Field Experiments Testing the Impact of Radio Advertisements on Electoral Competition

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Results from previous studies of campaign spending imply that equal-sized grants to both incumbents and challengers are a net benefit to challengers, who on average spend less money and derive greater marginal returns from each additional dollar. This study provides an experimental test of this proposition. Cities holding mayoral elections in November 2005 and 2006 were randomly assigned to broadcast nonpartisan radio ads that stated the names of the mayoral candidates, reminded listeners about the date of the upcoming election, and encouraged them to vote. Consistent with the findings of previous studies on the differential effects of incumbent and challenger campaign spending on election outcomes, we find that these radio ads produced substantially more competitive elections. The borderline statistical significance of our results, however, invites replication of this experiment.

The proposition that incumbents dominate elections ranks among the most robust empirical regularities in political science. Since World War II, reelection rates for members of the U.S. House have exceeded 90%. Jacobson (2004, 23) reports that between 1946 and 2002, only 1.7% of House officeholders seeking reelection have been defeated in primaries and only 6.1% have lost general elections. Incumbency is no less potent in municipal elections. Mayoral incumbents in cities and towns across the United States were reelected at a 91% rate in 2005 (Welfley, Slater, and Daniel 2005).

Scholars have advanced a variety of explanations for the electoral dominance of incumbents (cf. Ansolabehere and Snyder 2002). Some emphasize incumbent resources (Gerber 1998; Green and Krasno 1988, 1990), including opportunities to perform constituency service (Cain, Ferejohn, and Fiorina 1987; Fiorina 1977, 1989; Mayhew 1974), particularly in an era of weak party ties (Erikson 1972; Ferejohn 1977), reluctant entry by strategic challengers (Cox and Katz 1996, 2002; Jacobson and Kernell 1983), and redistricting (Cox and Katz 2002). One idea that runs through many of these explanations is that incumbents are better known than challengers (Mann and Wolfinger 1980). Jacobson, for example, points out that "[a]t the most basic level, people hesitate to vote for candidates they know nothing at all about. Among the most consistent findings produced by studies of congressional voters over the past generation is that simple knowledge of who the candidates are is strongly connected to voting behavior" (2004, 122). Jacobson (2004) goes on to argue that one important reason that incumbents do so well in House elections is that voters are more apt to remember their names; he reports that in surveys conducted between 1980 and 2000, 46% of respondents on average recalled the incumbent's name, but only 16% could recall the name of the challenger.

The name-recognition advantage that incumbents enjoy has been a central component of the longstanding debate about the impact of campaign spending on election outcomes. Scholars such as Jacobson (1978, 1985), Abramowitz (1991), and most recently Gerber (2004) argue that spending by challengers is more effective than spending by incumbents, perhaps reflecting the relative obscurity of challengers. Green and Krasno (1988, 1990),

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Levitt (1994), and Erikson and Palfrey (2000) find incumbent and challenger spending to have similar effects, with challengers enjoying a smaller edge in spending efficiency than suggested initially by Jacobson (1978). All of these studies, however, imply that policies which grant resources to both incumbents and challengers will advantage challengers because of diminishing marginal returns. The average incumbent outspends the average challenger, so an equal grant to both candidates works to the challenger's advantage.

Taken together, the literatures on incumbency advantage and campaign spending suggest the following hypothesis: efforts that boost constituents' familiarity with both challengers and incumbents will strengthen challenger performance and enhance electoral competition. Our study tests this hypothesis by examining the effect of nonpartisan radio advertisements broadcasted in randomly assigned jurisdictions holding municipal elections in November 2005 and 2006. Like congressional elections, municipal elections are typically low-salience affairs, a feature that arguably enhances the advantages that incumbents enjoy against their lesser-known challenger opponents. Our central hypothesis is that by elevating the salience of a mayoral contest and mentioning the names of all the mayoral candidates, including the names of overmatched challengers, the radio campaign increases electoral competitiveness.

The significance of this inquiry extends well beyond the specific effects of a single radio campaign. Although the mass media and radio in particular have for decades attracted scholarly attention (e.g., Berelson, Lazarsfeld, and McPhee 1954), the number of studies that systematically evaluate the effects of radio on voting behavior is surprisingly small. Geer and Geer (2003, 70) focus on voter reactions to positive versus negative radio advertising and acknowledge that radio's effects on voting behavior remain largely unaddressed. Nonexperimental studies find that candidates who broadcast ads on radio tend to do better than those who do not (McCleneghan 1987), but these studies leave open the question of whether radio advertising affects outcomes or merely represents a marker for better-funded and more professionalized campaigns. Descriptive studies find that candidates make widespread use of radio at both the federal and municipal levels (Herrnson 2000; Strachan 2003). Using survey data from two states with competitive Senate races in 2002 (Arkansas and Missouri), Overby and Barth (2006) propose other reasons why research on political radio advertisements may be important. Overby and Barth (2006) report that political advertisements broadcasted on radio exert greater influence over voting decisions than do television ads, possibly because they are perceived to impart more information than television as a result of greater repetition and length. But again, this nonexperimental evidence may not show convincingly that radio advertising alters candidate preference.

This article breaks new ground by conducting a randomized field experiment to assess the effects of nonpartisan radio advertising. Municipal elections have several advantages. First, they allow us to study the effects of radio in campaign environments that would naturally attract radio broadcasting on the grounds that other media (television or newsprint, for example) are usually prohibitively costly or difficult to target to a geographically compact area (Ansolabehere and Gerber 1994). Mayoral elections, due to their low salience, are also ideal laboratories within which to study the effects of radio advertising. The fact that these elections typically occur with little competition from other campaign communication makes it easier to isolate the effects of our intervention. Although the external validity of our results for federal legislative election years remains an open question, the experiment does provide useful information about low-salience elections in which radio communication occurs in a campaign environment amid few competing messages. Given the lopsided nature of municipal elections and legislative elections at the state and federal levels, the applicability of these findings is potentially quite broad.

