advertising in battleground states. If other factors are also present in battleground states, we attribute too much of the effect to advertising. As we find an effect of advertising that is close to zero, these biases cannot be the cause of the findings.

## RESULTS

Our main empirical finding is readily apparent in Table 1: residents of battleground states are no more likely to vote than residents of other states. Even if we attribute all of the possible differences in campaign activity and news coverage between battleground and non-battleground states to advertising – a move that surely overstates the true impact of advertising – the effect is essentially 0.<sup>7</sup> And this is in spite of a considerable difference in self-reported exposure to advertising (21.2%). This represents *prima-facie* evidence against the claim that advertising increases turnout. (The next section addresses concerns that covariate differences explain this pattern, but the substantive conclusion of Table 1 persists.)

To turn the percentages reported in Table 1 into estimates with valid standard errors, we use 2SLS to estimate the probability of respondent  $i$  reporting an intention to vote using

$$vote_i = \alpha + \beta ad_i + \epsilon_i \tag{1}$$

<sup>7</sup> All of our results are essentially identical if we use a more comprehensive measure of campaign exposure that accounts for both advertising and personal contact by a campaign. This is not surprising, because only 8% of the sample reports personal contact but not advertising exposure.

**Table 1.** Data for national Wald estimates

	Battleground	Non-battleground	Difference
Turnout	71.7	71.7	0.00
Ad exposure	53.4	32.2	21.2
Sample size	1,050	1,660	

**Table 2.** National sample Wald estimator

	OLS	Wald estimates	Hausman test <i>p</i> -Value	<i>N</i>
Full sample	0.167 (0.017)	0.007 (0.084)	0.048	2697
<i>vote</i> <sub>1</sub> = 0	0.128 (0.035)	0.005 (0.263)	0.633	772
<i>vote</i> <sub>1</sub> = 1	0.044 (0.012)	−0.055 (0.053)	0.045	1921

where *vote* is the post-campaign vote intention and *ad* is the indicator of recalled advertising exposure. 2SLS estimation of Equation 1 is numerically equivalent to the ratio form of the Wald estimator.

The estimates are presented in Table 2, along with the results of OLS estimation of Equation 1. We present the results for the full sample and for two subsets of respondents: those who initially planned to vote, and those who did not.

The OLS results in Table 2 show substantial correlation between exposure and turnout. In the full sample – and ignoring the potential endogeneity of advertising and initial vote intentions – the effect of advertising sufficiently memorable so as to be recalled by the respondent is 0.167 on a 0–1 scale (with a White standard error of 0.017). Accounting for the endogeneity of advertising exposure dramatically changes the point estimates. In the full sample the effect is cut by a factor of 24, to just 0.007. The instrument is strong, with a first-stage *F* statistic of 126 for the full sample.

We replicate Hillygus’s 2005 finding that the effect of advertising differs for respondents who initially plan to vote and those who do not. Table 2 reveals a 0.084 (standard error of 0.037) larger impact on a 0–1 scale of advertising among initial nonvoters relative to initial voters (as difference that is statistically significant at a *p*-value of 0.02). Moreover, as is the case in the full sample, accounting for the potential endogeneity of advertising in the initial nonvoter and voter samples reveals a very different estimate of advertising’s magnitude: 0.005 for initial nonvoters and −0.055 for initial voters.<sup>8</sup>

The last column of Table 2 presents statistical tests of the endogeneity of advertising exposure. We reject the null of no endogeneity at the 5% level in both the full sample and

<sup>8</sup> Evaluating the magnitude of the changes resulting from accounting for the endogeneity of advertising must be tempered by the fact that the estimates are imprecise. In the next section we increase the precision of our estimates by controlling for covariates known to predict turnout.

Table 3. National sample characteristics

Variable	Non-BG	BG	Mean difference	
			<i>p</i> -value	Values
Pre-campaign vote intention	0.707	0.723	0.364	{0, 1}
Post-campaign vote intention	0.717	0.717	0.978	{0, 1}
Advertising exposure indicator	0.322	0.534	0.000	{0, 1}
Political interest	0.575	0.578	0.832	{0, 0.33, 0.66, 1}
Strength of party identification	0.503	0.513	0.530	{0, 0.33, 0.66, 1}
Age	45.1	44.8	0.658	(18, 99)
Female indicator	0.508	0.492	0.441	{0, 1}
Hispanic indicator	0.106	0.061	0.000	{0, 1}
Black indicator	0.077	0.048	0.002	{0, 1}
Union indicator	0.125	0.109	0.197	{0, 1}
Church indicator	0.396	0.378	0.365	{0, 1}
Senate race in state	0.814	0.743	0.000	{0, 1}
Gubernatorial race in state	0.091	0.096	0.650	{0, 1}
Sample size	1660	1050		

in the sample of initial voters. Although the change in magnitude for the point estimates for initial nonvoters is similar, the estimates are too imprecise for us to reject the null of no endogeneity.

