The Effect of TV Ads and Candidate Appearances on Statewide Presidential Votes, 1988–96

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Although presidential campaigns have been mythologized in literature and cinema, most theories of elections relegate them to a secondary role, presuming they have little effect on outcomes. Direct tests of campaigning's influence on electoral college votes are rare, mostly because statewide data on the allocation of resources and voters' preferences have been hard to obtain. Many studies suggest a minimal effect, but it is possible that a more significant influence might be found with better data on the key dependent and independent variables. This study uses data on presidential candidates' appearances and television advertising purchases to conduct cross-sectional and pooled time-series analyses of their influence on statewide outcomes in 1988, 1992, and 1996. The data demonstrate that, despite the conditioning influence of other factors, campaigning affected statewide preferences as well as the electoral college vote.

American presidential election campaigns have always received much notoriety both in the news media and political science. This notoriety masks considerable scholarly debate over the importance of contemporary campaigns. Studies of congressional elections have consistently shown that candidate quality and campaign spending affect the vote (Franklin 1991; Herrnson 1989, 1995; Jacobson 1983, 1989, 1990; Jacobson and Kernell 1981), but studies of presidential races offer mixed evidence with respect to campaign effects. More often than not, explanations of presidential elections are sought in the distribution of party identification or in variations of the macroeconomy (Fair 1996; Lewis-Beck and Rice 1992; Rosenstone 1983; Tufte 1978). Moreover, many of these studies make a strong case that campaigns do little more than activate latent candidate preferences. For some scholars, presidential campaigns are of symbolic but exaggerated importance.

Despite strong arguments for minimal effects, the issue of presidential campaign influence remains an open question. Indeed, some of the most recent research suggests that campaigns—particularly television advertising and candidate events—do affect voters' preferences (Ansolabehere and Iyengar 1996; Holbrook 1994, 1996). At its core, however, the persistence of the debate is due to a paucity of data and disagreement about existing theories. Political science has yet to capture the data necessary to conduct detailed analyses of campaign activities. Most conspicuously, data on the effect of the candidates' television advertising and personal appearances are sparse in the election literature. Scholars studying campaigns also disagree on theoretical issues, such as what constitutes "campaigning" and "campaign effects." The literature's use of generalities like these has helped fuel controversy over the significance of presidential campaigns.

The resolution of this debate is important for our understanding of representative democracy in America. Campaigns are typically thought to be significant for both candidates and voters. From the perspective of candidates, campaigns are used to articulate issue and policy proposals that (they hope) will attract groups of voters. From the perspective of voters, campaigns convey information that can be used to evaluate prospective choices. More generally, campaigns provide the key link between public officials and voters; candidates make promises during campaigns that allow voters to hold winners, or their party, accountable in subsequent elections. In short, it is difficult to imagine a truly democratic process without campaigns. Therefore, questioning their significance is a serious challenge to the nature of democracy in America.

This study identifies the causes of the discrepancy in the literature and uses them to inform our definitions, expectations, and analyses. Its most outstanding feature is an extensive data set on statewide television advertising and candidate appearances from the 1988, 1992, and 1996 presidential elections, which is used to estimate campaign effects. Both cross-sectional and pooled time-series analyses show that campaigns influenced electorates and electoral votes, although this effect was conditioned by voter characteristics and the opposing campaigns. The findings suggest that campaigns may be critical agents of mobilization and persuasion.

A CRITICAL REVIEW OF THE LITERATURE ON CAMPAIGN EFFECTS

Scholarship on presidential campaign effects can be roughly sorted into three categories: descriptive, experimental, and survey research studies. Descriptive studies, which include the narratives of political reporters (Germond and Witcover 1985, 1989, 1993; Goldman et al. 1989, 1994; White 1961) and campaign consultants (Matalin, Carville, and Knobler 1994; Morris 1997; Rollins 1996), tend to emphasize the campaign's role in influencing the election outcome. The evidence mar-

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shaled is selective and subjective, however, and little effort is made at systematic analysis.

Experimental studies typically provide corroborative evidence for the campaign effects suggested by the descriptive works. Recent experiments have made extraordinary strides in replicating the circumstances voters face when they are exposed to actual political campaigns and in measuring the effects of different "manipulations." In particular, Ansolabehere and Iyengar (1996) and Lau and Redlawsk (1995) have conducted experiments which demonstrate that campaign information affects voters' issue preferences, candidate evaluations, and likelihood of voting. Although this work is more compelling than the descriptive studies, experimental analyses of campaign effects are not without faults. Specifically, while experiments allow researchers to isolate the influence of particular effects, critics question whether such a controlled environment is comparable to the hurly-burly of an actual campaign. In addition, it has been difficult to replicate the findings (see Finkel and Geer 1998; Geer and Lau 1998).

By far the most common and influential approach to studying campaign effects is to rely on survey data. These studies have overwhelmingly concluded that presidential campaigns have minimal effects on voters. Campbell and his colleagues (1960), for example, developed a compelling sociopsychological theory of voting based in part on the stability of voters' preferences during the Eisenhower election campaigns. Similarly, Key (1966) noted the lack of movement in response to presidential campaigns and argued that voters use macroindicators of incumbent performance when deciding for whom to vote. More recently, Bartels (1992, 1993, 1997), Finkel (1993), and Markus (1992) have revisited the question of presidential campaign effects and found little reason to question the continued validity of these early studies (see Finkel 1993 or Holbrook 1996 for a thorough review of this literature).

Even those who argue for minimal effects, however, would concede there is room for debate. More specifically, there are at least four explanations for the finding of survey research. First, it is possible that presidential campaigns produce effects but that these are off-setting. That is, the rough equality of resources imposed by the public funding requirement of the Federal Election Campaign Act (FECA), coupled with the rough equality of expertise and information, practically guarantees that the competing presidential campaigns will cancel each other out. One campaign may move voters under these conditions, but the other side will soon act to wipe out any gains (see Iyengar 1996 for a review).

Second, there are some methodological problems associated with using surveys to gauge campaign effects. For example, most studies examine the differences on some behavioral or attitudinal question between respondents who self-report high or low levels of exposure to the campaign (Iyengar and Petrocik 1998). This is fine so long as self-reports are accurate, but available evidence indicates that self-reported and actual exposure are only weakly correlated (Price and Zaller 1993). More important, this error in self-reports necessarily ameliorates campaign effects (Bartels 1993).

Another methodological issue is the endogeneity of exposure to the campaign and political attitudes. As one recent study put it:

> Those who choose to tune in to the campaign may differ systematically (in ways that matter to their vote choice) from those who do not. Respondents who recall having seen a presidential campaign advertisement in the 1992 NES survey, for instance, were more likely to intend to vote than those who did not (Wattenberg and Brians 1996). Was it exposure to advertising that prompted turnout, or was the greater interest in the campaigns among likely voters responsible for the higher levels of recall (Iyengar and Petrocik 1998, 2)?

A different concern with survey methods is the attenuated time frame. The most common source of data for presidential election campaigns is the NES survey, which generally begins in early September and runs through election day. Given the development of the "permanent campaign" (Blumenthal 1982; Franklin 1997), critical movement associated with campaigns could occur before the traditional Labor Day kick-off for the general election. This movement would be missed by the NES survey and absent from any estimate of campaign effects relying on these data.

Third, minimal effects may be a function of our preoccupation with changes in the presidential vote choice. While this is an appealing barometer for campaign influence, campaigns could affect the electorate in many other ways. Recent scholarship offers evidence that presidential campaigns mobilize turnout (Ansolabehere and Iyengar 1996), alter issue preferences and priorities (Alvarez 1997; Petrocik 1997; Wattenberg and Brians 1996), change perceptions of candidates (Petrocik 1997), and inform voters (Alvarez 1997; Wattenberg and Brians 1996).

Fourth, one foundation for the minimal effects perspective is the erroneous belief that campaign-based and structural explanations of presidential elections are mutually exclusive. That is, the success of forecasting models based on objective factors, such as party identification or variations in the macroeconomy, has been taken as evidence that campaigns do not matter. Recent studies by Bartels (1997), Gelman and King (1993), and Iyengar and Petrocik (1998), however, point to a growing realization that these are complementary explanations and that "campaigns matter because they tend to produce congruence between fundamental political conditions and predispositions, on the one hand, and vote intention, on the other" (Bartels 1997, 3).