This essay is organized as follows. First, we describe the procedure by which the experimental sample was created and the way in which observations were randomly assigned to treatment and control groups. Next, we describe the content and timing of two radio campaigns, an initial experiment in 2005 and a replication study in 2006. We then explain the statistical models used to test the hypotheses that these radio ads enhance competitiveness. After presenting our results, we use a Bayesian approach to update prior findings about the effects of campaign spending in light of the experimental results obtained here. We conclude by commenting on the theoretical and policy implications of our results and by suggesting directions for future research.

Experimental Design

Political scientists have increasingly turned to field experiments to isolate the impact of various activities on voter turnout (Gerber and Green 2000; Green and Gerber 2004) and voter preferences (Gerber 2004). Experimentation is a research method in which units of observation are assigned randomly to treatment and control groups. Field experiments, as distinct from laboratory experiments, study the effects of an intervention within a naturalistic

setting. In this case, the units of observation are cities, the intervention is a radio campaign, and the dependent variable is the closeness of the election as determined by the incumbent's share of the vote. This section describes two experiments. The first occurred in 2005 and generated results on which the initial version of this report was based. On the suggestion of reviewers, we conducted a replication study in 2006 using the same research design.

Sample Construction

Of the nation's 1,183 cities and towns with populations of over 30,000, 281 municipalities held mayoral elections in November 2005. The cost of broadcast radio advertisements in the various localities ranged from $9 per point in Billings, Montana, to $922 per point in Palmdale and Hawthorne, California.[1] The average cost per point in the population of election-holding cities was $164 per point. Due to resource limitations, we excluded cities and towns where cost for radio advertisements exceeded $111 per point, reducing the population to 151 localities. The average cost per point in the subpopulation of cities was $63.

In order to increase the statistical power of our experiment, we sought to create a sample of observations that, within experimental strata, were as homogeneous as possible. We gathered detailed information about the institutional and political characteristics of mayoral elections in each of the 151 cities and matched pairs of municipalities based on criteria thought to affect competitiveness. These matching criteria were voter turnout in the previous mayoral election, incumbent vote share in the previous mayoral election, whether mayoral elections are partisan or nonpartisan, and whether the 2005 mayoral election was contested. All of the cities and towns included in the final sample were municipalities in which the local executive is selected by popular vote (as opposed to appointment by the city or town council). Using the criteria described above, we identified 28 closely matched pairs of cities in an effort to make the treatment group as similar as possible in terms of observable characteristics. Once the matching exercise was completed, we randomly assigned one city in each pair to the treatment group and the other to the control group. The full 28-pair design is the basis of another report by the authors, which examines the effects of our radio campaign on voter turnout (Panagopoulos and Green 2006). For the purposes of this article, which focuses on electoral competitiveness, we restrict our attention to 33 cities—16 in the treatment group and 17 in the control group—in which an incumbent mayor ran opposed. As we demonstrate below, within the set of 33 observations, random assignment generated experimental groups that have closely balanced observable characteristics.[2]

In November 2006, 105 municipalities held mayoral elections. Using the same four matching criteria as in 2005, we repeated the matching exercise to create 11 pairs, half of which were randomly assigned to the treatment group. Of these, 16 cities—seven in the treatment group and nine in the control group—featured elections in which incumbent mayors ran opposed in 2006. The average cost per point for cities in the 2006 sample was $100, raising the average cost per point in the combined sample of 49 observations to $75.

The analyses that follow present details about the experimental results separately for each of the two experiments. We also combine the two experiments to present overall results for the 49 elections.

Radio Treatment

Localities in the treatment group were exposed to 60-second radio advertisements that presented a nonpartisan get-out-the-vote message to listeners.[3] The size of the media buy was varied in each municipality so that cities or towns were exposed to 50, 70, or 90 gross ratings points (GRPs) of radio advertising. In order to conserve money, purchase of 70 or 90 GRPs was restricted to less expensive media markets. Cities randomly assigned to the treatment group whose cost-per-point was less than $30 were treated with 90 GRPs. Cities randomly assigned to the treatment group whose cost-per-point was greater than $30 but less than $40 were treated with 70 GRPs. All other treatment

[1]There are 286 Arbitron Metro Markets in the United States corresponding to cities (or urban regions) of various sizes. Reaching the audience in each market is measured in ratings points, with 1 ratings point being equal to 1% of the number of total listeners living in the market. When running an advertising campaign, summing the number of points for all ads aired over the duration of the campaign determines the gross ratings points (GRP) which have been achieved. Theoretically, 100 GRPs means 100% of the market was exposed to the ad. But as some people may hear the spot multiple times and others not at all, advertisers have to measure other factors: *Reach* (the percentage of the market that have heard the spot one or more times) and *Frequency* (the number of times they have heard the spot). Thus, 100 GRPs can also mean 50% of the audience heard the spot an average of two times, or 25% heard it four times, or any other combination that equals 100.

[2]The winnowing of the larger population of cities to a smaller population of cities with affordable advertising rates does not introduce "selection bias," in the usual sense of that term. Our randomized experiment still provides unbiased estimates of the treatment effect within the population of inexpensive media markets.

