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The effects of lawn signs on vote outcomes: Results from four randomized field experiments☆

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ABSTRACT

Although lawn signs rank among the most widely used campaign tactics, little scholarly attention has been paid to the question of whether they actually generate votes. Working in collaboration with a congressional candidate, a mayoral candidate, an independent expenditure campaign directed against a gubernatorial candidate, and a candidate for county commissioner, we tested the effects of lawn signs by planting them in randomly selected voting precincts. Electoral results pooled over all four studies suggest that signs increased advertising candidates' vote shares. Results also provide some evidence that the effects of lawn signs spill over into adjacent untreated voting precincts.

1. Introduction

Lawn signs are one of the few campaign tactics deployed by candidates for every level of government in the United States. Inexpensive and relatively easy to deploy, lawn signs are a tactic available to even the most obscure and underfunded candidate for a down-ballot office. Indeed, the efflorescence of roadside lawn signs is often one of the few outward manifestations of a low-salience election.

Although campaign tactics ranging from door-to-door canvassing to robotic phone calls have been evaluated by a vast array of field experiments conducted during the past fifteen years (for summaries of this literature, see Green and Gerber (2015), Bedolla and Michelson (2012), and Green et al. (2013)), lawn signs have largely escaped scholarly attention. Panagopoulos (2009) finds that hand-held placards announcing Election Day promote voter turnout, but hand-held placards involve a human element that lawn signs lack, and non-partisan encouragements to vote are different from efforts to build vote support for a candidate. Current understanding of lawn signs derives largely from campaign how-to guides, which offer anecdote-driven recommendations from campaign professionals and candidates. However, these guides are equivocal on the question of whether lawn signs are effective. For example, *The Political Campaign Desk Reference* recommends planting signs as early as local election laws allow (McNamara, 2012, p. 171), whereas Blodgett et al. (2008) dismiss lawn signs as ineffective on the grounds that they do nothing to persuade undecided voters (p. 130). Shaw (2009) describes a number of campaigns that supposedly used lawn signs to great effect but concedes that "You never know what will work" (p. 152).

The present study represents the first rigorous evaluation of the effectiveness of lawn signs. Working in collaboration with four campaigns in different electoral contexts, we tested the effects of lawn signs by planting them in randomly selected voting precincts. The paper begins by describing the theoretical mechanisms by which lawn signs are hypothesized to affect vote choice and how those mechanisms may vary by electoral context. We then explain the experimental design and its implications for statistical analysis. We estimate a statistical model that allows for both direct exposure

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to signs in targeted voting precincts and indirect exposure to signs in adjacent voting precincts. Although no single experiment is conclusive statistically, electoral results from all four studies taken together suggest that the signs significantly increased advertising candidates' vote margins. Results also indicate that the effects of lawn signs spill over into adjacent untreated voting precincts. Working within a Bayesian learning framework, we show that even an initial skeptic would update her views in light of these four studies.

2. Theoretical backdrop

Researchers have long observed that electoral outcomes are correlated with exposure to signs (Kaid, 1977; Sommer, 1979). The literature on campaign effects offers at least three theoretical reasons for thinking that this relationship is causal, in other words, that lawn signs increase the share of the vote won by the advertising candidate.

In the context of low-salience elections or relatively unknown candidates, lawn signs may help build name recognition. Recent experiments suggest that mere exposure to candidates' names increases their popularity among voters (Kam and Zechmeister, 2013), although the effect of mere exposure seems to dissipate when voters are provided with other relevant information, such as candidates' occupation or incumbency status. Indeed, Kam and Zechmeister's quasi-experimental test of lawn signs on behalf of a fictitious candidate for city council is the only study we are aware of that directly assesses the effects of lawn signs on vote preference; they find that signs conveying only the candidate's name had a large effect on vote intentions expressed in a survey they conducted at the start of a low-salience election campaign (p.13). One testable implication of the name recognition hypothesis is that signs should have weaker effects in high-salience races, where name recognition is widespread. This hypothesis suggests that we should expect to find weaker effects of signage in a hard fought governor's election than in contests for lower office.

A second hypothesis is that the presence of signs is interpreted as a signal of candidate quality or viability (Krasno, 1994). Lawn signs suggest to voters that the advertising candidate's campaign has the resources and staffing necessary to purchase and deploy signage. Such costly signals are thought to influence vote choice (Potter and Gray, 2008) by creating bandwagon effects akin to those set in motion by pre-election polls (Ansolabehere and Iyengar, 1994). These signaling effects are thought to be especially strong when signs are displayed on private property because voters are influenced by their neighbors' candidate endorsements (Huckfeldt and Sprague, 1992).

