

POLITICAL COMMUNICATION: INSIGHTS FROM FIELD EXPERIMENTS

DONALD P. GREEN

Columbia University

Department of Political Science

7th Floor, International Affairs Building, 420 West 118th St.

New York, NY 10027

dpg2110@columbia.edu

ALLISON J. SOVEY

Yale University

Rosenkranz Hall, Room 437, New Haven, CT 06520

allison.sovey@yale.edu

JOEL MIDDLETON

New York University

Steinhardt School

246 Greene St., New York, NY 10003

joel.middleton@nyu.edu

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Donald P. Green is Professor of Political Science at Columbia University. The author of four books and more than one hundred essays, Green's research interests span a wide array of topics: voting behavior, partisanship, campaign finance, hate crime, and research methods. Much of his current work uses field experimentation to study the ways in which political campaigns mobilize and persuade voters, but he has also conducted experimental research on the effects of the mass media, civic education classes, and criminal sentencing. With Alan Gerber, he recently co-authored a textbook on this research method titled *Field Experiments: Design, Analysis, and Interpretation* (W.W. Norton, 2012).

Allison J. Sovey is a PhD candidate, pursuing a joint degree in the Departments of Political Science and Economics at Yale University. Her research focuses on political economy and political methodology. In political economy she works on economic policy and international trade, and in methods she focuses on causal inference. Her work has appeared in the *American Journal of Political Science* and she has been awarded the J. Jacqueline and Roger B. Kaufman Fellowship, the Falk Fellowship, and the Yale University Dissertation Fellowship.

Joel Middleton is visiting assistant professor of applied statistics at the Steinhardt School at New York University. His interests include design-based estimation and causal inference in randomized experiments. He also studies voter behavior and political persuasion, and he has 10 years experience designing surveys and experimental interventions for political organizations. He received a Ph.D. in Political Science from Yale University, a Masters in Statistics from The George Washington University, a Masters in Psychology from Brown University, and his BS from Lewis and Clark College.

Empirical investigation of cause and effect in the field of political communication has long been dominated by non-experimental research. From the 1940s through the 1990s, political campaigns' effects on voter opinion and behavior were studied using survey data that compared people with different levels of exposure to campaigns and sometimes tracked these differences over time (Berelson, Lazarsfeld and McPhee 1986; Campbell 1980; Patterson and McClure 1976; Johnston, Hagen and Jamieson 2004; Freedman, Franz and Goldstein 2004). The methodological sophistication of these studies has improved over time as scholars have deployed much larger surveys (Whiteley 1988; Romer, Kenski, Winneg, Adasiewicz, and Jamieson 2006) and made use of improved technologies for measuring exposure to political messaging (Freedman and Goldstein 1999; Goldstein and Freedman 2002).

Nevertheless, an inherent limitation of non-experimental research has been a growing source of concern. Because the analyst of observational data does not have control over the process by which people encounter political communication, any observed correlation between outcomes and exposure to political messages is subject to two interpretations: (1) exposure causes outcomes, or (2) exposure is systematically related to unobserved factors that cause outcomes. For example, exposure to political advertising by presidential candidates may predict vote choice either because ads change opinions or because ads happen to coincide with viewers' pre-existing political preferences. The latter interpretation is hard to rule out when political campaigns strategically target their messages or when voters decide which messages to attend to.

One answer to this conundrum is to conduct experiments, a research method in which subjects are randomly assigned to treatment and control groups with known probabilities. The attractiveness of the experimental method derives largely from the fact that when political messages are assigned at random, there is no systematic relationship between the experimental

intervention and subjects' political proclivities. Experiments in the field of political communication may be classified into two broad categories depending on whether they are conducted in the lab or field. Lab experiments expose subjects to messages in artificial settings. Although researchers often go to great lengths to recruit subjects from the general population, to make them feel at home in the lab setting, and to measure outcomes unobtrusively, applying the findings from laboratory studies to the world of politics requires supplementary assumptions. For example, in order to study the electoral effects of negative campaign tactics, Ansolabehere and Iyengar (1995) conducted a series of lab studies demonstrating that negative advertising depresses voter turnout, as measured by expressed intention to vote in post-intervention surveys. Applying the lessons of the lab to the world of electoral politics, however, requires one to assume that subjects in the lab respond to advertising in the same way that ordinary television viewers do and that subjects' stated vote intentions correspond to the way in which they would behave in a non-lab setting.