[3]Every effort was made to be certain the content of the message would be received as a nonpartisan appeal. During the course of the experiment, we were contacted by two individuals (a candidate and a journalist in two separate localities included in the treatment group) who expressed their view that the content of the appeal did not appear to favor any candidate or party expressly.

cities received 50 GRPs. This design implies that our statistical analysis must control for the cost-per-point of radio ads because intercity variation in the volume of GRPs is random within, but not across, strata. (In effect, we will be analyzing three distinct randomized experiments, each occurring within populations with different advertising rates.) Total media expenditures to conduct the experiment in the 49 jurisdictions included in the analysis amounted to $77,166.

Radio advertisements were broadcasted from November 1 through November 7 in 2005 and from October 31 to November 6 in 2006. Details about the size of the radio media buy in each municipality included in the treatment group are presented in Table 1. Advertisements were professionally recorded and produced by a partnering political consulting and media firm. A media consulting firm selected the radio stations within each market, favoring stations that reached a broad audience. The media schedule was designed to reach a demographically diverse but receptive audience. Ads were broadcasted during music and news/talk program formats across markets. Springfield, Massachusetts, is a typical example. A total of 70 gross ratings points were purchased to reach voters in Springfield. Ads were broadcasted to capitalize on peak audience times during the work week, early morning traffic (6–10 AM) and afternoon rush hour (3–7 PM) as well as throughout the day (10 AM–3 PM). Additional ads were aired during the weekend. A total of 41 GRPs were aired on WMAS FM (94.7), a popular music station. Twenty-nine GRPs were aired on WHYN AM (560), a news and talk radio station.[4]

Voters in each locality were urged to vote on Election Day, and the ads included the names, incumbency status, and party affiliations (where applicable) of the main candidates in each race. It should be stressed that the intervention was strictly nonpartisan in nature. We were especially sensitive to any elements that could be construed as negative commentary about incumbents.[5] The radio scripts were designed to pique voters' interest in the contest and provide the names of the candidates, but scripts make no evaluative remarks. For example, the following sample script was used for Syracuse, New York:

> Many people don't realize how important local government is. But think about it. Your local government is in charge of things that affect your life every day: police protection, transportation, garbage collection, tax assessment. From fire departments to libraries to safe drinking water—it's all part of local government.
>
> Here's where you come in: Voting. If you're a registered voter in SYRACUSE, you have an opportunity to shape the direction of your city by electing the mayor and other local officials. On Tuesday, November 8th, residents of SYRACUSE will vote to decide whether to RE-elect Democratic MAYOR MATTHEW DRISCOLL or to support his opponent Republican JOANNIE MAHONEY.
>
> Take part in shaping your city's future. Be sure to vote on November 8th.
>
> Paid for by the Institution for Social and Policy Studies, a nonpartisan organization that encourages citizens to take an active role in their communities.

Our hypothesis is that this nonpartisan communication, which places all candidates on an equal footing, boosts challengers' electoral performance by mitigating advantages in resources and name recognition that incumbents typically enjoy.

Statistical Models and Results

Random assignment ensures that, in advance of the experimental intervention, the treatment and control groups have the same expected levels of electoral competitiveness. One by-product of random assignment is that the background attributes of the observations in the experimental groups should be similarly distributed. This expectation is easily confirmed using regression. The dependent variable is the assigned level of GRPs, as described in Table 1, and the independent variables are the assignment strata and the four background covariates used to form the sample (past incumbent vote share, past turnout, partisan balloting, and statewide elections). This regression permits an F-test of the significance of these four covariates, which, as expected, is insignificant: $F(4,41) = 0.95$, $p = .44$. Expanding the randomization check to include the number of years the incumbent served in office and the number of candidates running the previous election leaves the result unchanged ($F(6,39) = 1.15$, $p = .35$). Having confirmed that random assignment produced balanced treatment

[4]It is difficult to translate the number of GRPs into the number of times that an ad was aired, but interested readers can examine the details of our ad purchases at the website http://research.yale.edu/vote/Radio%20Ad%20Buys.xls.

[5]If anything, incumbents could have been slightly advantaged by the radio script, which always mentioned incumbents first (see Krosnick, Miller, and Tichy [2003] on primacy effects).

Table 1 Sample and Matching Criteria

City	CPP Strata	Turnout (t−1)	Partisan	Incumbent Vote Share (t − 1)	Radio Buy (GRPs)	Year
Bristol CT	HIGH	40	Y	51	50	2005
Fall River MA	HIGH	39	N	51	50	2005
Chicopee MA	MODERATE	41	N	51	0	2005
Dayton OH	HIGH	45	N	52	50	2005
New Britain CT	HIGH	38	Y	52	50	2005
Greeley CO	MODERATE	51	N	52	0	2005
Provo UT	HIGH	26	N	52	0	2005
Springfield MA	MODERATE	34	N	53	70	2005
Middletown CT	HIGH	41	Y	53	0	2005
Fairfield CT	HIGH	56	Y	54	0	2005
Pittsfield MA	HIGH	45	N	54	0	2005
Nampa ID	MODERATE	19	N	54	0	2005
York PA	HIGH	22	Y	54	0	2005
Waterloo IA	LOW	25	N	55	90	2005
Attleboro MA	HIGH	26	N	57	0	2005
Sandy City UT	HIGH	24	N	58	50	2005
Syracuse NY	HIGH	48	Y	60	50	2005
West Haven CT	HIGH	42	Y	60	50	2005
Toledo OH	HIGH	43	N	61	50	2005
Jackson MI	HIGH	18	N	62	50	2005
Lima OH	LOW	45	N	62	0	2005
Norwalk CT	HIGH	45	Y	62	0	2005
Moorehead MN	LOW	37	N	62	0	2005
Council Bluffs IA	HIGH	27	N	65	0	2005
Fayetteville NC	MODERATE	21	N	66	70	2005
Scranton PA	HIGH	48	Y	67	50	2005
Stamford CT	HIGH	37	Y	67	0	2005
Torrington CT	HIGH	49	Y	68	0	2005
Worcester MA	HIGH	17	N	73	50	2005
Coeur d'Alene ID	LOW	26	N	76	0	2005
Fairborn OH	HIGH	30	N	77	50	2005
Holyoke MA	MODERATE	35	N	80	0	2005
Greenville NC	MODERATE	11	N	93	70	2005
Seaside CA	HIGH	53	N	39	50	2006
Gresham OR	HIGH	53	N	41	0	2006
Fontana CA	HIGH	39	N	47	0	2006
Montclair CA	HIGH	44	N	50	0	2006
El Cajon CA	HIGH	39	N	51	0	2006
Cathedral City CA	LOW	61	N	51	90	2006
San Luis Obispo CA	MODERATE	71	N	52	0	2006
Reno NV	LOW	51	N	54	90	2006
Richfield MN	HIGH	78	N	54	0	2006
Rochester MN	LOW	69	N	55	0	2006
Augusta GA	LOW	45	N	56	90	2006
Brooklyn Park MN	HIGH	64	N	57	50	2006
Bellevue NE	HIGH	37	N	66	50	2006
Ann Arbor MI	HIGH	60	Y	69	50	2006
West Sacramento CA	HIGH	69	N	69	0	2006
Louisville Metro KY	HIGH	51	Y	71	0	2006