These criticisms of the minimal effects findings of survey research must be taken seriously, but many of the problems cited are neither endemic nor insurmountable. This article offers solutions to some of the pitfalls that have plagued previous analyses. In particular, it provides better measures of campaigning (and exposure to campaigning), a clearer statement of what is meant by "campaign effects," a way to control for off-setting effects, and openness to the possibility that

campaigns often work *with* structural explanations by activating latent preferences. These, in turn, will help us engage more effectively the broader question of whether campaigns matter.

HOW CAMPAIGN EFFECTS MAY OCCUR

In addressing the minimal effects debate, I focus on how and how much presidential campaigns affect the aggregate distribution of voter preferences. Accounting for all aspects of presidential campaigns is beyond my reach. I examine only the most obvious and visible manifestations of the campaign: television advertisements and candidate appearances. Similarly, in concentrating on "effects," I draw bead on the extent to which support for the presidential candidates is affected by campaign activities. This excludes other areas in which campaigns influence voters and elections, as mentioned above. Still, the loss in breadth is more than compensated for by the increased clarity of analytical focus.

Messages and Voters

I proceed by considering how campaigns may influence electorates. Let us begin with the notion, popularized by the work of Hovland (1949, 1953), that persuasion is composed of three discrete steps: exposure, reception, and acceptance. Exposure may involve only physical proximity to a message. Reception, however, involves actually getting, taking in, or "cognizing" a message. Acceptance involves allowing the information contained within the message to influence one's preferences, opinions, or attitudes. Within this framework, it is plausible to posit that a campaign develops and disseminates messages to the electorate with the goal of getting enough voters in enough states to accept its information so that the candidate receives 270 electoral votes.

Even within this simple framework, some limits on the potential for campaign effects are clear. Television advertisements communicate messages directly to the electorate, but their influence is limited by the electorate's reception of the messages and their persuasiveness. On the other side of the ledger, personal appearances allow candidates to convey information to the mass electorate through the reportage of the news media, but this conveyance is selective. Candidates are thus limited by the media with respect to how (and how often) a campaign appearance is presented to the public. In addition to these limits, it is important to reiterate the earlier observation that presidential elections are likely to be characterized by competing messages that can cancel each other.

Less obvious factors are also at play. Building on the work of Converse (1964), Zaller (1991) contends that the characteristics of the electorate should influence the nature and magnitude of campaign effects. He argues that reception is a function of the "intensity" of the message *and* the political knowledge of individual voters. Although Zaller focuses on U.S. House races, there is an obvious analogue at the presidential level. In presidential elections, we would expect media activity to be so intense that most Americans are exposed to the campaign. Still, there are differences in the amount of advertising and appearances lavished on a given state. As Zaller points out for House races, these differences are of considerable interest to anyone seeking to gauge effects; states in which one side outcampaigns the other are, all other things being equal, more likely to show changes in candidate support.

In addition, given what we know about the effects of partisanship, we would expect that statewide electorates with a high proportion of independents are the most likely to show campaign effects consistent with this differential activity. The reasoning here is straightforward. Many partisans are knowledgeable, strong identifiers with a low probability of accepting messages inconsistent with their party identification. Independents and leaning partisans are somewhat less likely to receive messages, but are more accepting. Since presidential campaigns are the most intense in American politics, we should expect that even these less knowledgeable independents and leaning partisans will be exposed to and receive campaign messages. Moreover, it is these individuals who are most likely to be persuaded and thereby change the aggregate distribution of candidate preferences in a given state. It would seem logical, then, to focus attention on states in which presidential candidates expend substantial but unequal resources and in which the proportion of less partisan citizens is relatively high.

With regard to factors that condition voter receptiveness, there is some evidence that campaign effects are not linearly correlated with exposure to (or reception of) campaign messages (see Bartels 1988). Put another way, the effect of seeing one additional TV ad should be greater for someone who has seen only one ad than for someone who has seen twenty. This is often termed the "diminishing returns" or "ceiling effects" argument. Although Bartels finds some evidence for this contention, it is somewhat inconsistent with Zaller's work, which suggests that political campaigns are typically not intense enough (and the American electorate not engaged enough) to trigger this effect. Still, this article assumes that ceiling effects are another potential limitation to campaign influence and considers them in the design and analysis.

A final factor affecting receptiveness is timing; that is, the presumed relationship between timing and the intensity of messages. Messages are not uniformly distributed across the campaign. By all accounts, the volume increases as the campaign progresses and peaks in the final two weeks, when less informed and more independent voters turn their attention to the election. Therefore, one would expect that more messages are received by a greater portion of the electorate during the last few days, and this could improve the chance for changes in candidate support.

Influencing Voters: Mobilization and Persuasion

The preceding discussion presumes that messages are often accepted in political campaigns. This is not a

trivial presumption, and it is important to detail just how this acceptance occurs. Two possibilities exist. First, messages can activate the latent predispositions of partisans by (1) priming party identification, (2) priming perceptions of objective circumstances (such as the existence of peace or economic prosperity), or (3) priming issue agendas in which one party has a popular advantage. These activation (or mobilization) effects are generally seen as the most powerful influence of the campaign (Gelman and King 1993; Iyengar and Petrocik 1998). Second, the last two priming processes noted above can persuade independents or weaker partisans to vote for a particular candidate. These persuasive effects are believed to be less powerful (Finkel 1993).

The relative importance of campaign effects for mobilization and persuasion is of considerable interest to our understanding of elections, but controlling for mobilization when modeling campaign effects is difficult. One possibility explored later is to estimate the typical partisan vote for a state so that support close to this number on election day is taken as an indication of mobilization. Controlling for persuasion is also difficult. By considering the level of unattached voters in a statewide electorate, however, it is possible to construct a similarly rough control for persuasion (e.g., the percentage of undecided voters can be used in conjunction with campaigning information to estimate persuasive effects). Although some may still prefer the use of individual-level survey data to gauge mobilization and persuasion (see, e.g., Finkel's use of the 1980 NES panel survey), these controls give some leverage on the critical question of how campaigns affect electorates. Moreover, given the very useful properties described below of the aggregate-level data at hand, the limits of these aggregate conceptualizations of mobilization and persuasion seem acceptable.

DATA AND RESEARCH DESIGN

This study relies on an empirical analysis of state-level data because, for the first time, information on the allocation of specific campaign activities is available for media markets and states. This presents a rare opportunity to measure campaigning's effect on aggregate statewide results and votes in the electoral college—where presidential elections are decided—rather than on the national vote or individual voters. In particular, aggregating campaigning at the national level enormously increases the likelihood that the candidates' activities will cancel out.

The focus on aggregate data introduces concerns about the unit of analysis, the possibility of measurement errors, and ecological regression fallacy. Although campaign effects must originate at the individual level, there is no effort here to develop or estimate an individual-level model. In the absence of individual-level inference, there can be no ecological regression fallacy. Were I to offer an individual-level model, it would be specified the same as the aggregate version, but with a smaller, homoskedastic disturbance term, so long as the classic OLS assumptions hold (Luskin n.d.).

In focusing on how candidate appearances and television advertising affect statewide election outcomes, three pieces of information are essential: (1) schedules of candidate appearances, (2) television advertising purchases by media markets, and (3) trial ballot polling data from each state. The first two allow an estimate of the volume of campaigning in the various states, and the third enables the estimation of effect by serving as the dependent variable in multivariate models. These data are available for the last three presidential campaigns. I also have weekly estimates of the campaigning and candidate support variables for several battleground states from 1992 and 1996, which facilitates an examination of how statewide electorates responded to shorter term stimuli.

Because many of these data are unique, a brief overview is in order. Candidate appearance data are the more straightforward of the two key independent variables and come from three sources: the Hotline (a daily political newsletter published in Falls Church, Virginia), the *Washington Post*, and schedules kept by the campaigns themselves. Since candidates often make last-minute stops that may not be reported in the news media, the last source is the most reliable. As detailed in the Appendix, for Dukakis in 1988 and for Bush in 1988 and 1992, candidate schedules were obtained from campaign personnel. For Clinton in 1992 and 1996 and for Dole in 1996, schedules noted each day in the Hotline were checked against accounts in the *Post*. This information makes it possible to count the days the presidential candidates spent in each state and to measure their effect on support.

Several issues merit mention. First, the presence of the candidates in their hometown (Boston, Houston, Little Rock, or Russell), places of work (Boston, Little Rock, or Washington), vacation destinations (Camp David, Kennebunkport, or Jackson Hole), or debate sites are not counted unless campaign appearances were reported. Second, the analysis does not separate low-visibility appearances, such as fundraisers, from high-visibility appearances, such as stump speeches. Such a distinction was made in subsidiary analyses (not reported here) and showed little effect. Third, it is assumed that a visit to Fresno is as important to California as a visit to Los Angeles. At one point, appearances were weighted by the population size of the city, but this also did not affect the results. Finally, the District of Columbia is treated as a state because it carries three electoral votes.