By focusing on political communication in real-world settings, field experiments are designed to shorten the distance between the experimental setting and the political setting to which the researchers intends to generalize. The push for realism and unobtrusiveness stems from the concern that unless one conducts experiments in a naturalistic manner, some aspect of the experimental design may generate results that are idiosyncratic to the experimental design. The realism of a field experiment may be judged along four dimensions: whether the treatment used in the study resembles the intervention of interest in the world, whether the participants resemble the actors who ordinarily encounter these interventions, whether the context within which subjects receive the treatment resembles the context of interest, and whether the outcome measures resemble the actual outcomes of theoretical or practical interest. Field experiments

vary in their degrees of realism but all share in common the randomized deployment of treatments in naturalistic settings.

The aim of this chapter is to provide a brief overview of the field experimental literature in the domain of political communication. Recognizing that many readers may be unfamiliar with this style of research, we begin by describing some important features of experimental designs. This section discusses some of the challenges of conducting experiments in the field and explains how designs strike a balance between scholarly objectives and practical considerations. Next, we summarize the substantive contributions of some illustrative field experiments in three substantive domains. First, we consider public information campaigns designed to encourage voters to hold public officials accountable for performance in office. Second, we discuss individually-targeted information designed to encourage voters and taxpayers to comply with social norms. Finally, we review recent attempts to study the electoral effects of television and radio advertisements. The examples illustrate how field experiments may contribute to a broad range of important theoretical and policy debates.

2. Overview of Field Experimental Designs

In terms of research design, field experiments related to political communication may be classified along three different dimensions. The first dimension describes the unit of randomization. At the “micro” end of the spectrum are experiments in which individuals are each assigned to treatment or control conditions. On the other end of the spectrum are “place based” experiments (Boruch et. al. 2004; Boruch et. al. 2005) in which clusters of individuals, such as media markets or legislative districts, are assigned to treatment or control. A second

dimension is whether subjects are presented with different treatments over time. A design that is strictly between-subjects focuses on a single point in time, comparing subjects who were randomly assigned to treatment or control. An alternative design traces subjects (which may be aggregate units, such as regions) over time as they respond to different treatments. A third dimension is the degree of correspondence between the assigned and actual treatment. In some settings, those implementing the intervention treat everyone in the treatment group and no one in the control group. In other settings, the experiment encounters some degree of noncompliance; some members of the assigned treatment group do not receive the treatment, or some members of the control group receive the treatment.

Before considering examples that illustrate these design dimensions, let's first consider some of the practical constraints and statistical considerations that lead researchers to implement different designs. Ordinarily, researchers aim to include as many units as possible in their experiments, since precision increases as the number of units in treatment and control increases. If an electoral constituency contains 100,000 voters, one would ideally like to assign them individually to treatment and control. Sometimes individual assignment is impossible. For example, given current technology, broadcast media send messages to geographically defined groups of voters; one cannot target randomly selected voters inside a media market. Sometimes the researcher aims to answer a question that lends itself to place-based randomization: instead of seeking to assess the effect of viewing a campaign message when one's neighbors do not, the researcher may wish to assess the effects of viewing a message when neighbors view it as well.

Second, the use of between-subjects designs makes sense when researchers have many experimental units, limited capacity to vary treatments over time, or limited capacity to measure outcomes over time. When only a small number of observations are available for experimental

assignment and the experiment may be conducted repeatedly over time, researchers may compare observations both cross-sectionally and cross-temporally. The latter design is sometimes a matter of necessity when field-based research partners (e.g., political campaigns) refuse to conduct an experiment unless all units receive the treatment at some point. The so-called “stepped wedge” design rolls out the treatment in stages to selected subjects, until eventually every subject receives the treatment.