and control groups, we now estimate the effects of the experimental ad campaign on competitiveness.

The dependent variable in our analysis is the difference between the vote percentage won by the incumbent in 2005 or 2006 and his or her vote percentage in the previous election. This dependent variable has the advantages of simplicity and efficiency: conceptually, we want to know whether the ads cause the incumbents' electoral performance to improve or deteriorate; statistically, examining change rather than levels greatly reduces the amount of noise in the outcome variable. As we point out in the appendix, other constructions of the dependent variable, such as the vote margin separating the incumbent from the closest challenger, produce substantively similar conclusions.

In order to estimate the effects of the radio buys (as measured in GRPs), linear regression was applied to two nested models. The first includes three regressors: radio GRPs and two strata dummies that account for the fact that random assignment was conducted within price strata.

$$\begin{aligned} &IncumbentVoteShare_t - IncumbentVoteShare_{t-1} \\ &\quad = \beta_0 + \beta_1 RadioGRPs + \beta_2 ModerateCostStratum \\ &\qquad + \beta_3 HighCostStratum + u \end{aligned} \tag{1}$$

Equation (1) expresses change in incumbent vote share as a linear function of the treatment, covariates, and a disturbance term (u). Because the level of radio GRPs is randomly assigned, it is statistically independent of the disturbance, which satisfies the key assumption necessary for unbiased causal inference.

The second specification includes as covariates past turnout, partisan balloting, and statewide elections (past incumbent vote share is already part of the model).

$$\begin{aligned} &IncumbentVoteShare_t - IncumbentVoteShare_{t-1} \\ &\quad = \beta_0 + \beta_1 RadioGRPs + \beta_2 ModerateCostStratum \\ &\qquad + \beta_3 HighCostStratum + \beta_4 PastVoterTurnout \\ &\qquad + \beta_5 PartisanBallot + \beta_6 StatewideElection + u \end{aligned} \tag{2}$$

Both equations (1) and (2) satisfy the requirements for unbiased estimation of β_1. The advantage of including covariates in equation (2) is that it potentially reduces the standard errors associated with the estimate of β_1.

The results of the two regressions are shown in Table 2, which presents results for the 2005 experiment, the 2006 replication, and both studies analyzed jointly.[6] The estimates associated with equation (1) are reported in the columns with the heading "Strata Only." In both years, the estimated effect of the radio ads is negative, which is consistent with the underlying hypothesis that these radio ads improve the electoral fortunes of challengers. Although the magnitude of the point estimate is larger in 2005 than 2006, possibly reflecting the fact the greater volume of political communications during an even-numbered year, the difference between the coefficients is small in relationship to their standard errors, which makes pooling across years appropriate. The pooled regression implies that each one-point GRP purchase lowers the incumbent's vote share by .078 percentage-points (SE = .059). This estimate implies that in cities where 50 GRPs were purchased, the incumbent's vote margin (relative to the previous election) declined by 3.9 percentage-points; 70 GRPs lowered the incumbent's performance by 5.5 percentage-points; and 90 GRPS lowered the outcome by 7.0 percentage-points. These estimates far outstrip the apparent turnout effects of the radio ads, which appear to be less than 3 percentage-points per 100 GRPs.[7] It appears that the vote choice effect we observe is primarily driven by preference change rather than by mobilization.

In an effort to dampen some of the variability associated with these elections, the results presented under the heading "Strata and Covariates" control for the background covariates listed in equation (2). The results are essentially unchanged in terms of the magnitude and statistical precision of the estimated treatment effect (−.088, SE = .062). Unfortunately, these control variables do not improve the precision with which the treatment effect is estimated, as the RMSE increases slightly when these additional degrees of freedom are consumed. Ordinarily, one might not report the results for this regression model, but we do so in order to replicate exactly the analyses that we earlier reported after the 2005 experiment.[8] The results suggest both the stability of the estimates over time and the fact that controlling for background characteristics seems to lead to estimates that, if

[6] When estimating the model using data for both years, we also include a dummy variable for year. We have also estimated the pooled model including interactions between year and each of the cost strata. The estimated treatment effects are slightly stronger than what is reported in Table 2.