Finally, signs may convey information that guides vote choice. Three of the four signs described below used text and graphics to convey the ideological location or partisan affiliation of the advertising candidate. In electoral contexts where voters know relatively little about the candidates, such cues may have strong effects on vote choice (Mann and Wolfinger, 1980; Popkin, 1994). For example, in their experimental study Schaffner and Streb (2002) found that surveys that merely included candidates' party affiliations profoundly affected the distribution of vote preferences, as this cue allowed respondents to better express their own party preferences. Our theoretical predictions concerning the mechanisms at play in each of the four experiments are summarized in Table 1.

Are there theoretical reasons to be skeptical about the effects of lawn signs? Two important caveats are suggested by the literature on vote choice. First, lawn signs represent an impersonal mode of campaign communication akin to direct mail or automated phone calls, the persuasive effects of which have occasionally proven to be significant (Gerber, 2004; Rogers and Middleton, 2012) but have just as often proven to be limited (Cardy, 2005; Shaw et al., 2012; Cubbison, 2015). Second, the effects of signage may decay during the time that elapses between exposure and the expression of vote preference. The campaigns described below deployed signs in residential precincts, not immediately outside polling locations. To the extent that information diminishes in salience or is forgotten altogether, the effects of signage may fail to manifest themselves in the actual vote tally.

Taken together, these competing hypotheses offer compelling reasons for believing either that lawn signs work or that they do not, divergent conjectures reflected in the campaign manuals cited above. After presenting the results of our four experiments, we will return to these prior beliefs about the efficacy of lawn signs and update the views of optimists, skeptics, and agnostics in light of the evidence.

3. Experimental design

The unit of analysis in each of the experiments was the voting precinct. Voting precincts are the lowest level of aggregation at which voting choices are made public, so the effects of the lawn signs on voters' political preferences can be measured directly. Because voting precincts are relatively contained geographic areas, the residents of a precinct can be thoroughly exposed to a large dose of signs. Our design faced two relatively minor complications. First, in Experiments 1, 2, and 4, some districts were designated either as "must-treat" or "untreatable." Because these units could not be randomly assigned to treatment conditions, they are excluded entirely from our analyses. Second, in Experiments 1 and 4, we encountered some failure-to-treat: some units assigned to get lawn signs did not receive them. We will conduct all analyses according to treatment assignment, not treatment receipt.[1]

Experiment 1 took place across two counties in upstate New York containing a total of 97 voting precincts, 88 of which were treatable. In Experiment 2, our initial sample size was 128 precincts in the City of Albany. However, because some precincts were regarded by the campaign as "must-treat" locations, our experiment was restricted to 69 precincts. Experiment 3 took place in 5 of 9 Fairfax County, Virginia districts, comprising a total of 131 precincts. Experiment 4 was conducted in 88 of Cumberland County, Pennsylvania's 107 voting precincts, as the remainder were designated as "untreatable" by the campaign.

In all four experiments, precincts were assigned to treatment conditions using restricted randomization (Morgan and Rubin, 2012). In order to address the issue of spillover, whereby voters in one voting precinct are exposed to experimental lawn signs in a neighboring precinct, the randomization protocol ensured that two neighboring precincts could not be assigned to direct treatment at the same time. The precise algorithm used to allocate treatment assignments to units was different for each of the four experiments; the Albany, Virginia, and Pennsylvania experiments also included covariate information in the randomization protocols in order to increase statistical power. These procedures induced differential probabilities of assignments. For example, centrally located precincts are less likely to be directly treated. We address the statistical complications of differential treatment probabilities by including inverse probability weights. For complete descriptions of the

[1] Our estimates therefore gauge the intent-to-treat effect, or the effect of assigned (rather than actual) treatment (see Gerber and Green (2012, Chapter 5)). The discrepancy between actual and assigned treatment is small, and therefore the intent-to-treat effect understates the average treatment effect only by a factor of $22/23 = 0.96$ in Experiment 1 and $17/20 = 0.85$ in Experiment 4.

Table 1
Expected mechanisms.