A final design dimension concerns the relationship between the assigned treatment and the treatment that subjects actually receive. Sometimes the aim of a study is to evaluate the effect of an attempt to expose subjects to a form of communication. For example, studies that evaluate the effects of direct mail (Gerber, Green, and Green 2003; Gerber, Green, and Larimer 2008) assess the effect of sending mail, not the effect of actually reading what is sent. On the other hand, sometimes researchers have a different objective: they wish to study the effects of actually receiving a message. For example, studies that examine the effects of receiving a phone call from a political campaign must contend with the fact that only a portion of those who are randomly assigned to receive a call are in fact reached by canvassers. An experiment that encounters noncompliance cannot estimate the average effect of receiving the treatment for all subjects; instead, it can only estimate the effect of the treatment among “compliers,” those who receive the treatment if and only if they are assigned to the treatment group. (For an extended discussion that defines compliers and reviews the assumptions necessary for estimation of the average treatment effect among compliers, see Gerber and Green 2012.) Whether the average effect among compliers is of theoretical interest depends on one’s research aims. If the aim is to estimate the average treatment effect of a policy that will expose everyone to the treatment, the

answer is no; if the aim is to estimate the effect on compliers of an intervention that encourages exposure, the answer is yes.

Let's now consider three examples of research designs that vary along these dimensions and the reasons that scholars might choose one design over another. Shaw et al. (2011) report the results of a study that tests the persuasive effects of pre-recorded messages on voting. The message in this experiment was an endorsement by the Republican governor of Texas, Rick Perry, of a conservative member of the State Supreme Court, who was facing reelection in a low-salience primary election. In order to assess the effects of the pre-recorded phone call on voter turnout, the calls were randomly assigned to more than 200,000 households with a history of voting in Republican primaries. To ascertain the effects on vote choice, the researchers conducted a parallel experiment in which the unit of assignment was the voting precinct. In the turnout experiment, outcomes were measured using official records indicating whether each individual voted; in the vote choice experiment, they were measured based on the number of votes that treatment and control precincts cast for the endorsed candidate. This study illustrates how the availability of outcome measures guides research design.

Another Texas experiment illustrates the advantages of tracing outcomes over time. Gerber et al. (2011) report the results of a multi-million dollar experiment in which eighteen Texas media markets were randomly assigned to varying levels of television and radio advertising. Because the number of media markets is fairly small, cross-sectional comparisons of treated and untreated markets were subject to considerable sampling variability. In this case, the treatments were rolled out gradually week by week, and outcomes were measured daily using tracking polls. The statistical power of this study derives in large part from the ability to track opinion in each media market over time. A final pair of examples illustrates the use of an

encouragement design and the way in which noncompliance affects the interpretation of the results. Albertson and Lawrence (2009) report the results of an experiment that randomly encouraged subjects to watch a Fox News debate on affirmative action and gauged its effects on support for a ballot proposition on this issue, and Washington, Mullainathan, and Azari (2010) conducted an experiment that randomly encouraged New York voters to view a debate between mayoral candidates. Post-debate surveys gauged whether respondents in the treatment and control groups watched the debate. In both experiments, some subjects in the control group watched the treatment shows without being encouraged to do so, and some members of the treatment group failed to do so despite encouragement. These experiments therefore assess the effects of the experimental shows for compliers, those who view the shows if and only if encouraged to do so.

3. Illustration of Substantive Contributions

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This section reviews research from an assortment of different domains in order to illustrate the breadth of field experimental applications.

3.1: Public information campaigns designed to promote electoral accountability

In developing countries, the quality of governance tends to be poor even in countries where elections are conducted in an open and fair manner. Why does corruption and incompetence persist when voters regularly have the opportunity to punish candidates and political parties that misbehave in office? One hypothesis is that voters fail to punish official misconduct because

they lack information about elected officials' levels of integrity and competence. This hypothesis lends itself to a field experiment in which voters are presented with "report cards" that grade officials' performances.