Data on candidate appearances have been gathered before (most notably, Kelley 1983), but data on television advertising come from the campaigns themselves and are completely unique. They encompass all TV advertising purchased by the campaigns between September 1 and election day.

The analysis conceives of TV advertising in terms of audience exposure. The volume of advertising in each state is measured in gross rating points (GRPs), a yardstick commonly used by advertising companies and communication scholars. Ordinarily, market cost discrepancies make it treacherous to compare advertising dollars spent in Sioux Falls, for example, with those

spent in New York City. GRPs provide a measure of audience reach independent of market costs. Roughly speaking, one hundred GRPs represent 100% of voters in a market seeing an advertisement once. (Fifty GRPs thus represent half a market's voters seeing an ad.) For the present analysis, statewide GRPs are calculated by multiplying the number of GRPs bought in a market by the percentage of the state's eligible voters in that market, repeating the procedure for all markets in a state, and then summing these numbers.[1]

This measure, although common elsewhere, has rarely been used in political science. The few studies that have gathered data on political ads tend to use the number of spots as a measure of exposure. Goldstein (1997) offers the most sophisticated analysis of this sort; he relies on the number of spots shown in markets (broken down by day-part) in conjunction with survey data on self-reported media consumption to estimate both individual- and aggregate-level exposure to political advertising. Although powerful, this approach is not feasible here because the relevant data are not available. GRPs are designed to account for the day-part and repetition factors that make exposure to TV ads so difficult to measure.[2]

While comprehensive and detailed, the media buy data are not perfect. In particular, estimates of party and independent expenditures were incomplete and had to be omitted from the final analysis. Given the amount of spending from these sources—as high as one-third of all spending in 1996, according to the Annenberg School of Public Policy—this causes concern. Still, media buys made by the campaigns themselves constituted the bulk of TV advertising during the general election campaign, and available evidence suggests that the purchasing patterns of the parties and independent groups closely followed those of the campaigns.

There is also the question of aggregation. Advertising data are observed at the media market level but are aggregated to the state level. This means that voters in a state could be assigned a high TV ad score even if advertising were concentrated in one or two markets (since this would increase the overall average). Because all analyses are aggregate, however, with states consistently serving as the unit of analysis, the use of statewide averages does not introduce error. Estimation problems could occur if (1) intense campaigning is concentrated in one or two markets and (2) the persuasive effects of campaigning are not linearly correlated with exposure.[3] More detailed analyses revealed no evidence of such nonlinearity. The Appendix presents both the appearance and TV advertising data, along with a description of the sources for each and a brief discussion of the validation procedures.

To discern the effects of this campaigning, trial ballot results and election day vote totals are used to gauge the preferences of statewide electorates. The Republican share of the two-party vote is generally used as the dependent variable, so that candidate activities are judged by their ability to move voter preferences. In addition, tracking polls conducted for the Republican National Committee (RNC) in the battleground states facilitate pooled time-series analyses of voter preferences across the 1992 and 1996 campaigns.

As noted above, the data span the ten weeks between September 1 and election day. Because quality data are simply not available before this period, campaigning through August 31 is excluded from the analysis. Unquestionably, these early campaign activities set the stage for what follows and are important in their own right (Blumenthal 1982; Franklin 1997). September 1 is the date on which the presidential campaigns receive their check from the Department of Treasury, however, and serves as both a legal and historical starting point for the fall campaign.

Cross-Sectional Models

The pooled cross-sectional model is specified as follows: Rep share of the two-party vote = α + β1(Rep − Dem GRPs) + β2(Rep − Dem appearances) + β3 (GRP diff × appearance diff) + β4(GRP diff × undecideds) + β5(appearance diff × undecideds) + β6(undecideds) + β7(Perot support) + β8 (average Rep presidential vote) + β9(GRP diff × average Rep vote) + β10(appearance diff × average Rep vote) + β11(1992) + β12(1996) + e.

The measure of the vote is calculated by taking the Republican share of the vote and dividing by the sum of the Republican and Democratic shares. This controls for the importance of independent candidates, which is an especially attractive feature, given the presence of Ross Perot in the 1992 race and of Perot and Ralph Nader in the 1996 race.

There are two groups of critical independent variables. The first is the measures of TV advertising and candidate appearances. These are captured by difference terms, so that greater Republican campaigning should positively correlate with greater support for the Republican candidate.[4] The second is interactions between (1) the campaign measures themselves, (2) the campaign measures and the percentage of undecided voters as measured in September polls, and (3) the campaign measures and the average Republican pres-

[1] For example, if a campaign purchases 400 GRPs in a market that contains 25% of the state's voters and 100 GRPs in a market containing 75% of the state's voters, the statewide point buy is 175 GRPs [(0.25 × 400) + (0.75 × 100) = 175].

[2] Two points should be made about the GRP estimates. First, advertising time is not always purchased in the state for which the ad is targeted. For instance, one must purchase time in New York City to reach northern New Jersey audiences. Whenever possible, GRPs targeted for a particular state are attributed to that state. Second, GRP estimates include spot, regional, and national network buys, with the last of these encompassing prime time, sporting events, national cable, and CNN buys.

[3] That is, if New York City receives 5,000 GRPs while the rest of the state receives none, then the statewide average will be correct but the effect on vote choice may be less than expected, since some people are saturated while others are not exposed at all.

[4] Models using separate terms for Republican and Democratic campaigning were also estimated and revealed minor differences between the parties. The models relying on difference terms were ultimately chosen because they are more parsimonious without sacrificing significant information.

idential vote in the state.[5] The first set of interactions captures the possibility that campaigning has a cumulative effect, with personal appearances reinforcing (and perhaps even magnifying) TV advertising. The second set captures the possibility that differential campaigning serves to persuade undecided voters—both independents and unconvinced partisans—to vote a certain way. For example, a largely undecided electorate that receives a one-sided message flow from a Republican candidate is expected to increase its support for the Republican. The third set captures the possibility that differential campaigning is most effective when there is a large pool of available partisans to mobilize. Because the average Republican vote is only an approximation of latent partisanship,[6] and because persuasive effects can also be captured by this variable, it provides a suggestive but imperfect test of mobilization's singular effect.

I should note that the computation of interactions between campaigning differentials and between campaigning differentials and the average Republican presidential vote is not straightforward. Because the campaigning terms are differentials, negative numbers can occur and cause multiplicative interactions to yield improper expectations. For example, a negative campaigning score, meaning a Democratic advantage, coupled with a relatively high average Republican vote would have us expect a very low Republican vote for a given state. Therefore, the difference measures are rescaled so that they are all positive, with the most pro-Democratic scores beginning at zero. Interactions are then computed so that larger sums should correlate positively with increases in the Republican share of the vote.

Several other variables serve as important controls. First, the percentage of undecided voters, taken from September trial ballot results, controls for the possibility that Republican support is affected by the relative percentage of undecided voters regardless of campaign activity. Since movement over each of the 1988–96 campaigns was generally favorable to Republican candidates, the expectation is that higher percentages of undecided voters should positively correlate with greater support for Republican candidates.

Second, statewide historical vote averages help control for the possibility that candidates do well in states where they always do well. In a sense, this measure accounts for mobilization that is independent of campaigning differences and helps comment on the argument that campaigns are crucial to what heretofore has been regarded as inevitable mobilization processes (Gelman and King 1993; Iyengar and Petrocik 1998).

Third, Perot's rise during October 1992 (and, to a lesser extent, 1996) is controlled for as a possible explanation for changes in the Republican share of the two-party vote. The underlying argument is that he attracted leaning Democrats and independents during the campaign who otherwise would have voted for Clinton (Germond and Witcover 1993; Goldman et al. 1994). Greater support for Perot, as indicated by his standing in polls during the final two weeks of the campaign, is therefore presumed to correlate with a greater share of the two-party vote for Republicans.