[7] Depending on the specification, the estimates combining 2005 and 2006 data range from approximately 1 to 3 percentage-points, with a 3 percentage-point standard error. For similar results using only 2005 results, see Panagopoulos and Green (2006).

[8] An earlier version of this essay also reported results using Iteratively Reweighted Least Squares (IRLS) in order to dampen the effects of outliers. The IRLS results for 2006 with and without covariates are −.087 (SE = .070) and −.088 (SE = .077), respectively. These results are downplayed here only because they are so similar to the OLS results we report.

TABLE 2 Ordinary Least Squares Estimates of the Effects of Radio Advertisements (in GRPs) on the Change in the Share of Vote Won by the Incumbent

	2005		*2006*		*Pooled*	
	Strata Only	***Strata and Covariates***	***Strata Only***	***Strata and Covariates***	***Strata Only***	***Strata and Covariates***
Independent Variables						
Gross Ratings Points	−.103	−.116	−.059	−.112	−.078	−.088
	(.079)	(.083)	(.112)	(.136)	(.059)	(.062)
Turnout in Prior Mayoral Election		.045		−.197		−.085
		(.286)		(.316)		(.194)
Partisan ballot?		−10.720		8.138		−6.216
(1 = yes, 0 = no)		(7.410)		(15.691)		(5.899)
Statewide election?		−1.623		18.387		1.630
(1 = yes, 0 = no)		(8.166)		(21.133)		(6.889)
Moderate Cost-per-point	−4.251	−9.283	−13.618	−11.299	−5.588	−8.569
Stratum Dummy	(5.825)	(6.568)	(13.303)	(15.559)	(5.124)	(5.649)
High Cost-per-point	11.860	6.771	13.598	16.647	13.319	11.095
Stratum Dummy	(7.304)	(7.901)	(9.204)	(11.068)	(5.242)	(5.597)
Year Dummy					8.118	7.125
(1 = 2006, 0 = 2005)					(4.014)	(6.382)
N	33	33	16	16	49	49
RMSE	13.42	13.43	12.58	13.80	12.82	12.86
R^2	.17	.25	.24	.32	.27	.32

Notes: The dependent variable in the analyses is *change in incumbent vote share*, which, for the entire sample, ranges from −37 to 30, with a mean of 0.3 and standard deviation of 14.4. Standard errors in parentheses. Sample restricted to cities with incumbents running against at least one opposing candidate. See appendix for additional regression diagnostics.

anything, are larger in magnitude than those based on equation (1).

Taken together, the OLS point estimates suggest that the treatment had a strong negative effect on incumbent vote share. However, the coefficient of −.078 with a standard error of .059 implies a one-tailed p-value of .098. Bootstrapping the regression confirms that 91.6% of the 100,000 replications generate negative estimated treatment effects. Evidently, the statistical precision of the treatment effect falls just short of conventional $p < .05$ levels. As Gill and Walker (2005) point out, the classical approach to hypothesis testing is equivalent to a Bayesian framework for the special case in which the analyst has noninformative priors (i.e., priors with infinite variance). That special case arguably does not apply here, as this experiment was inspired by widely held prior beliefs about the relative effects of campaign spending by incumbents and challengers. The task for the next section is to specify the priors implied by the extant literature and show how one might update them using the experimental results reported here.

Placing the Experimental Results in Bayesian Perspective

Gill (2002) and Gill and Walker (2005) argue forcefully for the incorporation of priors into assessments of causal effects. The central Bayesian argument that runs through their work is that posterior assessments of causal effects are a blend of prior beliefs and new information. So long as one is able to characterize one's priors in terms of a probability distribution, it is relatively easy to generate a posterior distribution that is a function of both the priors and the experimental results. As Gill and Walker (2005) point out, the practical challenge is characterizing priors with sufficient specificity to support this calculation. In this section, we characterize the prior distribution in general terms by examining the spending effects implied by reported research results, drawing on works with very different modeling assumptions and research designs. We then examine what kinds of posterior distributions would emerge from a range of different assumptions about the location and dispersion of the prior distribution.

Extracting priors from the campaign spending literature is complicated by the fact that a great deal of uncertainty surrounds the proper way to estimate the effects of incumbent and challenger spending. As Gerber (2004) points out in his path-breaking work connecting the observational literature on campaign spending to the experimental evaluation of how dollars translate into votes, the methodological deadlock that has beset the observational literature is precisely the reason for the recent turn to field experimentation.

That said, a closer look at the implications of research findings derived from very different estimation approaches show them to be surprisingly similar with respect to the core hypothesis of this article. Table 3 reports the estimates and implications from three well-known studies of campaign spending in U.S. House elections, all of which regress vote outcomes on the log of challenger spending and the log of incumbent spending.[9] Using instrumental variables regression, Jacobson (1990, 340; column 5) obtains a coefficient for challenger spending of 2.877 and for incumbent spending of 1.523. Erikson and Palfrey (2000, 604; column 1), applying OLS to the most closely contested races so as to minimize the endogeneity problem, obtain estimates of 4.11 and 4.04 for challenger and incumbent spending, respectively. Analyzing a sample of repeat challenger-incumbent contests, Levitt (1994, 788; column 3) obtains 1.04 and 0.61. Although the coefficients differ, they have similar implications when it comes to predicting the effects of a grant of free air time to both candidates.