Study	Election context	Election closeness	Salience	Sign type	Expected mechanism[a]
Experiment 1	General	Contested	Medium	Road Sign	Signs signal name recognition and viability
Experiment 2	Municipal Primary	Landslide	Low	Yard Sign	Signs signal name recognition, partisan endorsement, and support among neighbors
Experiment 3	General	Toss up	High	Road Sign	Signs signal ideological cues
Experiment 4	County Primary	Contested	Low	Road Sign	Signs signal name recognition, viability, and ideological cues

[a] The sign used in Experiment 1 does not mention party or ideology, only the candidate's name. We expect that the sign operates primarily through the viability channel, though given the medium salience of the election, it may also increase name recognition. In Experiment 2, the sign was planted in supporters' yards and mentioned the candidate's name, party, and office sought. We expect the main mechanism triggered by the sign is the support among neighbors signal, though name recognition and party endorsement may also be at work. The sign used in Experiment 3 was designed to mimic signs used for house or garage sales, suggesting that the advertising candidate's opponent was "For Sale." This sign primarily signals ideology. In Experiment 4, the sign displayed the candidates' names and office sought, with the text "The Conservative Team," including no direct mention of party beyond a graphic depicting an elephant. Given the low salience of the election, we expect that the sign operates through the name recognition channel, though it may also signal viability and ideology.

restricted randomization procedure used in each experiment, see the appendix. The number of precincts in each condition in each experiment is summarized in Table 2.

Our experimental treatments differed along two dimensions: content and placement. Below we describe the signs and the manner in which they were deployed (See Fig. 1).

3.1. Experiment 1: treatment

The lawn sign used in Experiment 1 was designed to be conventional in all respects. The size and shape were the standard 18 × 24 inch rectangle. The colors were white type on a blue background, which is a common format, especially for Democrats. The candidate's last name commanded the largest font. Somewhat smaller font was used for his first name and the word "Congress," which appeared in blue type on a white background. The bottom of the sign gave the candidate's campaign website. The sign contained no graphics or photos.

Signs were deployed by researchers working with the campaign two weekends prior to Election Day. When planting the lawn signs in the treatment precincts, the guiding principle was to hew closely to the procedures used by actual campaigns. This meant searching for locations where the signs were likely to be seen by as many potential voters as possible. Upon arriving in a treatment precinct, the campaign team looked for public land around the largest intersections and busiest roads. Given the paucity of public land in some precincts, the team frequently placed clusters of signs - usually around four or five - in these high-profile locations. When the most prominent areas had been treated with clusters of signs, the team continued to place clusters of signs in other high-visibility public locations until the allocation of 40 signs was exhausted. One precinct assigned to treatment was not actually treated because the campaign team could not find any suitable roads on which to plant signs.

3.2. Experiment 2: treatment

This experiment also used a conventional lawn sign. The size and shape were the standard 18 × 24 inch rectangle. The colors were white type on a blue and green background. The candidate's last name is displayed in large type face with "Democrat for Mayor" below in smaller font. No further information was included on the sign.

Table 2
Treatment assignments.

Study	Control	Adjacent	Treated	Total
Experiment 1	16	49	23	88
Experiment 2	13	41	15	69
Experiment 3	25	76	30	131
Experiment 4	24	44	20	88

An important difference between Experiment 2 and the other experiments concerns the distribution of signs. In this study, signs were given to supporters residing in treatment locations to display on their own private lawns. This distinction is important for the interpretation of the results, as signs planted on private land may convey an endorsement by the landowner, whereas signs planted by campaign workers do not necessarily imply support by local residents. The signs in this campaign were placed by residents during the last four weeks of the election.

3.3. Experiment 3: treatment

The sign used in the Virginia experiment was quite different — it was a negative sign attacking an opponent rather than a positive sign supporting a candidate. Moreover, the sign was visually arresting, as it mimicked a "For Sale" sign commonly used for selling automobiles or houses. The text of the sign read: "For Sale: Terry McAuliffe. Don't Sellout Virgina [sic] on November 5". A notice at the bottom indicated that the sign was "Paid for by FreedomWorks for America and not authorized by any candidate or candidates' committee. FreedomWorks for America — 202-942-7642". The sign was designed to highlight the fact that the opposing candidate was Democratic fundraiser before becoming a candidate for higher office.

Three weeks prior to the election, the signs were placed in clumps of five in three locations within each treatment precinct. Signs were successfully planted in all assigned treatment locations. We were able to verify the exact placement of signs because the volunteers placing them were instructed to take geotagged photos of the clumps of signs with their smartphones.