Finally, dummy variables are introduced for the specific elections and each of the states to control for unit effects in the pooled models. The election dummies, 1992 and 1996, proved useful. The state terms were insignificant, however, having little effect on the coefficient estimates (their deletion did not alter the significance of any coefficients by more than 10%). Therefore, they are excluded from the equations presented here.[7]

Given the cross-sectional nature of the data and a continuous dependent variable, ordinary least-squares (OLS) techniques are used to estimate the model.[8] Within this framework, three issues are troublesome for the present study. First, it may be that the volume of campaigning is endogenous to, or simultaneous with, candidate support. That is, resources are allocated where the vote is most likely to be high, as determined by the polls or previous voting patterns. This study presumes that this does occur but that the control variables—especially the average statewide vote—largely pick up the tendency of a state to be "receptive" to particular candidates. It is also the case that candidates, especially underdogs, are forced to campaign in states where their chances of doing well are not strong, so that endogeneity is not always (or even often) a concern.

Second, campaigning in a given state is not independent from campaigning in other states. This is a form of spatial as opposed to temporal autocorrelation, and it suggests an underestimation of the standard errors of the regression coefficients for the models. Not much can be done about this potential problem, except for focusing on alternative measures of significance, such as the simulations presented below.

[5] The average statewide Republican presidential vote was calculated by averaging the Republican share of the two-party vote in the six presidential elections between 1964 and 1984. The analysis presented here was replicated using averages derived from different ranges of elections with minimal differences, but 1964–84 was chosen because 1964 is generally regarded as the first election of the post–New Deal party system (Petrocik 1981).

[6] Why not use the party identification distribution in the states to get at mobilization and persuasion more directly? To be blunt, the numbers on party identification at the state level appear unreliable. Exit polls provide the most obvious data for measuring this, but CBS/*New York Times* numbers from 1988 and 1992 do not correlate strongly with each other or with votes in statewide elections for those years. Therefore, I use the alternative measures described in the text.

[7] Initially, the models also included a term for the Republican share of the two-party vote in the September trial ballots, which was meant to test the possibility that early support largely determines actual votes. This variable was excluded from the final model because it was insignificant, and its deletion did not affect the significance of the remaining regression coefficients.

[8] Preliminary diagnostics support this decision by verifying the classic assumptions of linear regression for these data. Scatterplots of the residuals do not suggest any problems associated with heteroskedasticity (an observation confirmed by more demanding White tests), and the errors do not appear to be autocorrelated. Multicolinearity does not appear to be an issue either, as the variance inflation factors (VIF) derived from the diagonal elements of the inverse of the correlation matrix do not indicate significant colinearity among the variables (VIF < 10 after standardizing the variables).

Third, one can argue that weighted least-squares (WLS) techniques are appropriate here because of the population differences among states. It is unclear, however, that estimating the effect of campaigning in the states should take these differences into account. In fact, to some degree the electoral college does this after the fact. To be safe, WLS estimates were derived from these data and were consistent with the OLS estimates.

Pooled Time-Series Models

Cross-sectional models cannot address the dynamic nature of campaigning. Does the absence of change in a candidate's standing reflect stability, or does it mask significant ebb and flow that sums to zero? Therefore, the weekly effects of campaigning are estimated using pooled time-series analysis. There are ten time points (T) for both the 1992 and 1996 presidential campaigns, each representing one week. Furthermore, there are nineteen cross-sections (C) from 1992 and seventeen from 1996, each representing a battleground state (the essential tracking polls were unavailable for nonbattleground states in these years and were completely unavailable for 1988). The 1992 battlegrounds, as defined by the Bush-Quayle campaign, were Colorado, Connecticut, Georgia, Illinois, Kentucky, Louisiana, Maine, Michigan, Missouri, Montana, New Jersey, New Mexico, North Carolina, Ohio, Pennsylvania, South Dakota, Tennessee, Texas, and Wisconsin. For 1996, the Dole-Kemp campaign identified them as Arizona, California, Colorado, Florida, Georgia, Kentucky, Louisiana, Michigan, Missouri, Montana, New Jersey, New Mexico, Nevada, Ohio, Pennsylvania, South Dakota, and Tennessee.[9]

For each year, the pooled time-series model, incorporating terms for cross-sections and time variation, is as follows: (Rep share of two-party vote)$t = \alpha t + \lambda t + \mu n + \beta 1$(Rep support)$nt\text{-}1 + \beta 2$(Rep - Dem GRPS)$nt + \beta 3$(Rep − Dem appearances)$nt + \beta 4$(GRP diff × appearance diff)$nt + \beta 5$(GRP diff × undecideds)$nt + \beta 6$(appearance diff × undecideds)$nt + \beta 7$(undecideds)nt-1 + $\beta 8$(Perot support)nt-1 = 1 + $\beta 9$(GRP diff × average Rep vote)$nt + \beta 10$(appearance diff × average Rep vote)$nt + \beta 11$(average Rep presidential vote)$nt + \beta 12$(state 1)$t + \ldots \beta 12+n$(state n)$t + \xi nt$, for $n = (1, \ldots, N)$ cross-sections and $t = (1, \ldots, T)$ time series, where λt and μn are assumed to be "fixed" conditionally in the intercept, while ξnt is assumed to be random (Sayrs 1989).

The dependent variable is the Republican share of the statewide two-party vote, as estimated by tracking polls. The vote measure for the pooled time series averages the trial ballot results from tracking polls for a given week and follows the same form as the cross-sectional measure. Thus, undecided (as well as third-party) voters are omitted, which makes comparisons across time more valid.[10]

The critical independent variables are also similar to those in the cross-sectional models, with minor changes. Most notably, when used in interactions, the measure of undecided voters is lagged by one week so that the effect of campaigning variables and electoral indecision is conditioned by the factors that existed as the campaigning occurred. The measure of Perot's support is also lagged by a single week. In addition, the Republican share of the statewide two-party vote from the previous week is added to control for the possibility that existing support is a function of prior support.

Although tracking data present an extraordinary opportunity to measure the campaign's effect on support, a problem when estimating a typical OLS model for pooled time-series data is the presence of heteroskedastic errors in the residuals. These represent the effects of each of the cross-sections (states) on the full pool. One way to correct for such errors is to estimate least-squares dummy variable (LSDV) models, which use an intercept to capture those effects unique to the cross-sections and those that may be unique to time. LSDV models are not the only models appropriate to estimate such effects, but they are relatively simple (Sayrs 1989). In using this approach for the pooled time-series analyses, I replaced the estimates of the coefficients for the full pool with coefficients from the dummy variables.

It is important to observe, as Stimson (1985) points out, that the appropriateness of the LSDV models depends upon homogeneity among the cross-sections. This is not directly testable in OLS but is assessed by estimating the robustness of the coefficients against the introduction or omission of cases and time points. Since the maximum change in the significance of any coefficient caused by adding or deleting a single case is only 9%, there is some assurance that the LSDV models are reasonably robust. Aside from this, the main disadvantage of LSDV is that under certain conditions the estimates of the standard errors of the regression coefficients will be inflated (Beck and Katz 1995). Panel-corrected standard errors (PCSEs) have been calculated to correct for this inflation.[11]

[9] The omission of nonbattleground states is regrettable, but it is not clear that their presence would substantively alter the results. Since these states are more partisan and received less intense campaigning, one can argue that campaign effects would have been less significant, suppressing the overall estimates. Note, however, that strongly partisan states such as Nebraska or Hawaii can still have a relatively large independent (and undecided) population. When subjected to one-sided (if relatively less intense) campaigns from the dominant party, this could lead to significant movement toward that party over the campaign.

[10] Most tracking polls ask the following question of undecided respondents: "Do you lean toward either candidate?" They then collapse leaning supporters with committed supporters, which reduces the percentage of undecided voters in the sample. I treat leaners as supporters and only use tracking poll results in which the follow-up question was asked. This is because (1) there is a strong correlation between leaning toward a candidate and voting for that candidate, (2) many more polls ask and report the collapsed results than do not, and (3) consistency in the question frame is desirable, since the percentage of undecided voters is used as a control variable.

[11] The most common alternative for modeling pooled time-series data—the Parks method, which employs generalized least-squares (GLS) techniques—often underestimates standard errors and thus inflates t-statistics. In fact, Beck and Katz (1995) contend that the Parks method is unusable unless there are substantially more time

RESULTS

Univariate Results

Summary statistics are presented in Table 1. Due to the substantial variance in the statewide campaign totals, median, minimum, and maximum values are included to provide a better description of the data. All told, TV advertising averaged about 2,800 GRPs per state and consumed 62% of available funds. Given that public funding for presidential campaigning is indexed for inflation, it is somewhat surprising that the volume of campaigning differed from year to year. On the whole, Republican presidential candidates enjoyed an average advantage of 505 GRPs—which represents eight additional TV advertisements seen by the typical voter in each state. This advantage is driven by Bush's 1992 campaign, which purchased an average of 4,957 GRPs per state. During that campaign, Bush bought a great deal of national television advertising time, which registers in all states and drives up the state-by-state average. Interestingly, there are several instances of significant TV advertising advantages in battleground states (see the Appendix). California was an especially inviting target for the Republicans; Bush purchased almost 2,700 more GRPs than Dukakis, and Dole purchased more than 2,000 more than Clinton. Conversely, the Democrats were more active in Washington and Oregon, where both Dukakis and Clinton had large advantages in GRPs over Bush and Dole, respectively.