Two recent studies, one conducted in Mexico and another conducted in India, assess the effects of providing this type of information. A study by Banerjee et al. (2010) evaluates a multifaceted information campaign designed to inform slum dwellers in Delhi about the performance of incumbent state legislators. Legislators were graded based on their attendance at legislative sessions and local meetings, how they spent discretionary budgets, and their personal characteristics (such as whether they faced serious criminal charges). The researchers distributed report cards summarizing this information via door-to-door canvassing, group meetings, and newspapers in randomly selected polling precincts. Polling station outcomes were used to measure the treatment's effects on voter turnout and vote share for the incumbents. A similar experiment by Chong et al. (2011) gauges the effect of providing Mexican voters with information about malfeasance in local governments. Information about integrity and performance was culled from reports from Mexico's federal government, which regularly audits municipal finances. Some municipalities received high marks (they spend authorized funds appropriately and without accounting irregularities symptomatic of corruption), and others did not. The researchers randomly assigned precincts within audited municipalities to one of four experimental conditions. Control precincts received a flyer shortly before the election with information about when and where to vote; treatment precincts received a flyer with this information plus a brief summary of the federal audit's findings. Specifically, the treatments reported one of three things: the auditor's assessment of the corruption in the municipality, how much of federally authorized funds the municipality spent (an indicator of bureaucratic

competence), and how much of federally authorized money aimed at the poor the municipality spent. The outcome was the share of the vote that each precinct cast for the incumbent's party.

Banerjee et al. found that in areas where incumbent quality was poor, report cards significantly reduced votes for the incumbent. This finding is echoed by Chong et al., who find that in areas where auditors discovered corruption, leaflets that publicized corruption had substantial electoral effects, significantly reducing the vote share won by each incumbent's party in the subsequent election. Revelations about a government's failure to spend earmarked funds had no apparent effect. The implication seems to be that credible information causes voters to punish corrupt behavior, but voters are uncertain about or unmoved by allegations of bureaucratic incompetence, regardless of whether it means that the poor failed to receive authorized federal funds. The results of both studies seem to suggest that information with clear evaluative implications, such as charges of corruption, can lead voters to punish incumbents. This conclusion is bolstered by the findings of Feraz and Finan (2008) who find that random audits of Brazilian municipalities that came to a critical verdict about municipal governance led to electoral reprisal against the incumbents, especially in areas where mass media publicized this information.

3.2: Individually targeted shaming campaigns to encourage compliance with norms

One of the longstanding puzzles in social science is the paradox of collective action: despite the fact that one person's contribution is extraordinarily unlikely to affect whether the collective goal is realized, individuals nevertheless vote, volunteer, and make other personal sacrifices for collective causes. One way to resolve this theoretical puzzle is to posit that people receive

psychic benefits from doing the right thing, such as performing a civic duty. This hypothesis holds that the personal sacrifice of time or money is offset by two kinds of “selective incentives” (Olson 1965): the intrinsic satisfaction that comes from upholding a social norm and the extrinsic utility of winning social approval (or avoiding social disapproval) from others. This theoretical argument has important practical implications: rather than offer material inducements, those seeking to encourage collective action could use tactics that increase the motivation to comply with social norms.

A series of recent field experiments has tested the effectiveness of encouragements that make social norms salient. Fellner, Sausgruber, and Traxler (2009) collaborated with an Austrian tax collection agency to examine the conditions under which people who own televisions without paying the mandatory annual fee will pay the fee when confronted by an official letter from the tax collection agency. The researchers randomly varied the content of the mailings, emphasizing either (1) a threat of prosecution for tax evasion, (2) a fairness appeal to pay one’s fair share rather than forcing others to bear one’s tax burden, or (3) information stating the descriptive norm that 94% of households comply with this tax. These interventions were designed to accentuate three theoretically relevant factors: fear of punishment, concern for fairness, and conformity with perceived norms. The outcome was whether those who were apparently in arrears on their TV tax paid the tax in the wake of the letter. Their findings suggest that the letter’s effectiveness was enhanced by threats of prosecution, but not by appeals to fairness or descriptive norms. (For other large scale experiments on the effects of warning letters from tax collection agencies, see Kleven et al. 2010 and Slemrod, Blumenthal, and Christian 2001.)