ROBUSTNESS CHECKS

We have argued that the apparent impact of advertising may be an artifact of endogenous advertising strategies. We now examine the robustness of these results by introducing individual-level covariates and assuming several parametric forms for the relationship with turnout. None of these investigations changes the substantive conclusions evident in Table 1.

Are sample differences responsible for the results?

The Wald estimator attributes all of the difference in turnout between battleground and non-battleground states to differences in advertising exposure. Although we argue above that alternative causal paths linking battleground status and turnout cannot account for our results, the exclusion restriction also fails if residents of battleground and non-battleground states differ in ways known to predict turnout.

Table 3 shows that the covariates are well balanced between the two subsamples. Only the indicators for Black, Hispanic, and a senate race in the state differ significantly. Because Blacks and Hispanics both constitute larger parts of the sample in non-battleground states, and because they are also less likely to participate in elections

(e.g. Abramson and Claggett 1984 and Verba, Schlozman, and Brady 1995), failing to adjust for these two covariates should *increase* the estimated effect of advertising exposure. As we expect senate races to attract voters’ interest, the fact that more individuals in non-battleground states were also exposed to a senate election is a more plausible threat to the Wald estimator.

Although comparing mean differences suggests that covariate imbalance is not problematic, estimating models controlling for covariates increases the precision of estimates of interest and ensures the robustness of our results. Ideally, we would proceed non-parametrically and compute the Wald estimator for each possible vector of covariates. Unfortunately, this approach is infeasible due to the lack of data. As an alternative we utilize several functional forms. The impact of these assumptions is likely inconsequential, as the substantive conclusions do not change much when we vary the functional form.

We employ two models to control for covariates. First, we use 2SLS to estimate the linear probability model:

$$vote_i = \alpha + \beta ad_i + \gamma' \mathbf{x}_i + \epsilon_i,$$

where  $\mathbf{x}_i$  is the vector of covariates reported in Table 3 for individual  $i$  (including initial vote intention for the full sample estimators). Second, we consider the system:

$$\begin{aligned} vote_i = 1 & \text{ if } \alpha_1 + \beta ad_i + \gamma_1' \mathbf{x}_i + \epsilon_i \geq 0 \\ ad_i = 1 & \text{ if } \alpha_2 + \delta BG_i + \gamma_2' \mathbf{x}_i + v_i \geq 0, \end{aligned}$$

where  $(\epsilon, v) \sim \mathcal{N}(0, \Sigma)$  and

$$\Sigma = \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix}.$$

This bivariate probit model is estimated using maximum likelihood.<sup>9</sup>

Table 4 presents the effects of advertising exposure in models with covariates. (The (online) appendix presents complete tables of coefficient estimates.) All reported estimators (except for the Wald estimator in column 3) control for the variables listed in Table 3. In each case we present results both for the full sample and separately for the subsamples of initial voters and nonvoters.<sup>10</sup>

The first two columns present the average effect of advertising exposure on turnout intention treating advertising exposure as exogenous using OLS (with heteroskedasticity-robust standard errors) and probit. The substantive results are similar across specifications. For initial voters the impact of advertising is essentially zero, but for initial non-voters there appears to be a modest and statistically significant positive impact.

The last two columns present the results from the IV estimates with covariates. Accounting for the endogeneity of advertising reveals no positive relationship between turnout and advertising. The results reported in Table 1 are robust to controlling for

<sup>9</sup> Greene 2003 discusses the bivariate probit model for a recursive system in section 21.6.6. Conveniently enough, the endogenous regressor has no impact on the form of the log-likelihood.

<sup>10</sup> The sample size is slightly larger for the Wald estimates because of some covariates are missing for some respondents.