Few comparable partisan differences were apparent in the travels of candidates, who averaged 2.2 appearances per state. Some notable extremes are Dole's advantage of fourteen more appearances than Clinton in California and Bush's advantage of six more than Dukakis in New Jersey. A favorite destination for Democrats seems to have been New York, where Dukakis made five more appearances than Bush, and Clinton made six more appearances than Dole. One general finding is that incumbents made fewer appearances than challengers. The data do not allow us to test whether Bush and Clinton were presiding over an inordinate number of ceremonies at the White House when they ran for reelection, but one suspects that all incumbents follow some version of the "Rose Garden" strategy.

As expected, the preference data show that Republican candidates picked up support over each of the three presidential campaigns, with Bush's gain of 3.1 points in 1988 constituting the greatest movement. The most volatile states were those known to be Republican strongholds: Alaska, Indiana, South Dakota, and Utah were especially prone to Republican swings during the campaigns. It is also worth noting that the Republican candidates had lop-sided campaigning advantages in these states, which were probably written off by the Democrats as unwinnable. Although this partially supports the inclusion of an average statewide vote term, movement in other Republican states was uneven. Furthermore, substantial movement occurred in more competitive states, which leads one to view these preliminary, univariate results with caution.

Cross-Sectional Results

Table 2 presents the cross-sectional models for each year as well as the pooled model. It shows that campaign effects were often statistically and substantively significant.[12] For example, note the significance of the direct effects for the pooled model, seen in the total column. Put plainly, an increase of 500 GRPs—not unusual in presidential campaigns—would boost a candidate's share of the vote by 2.2 points. As for old-fashioned campaign stumping, an extra day in a state would raise a candidate's standing by 0.8 points.[13]

In general, the interactive effects shown in the total column of Table 2 are also statistically significant. In particular, the interactions between campaigning differentials and the percentage of undecided voters had a strong effect on candidate support. In fact, in only one instance—*Appearances* × *% Undecided* in 1996—did an interaction of this class fail to achieve statistical significance. To put these effects in more understandable terms, an increase in a candidate's statewide TV advertising advantage of 500 GRPs and a simultaneously five-point increase in the percentage undecided would raise his share of the vote by 1.3 points. Similarly, an extra three days of campaigning in a state by a candidate, coupled with an electorate that was five points more undecided, would raise his share of the vote by 2.1 points. This indicates that campaigning persuades uncommitted voters.

The interactions between campaigning and the average statewide vote also produce significant effects. For example, an increase of 500 GRPs in a Republican candidate's TV buy coupled with a five-point increase in the average statewide Republican vote would result in a 1.6-point increase in his vote total. In the same vein, three additional appearances coupled with a five-point increase in the average Republican vote would yield a 2.7-point change in his vote total. Since we are controlling for the average presidential vote in the state, the significance of these interactions indicates that mobilization can be affected by differences in

points (T) than cross-sectional units (N). Since I have substantially more cross-sections than time points—17 and 19 states for 1992 and 1996, respectively, and only ten weeks of campaigning—I proceed within the simple LSDV model framework.

[12] Significance estimates for the coefficients do not change much with simpler specifications but the coefficient estimates themselves appear fairly volatile. For example, a model with only the campaign variables and controls for statewide average vote and year effects produces coefficient estimates only 30% as large as the estimates presented here. This suggests a cautious interpretation of the larger estimates found in the full model.

[13] As in the cross-sectional analyses, models using separate terms for Republican and Democratic campaigning were also estimated, and slight differences emerged. Democratic appearances were marginally (but not significantly) more effective than those by Republicans, and Republican TV ads were marginally (but not significantly) more effective than those of Democrats. Again, the models relying on difference terms were chosen because they are more parsimonious without sacrificing significant information.

TABLE 1. Summary Statistics for Presidential Campaigning and Candidate Support in the States, 1988–96

Variable	Mean (Standard Deviation)	Median	Minimum	Maximum
Total				
Republican TV Ads[a]	3,066 (2,624)	2,300	300	9,760
Democratic TV Ads	2,561 (2,181)	1,558	700	9,960
Republican Appearances	2.2 (3.3)	1	0	23
Democratic Appearances	2.2 (3.0)	1	0	19
% Republican Support in September	39.3 (10.3)	40	5	65
% Democratic Support in September	43.8 (8.1)	45	24	86
% Independent Support in September	6.2 (5.6)	5	0	23
% Undecided in September	10.9 (6.5)	9	0	26
% Republican Vote	44.3 (10.3)	44	9	67
% Democratic Vote	45.0 (8.7)	44	25	85
% Independent Vote	9.9 (8.4)	9	0	30
Change in Republican Share of the Two-Party Vote over the Campaign[b]	2.3 (3.6)	2.8	−7	11
Average Republican Vote (1964–84)	53.4 (6.8)	53	22	64
Number of cases = 153				
1996				
Republican TV Ads[a]	2,447 (2,288)	700	700	8,226
Democratic TV Ads	2,696 (2,138)	1,499	1,000	9,537
Republican Appearances	2.7 (4.0)	1	0	23
Democratic Appearances	2.5 (2.9)	1	0	13
% Republican Support in September	37.3 (8.3)	37	5	54
% Democratic Support in September	46.7 (8.1)	48	32	81
% Independent Support in September	5.2 (2.1)	5	2	9
% Undecided in September	11.0 (4.1)	10	4	26
% Republican Vote	41.3 (8.3)	41	9	54
% Democratic Vote	47.9 (8.6)	48	33	85
% Independent Vote	9.1 (2.4)	9	2	14
Change in Republican Share of the Two-Party Vote over the Campaign[b]	1.9 (3.4)	1.9	−7	9
Number of cases = 51				
1992				
Republican TV Ads[a]	4,957 (2,655)	3,400	2,300	9,760
Democratic TV Ads	3,013 (2,949)	1,200	700	9,960
Republican Appearances	1.8 (2.1)	1	0	9
Democratic Appearances	1.8 (2.0)	1	0	8
% Republican Support in September	34.2 (5.1)	34	21	46
% Democratic Support in September	40.9 (6.2)	41	24	53
% Independent Support in September	12.4 (4.0)	12	6	23
% Undecided in September	12.6 (4.1)	12	7	24
% Republican Vote	37.7 (6.5)	37	9	50
% Democratic Vote	42.1 (8.6)	43	25	85
% Independent Vote	19.6 (5.8)	21	4	30
Change in Republican Share of the Two-Party Vote over the Campaign[b]	1.8 (3.9)	3	−4	11
Number of Cases = 51				

Continued on Next Page

the amount of campaigning to which the electorate is exposed.

Even though interactive effects between TV advertising and candidate appearances are insignificant, the total effect of campaigning, taking into account both direct and interactive effects discussed above, is notable. The pooled model indicates that an increase of 500 GRPs in TV advertising would net a candidate about 2.8 points in the polls, while three additional appearances would increase his support by 2.5 points.

TABLE 1. (Continued)

Variable	Mean (Standard Deviation)	Median	Minimum	Maximum
1988				
Republican TV Ads[a]	1,795 (1,723)	1,108	300	5,994
Democratic TV Ads	1,976 (791)	1,682	1,240	3,750
Republican Appearances	2.2 (3.5)	1	0	15
Democratic Appearances	2.5 (3.8)	1	0	19
% Republican Support in September	46.3 (7.8)	47	14	65
% Democratic Support in September	43.7 (7.8)	42	35	86
% Independent Support in September	1.0 (1.2)	1	0	4
% Undecided in September	9.0 (9.3)	9	4	17
% Republican Vote	53.9 (8.1)	55	14	67
% Democratic Vote	45.0 (7.9)	44	32	83
% Independent Vote	1.1 (0.9)	1	0	4
Change in Republican Share of the Two-Party Vote over the Campaign[b]	3.1 (3.8)	3.5	−5	11
Number of cases = 51				

[a]Television advertising is measured in gross rating points.
[b]The Republican vote in November is subtracted from Republican standing in September, so that negative values indicate a decrease in the Republican share of the vote.