3.4. Treatment: experiment 4

The sign used in Pennsylvania promoted two candidates for County Commissioner: Gary Eichelberger and Rick Schin. It was headlined, "The Conservative Team" and featured an elephant graphic to signal the candidates' Republican affiliation, though the word "Republican" is not on the sign. A total of 200 signs were planted in the two weeks prior to election day by a campaign worker. We encountered some failure-to-treat: three precincts selected for treatment did not receive signs. We nevertheless analyze the experiment according to the randomly assigned treatments, ignoring noncompliance altogether.

4. Statistical model

One of the core assumptions required for unbiased causal inference is the stable unit treatment value assumption (Rubin,

Fig. 1. Signs.

1986), which implies that subjects are affected solely by the treatment to which they are assigned; treatments assigned or administered to others are assumed to be inconsequential. This assumption is jeopardized when treatments spillover as the result of communication or contagion. Because lawn signs can be seen by anyone who passes by, untreated precincts may be affected by the treatments that neighboring precincts receive. In effect, there may be potential outcomes other than "treated" and "untreated"; some precincts may be partially treated. To reestablish "stable" potential outcomes, we must develop a model of potential outcomes that includes this intermediate case.

Let $Y_i(d)$ be the potential outcome of each precinct i, where d indicates one of three possible inputs: direct treatment with lawn signs, indirect treatment because lawn signs have been planted in an adjacent precinct, and no treatment. Following the language of Aronow and Samii (2013), we are implementing an exposure model in which the potential outcomes of each precinct can take on only one of three values. $Y_i(d)$ is called a potential outcome because it is the outcome that a precinct would manifest if it were to receive the input d. Only one of the three potential outcomes is actually observed, depending on the actual deployment of lawn signs; the other two potential outcomes remain unknown. Nevertheless, we can define the causal effect of direct treatment versus no treatment for each precinct as $Y_i(direct)-Y_i(none)$ and the causal effect of spillover treatment versus no treatment for each precinct as $Y_i(spillover)-Y_i(none)$. Although these precinct-level causal effects cannot be observed or estimated, we may define and estimate two average causal effects across all precincts. The first is the average treatment effect of direct treatment versus control (i.e., the direct treatment effect). The second is the effect of adjacency versus control (i.e., the spillover effect).

Recovering these estimands from our experiment is complicated by the fact that voting precincts have varying probabilities of assignment to treatment and to adjacency-to-treatment. As discussed in Gerber and Green (2012, chapters 4 and 8), when assignment probabilities vary, the effect of treatment cannot be recovered using a comparison of unweighted average outcomes. For example, comparing the average vote margin in precincts assigned to receive the treatment to the average vote margin in precincts assigned to the non-adjacent control group yields biased and inconsistent estimates of the average effect of lawn signs. An asymptotically unbiased estimation approach reweights the data before computing group averages (see Gerber and Green, 2012, chapter 3). The weight for each observation in experimental group $d \in \{direct, spillover, none\}$ is the inverse of the probability of it being assigned to group d. (Observations with probabilities of zero or one, i.e., the untreatable or must-treat precincts, are necessarily excluded.) When using regression to estimate average treatment effects, we use weighted least squares rather than ordinary least squares.

Our basic regression model is equivalent to comparing weighted means:

$$Y_i = \beta_0 + \beta_1 D_{1i} + \beta_2 D_{2i} + u_i \tag{1}$$

where Y_i is candidate vote share, D_{1i} is an indicator variable scored 1 if the voting precinct is assigned to lawn signs, D_{2i} is an indicator variable scored 1 if the voting precinct is adjacent to a precinct assigned to lawn signs, and u_i is the unobserved disturbance term. Here, β_1 represents the average effect of direct treatment (compared to no direct or indirect treatment), and β_2 represents the average effect of spillover from an adjacent treatment (compared to no direct or indirect treatment).

In order to improve the precision with which these causal parameters are estimated, we augment our regression model to include covariates that are expected to be predictive of vote outcomes. Different sets of covariates were available in each experiment. We strove to include results from past elections of the same type where possible (i.e., past congressional elections in Experiment 1, a past mayoral election in Experiment 2, etc.) in addition to past presidential vote margin. In Experiment 2, we also included the campaign's measure of the number of registered Democrats in each precinct. The full list of covariates used in each experiment is listed at the foot of results tables below.