A rather different pattern of results emerges when letters encourage voter turnout. A series of experiments beginning with that of Gross, Schmidt, Keating, and Saks (1974) has tested the effects of messages that manipulate the psychic benefits of voting by either encouraging people to do their civic duty or by reminding them that voting in elections is a matter of public record and that failure to vote will be a source of embarrassment. Gerber, Green, and Larimer (2008, 2010), for example, conducted a series of experiments in which tens of thousands of registered voters in Michigan were sent a letter that either (1) encouraged them to do their civic duty and vote in an upcoming election, (2) presented voters with records indicating whether they had voted in past elections and threatened to monitor whether they voted in an upcoming election, or (3) presented voters with records indicating whether they and their neighbors had voted in prior elections and threatened to update everyone about who voted in an upcoming election. Appeals to civic duty alone boosted turnout, but this effect was dwarfed by the powerful effects of disclosing voting records. These results have been replicated in other settings, again using very large samples (Mann 2010; Panagopoulos 2010; Sinclair, McConnell, and Green 2012). Other field experiments have shown that turnout increases when messages encourage voters to join an honor roll of neighbors who vote (Panagopoulos 2010) or thank voters for their past participation or interest in politics (Panagopoulos 2011), suggesting that voters participate in elections in part to win the gratitude of others and to maintain positive self-images. Although direct mail that reminds people to vote in upcoming elections tends to have negligible effects (Green and Gerber 2008; Bedolla and Michelson 2012), mail that makes social norms salient tends to produce substantial increases in turnout.

3.3: Persuasive effects of TV and radio

Since the 1940s, scholars have attempted to measure the effects of TV and radio communication on political attitudes but have rarely employed experimental designs in naturalistic settings to do so (for exceptions, see Ball-Rokeach, Rokeach and Grube 1984). Recently, several experiments have demonstrated that field-based tests are possible and shed light on longstanding theories of opinion change.

Inspired by the large non-experimental literatures on campaign spending and name recognition, Panagopoulos and Green (2008) conducted a study of mayoral elections in which the experimental intervention was a series of nonpartisan radio advertisements that mentioned the names and party affiliations of both mayoral candidates. The underlying hypothesis was that low levels of name recognition prevent challengers from running competitively against incumbents; by mentioning both candidates in an even-handed way, the intervention was expected to narrow incumbents' margins of victory. This prediction was borne out by electoral results from more than three dozen mayoral elections over a two year period. Although these statistical results fall short of conventional levels of statistical significance, they are bolstered by a follow-up study using a similar intervention with candidates for the House of Representatives (Panagopoulos and Green 2011).

One limitation of the Panagopoulos and Green (2008) study and related experiments that assess the effects of nonpartisan TV advertisements on voter turnout (Green and Vavreck 2008; Panagopoulos and Green 2011) is that they do not test the effects of partisan campaign advertisements aired by candidates. Gerber et al. (2011) attempt to fill this gap. Their experiment assesses the impact of millions of dollars of TV and radio advertising deployed by the campaign to re-elect Governor Rick Perry of Texas in 2006. As noted above, the study

randomly assigned different amounts of TV and radio ads on a weekly basis in 18 media markets over a three week period in January of 2006, at the start of campaign. Using daily tracking polls with approximately 1,000 respondents each day, the evaluation was able to gauge over-time movement in voters' preferences as advertising was switched on or off in each market. The results showed a powerful but short-lived effect of TV ads. The statistical results suggest that 1000 gross ratings points of TV advertising raised Perry's vote support by approximately 5 percentage-points during the week in which the ads were aired; a week later, the ads had no apparent effect. Perhaps surprisingly, the TV ads' effects were unaffected by whether TV ads were aired by Perry's principal opponent, suggesting that one-sided and two-sided communication had roughly the same effect.

The fact that advertising had a momentary effect presents an interesting anomaly from the standpoint of rational learning theories, which characterize the public as Bayesian information processors (Gerber and Green 1998). A Bayesian learning model would imply that an ad would move opinion markedly only if it conveyed a large amount of new information, in which case its effects should decline slowly unless the overall information environment was continually bombarding voters with important pieces of new information. But the Perry ad in this case was a feel-good one that conveyed little substantive content, and the informational environment during this period was fairly placid. Evidently, candidate ads can produce large momentary effects that are not easily reconciled with models of rational information processing.

4. Directions for Future Research

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In the domain of political communication, field experimentation remains relatively rare, but recent research suggests that this type of inquiry is both feasible and informative. In this section, we suggest some fruitful directions for future investigation, building on the brief literature review presented above.