Interestingly, variation in the significance of these effects is moderate across the three elections. In fact, the differences are small enough to discourage any causal explanations for their occurrence. For instance, the interactive effect of TV ad differentials and the average statewide vote is smaller for 1992 than for 1988 or 1996, but the notion that this difference is caused by the relative ineffectiveness of Bush and Clinton TV ads seems tenuous at best.

Table 3 demonstrates that the effect of differential campaigning is even more apparent in the electoral college. If one uses the models to predict specific statewide outcomes, then one can see how changes in campaigning differentials may have affected the electoral college vote. For example, an increase of 500 GRPs in the TV ad buy differential across the states would give the Republicans an average of 123 more electoral votes (130 in 1996, 154 in 1992, and 83 in 1988). A similar increase by the Democrats would give them an average of 110 more electoral votes. As for candidate appearances, a three-visit increase in the differential would give the Republicans an average of 64 more electoral votes and the Democrats an average of 87 more electoral votes. Of course, such a shift in the relative amount of campaigning is highly unlikely. Still, this exercise demonstrates that electioneering can move voters and that the reactive nature of resource allocation in contemporary presidential campaigns is probably smart politics.

The remaining coefficients in Table 2 indicate that the control variables had fairly strong effects on the vote. States in which (1) many voters were undecided, (2) Perot had strong support, and (3) Republican presidential candidates had run well in the past tended to see higher Republican vote totals. This third finding indicates that some mobilization occurred irrespective of campaigning, although the significance of the interactions suggests that mobilization occurs both through the stimulus of campaigning differentials and through other processes not captured here. The terms capturing specific election effects were also significant: The GOP vote was substantially lower in 1992 (−2.3) and 1996 (−5.0) than in 1988. Although not unexpected, these findings reinforce the need to consider the distinct contexts of different election years.

On the whole, the models provide powerful explanations of the elections, accounting for 78% of the variance. It is surprising that further analyses revealed no ceiling effects associated with campaigning. First, a visual inspection of the scatterplots between campaigning differentials and the vote suggest the relationship is roughly linear. Second, Box-Cox transformations produce a test statistic λ of 0.7,[14] since we know that this statistic approaches one as a relationship becomes linear (Johnston 1984), there is additional reason to doubt that there are ceiling effects. In light of these facts, I assume a linear relationship between campaigning variables and the vote in these elections. While unexpected, this reinforces the decision to use OLS methods to estimate the model.

Although the key coefficients are generally significant, these findings cannot be taken to mean that it was easy to increase support. The candidates were sensitive to small shifts, and effects were rarely allowed to

[14] In a Box-Cox transformation, a variable Z is transformed $(Z^{\lambda} - 1)/\lambda$. Since the limit of this as λ approaches zero is $\ln Z$, it is defined to be $\ln Z$ when $\lambda = 0$. If all variables in a linear functional form are transformed in this way and λ is then estimated (in conjunction with the other parameters) via a maximum likelihood technique, significance tests can be performed on λ to check for special cases. If $\lambda = 0$, for example, the functional form becomes Cobb-Douglas in nature; if $\lambda = 1$, it is linear.

TABLE 2. Republican Candidates' Share of the Major Party Presidential Vote as a Function of Campaigning, 1988–96

Variable	Total	1996	1992	1988
Television Ad difference (Rep. − Dem. in 100s of GRPs)	0.430 (0.242)*	0.740 (0.339)*	0.291 (0.170)*	0.472 (0.268)*
Appearance difference (Rep. − Dem.)	0.766 (0.401)*	1.043 (0.534)*	0.543 (0.331)	0.723 (0.419)*
TV Ad × Appearances	0.066 (0.047)	0.071 (0.034)*	0.042 (0.034)	0.049 (0.029)*
TV Ad × % Undecided	0.032 (0.009)***	0.057 (0.020)**	0.011 (0.005)*	0.020 (0.008)**
Appearances × % Undecided	0.089 (0.043)*	0.062 (0.038)	0.088 (0.018)***	0.091 (0.029)**
TV Ad × Avg. state vote	0.006 (0.003)*	0.005 (0.003)*	0.003 (0.002)	0.008 (0.003)**
Appearances × Avg. state vote	0.017 (0.010)*	0.007 (0.003)*	0.023 (0.012)*	0.019 (0.010)*
% Undecided	0.009 (0.004)*	0.006 (0.003)*	0.004 (0.002)*	0.010 (0.004)**
Perot Support	0.002 (0.001)*	0.002 (0.001)*	0.003 (0.001)**	—
Average Statewide Rep. Vote	0.014 (0.002)***	0.010 (0.007)	0.031 (0.013)*	0.019 (0.004)***
1992	−2.324 (0.919)**	—	—	—
1996	−5.001 (0.178)***	—	—	—
Intercept	−0.326 (0.741)	−0.130 (0.107)	−0.061 (0.125)	0.004 (0.088)
Number of cases	153	51	51	51
Adjusted R^2	0.777	0.775	0.723	0.803
Standard error	0.041	0.044	0.033	0.036
F-statistic	53.513***	18.182***	13.818***	30.055***

Note: Cell entries are unstandardized regression coefficients; standard errors are presented in parentheses. *p = .05, **p = .01, ***p = .001; one-tailed. For the pooled model, dummy variables for 1992 and 1996 are introduced to control for differences across years. Variables controlling for the unique effects of the states were deleted from the pooled model primarily because this did not much affect the regression coefficients for the remaining variables (average change in the significance of the regression coefficient was 9%). Regressions done on SPSS for Windows, Release 8.0.

accumulate. In fact, presidential election outcomes fell somewhere in the middle range of potential results (suggested by different forecasting models and political pundits) due to the tendency of Republican and Democratic campaigns to target the same states, allocate similar resources in those states, and craft TV ads that were roughly comparable in terms of quality. The next section pursues this theme of campaign mimicry and tests more directly whether statewide poll standing is significantly influenced by the presumed tit-for-tat nature of presidential campaigning.

Pooled Time-Series Results

Table 4 presents the results of LSDV models for the ten weeks of the 1992 and 1996 campaigns (again, statewide tracking polls were not available for 1988). The goodness of fit and diagnostic statistics indicate that the models are well specified and explain much of the variance. As with the cross-sectional models, campaigning—especially TV advertising—had strong direct effects. For example, an increase of 500 GRPs by a candidate would increase his statewide support by an average of 1.6 points from the previous week. Similarly, an additional three appearances would produce a 2.2-point improvement in a candidate's support. These findings validate the results of the cross-sectional analysis.

The interactive effects of campaigning were strong as well, especially in 1992. In particular, those between campaigning and the percentage of undecided voters typically caused statistically and substantively important changes in support. On average, an increase of 500 GRPs for a candidate and an additional 5% of undecided voters would cause a 1.3-point increase in support from the previous week. Along the same lines, a three-day increase in a candidate's appearances and an additional 5% undecided would cause a 1.7-point increase.

Slightly less significant but nonetheless important were the interactive effects between campaigning and the average statewide vote. Here, an additional 500 GRPs and a five-point increase in the party's average vote would cause a 1.8-point increase in the candidate's support. By the same token, an extra three days coupled with a similar increase in the party's average vote would produce a 2.05-point increase.[15]

Interestingly, the data indicate that campaign effects were stronger in 1992 than in 1996. Unlike what was found in the cross-sectional data, the difference is statistically significant. This fits well with the impressionistic observation that effects were less likely in

[15] The data indicate that "persuasive" effects (as indicated by the interactions between undecided and campaigning differentials) are roughly equal to the "mobilizing" effects (as indicated by the interaction between average statewide presidential vote and campaigning differentials), but comparisons are only suggestive. One should bear in mind that the measures imperfectly capture the underlying concepts; the persuasion measures include some mobilization effects, and vice versa.

TABLE 3. Simulated Effects of Increased Campaigning on Electoral College Votes, 1988–96

Various Electoral Scenarios	Electoral Votes Average	1996	1992	1988
Estimated Republican total[a]	252	189	173	393
Estimated Democratic total	286	349	365	145
Actual Republican total	251	159	168	426
Actual Democratic total	287	379	370	112
Republican Gain Due to an Additional 500 GRPs per State	123	130	154	83
Democratic Gain Due to an Additional 500 GRPs per State	110	68	60	201
Republican Gain Due to an Additional Three Appearances per State	64	77	87	28
Democratic Gain Due to an Additional Three Appearances per State	87	71	66	123

Note: Cell entries represent electoral votes. The effects of additional campaigning are calculated by substituting hypothetical campaigning values into the models of candidate support presented in Table 2.
[a]These values are derived by allowing the models to predict the outcomes in each state based on the actual allocation of resources.