**Table 4.** Estimates of the effect of advertising exposure on turnout

	OLS	Probit	Wald estimates	2SLS	Bivariate probit
Full sample	0.033 (0.013)	0.024 (0.010)	0.007 (0.084)	-0.094 (0.059)	-0.059 (0.044) (-0.142, 0.033)
<i>N</i>	2346	2346	2697	2346	2346
<i>vote</i> <sub>1</sub> = 0	0.101 (0.039)	0.076 (0.030)	0.005 (0.263)	-0.225(0.273)	-0.145 (0.166) (-0.318, 0.215)
<i>N</i>	631	631	772	631	631
<i>vote</i> <sub>1</sub> = 1	0.008 (0.012)	0.005 (0.010)	-0.055 (0.053)	-0.074 (0.052)	-0.062 (0.047) (-0.180, 0.013)
<i>N</i>	1715	1715	1921	1715	1715

covariates using several parametric forms for the relationship. (For ease of comparability the table also includes the Wald estimates from Table 2.)

To calculate the predicted impact of advertising on vote choice for the bivariate probit case (column 5 in Table 4), we use the average treatment effect:

$$ATE = \frac{1}{n} \sum_{i=1}^n \Phi(\alpha_1 + \beta + \gamma_1' \mathbf{x}_i) - \Phi(\alpha_1 + \gamma_1' \mathbf{x}_i).$$

The ATE is the mean difference of the predicted probability for each respondent *i* reporting an intention to vote given the set of covariates and assuming *ad*<sub>*i*</sub> = 1 and the predicted probability for each respondent *i* reporting an intention to vote given the set of covariates and assuming *ad*<sub>*i*</sub> = 0. For statistical inference we bootstrap this average. A single bootstrap iteration of the average treatment effect requires: sampling with replacement from the data matrix, estimating the model on the data sample, and calculating the average treatment effect. Because each sample yields an estimate of the average treatment effect, repeating the process generates the bootstrap standard error and bootstrap bias corrected confidence interval for the average treatment effect across the samples reported in Table 4.

Table 4 reveals that adding control variables fails to change the substantive conclusions regarding the impact of advertising exposure evident in the Wald estimates. In no case is the estimated effect statistically significantly different than zero. For initial voters the estimated effects are remarkably stable across models, and the 95% confidence interval rules out substantively important positive effects in every case. In contrast to the OLS and Probit estimates, the IV estimates allow for substantively large negative effects. For initial non-voters the estimates are imprecise and confidence intervals permit any reasonable value.

A Hausman test rejects exogeneity in the linear probability model at the 3% level for the full sample and at the 11% level for the sample of initial voters. The *p*-value for Hausman test for the sample of initial nonvoters is only slightly higher at 0.212. For the bivariate probit and the full sample our estimate of  $\rho$  is different than zero at the 4% level, so exogeneity is also rejected in this case. Exogeneity is rejected for the initial nonvoter and voter samples at 14% and 20% respectively.

Are we missing countervailing effects?

Another way our estimates of the previous section could be misleading is if advertising has large effects on turnout in both directions: advertisements mobilize some voters at the same time they demobilize others. Such countervailing effects are a serious possibility given that 12% of the respondents change their intention during the campaign. If these changes are concentrated in battleground states then they could indicate a campaign effect (whose sign differs between the two offsetting voter groups).

This is not what happened. Although 12% of respondents in battleground states changed their intention, about 11% of respondents in non-battleground states also changed their intention. The  $t$ -statistic for this difference is only  $-0.760$ . These results are very similar to those for final vote intention. Controlling for covariates does not affect these results.

Are the results due to measurement error?

Another possible objection is that the results are the consequence of using variables that mismeasure advertising exposure because our measure is based on the respondent's recall of advertising exposure. It is well known that recall measures can be noisy (see, for example, Ansolabehere, Iyengar, and Simon 1999, Niemi, Katz, and Newman 1980, Price and Zaller 1993, and Vavreck 2005). In our application the measurement error is non-classical – respondents not intending to vote cannot underreport their intended voting behavior (as they are already indicating that they do not intend to vote) and respondents intending to vote cannot overreport their intended voting behavior. When measurement error takes this form, the OLS estimates are biased toward zero, but the IV estimates are biased *away* from zero (Kane, Rouse, and Staiger 1999). So, again, this bias works *against* our findings and cannot account for the results we report in Tables 2 and 4.<sup>11</sup>

#### The New Jersey Sample

As a final robustness check we investigate the relationship between advertising exposure and turnout for New Jersey residents. New Jersey is worthy of individual attention because the state is divided into two non-native media markets. The northeastern half lies in the New York media market and the southwestern half receives broadcasts from Philadelphia (*Table and Cable Television Factbook* 2000). Because the Philadelphia market is in a battleground state and the New York market is not, the natural division of New Jersey residents according to their media market offers the opportunity to investigate the relationship among residents from the same state. Because all of these voters have the same importance to the electoral college outcome, face-to-face mobilization efforts should not vary by media market. To the extent that the results for the New Jersey

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<sup>11</sup> Another form of measurement error in advertising exposure comes from our coding it as a dichotomous variable. Again, this leads to bias away from zero (Angrist and Imbens 1995). The bottom line is that measurement error probably *does* affect our point estimates, but it certainly *does not* drive the substantive conclusions.