Experiments have much to contribute to longstanding debates about the capacities of voters and the quality of public opinion. Social scientists have long been critical of the public's limited awareness of political facts (Delli Carpini and Keeter 1997), its incoherent configuration of policy attitudes (Converse 1964), and its lack of understanding of constitutional principles (McClosky and Brill 1983). On the other hand, scholars have often praised the public's ability to adjust its policy views in light of current conditions (Page and Shapiro 1983), to punish candidates based on retrospective performance evaluations (Fiorina 1981), and to use decision shortcuts in order to sort out policy choices (Brady and Sniderman 1985; Lupia 1994). These claims all rest on observational data, and their characterization of the public is situated in a party system that narrows the range of "treatments" (candidate choices, messages, and messengers) to which citizens are exposed. In such cases, experiments are informative for two reasons: they overcome problems of unobserved heterogeneity, and they deploy treatments that would not ordinarily occur. For example, when left to its own devices, the political system rarely if ever generates radio ads that publicize the names of both parties' congressional candidates. If researchers want to test theories about the effects of these out-of-equilibrium forms of communication, they have no choice but to orchestrate these interventions themselves.

The recent line of field experiments that provides voters with information relevant to electoral choice could be expanded to address a variety of longstanding propositions about voter decision making. We earlier reviewed interventions that provided information designed to assist

voters in forming a retrospective performance evaluation (Fiorina 1981), but the same experimental paradigm could be extended to other messages: issue positions, partisan cues, social identities, or endorsements by reference groups. Some initial attempts to conduct these sorts of theory-guided messaging tests may be found in the experimental literature on voter mobilization (Trivedi 2005; Bedolla and Michelson 2012); the next step is to extend this line of research to voter persuasion. This research agenda could also explore the possible interaction between information and other features of the political environment. For example, in India, caste tends to play a dominant role in shaping party preferences, and an interesting research question is whether interventions that lower the salience of caste have the effect of increasing the influence of issue- or performance-based evaluations (Banerjee et al. 2011). Similarly, one might conduct experiments in an array of different electoral settings to see whether the influence of information about incumbent performance depends on the availability of a viable alternative candidate.

Recent studies of attempts to increase compliance with social norms also point the way toward more systematic theoretically-guided inquiry. The existing literature has focused primarily on *prescriptive* norms (what one ought to do) and *descriptive* norms (what others tend to do), and only recently have messaging experiments focused on other psychological propositions, such as idea that people are more likely to take action in the wake of an encouragement when they form of mental image of the steps they would take in order to perform the behavior (Rogers and Nickerson 2010) or express a verbal commitment to perform the behavior that they are later reminded of (Michelson, Bedolla, and McConnell 2009). Again, such messages could be studied in conjunction with other factors by concurrently varying source credibility or reference group cues.

Experimental investigations of mass media effects need to diversify along several dimensions. Field experiments have scarcely tested the relative effects of positive versus negative tone in candidate messaging. (For experiments that speak obliquely to this comparison with regard to voter turnout, see Niven 2006; Gerber, Green, and Green 2003). Researchers have only begun to test the rate at which messaging effects decay, and no experimental studies have tested whether the random introduction of ads on a specific topic changes the salience of that topic either in the minds of voters or in the media's campaign coverage. And beyond the traditional mass media, experimental researchers have paid relatively little attention to Internet-based advertising or messages deployed through social media, such as Facebook. (For experimental attempts to gauge the effects of e-mail messaging on voter registration see Bennion and Nickerson 2010; see Bond, Jones, and Fowler 2011 on the effect of Facebook messaging on voter turnout.) Despite the fact that Internet ads and applications are increasingly able to target specific individuals or small geographic regions, experimental researchers are only beginning to study the effects of political messaging. Given the immense reach of the Internet and the growing range of political actions that can be expressed through on-line behavior (e.g., petitions, donations, volunteering), experimental opportunities abound.

Can these opportunities be turned into actual field experiments? Prior to the 1990s, one might have supposed the answer to be no, but researchers in recent decades have steadily expanded the domain of what is thought possible. An increasing number of governments and nongovernmental organizations have been persuaded to use field experiments in order to evaluate their public outreach efforts. In the world of campaign politics, scholarly collaborations have occurred when candidates, interest groups, and political parties have sought to harness the power of randomized experiments in order to evaluate persuasive messages, fundraising appeals,

and voter mobilization efforts. The field of political communication is gradually being transformed by this research method and the rigor that it brings to causal inference.

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