1996, given the presence of a popular incumbent in the race. It also accords with Holbrook's (1996) more generalized account for why this was the case, as will be discussed later.

The interactive effects were significant, but many of the control variables also elicited direct effects. For example, the percentage undecided significantly correlated with greater support for Republican candidates. This is plausible, since Bush and Dole rallied late in their campaigns and in so doing undoubtedly converted many undecideds to the Republican cause. Candidate support across a given week also correlated with the previous week's support, which indicates that states where a ticket was doing well were states where it continued to do so. Similarly, the average statewide vote correlated with the week-by-week measure of candidate support, although the relationship only achieved statistical significance for 1992.

In contrast to the cross-sectional analysis, Perot support did not significantly affect the Republican share of two-party support. Perhaps the focus on battleground states in the time series influenced this result, as it omitted Republican and Perot strongholds such as Idaho, North Dakota, Utah, and Wyoming, where the correlation between their votes was high. Another discrepancy between the cross-sectional and time-series analyses is that the coefficients for the state dummy variables, which were included to account for unit effects, tended to be significant in the time series. This suggests that there were state effects over and above the effect of the average statewide vote. While intriguing, these findings have been excluded from Table 4 in order to focus on the effects of the campaigning variables.

The campaign effects uncovered here are striking, but it is possible that the parameter estimates underpinning this analysis are not constant across the time series. More specifically, campaigning may have had a greater effect closer to the election, as voters turned their attention to the race. Tests for parameter constancy were conducted by establishing break points in the series and comparing parameter estimates (similar to the Chow test used for cross-sectional data). Different effects across the campaign are difficult to assess because of the limited data points in the time series, but these tests revealed some differences. Most notable were changes in the last two weeks, when the relationship between campaigning and preferences strengthened. For example, the TV ad differential coefficient increased by almost 25% in the last two weeks of the 1996 and 1992 campaigns. This supports the conventional wisdom in politics that "nobody tunes in until after the World Series," but it contradicts the popular notion that voters make up their mind early in the campaign.

This finding also appears to raise doubts about the role of campaigns assumed by forecasting models. Since information about objective, exogenous factors favored the front-runner in 1992 and 1996, late movement coincident with the campaign activities of the underdog candidate seems at odds with the assumption that voters use the campaign (if at all) to acquire information about these factors. Put differently, if voters "learn" over the campaign and the vote distribution gradually converges to a predictable outcome, then what do we make of major vote swings in the last two weeks that favor the losing candidate?

Of course, not all would agree that this finding is incompatible with the logic of forecasting models. Gelman and King (1993), for example, contend that accelerated volatility in the last weeks of the campaign is consistent with forecasting models that assume campaigns to be conduits of exogenously determined information. More to the point, they imply that late movement is caused by disaffected partisans returning home, which causes a predictable boost in the losing candidate's vote total. Despite these protestations, however, the direction of the movement and the significance of campaigning shown here strongly suggest that these activities are, at the least, important triggering mechanisms.

All told, the LSDV models demonstrate that a good week of campaigning can move a statewide electorate. Although the time series focuses on battleground states, which may have been less partisan and more susceptible to movement, it is unlikely that other states were immune to these effects. Furthermore, campaign effects persist despite the fact that the tracking polls used for the pooled time series roll several nights of polling into weekly "averages," which creates errors almost twice as large as those associated with stand-alone polls that draw a similar number of respondents (Asher 1991).

The models also suggest that presidential candidates

TABLE 4. Weekly Changes in Republican Share of the Major Party Presidential Vote as a Function of Campaigning, 1988–96

Variable	LSDV Regression Coefficients (panel-corrected standard errors) 1996	1992
Television Ad Difference (Rep.-Dem.)	0.232 (0.111)*	0.387 (0.167)*
Appearance Difference (Rep.-Dem.)	0.514 (0.215)**	0.954 (0.499)*
TV Ad × Appearances	0.050 (0.020)**	0.115 (0.065)*
TV Ad × % Undecided (t − 1)	0.023 (0.012)*	0.039 (0.014)**
Appearances × % Undecided (t − 1)	0.049 (0.039)	0.097 (0.038)**
TV Ad × Average State Vote	0.005 (0.003)*	0.009 (0.004)*
Appearances × Average State Vote	0.008 (0.005)	0.018 (0.008)*
% Undecided (t − 1)	0.159 (0.056)**	0.172 (0.049)***
Average Statewide Republican Vote	0.116 (0.090)	0.292 (0.149)*
Perot Support (t − 1)	0.074 (0.111)	−0.003 (0.058)
Republican Share of Previous Week's Vote	0.649 (0.071)***	0.609 (0.076)***
Intercept	10.726 (5.130)	3.470 (7.971)
Number of cases	116	118
Adjusted R^2	0.498	0.615
Standard error	2.604	2.387
Durbin's h-statistic	2.112	2.311
F-statistic	11.448***	18.155***

Note: Cell entries are unstandardized regression coefficients from least-squares dummy variable (LSDV) regressions; panel-corrected standard errors (Beck and Katz 1995) are presented in parentheses. *p = .05, **p = .01, ***p = .001; one-tailed. Variables controlling for the unique effects of the states were included in the model and were typically significant, but they are omitted from this table (provided upon request). Regressions were done on SPSS for Windows, Release 8.0.

could have improved their electoral votes dramatically through the cumulative influences of either more effective activities or repeated weekly advantages. The difficulty is that the effectiveness and geographic allocation of campaigning were similar in 1992 and 1996. In terms of effectiveness, separating out campaigning influences by candidate reveals that the coefficients associated with the activities of the Republican and Democrat were roughly equal (the largest of these small differences was the greater effect associated with Democratic appearances). In terms of volume, a look at the amount of campaigning from week to week shows that the opposition usually matched increases in GRPs or appearances within two weeks. (This may have been less common in nonbattleground states, which tend to be more reliably partisan and less of a campaign target.) Thus, campaigning advantages were difficult to achieve. Moreover, most states were predisposed toward a particular candidate anyway, so the probable electoral vote outcome could only have been reversed by a sweep of the close states. Given what we know about differences in campaigning effectiveness and volume, such a sweep was possible but unlikely.

CONCLUSION

This article began by asking whether and how presidential campaigns influence elections. Cross-sectional and pooled time-series models of candidate support were estimated from extensive statewide data on the candidates' TV advertising and appearances from the 1988, 1992, and 1996 presidential campaigns. The cross-sectional models showed that a candidate's activities in a state were positively correlated with his vote. In addition, simulations derived from these models suggested that changes in the relative level of campaigning between the candidates *could* have significantly affected electoral vote totals. In a more detailed examination, pooled time-series models used weekly data to corroborate the relationship between statewide support and campaigning. Furthermore, both analyses showed that the interactions between campaigning differentials and (1) the percentage of undecided voters and (2) the average statewide presidential vote had significant effects on candidate support. Campaign effects, therefore, appear to have been both direct and conditioned by the receptivity of the electorate.

Too much should not be made of the campaign effects discovered here—no elections would have been reversed without implausible changes in the distribution of campaigning in several key states—but the results indicate that previous studies of presidential campaigning may have been limited by some of the problems identified earlier. For instance, a quick inspection of the Appendix shows that aggregating statewide data to the national level can mask discrepancies across the states, which may affect the estimation of effects. In addition, this study also offers evidence that campaigns influence both mobilization and persuasion and that these processes merit particular attention at both the conceptual and empirical design phases of future analyses.

There remains, of course, much room for debate. Some scholars may consider the effects found here to be minor, while others will deem them significant. My own view is that these findings—especially the simulations and (to a lesser degree) the late campaign volatility—lend support to the latter interpretation. I

also believe there is little reason to suggest that presidential campaigning was decisive in these elections.

Beyond the larger question of whether campaigns matter, there are several reasons to view these particular estimates of effects with caution. First, there is the time frame. The analysis does not account for campaigning before September 1, and the 1996 election demonstrated that period can be critical (Franklin 1997; Morris 1997; Woodward 1996). Second, the estimates of campaign effects do not account for local media coverage of TV advertising or appearances, which could reinforce or undermine campaign messages. Also not considered are the debates, blunders, and events that drive media coverage of the campaign. Third, campaign effects can occur at a more subtle level than is considered here. As suggested earlier, campaigns can potentially affect voter turnout and information about candidates and issues. Moreover, this study assumes that campaigns prime voters' perceptions of politics and, quite possibly, their candidate preferences, but it does not directly examine the possibility that campaigns influence elections through other prepersuasive processes, such as the framing of specific issue agendas.