Consider the effects of a grant of $7,500 (the average cost of 100 GRPs, in 2005 dollars) to a challenger spending $10,000 and an incumbent spending $100,000. Table 3 calculates the net vote gain according to each of the three sets of research findings and shows that challengers gain votes at a rate of $1.61 per vote (Erikson and Palfrey), $2.17 per vote (Jacobson), or $6.73 per vote (Levitt).[10] The cost per vote for the challenger rises when we next consider a scenario in which the challenger spends $25,000 against an incumbent who spends $100,000. Now the cost-per-vote estimates are $4.50, $5.56, and $16.48. When a challenger is overmatched in terms of spending, all of these models imply that an exogenous grant to both candidates greatly benefits the challenger.[11]

How do these cost-per-vote figures compare with comparable figures from our experiment? The average city had approximately 50,000 registered voters, of whom approximately 20,300 voted.[12] The most precisely estimated effect of radio from Table 2 is .078 per GRP, which implies that 100 GRPs produces a gain of 1,583 votes for the challenger. The average cost of one GRP was approximately $75. Thus, the cost per challenger vote is $7,500 ÷ 1583 = $4.74 in 2005 dollars. Given that the typical mayoral election features a grossly overmatched challenger, this figure is quite consistent with the extrapolations derived from the campaign spending literature.

Let us now calculate what the cost-per-vote estimates in Table 3 imply for the estimated treatment effects reported in Table 2. Radio ads are assumed to be tantamount to a name-recognition enhancing expenditure of $7,500 on behalf of both the challenger and the incumbent, as both names are mentioned. For the scenario in which the challenger spends $25,000 against an incumbent spending $100,000, the Erikson and Palfrey estimates imply a radio treatment effect of .082 per GRP. Jacobson's estimate implies a radio treatment effect of .066, and Levitt's estimates imply .022.

Since the three regressions are based on some of the same House elections data, the estimates are not independent and cannot be aggregated in any straightforward way. However, we can simulate the effects of stipulating one prior or another on the posterior distribution that emerges from the radio experiment results. Table 4 shows how the posterior estimates and their standard errors (Gill 2002, 138) vary across simulated priors, each patterned after the three campaign spending essays. The rows of the table reflect different assumptions about the mean of the prior distribution over possible radio treatment effects. In this table, higher means imply stronger net effects for challengers. The columns of the table reflect different assumptions about the standard deviation of the prior distribution. Higher standard deviations imply greater initial

[9] Jacobson (1990) and Erikson and Palfrey (2000) use the log of spending plus a constant of $5,000, using 1978 dollars. Levitt (1994) recodes spending below $1,000 to $1,000 before taking logs, using 1990 dollars.

[10] The cost per vote for the challenger is calculated as follows. First, a grant of $7,500 is recalculated in 1978 dollars (Erikson and Palfrey, Jacobson) and in 1990 dollars (Levitt). The real value of the grant is used to calculate incumbent/challenger gains. For example, the gains to a challenger who initially spends $10,000 in Jacobson's model is (ln(initial spending of $10,000) + (additional spending $7,500 converted to 1978 dollars) – ln(initial spending of $10,000)) multiplied by the coefficient of challenger spending. The net vote gain is calculated by subtracting the corresponding vote gain for the incumbent. The cost per vote for the challenger is the value of $7,500 in 1978 dollars divided by the net vote gain of the challenger.

[11] Note that the fact that we deployed our ads at the very end of the campaign meant that candidates were unable to respond strategically to the new equilibrium in time to affect the outcome. Table 3 is essentially calculating the shift in equilibrium that occurs before incumbents have a chance to respond, which is realistic in this situation but not in all situations involving in-kind grants.

[12] Calculations assume 70% of the total population is eligible to vote and 75% of the eligible population is registered. In our sample of cities, the average turnout rate among those registered to vote is 40.6%.

TABLE 3 Cost Per Vote Gained by Challengers Implied by Three Regression Analyses of Campaign Spending

Incumbent Spending	Challenger Spending	Incumbent Spending Effect[a]	Challenger Spending Effect[a]	Incumbent Gains from $7,500[b]	Challenger Gains from $7,500[b]	Number of Voters	Incumbent Vote Gain	Challenger Vote Gain	Net Vote Gain	Cost Per Vote If Both Candidates Spent an Additional $7,500
					Erikson and Palfrey (2000)					
$100,000	$10,000	−4.04	4.11	−0.098	0.904	190,000	−187	1718	1531	$1.61
$100,000	$25,000	−4.04	4.11	−0.098	0.386	190,000	−187	733	546	$4.50
$100,000	$50,000	−4.04	4.11	−0.098	0.197	190,000	−187	375	188	$13.05
					Jacobson (1990)					
$100,000	$10,000	−1.523	2.877	−0.037	0.633	190,000	−70	1202	1132	$2.17
$100,000	$25,000	−1.523	2.877	−0.037	0.270	190,000	−70	513	443	$5.56
$100,000	$50,000	−1.523	2.877	−0.037	0.138	190,000	−70	263	192	$12.80
					Levitt (1994)					
$100,000	$10,000	−0.61	1.04	−0.030	0.425	190,000	−57	807	750	$6.73
$100,000	$25,000	−0.61	1.04	−0.030	0.191	190,000	−57	363	306	$16.48
$100,000	$50,000	−0.61	1.04	−0.030	0.100	190,000	−57	190	133	$37.96
Spending Effect Implied by Radio Experiment					0.078	20,300		1583		$4.74

[a]Spending effects are the gains, in percentage points, of a one-unit increase in the log of spending.
[b]Dollar amount in 2005 dollars. Vote gains are calculated by converting this figure to 1978 dollars (Erikson and Palfrey 2000; Jacobson 1990) or 1990 dollars (Levitt 1994).