Equation (2) shows the covariate-adjusted specification used in Experiment 1. The covariates included the vote margin for Barack Obama in the 2008 presidential election (V_i^{P2008}) and for the Democratic congressional candidates in the 2006, 2008, and 2010 elections (V_i^{C2006}, V_i^{C2008}, and V_i^{C2010}):

$$Y_i = \beta_0 + \beta_1 D_{1i} + \beta_2 D_{2i} + \gamma_1 V_i^{P2008} + \gamma_2 V_i^{C2006} + \gamma_3 V_i^{C2008} + \gamma_4 V_i^{C2010} + u_i^{'} \quad (2)$$

The regression coefficients for the covariates (γ_1, γ_2, γ_3, and γ_4) have no causal interpretation; the reason to include these covariates is to reduce disturbance variability and eliminate chance imbalances among experimental groups. The results below indicate that these covariates were highly predictive of outcomes in Experiment 1 and greatly improve the precision with which the direct treatment and spillover effects are estimated. Covariates also proved to be highly prognostic of outcomes in Experiment 3, which also took place in a general election. Because the standard errors in Experiments 1 and 3 are so much smaller after controlling for covariates, our interpretation focuses primarily on the covariate-adjusted estimates. In Experiments 2 and 4, the covariates were less predictive of outcomes and do little to improve the precision of our estimated treatment effects. We nevertheless focus our attention on the covariate-adjusted estimates when interpreting our results and summarizing all four studies via fixed-effects meta analysis.

When conducting hypothesis tests, we will focus exclusively on randomization-based tests of the joint hypothesis of no direct or indirect effects. The procedure is similar in all four experiments. We first obtain an observed *F*-statistic based on Equation (2), where the restricted model constrains β_1 and β_2 to both equal zero. We then simulate the distribution of this *F*-statistic under the sharp null hypothesis of no effect by recomputing the *F*-statistic under 10,000 possible (restricted) random assignments. Our *p*-value reflects the proportion of random assignments in which the simulated *F*-statistic exceeds the observed *F*-statistic.

5. Results

5.1. Experiment 1: results

Table 3 shows the weighted regression estimate of the effects of direct and indirect treatment using the regression specifications in Equations (1) and (2). Without covariates, the estimated effect of direct treatment on vote share is 2.5 percentage points (robust SE = 2.7), and the estimated spillover effect is 3.7 percentage points (robust SE = 2.7). The estimates sharpen considerably when the regression model is augmented with past vote outcomes as covariates. The estimated effect of direct treatment remains 2.5 percentage points, but the standard error falls sharply (robust SE = 1.7). The estimated spillover effect decreases to 1.8 percentage points, and its standard error falls to 1.6 percentage points. Using randomization inference, we fail to reject the joint null hypothesis of no direct or indirect effects (p=0.22). The results suggest that the signs exerted a direct treatment effect, although the effect falls short of conventional levels of statistical significance. The estimates also provide some tentative evidence of spillovers from treated to adjacent precincts.

Table 3
Impact of lawn signs on vote share (Experiment 1).

	Vote share	
	Model 1	Model 2
Assigned lawn signs (n = 23)	0.025 (0.027)	0.025 (0.017)
Adjacent to lawn signs (n = 49)	0.037 (0.027)	0.018 (0.016)
Constant	0.390 (0.020)	0.015 (0.031)
Covariate adjustment	No	Yes
N	88	88
R^2	0.031	0.823

Covariates: congressional vote margin '06, '08, '10 and presidential vote margin '08. Standard errors in parentheses.

Table 4
Impact of lawn signs on vote share (Experiment 2).

	Vote share	
	Model 1	Model 2
Assigned lawn signs (n = 15)	0.009 (0.054)	−0.014 (0.057)
Adjacent to lawn signs (n = 41)	0.012 (0.046)	0.004 (0.045)
Constant	0.659 (0.039)	0.287 (0.131)
Covariate adjustment	No	Yes
N	69	69
R^2	0.001	0.253

Covariates: registered democrats and mayoral vote margin '05 and '09. Standard errors in parentheses.

5.2. Experiment 2: results

The Albany signs campaign was expected to produce especially large effects, as the signs themselves were planted in supporters' yards rather than along public roadways. The statistical results, however, turned out to be murky (Table 4). Without adjustment, the signs appeared to increase vote share for Sheehan by 0.9 percentage points, but with adjustment, appeared to decrease her vote share by 1.4 points. Ironically, controlling for covariates increases our estimated standard errors. In either model, the standard errors are so large that we come away without a clear sense of the average treatment effects. Evidently, precinct-level studies in primary elections require a much larger population of precincts because one cannot rely on covariates to improve precision. The randomization inference test of the joint hypothesis of no direct or indirect effect yields a p-value of 0.90.

Table 5
Impact of lawn signs on vote share (Experiment 3).

