

CHAPTER 9

Field Experiments on Voter Mobilization: An Overview of a Burgeoning Literature

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Abstract

This essay reviews the ways in which field experiments have been used to study political participation. We begin by charting the intellectual history of field experimentation in political science. We explain why the advent of field experimentation in recent years represents an important advance over previous work, which relied principally on nonexperimental survey research. Our review of the

experimental literature on political participation focuses on two broad research domains related to voter mobilization: the effects of different modes of communication (e.g., face-to-face conversations, phone calls, and mail) and the effects of different messages (e.g., those that stress social norms, express gratitude, or urge the expression of implementation intentions). In the final section, we discuss some open questions and new ways that field experiments may illuminate the study of voter turnout and political behavior more generally.

Keywords

Field experiments; Political participation; Voter mobilization; Voting behavior

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In recent years the focus of empirical work in political science has begun to shift from description to an increasing emphasis on the credible estimation of causal effects. A key feature of this change has been the growing prominence of experimental methods, and especially field experiments.

In this chapter we review the use of field experiments to study political participation. Although several important experiments address political phenomena other than voter participation (Bergan, 2009; Broockman and Butler, 2015; Butler and Nickerson, 2011; Broockman, 2013, 2014; Grose, 2014; Kalla and Broockman 2016), the literature measuring the effect of various interventions on voter turnout is the largest and most fully developed, and it provides a good illustration of how the use of field experiments in political science has proceeded. From an initial focus on the relative effects of different modes of communication, scholars began to explore how theoretical insights from social psychology and behavioral economics might be used to craft messages and how voter mobilization experiments could be employed to test the real world effects of theoretical claims. The existence of a large number of experimental turnout studies was essential, because it provided the background against which unusual and important results could be easily discerned.

We begin by describing the intellectual context of the modern emergence of field experiments to study voter turnout. We discuss the state of the literature on campaign effects and voter mobilization around the time of the reintroduction of field experimentation to study political behavior. We discuss some of the methodological reasons why this change represents an important advance over previous work. Our literature reviews focus on two broad areas of research: the effects of different modes of communication (face-to-face conversations, phone calls, and mail) and the effects of different messages. In the final section we discuss some open questions and new directions for applications of field experiments to the application of field experiments to voter turnout and the study of political behavior more generally.

1. INTELLECTUAL CONTEXT FOR EMERGENCE OF FIELD EXPERIMENTS IN POLITICAL SCIENCE

1.1 The development of field experimentation in political science

The first political-science field experiments were conducted by Harold Gosnell in the 1920s. Gosnell, one of the foremost empirical political scientists of the first half of the 20th century, showed an early appreciation for the challenge of identifying the effects of voter mobilization efforts. He notes that the fall of 1924 featured a great deal of get-out-the-vote (GOTV) activity, including a National League of Women Voters' door-to-door canvassing effort and a campaign by two million Boy Scouts to remind citizens of their duty to vote. However, he recognized that any correlation between turnout and mobilization activity cannot be taken to demonstrate that a causal relationship exists. Foreshadowing the concerns about causal identification that now suffuse work on voter turnout, Gosnell writes:

What was the net effect of all this publicity regarding the election? Did a higher proportion of the eligible voters take part in the electoral process? The only candid answer to these questions is that we do not know... It is true that in some states a larger portion of the adult citizens voted in 1924 than in 1920, but what part of this increase, if any, can be traced to a single factor like the get-out-the-vote movement?

Gosnell (1927, p. 2)

Gosnell took up this challenge and conducted the earliest field studies of voter mobilization. He investigated the effects of GOTV mailings on turnout in the presidential election of 1924 and the 1925 Chicago mayoral election (Gosnell, 1927). Although it remains unclear whether Gosnell employed random assignment of the GOTV treatment in his study, other aspects of his research protocol such as measurement of outcomes using the administrative voter records have become familiar features in the modern experimental literature.¹ Three decades after Gosnell, Eldersveld (1956) conducted a series of randomized field experiments to measure the effects of different modes of campaign contact on voter turnout. Eldersveld assigned treatments at the household level and, using post-election administrative records, measured the effect of mail, phone, and canvassing on voter turnout in Ann Arbor, Michigan. While these early experiments have many features of contemporary work, the studies were seldom cited and had little effect on the trajectory of subsequent research. In the decades after Eldersveld, field experimentation was treated as an unusual curio and, when the method was considered at all, it was dismissed as impractical or of limited application. Although lab and survey experiments gained popularity during the 1980s and 1990s, experiments in naturalistic settings

¹ Gosnell canvassed some Chicago neighborhoods and assembled a collection of matched pairs of streets. He selected one of the pair to get the treatment, but it is not clear what method Gosnell used to decide which of the pair was to be treated.

remained rare; no field experiment on any subject was published in a major political science journal during the 1990s.

The modern tradition of political-science field experimentation began with a series of experimental studies of campaign activity (Gerber and Green, 2000; Gerber et al., 2001). The turn to field experiments can be understood in part as a response to persistent methodological concerns regarding the then-dominant approaches employed in important political behavior literature. To provide an appreciation for the context in which field experimentation developed in political science, we briefly review the state of the literature on campaign effects at the time of the authors' 1998 field experiment on voter turnout in New Haven. Although this literature includes some of the very best empirical studies of their time, the work suffered from important methodological weaknesses and often produced sharply conflicting results. The appeal of field experiments stems in part from its ability to address many of the deficiencies in the prior literature.