TABLE 4 Posterior Distributions of the Effects of Radio Advertisements, by Prior Mean and Standard Deviation

Prior	*Standard Deviation of Prior Distribution*					
(Mean)	***0.500***	***0.250***	***0.200***	***0.150***	***0.100***	***0.050***
0	0.077	0.074	0.071	0.067	0.057	0.032
	(0.059)	(0.058)	(0.057)	(0.055)	(0.051)	(0.038)
0.025	0.077	0.075	0.073	0.071	0.064	0.047
	(0.059)	(0.058)	(0.057)	(0.055)	(0.051)	(0.038)
0.050	0.077	0.076	0.075	0.074	0.070	0.061
	(0.059)	(0.058)	(0.057)	(0.055)	(0.051)	(0.038)
0.075	0.078	0.078	0.078	0.077	0.077	0.076
	(0.059)	(0.058)	(0.057)	(0.055)	(0.051)	(0.038)
0.100	0.078	0.079	0.079	0.081	0.083	0.091
	(0.059)	(0.058)	(0.057)	(0.055)	(0.051)	(0.038)

Note: Cells represent the mean and standard deviation of the posterior distribution of challenger vote gain per GRP based on prior assumptions and on the OLS results presented in Table 2 (b = .078, SE = .059). The entries are scaled in the same units but opposite sign as the regression coefficients in Table 2.

uncertainty about either the credibility of the observational point estimates or their relevance for our application. With very high standard deviations (column 1), the posterior distributions are very similar to the estimates reported in Table 2. As the standard deviation decreases, the posterior results are more strongly influenced by the prior mean. As the lower right-hand corner of Table 4 indicates, the posterior distribution has a high mean and small variance when either Jacobson-type or Erickson/Palfrey-type priors are assumed. A prior of .075 with a standard deviation of .05, for example, implies a posterior distribution with a mean of .076 and standard deviation of .038. With weaker Levitt-type priors of .025, the posterior mean remains substantively large (ranging from .047 to .077), but the t-ratio hovers around 1.25. Thus, the empirical results presented here range from convincing to suggestive, depending on which priors one forms based on the campaign spending literature.

It should be stressed that even if one's subjective priors were ex ante exactly in line with the eventual results of the 2005 and 2006 experiments, scientific progress is gauged by the degree to which the posterior mean and variance differ from the prior mean and variance. Thus, the case where Jacobson-type priors of .075 with a standard deviation of .05 meld with new data showing a point estimate of .078 with a standard error of .059 produces an advance even though the posterior mean is .076. Although at first glance it appears that the experiment has simply confirmed what one already suspected based on the literature, there is a profound difference between a posterior distribution of .075 with a standard deviation of .05 (the state of knowledge before the new data) and a posterior distribution of .076 with a standard deviation of .038 (the state of knowledge after seeing the new data). In the former case, the betting odds that this intervention has a positive effect are 14 to 1; in the latter case, the odds are 43 to 1.

Conclusion

As the first field experiment to examine the effects of political advertising on radio, this study offers a number of methodological and substantive insights. In terms of methodology, this experiment demonstrates the feasibility of studying radio's effects using random assignment in real-world settings. The research paradigm used here is a systematic and reproducible method that can be applied to further research on radio and other forms of mass communication.

To date, field experiments on the effects of the mass media are rare, as most researchers rely on survey research and laboratory experiments. This lopsided balance in favor of survey and laboratory approaches does not necessarily reflect the superiority of these methods. Laboratory experiments leave open the questions of whether listeners in a simulated environment absorb radio messages in the same way that they would under ordinary conditions and whether one can measure outcomes in an unobtrusive and externally valid manner. Surveys are often more expensive than field experiments but not necessarily more reliable. Exposure to radio advertisements is potentially correlated with unobserved causes of voting, particularly if media campaigns are directed at certain segments of the electorate. Absent random assignment, a survey draws on strong substantive assumptions in order to generate causal inferences.

Observational literatures, such as extant work on campaign finance, reach a point at which the remaining uncertainty is not sampling variability, but rather whether the modeling assumptions are correct. Even with a very large number of observations, such that the nominal standard errors have essentially been reduced to zero, there remains the open question of whether the underlying results are biased (Gerber, Green, and Kaplan 2004). To advance a literature that has reached this point, one must adduce experimental evidence (or perhaps evidence based on natural experiments or near-random assignment). That is the spirit in which this article is written. The observational literature strongly implies but has by no means settled the hypothesis that an equal grant of resources to both challengers and incumbents will generate a net benefit to challengers. Our article does not settle the matter either,

but it presents both novel evidence regarding this hypothesis and a fresh research paradigm for evaluating it.

By showing that field experimentation is possible, this study opens up a new and potentially valuable methodological path. At the same time, this field experiment has several limitations, most notably problems of statistical power. Because this is the first study to evaluate the impact of radio advertisements on electoral behavior, its statistical power was difficult to calculate ex ante. Now that the research community has a sense of what to expect from a study of this kind, we and other scholars can design follow-up studies that, in conjunction with ours, will have smaller standard errors. One of the great advantages of randomized experimentation is the potential for accumulating experimental evidence, thereby converging on an underlying parameter with ever-greater precision. The Bayesian perspective taken in this article breaks away from the classical framework in which effects are declared significant or nonsignificant and instead provides a framework within which causal conclusions can be continuously updated.

This updating process includes the exploration of possible interactions between advertising and electoral context. Additional research must investigate whether the results change when the messages are partisan, when advertising is directed toward other types of elective offices, and when the challengers have varying levels of prior name recognition. This more nuanced line of experiments will illuminate the mechanisms for the posited advertising effect and the scope conditions within which the effect is likely to obtain.