	Vote share	
	Model 1	Model 2
Assigned lawn signs (n = 30)	0.042 (0.016)	0.018 (0.009)
Adjacent to lawn signs (n = 76)	0.042 (0.013)	0.018 (0.007)
Constant	0.302 (0.011)	0.780 (0.025)
Covariate adjustment	No	Yes
N	131	131
	0.094	0.825

Covariates: gubernatorial vote margin '09 and presidential vote margin '12. Standard errors in parentheses.

Table 6
Impact of lawn signs on vote share (Experiment 4).

	Vote share	
	Model 1	Model 2
Assigned lawn signs (n = 20)	−0.013 (0.028)	−0.012 (0.026)
Adjacent to lawn signs (n = 44)	−0.023 (0.022)	−0.020 (0.021)
Constant	0.548 (0.017)	0.532 (0.082)
Covariate adjustment	No	Yes
N	88	88
R^2	0.012	0.172

Covariates: gubernatorial vote margin '02, '06, '10 and presidential vote margin '00, '04, '08. Standard errors in parentheses.

5.3. Experiment 3: results

Table 5 reports the results of the anti-McAuliffe sign campaign. To maintain consistency, we report the effects of the signs on the vote share of McAuliffe's opponent, Ken Cuccinelli. The results suggest, as expected, that the signs increased Republican vote shares. The inclusion of covariates reduces the estimated standard errors substantially, so we focus on those estimates. Cuccinelli's vote share increased by 1.8 percentage points in treated precincts and 1.8 percentage points in adjacent precincts. A randomization inference test of the joint null hypothesis that neither direct nor adjacent signs affected outcomes generates a p-value of 0.02.

5.4. Experiment 4: results

We expected that signs would be especially effective in the Pennsylvania experiment, as name recognition of the candidates was thought to be low. However, focusing on the covariate-adjusted estimates in Table 6, directly treated precincts saw 1.2 percentage points lower vote share for Eichelberger and Schin; indirect treatment decreased vote share by 2.0 points. The standard errors associated with both estimates are quite large because covariates again fail to predict outcomes in this primary election. The randomization inference test shows that we cannot reject the null hypothesis no direct or indirect effects ($p = 0.77$).

5.5. Effects on turnout

We find that lawn signs had essentially no effect on turnout. Pooling the covariate-adjusted estimates according to Equation (3) below, we find that direct effect of lawn signs on total votes cast in a precinct was 7.2 votes, with a standard error of 9.5 votes. We interpret this null finding on turnout to mean that any positive impact on vote share operates primarily though a persuasion mechanism, not through mobilization.

6. Bayesian integration

Considered separately, each of the four experiments provides equivocal evidence about the effects of lawn signs. That is not surprising, given that each study is somewhat underpowered due to the fact that relatively few precincts end up in the pure control group after we allow for possible spillover effects from treated precincts to adjacent precincts. One way to address the lack of power, however, is to conduct a series of replication studies and to pool the results.[2] This approach presupposes that the average of the effects across the four studies is a quantity of interest. If, however, the effects differ systematically depending on electoral context, then this average might mask theoretically relevant variation. In this analysis, we set aside differences in electoral context and features of the signs themselves in order to answer the overarching question of how well signs typically work across elections like the four we have studied.

[2] We do note, however, that the analytic choice to pool the results was not specified *ex ante* in our preanalysis plans.

Table 7
Meta-analysis: pooled vote share results.

	Direct	Direct SE	Indirect	Indirect SE
Experiment 1	0.025	(0.017)	0.018	(0.016)
Experiment 2	−0.014	(0.057)	0.004	(0.045)
Experiment 3	0.018	(0.009)	0.018	(0.007)
Experiment 4	−0.012	(0.026)	−0.020	(0.021)
Pooled results	0.017	(0.007)	0.015	(0.006)

In order to estimate the pooled average treatment effect, we conducted a fixed-effects meta-analysis of the four studies (Borenstein et al., 2009). This estimator is equivalent to the precision-weighted average of the four estimated direct treatment effects, where "precision" in this context refers to the inverse of the squared estimated standard error. Let the estimated standard error of the jth study be denoted σ_j; the weights are $W_j = 1/\sigma_j^2$, and the precision-weighted average of the four estimated average treatment effects, μ_j, is

$$\mu_{pooled} = \frac{\sum_1^J \mu_j * W_j}{\sum_1^J W_j} \tag{3}$$

As shown in Table 7, the pooled estimate of average effect of lawn signs in directly treated precincts is 1.7 percentage points, with a standard error of 0.7 percentage points. The corresponding pooled estimate for the average effect of adjacency is 1.5 percentage points, with a standard error of 0.6 percentage points. It appears that signs on average raise vote shares by just over one percentage point.