These caveats, however, suggest that the analysis is as likely to understate campaigning effects as it is to overstate them. In addition, it is improbable that the introduction of more controls or the consideration of indirect effects would alter the fundamental relationships between campaigning and voter preferences established here. The core finding that presidential campaigning influences statewide support for candidates seems secure. The more interesting question becomes whether these effects typically make an important difference in presidential election outcomes. Some of the present data suggest they do not, but the answer requires empirical analysis beyond the scope of this discussion and would seem to depend upon the magnitude of campaigning discrepancies and (perhaps more important) the closeness of the race. This latter point echoes Holbrook (1996), who argues that campaigns matter when conditions are ambiguous (1960, 1976, 1988, 1992) but not when one side is strongly favored (1964, 1972, 1984, 1996). The present findings, especially the significance of campaign effects in the time-series analyses for 1992, validate this argument. Empirical analyses that examine the joint effects of campaigning discrepancies and closeness may be the next step in resolving the controversy that surrounds the influence of presidential campaigns.

APPENDIX

The advertising and appearance data are aggregated over the ten weeks of the campaigns. As stated in the text, they encompass the candidates' campaigns and do not include campaigning by the national party or independent expenditures. The weekly campaigning data from the battleground states in 1992 and 1996, featured in the time-series analyses, will be provided by the author upon request.

For 1988, information on Bush's media buys were provided by Market Opinion Research, a survey research firm headed by Bush pollster Robert Teeter. Data on Dukakis media buys were provided by Yellin Communications, a firm that made most of the Democratic purchases during the general election campaign. For 1992, estimates for both Bush and Clinton were provided by the Republican Party's November Company, which handled advertising placements for Bush's campaign. Additional data on Clinton's 1992 buys were culled from media sources. The 1996 estimates for Dole and Clinton were provided by the Republican Party's New Century Media, which handled advertising placements for Dole's campaign.

Although attempts to acquire data from Clinton's campaigns proved unsuccessful, Republican and (in 1992) media estimates of Clinton buys are undoubtedly more reliable than in past elections. The main improvement was the use of satellite tracking systems to follow Clinton's advertising campaign. Advertisements are like supermarket products in that each is given a "bar code" as it is beamed to a satellite for transmission. These bar codes, in turn, allow for tracking of the ads. This service has proven invaluable to political campaigns, which (for a fee) no longer have to call a multitude of television station programmers or hear from the field to discern where the opposition is on the air. Perhaps more important, campaigns can monitor their own ads to ensure they are getting what they pay for. Many experts argue that ad monitors pay for themselves in presidential campaigns simply by catching (and allowing for the correction of) mistakes made by television stations in airing the correct ad at the correct time.

Despite the impressive new technology, all data on television advertising purchases were validated. First, measures of a candidate's media buys were compared to estimates of these buys by the opposition or the media (except in 1996, for which no data on Republican TV buys were available from the Clinton-Gore campaign). The zero-order correlation coefficient between the estimates was 0.79. Second, measures of the buys were correlated with the candidates' electoral college strategies and my own estimates of statewide costs per persuadable voter. The zero-order correlation between the two estimates was 0.61.

Although my campaigning data can now be released to the scholarly community, the polls used for measures of candidate support remain proprietary. Precampaign support for the candidates is estimated from private surveys conducted by the Republican presidential campaigns and from surveys (some public and some private) conducted by Mason-Dixon and several news media consortiums. Tracking poll data used in the time-series analyses were provided by the Republican presidential campaigns and the RNC.

Table A-1. Presidential Campaign Data, 1988–96

	1996		1992		1988	
State	Rep. TV	Dem. TV	Rep. TV	Dem. TV	Rep. TV	Dem. TV
AL	700	1123	3400	700	300	1240
AK	700	1000	3400	700	300	1240
AZ	5101	5869	4320	700	300	1240
AR	700	1220	2300	1220	2944	1526
CA	4814	2752	2300	700	5994	3308
CO	6724	5075	7750	8530	4603	2395
CT	4748	4500	8840	6045	1108	2665
DE	700	1000	7125	700	300	1566
DC	700	1000	3400	700	300	1240
FL	5429	4582	5770	700	300	1240
GA	5921	3974	8800	7350	1764	1240
HI	700	1000	2300	700	300	1240
ID	700	1000	2300	700	300	1240
IL	1474	1434	6050	2650	4819	3750
IA	3848	4046	3390	2090	640	2536
IN	1925	1499	3390	1000	361	1240
KS	700	1025	3390	700	1554	1240
KY	6189	6854	7700	4300	2495	2096
LA	5650	5969	9290	8580	1378	1351
ME	700	2045	6788	4010	3001	1528
MD	700	1000	2300	2500	300	2611
MA	700	1000	2300	700	300	1682
MI	6156	5148	9760	9960	5081	2615
MN	700	2013	2300	1000	300	1896
MO	1687	2200	8750	8725	4343	2634
MS	700	1000	3400	700	300	1240
MT	2743	3365	7910	6700	3347	2293
NV	6449	6991	3400	700	300	1240
NE	700	1000	2300	700	300	1240
NH	1043	1000	3400	700	300	1240
NJ	3597	3810	9340	7145	4828	3159
NM	4877	4617	8900	6400	3916	1750
NY	2311	2685	2300	1800	3044	2335
NC	700	1000	6800	6400	2110	1558
ND	700	1268	3400	700	300	2419
OH	4980	5626	9100	8760	5259	2078
OK	700	1000	3400	700	2318	1240
OR	1615	2606	2300	2750	300	2331
PA	6542	7050	9150	7500	2660	3693
RI	700	1000	2300	700	300	2417
SC	700	1010	3400	700	300	1240
SD	2894	2964	7500	4150	3542	2968
TN	8226	9537	5900	4350	1600	1240
TX	700	1165	5660	1200	2705	2585
UT	700	1000	2300	700	300	1240
VT	700	1000	3400	3300	2893	3208
VA	1654	1171	4350	700	371	1240
WA	700	2256	2300	2750	3691	2430
WV	700	1000	2300	700	1909	3737
WI	700	3065	8600	6400	970	2831
WY	700	1000	2300	700	300	1240
	Rep. App.	Dem. App.	Rep. App.	Dem. App.	Rep. App.	Dem. App.
AL	1	1	1	0	0	0
AK	0	0	0	0	0	0
AZ	3	4	0	0	0	0
AR	0	3	0	2	1	1
CA	23	9	1	3	15	19
CO	6	6	2	2	5	4
CT	3	1	1	2	5	3
DE	0	0	1	1	1	0
DC	7	13	0	0	1	5
FL	11	5	3	5	0	0

Table A-1. (Continued)

	1996		1992		1988	
	Rep. App.	Dem. App.	Rep. App.	Dem. App.	Rep. App.	Dem. App.
GA	6	1	3	5	1	1
HI	0	0	0	0	0	0
ID	0	0	0	0	0	0
IL	6	5	2	3	12	11
IA	3	2	1	1	1	0
IN	1	0	0	2	1	0
KS	3	0	0	0	0	0
KY	5	5	4	3	4	2
LA	4	2	4	2	0	2
ME	0	1	0	1	1	1
MD	0	0	2	2	1	2
MA	0	2	1	1	3	12
MI	5	6	9	8	9	8
MN	0	1	0	0	0	0
MO	5	3	5	6	8	7
MS	0	0	1	1	0	0
MT	0	0	1	0	1	1
NV	3	1	0	1	0	0
NE	1	0	0	1	1	0
NH	1	3	0	0	0	0
NJ	6	3	5	5	9	3
NM	3	5	2	2	1	0
NY	1	7	1	2	1	6
NC	2	1	3	5	1	2
ND	0	0	0	0	0	0
OH	9	10	6	7	11	7
OK	0	0	2	0	1	0
OR	0	2	1	2	1	2
PA	5	2	4	3	4	5
RI	0	1	0	0	0	1
SC	1	0	1	2	0	0
SD	1	2	2	1	1	3
TN	6	2	3	0	1	1
TX	2	4	7	2	6	9
UT	1	0	1	1	0	0
VT	0	0	0	1	1	0
VA	2	1	3	3	0	1
WA	0	2	1	1	4	3
WV	0	1	0	0	0	0
WI	0	1	5	4	1	3
WY	0	7	0	0	0	0

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