Another limitation of the current study is that it fails to exploit the full power of radio as a medium. Budgetary constraints prevented us from broadcasting more than 90 gross ratings points in any given market. An expanded study would allow us to procure more comprehensive coverage in the treatment markets. The ease with which radio ads are produced also makes it possible to vary message content in future experiments. Voters in the current study, for example, were exposed exclusively to nonpartisan get-out-the-vote messages. Still greater opportunities exist to harness the demographic targeting potential of radio in subsequent experiments to study effects on select audiences using, for example, Spanish-language stations.

Despite these limitations and the need for additional research, our findings have important substantive implications. For more than a quarter century, political scientists have argued that differential name recognition is an important source of incumbency advantages. A corollary argument is that challengers have more to gain from campaign spending than their better-known incumbent adversaries. As Gary Jacobson puts it, "[b]ecause voters are demonstrably reluctant to vote for candidates they know nothing about, challengers have a great deal to gain by making themselves better (and, of course, more favorably) known to the electorate. Their level of campaign activity . . . thus has a strong influence on how well they do at the polls" (1990, 335). If name recognition is indeed the active ingredient that causes challengers to reap disproportionate gains from campaign spending, an exogenous intervention that publicizes the candidates' names in low-salience elections should, on average, benefit challengers. Our experiment provides important new support for this core hypothesis.

This is by no means the only type of experiment that one could perform in order to test the name recognition hypothesis. Gerber's (2004) study of campaign mail's effect on vote choice suggests an experimental paradigm whereby one could test the differential effects of two alternative messages, one that focuses solely on the incumbent's drawbacks without mentioning the challenger and another that does both. The name-recognition hypothesis suggests that an obscure challenger should make especially large gains in the latter condition, as a result of increased name recognition. The name-recognition hypothesis could be tested using an array of standard campaign tactics, ranging from robotic phone calls to voter guides.

From the standpoint of public policy, our findings bolster the longstanding argument that even-handed campaign finance laws can enhance electoral competitiveness. As Primo, Milyo, and Groseclose (2006) point out, this hypothesis has received surprisingly little attention from observational researchers, despite the adoption of public financing laws in seven states. Their analysis of gubernatorial competitiveness between 1978 and 2004 provides some limited support for the idea that public funding enhances competitiveness, but the small number of observations and high level of visibility of gubernatorial candidates make this an imperfect test. It may be years before newly enacted public funding laws generate statistically reliable changes in the electoral competitiveness of legislative elections.[13] In the meantime, students of electoral politics may use experiments to speak to this central and enduring policy question.

By promoting awareness of upcoming elections and providing minimal information about the candidates, subsidies to both candidates—such as nonpartisan radio ads—appear to reduce the advantages of incumbency. To date, this implication has been at the forefront of the campaign finance literature but never tested directly. The present study contributes the first direct test and confirmation of this claim.

[13] As Mayer and Wood (1995) point out, the effects of public funding are also contingent on the emergence of challengers. They find that public financing narrowed the spending gap between challengers and incumbents in Wisconsin between 1964 and 1990, but more incumbents over time ran uncontested.

APPENDIX TABLE 1 Regression Diagnostics for the Pooled Regressions Presented in Table 2

Diagnostic Test	Pooled Regression with Strata Dummies	Pooled Regression with Strata Dummies and Covariates
Breusch-Pagan/Cook-Weisberg test for heteroskedasticity	p = .14	p = .33
Shapiro-Wilk W test for normality of residuals	p = .79	p = .94
Inter-quartile range test for residual outliers	IQR = 17.4, no outliers	IQR = 16.0, no outliers
Test of whether relationship between GRPs and Y is linear	F(2,42) = .35, p = .71	F(2,39) = .34, p = .71

APPENDIX TABLE 2 Ordinary Least Squares Estimates of the Effects of Radio Advertisements (in GRPs) on the Margin of Victory between First- and Second-Place Candidates for Mayor

	2005		*2006*		*Pooled*	
	Strata Only	*Strata and Covariates*	*Strata Only*	*Strata and Covariates*	*Strata Only*	*Strata and Covariates*
Independent Variables						
Gross Ratings Points	−.126	−.182	−.107	−.222	−.116	−.154*
	(.098)	(.088)	(.145)	(.153)	(.077)	(.074)
Turnout in Prior Mayoral Election		−.061		−.257		−.244
		(.328)		(.360)		(.238)
Partisan ballot?		−20.270		36.418		−10.001
(1 = yes, 0 = no)		(8.008)		(19.112)		(7.148)
Statewide election?		−.119		32.873		3.105
(1 = yes, 0 = no)		(8.811)		(23.736)		(8.244)
Moderate Cost-per-point	19.768	7.568	−15.325	−8.490	14.374	7.699
Stratum Dummy	(7.242)	(7.084)	(17.227)	(17.438)	(6.669)	(6.850)
High Cost-per-point	30.559	18.287	21.925	32.408	27.265	23.224
Stratum Dummy	(9.080)	(8.495)	(11.919)	(12.369)	(6.824)	(6.733)
Incumbent Vote Percentage		.394		.201		.515
in Prior Election		(.282)		(.541)		(.239)
Year Dummy					6.083	9.160
(1 = 2006, 0 = 2005)					(5.225)	(7.638)
N	33	33	16	16	49	49
RMSE	16.68	14.27	16.30	15.41	16.69	15.39
R^2	.38	.61	.28	.57	.32	.47

Notes: The dependent variable is the *margin of victory* between the first- and second-place candidates in the mayoral election, which for the entire sample has a mean of 28.8 and a standard deviation of 19.4. Standard errors in parentheses. Sample restricted to cities with incumbents running against at least one opposing candidate. *Estimated treatment effect is $p < .05$, one-tailed test.

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