In order to quantify what one learns from this succession of four studies, Fig. 2 traces the process by which three different observers update their priors in light of the experimental evidence (Gill, 2002; Hartigan, 1983). The leftmost density plots display the priors of an agnostic observer (row 1), an observer whose priors make her optimistic about the effects of signs (row 2), and an observer whose priors make her skeptical about the effects of signs (row 3). The agnostic and the optimist are assumed to have diffuse priors whose standard deviations are 5 percentage points, whereas the skeptic's prior has a standard deviation of 1 percentage point, reflecting her confidence that lawn signs have negligible effects. Moving from left to right, the density plots show how these priors evolve in the wake of each successive experiment. The rightmost plots in each row show the posterior distributions for each observer. Although the three observers' posteriors differ, they do not differ by much; the experimental evidence has largely displaced the prior views that these observers in advance of these studies. The agnostic observer (row 1), concludes that there is a 98.8 percent chance that lawn signs increase the vote share of the advertising candidate. The optimistic observer (row 2) puts this probability at 0.991, and even the initial skeptic (row 3) concludes that this probability is 0.966. Whichever prior view comes closest to the reader's own priors, it seems apparent that the experimental evidence contributes importantly to the posterior sense of the efficacy of lawn signs, even if questions remain about the conditions under which the effect tends to be larger or smaller.

7. Conclusion

Unlike prior research on lawn signs, which mainly described the correlation between election outcomes and the prevalence of

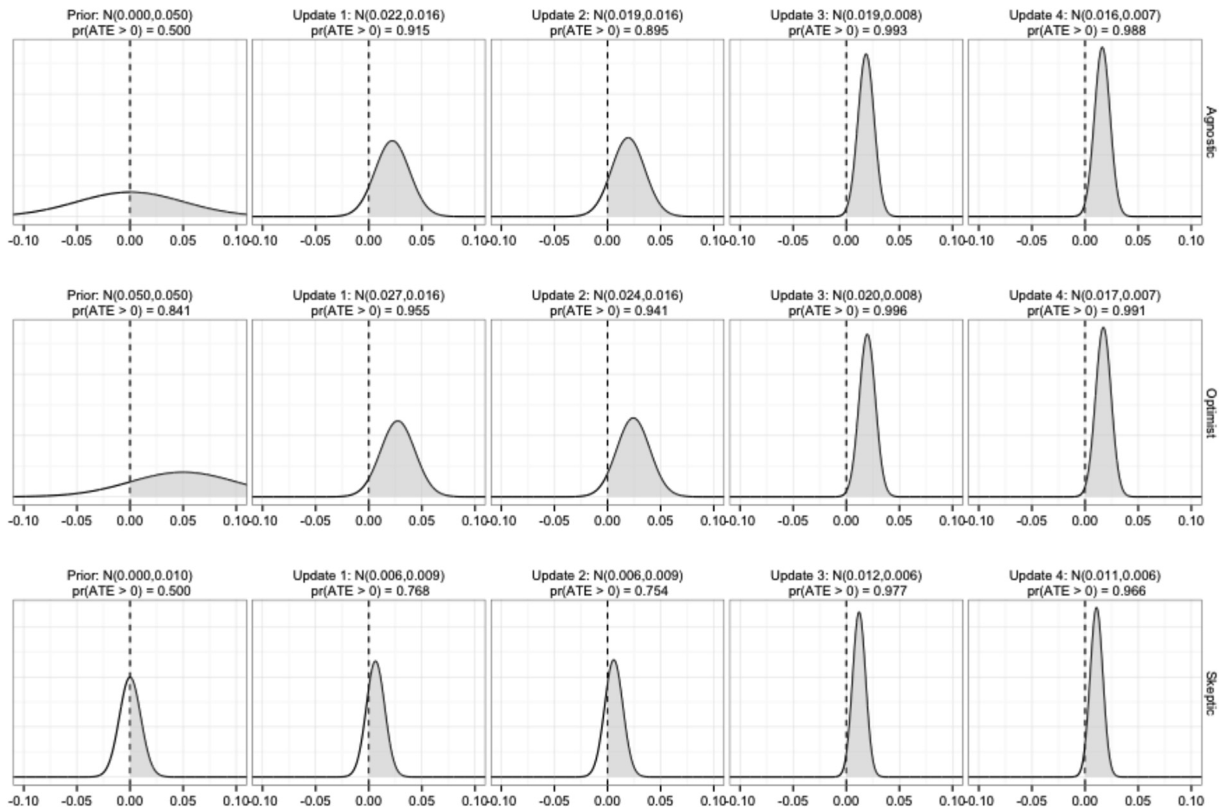

Fig. 2. Bayesian integration of four lawn signs experiments.