**Table 5.** Data for the New Jersey Wald estimator

	PA market	NY market
Turnout	74.7	75.8
Ad exposure	66.7	47.6
Sample size	111	328

sample are similar to those of the national sample, our confidence that the results are about advertising are increased.

Although the level 5 of self-reported intended turnout among residents in the Philadelphia and New York media market are virtually identical (75% and 76% respectively), residents of the Philadelphia media market are 20% more likely to report being exposed to many political advertisements. Because the observed difference is nearly identical to the difference between battleground and non-battleground state residents reported in Table 3, our confidence in our identification assumption is increased. Table 5 presents the marginal percentages.

The basic results from the Wald estimator persist when we introduce respondent-level controls and account for advertising endogeneity using a 2SLS estimator (see the (online) appendix). There appears to be no causal relationship between advertising and turnout.

#### CAVEATS AND CONCLUSIONS

If exposure to campaign advertising makes an eligible voter significantly more likely to vote, then we should see significantly greater turnout in battleground states. We do not. This result is robust to different specifications for control variables and to interesting subsamples. These results suggest that the causal effect of advertising on turnout is very small, at least in our sample.<sup>12</sup> This qualification is important. Any data analysis can be informative at most about the sample that is studied. There are consequently several ways that our results might differ from those of other samples.

The first issue concerns the representativeness of the panel we use. Although Knowledge Networks used probability sampling to choose respondents and to choose the subsample asked the political questions we use, there is nonresponse at both stages. Thus our sample is not fully representative of the population at large and it is possible that people who did not answer the questions were precisely the people for whom campaigns are effective. So it is possible that there are important campaign effects outside of our sample. Without data on these individuals we cannot know.

Suggestive evidence that this is not the case is provided by inspecting the average turnout in battleground and non-battleground states according to the turnout statistics

<sup>12</sup> Independently and simultaneously, Huber and Arceneaux 2006 use a similar identification strategy to the 2000 Annenberg National Election Study and find substantially identical results.

for the 2000 election as reported by the U.S. Census Bureau. Using the ratio of voters over the total state population as a measure of turnout reveals a turnout of 57.83% for battleground states or 57.44% for non-battleground states ( $p$ -value of 0.58). Using the total number of citizens in the state as the denominator yields an average turnout of 60.82% and 60.45% respectively ( $p$ -value of 0.58).

The second issue is a more subtle econometric point. Our instrumental variables estimator estimates the causal effect of advertising exposure only for those respondents whose exposure status would have been different had they resided in a different kind of state (Angrist, Imbens and Rubin 1996). A zero causal effect for this subpopulation is consistent with a nonzero effect for people who would see advertisements wherever they lived. Thus, our estimates are conditional on the strategies that parties use to select which voters to target. Different campaign advertising strategies might well lead to important campaign effects.<sup>13</sup> Still, this local average treatment effect is the relevant effect for some policy questions. For example, a candidate who must decide to allocate his or her remaining budget between two states cares about precisely this subpopulation.

These caveats aside, the results of our study clearly demonstrate that simply asserting that advertising is exogenous is insufficient, and analyses that fail to examine this possibility are potentially severely biased. Using an exclusion restriction that makes use of the fact that battleground state residents are significantly more likely to be exposed to advertising but state residence likely has no independent impact on turnout, we show that advertising does not result in increased turnout. Consequently, understanding the “surge” associated with presidential elections. As well as the purpose and implications of the massive amount of advertising in contemporary election contests, likely requires looking elsewhere.

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<sup>13</sup> This observation may help to reconcile our results with the results of the observational study of Lassen 2005 and the experimental study of Horiuchi, Imai, and Taniguchi 2005, both of which find that citizens who are randomly selected to receive information about the candidates in an election are more likely to vote than are citizens who do not get the information. In our data, citizens are targeted by candidates. If candidates preferentially target citizens who are relatively unresponsive to new information, then we should expect to see smaller campaign effects in our sample than in the randomly selected samples of Lassen 2005 and Horiuchi, Imai, and Taniguchi 2005.

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