signage, this paper attempts to assess the causal effect of signs on election outcomes using a randomized experimental design. Experiments in which geographic units are the unit of assignment present some special technical challenges insofar as random assignment procedures require the use of GIS “shapefiles” and analytic tools. In order to assist researchers seeking to conduct this type of research, we have made our data and accompanying code available in the Supplemental materials.

Experiments of this type also present special estimation challenges given the possibility that the effects of signage spill over from treated precincts to neighboring precincts. Unbiased estimation requires the researcher to take account of the probability that each precinct is exposed to spillovers, which in turn requires simulating large numbers of possible random assignments. When estimating spillover effects, we have taken a cautious design-based approach, relying as much as possible on decisions made at the design stage rather than on modeling choices made after results have been obtained. Our randomization procedure assigned units to two levels of treatment: precincts that receive signs and adjacent precincts that otherwise are untreated. More gradations of spillovers are possible (e.g., adjacent to adjacent to treated), as are precinct linkages that are guided by topography or road networks rather than adjacency. These are directions for future work.

Although this series of experiments leaves many questions unanswered, it is also apparent that the new evidence represents an important advance over the conjectures that previously dominated the discussion of campaign signs. Pooling over the four experiments, it appears that signs typically have a modest effect on advertising candidates' vote shares – an effect that is probably greater than zero but unlikely to be large enough to alter the outcome of a contest that would otherwise be decided by more than a few percentage points. This finding puts lawn signs on par with other low-tech campaign tactics such as direct mail that generate reliable persuasion effects that tend to be small in magnitude (Gerber, 2004).[3]

From a theoretical standpoint, these findings shed light on the conditions under which voters are swayed by campaign communication. Clearly, further experimentation is needed to refine our understanding of causal mechanisms, but for now we advance some tentative conclusions. First, although signs may promote name recognition, this mechanism does not seem to be a necessary condition for signs to exert an effect on vote shares. Signs seem to have been effective in the Virginia gubernatorial election, where levels of name recognition were quite high, party cues were abundant, and where the signs mentioned only the name of the opponent. Second, signs do not seem to be especially effective when they provide partisan or ideological cues. Signs appear to have had weak effects in Pennsylvania, where ideological labels were used, and Albany, where party labels were used. Conversely, the congressional candidate in New York seemed to benefit from signs that made no mention of his party or ideology. Third, our one test of yard signs found weak effects, suggesting that the cues from neighbors failed to generate meaningful bandwagon effects. The remaining hypothesis is that signs work because they signal viability. The evidence here is ambiguous because all four signage campaigns could be said to signal viability. Future investigation of this causal mechanism might assess whether signs deployed near polling places work especially well when randomly accompanied

3 Considering only the direct effect, we estimate the cost per vote across all four experiments to be \$3.18, with a 95% confidence interval extending from \$1.70 to \$13.71. This figure is calculated from total turnout (241,613), the direct effect (1.7 points), and the total cost (\$13,045): \$13,045/(241,613 * 0.017) = \$3.18. If we include indirect effects in this calculation, the cost per vote drops to \$1.69.

by signs in other areas of the same precincts, the latter signaling substantial campaign effort and resources.

By conducting a series of experiments in different settings, we have also sought to address questions of generalizability that inevitably arise due to the many ways in which signs' effects may interact with features of the electoral context. Each of our studies generated estimated average treatment effects that fall within the margin of sampling variability of the other studies, suggesting that meaningful systematic variation in treatment effects across contexts may be limited. Still, it remains to be seen whether the pattern of results we obtained hold up when experiments are conducted other contexts. Additionally, future experiments should be conducted at a much larger scale, so that both average and heterogeneous effects can be estimated with greater precision. Given the ubiquitous use of signage in campaigns worldwide, it is unfortunate that it has so rarely been the object of field experimental research. We hope that the present studies will provide the substantive impetus and methodological template for more work of this kind.

Appendix A. Supplementary data

Supplementary data related to this article can be found at http://dx.doi.org/10.1016/j.electstud.2015.12.002.

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