At the time of the New Haven field experiments, the literature that attempted to measure the effect of campaign spending on election outcomes included perhaps a dozen major studies using a variety of empirical strategies. With few exceptions, one common feature of this literature was that the studies did not examine the effect of particular campaign activities but rather explored the correlation between reported campaign spending [as compiled by the Federal Election Commission (FEC)] and candidate vote shares.² The pioneering work employing the newly available FEC data was conducted by Jacobson, who estimated spending effects by regressing election outcomes on incumbent and challenger spending levels (Jacobson, 1978, 1985, 1990, 1998). A key assumption of this approach is that spending levels do not adjust to unmeasured aspects of the political context. Intuition, however, suggests that incumbents tend to increase their spending when facing a tough race. This concern was heightened by the major finding of this line of work that incumbent spending frequently had a negative relationship with incumbent vote share. There were two major responses to the threat of bias. First, some studies proposed instrumental variables for candidates spending levels (Green and Krasno, 1988; Gerber, 1998). Second, it was proposed that omitted variables regarding election conditions could be eliminated through a panel approach. Levitt (1994) examined the subset of races that involved the same candidates facing each other in the same district on more than one occasion. Using the subset of races involving repeat-pairs, Levitt measured the relationship between the change in vote share and the change in spending levels, producing estimates that were unrelated to differences in candidate or district attributes that might be lurking in the error term of a cross-sectional regression.

² There were some exceptions, for example, Ansolabehere and Gerber (1994).

Using the results from several of the leading studies, we can calculate the implied cost per vote.³ Table 1 shows that the results produced by alternative estimation strategies are dramatically different. The estimated cost of moving the vote margin by a single vote ranges from as little as 20 dollars to as much as 500 (Gerber, 2004). This range seems to span all plausible estimates. Further, it is not clear which study ought to be believed, as each relies on assumptions that, while plausible, are far from airtight. The dramatically inconsistent results, and the sensitivity of the estimates to modeling assumptions, suggest the usefulness of attempting a fresh approach to measuring campaign effects.

The turn to experiments represents one such attempt. The campaign-spending literature attempts to draw conclusions about the effectiveness of spending using overall campaign spending as the independent variable. However, overall spending is the sum of spending on a variety of different activities. Thus, it might be possible to gain insight into the effect of spending overall by measuring the effectiveness of spending on particular components of campaigns, such as voter mobilization efforts. This suggests the usefulness of obtaining a ballpark estimate of the cost of inducing a supporter to cast a ballot. As the literature on campaign-spending effects developed, a parallel literature examining the effects of campaign mobilization was developing as well. This literature progressed on an independent track and, despite its relevance, no connection was drawn to the aggregate spending literature. What did the observational and experimental works on voter mobilization say about the votes that could be produced through voter mobilization efforts?

Prior to the 1998 New Haven experiments, a small field experimental literature addressed the effects of campaign activity on voter turnout. Table 2 lists and summarizes the results of these studies. Gosnell's study in the 1920s was by far the largest. Gosnell measured the effect of a nonpartisan mail campaign in Chicago's 1924 and 1925 elections. Eldersveld followed three decades later with studies of the effect of different modes of contact on turnout levels. He examined the effect of voter mobilization in a pair of local

Table 1 Approximate cost of adding one vote to candidate vote margin

	Incumbent	Challenger
Jacobson (1985)	\$278/vote	\$18/vote
Green and Krasno (1988)	\$22/vote	\$19/vote
Levitt (1994)	\$540/vote	\$162/vote
Erikson and Palfrey (2000)	\$68/vote	\$35/vote

2015 dollars. Calculations are based on 190,000 votes cast in a typical House district. For House elections, this implies that a 1% boost in the incumbent's share of the vote increases the incumbent's vote margin by 3800 votes.

Adapted from Gerber, A.S., 2004. Does campaign spending work?: Field experiments provide evidence and suggest new theory. *Am. Behav. Sci.* 47, 541–574.

³ The cost-per-vote in this context refers to the cost of reducing the vote margin by one vote. In the context of the turnout literature described below, cost-per-vote refers to the cost of mobilizing one additional voter.

Table 2 Voter mobilization experiments prior to 1998 New Haven experiment

Study	Date	Election	Place	No. of subjects (including control group)	Treatment	Effects on turnout (%) ^a
Gosnell (1927)	1924	Presidential	Chicago	3969 registered voters	Mail	+1
Gosnell (1927)	1925	Mayoral	Chicago	3676 registered voters	Mail	+9
Eldersveld (1956)	1953	Municipal	Ann Arbor	41 registered voters	Canvass	+42
Eldersveld (1956)	1954	Municipal	Ann Arbor	43 registered voters	Mail	+26
				276 registered voters	Canvass	+20
				268 registered voters	Mail	+4
				220 registered voters	Phone	+18
Miller et al. (1981)	1980	Primary	Carbondale, IL	79 registered voters	Canvass	+21
				80 registered voters	Mail	+19
				81 registered voters	Phone	+15
Adams and Smith (1980)	1979	Special city council	Washington, DC	2650 registered voters	Phone	+9
Greenwald et al. (1987)	1984	Presidential	Columbus, Ohio	60 registered voters	Phone	+23

^aThese are the effects reported in the tables of these research reports. They have not been adjusted for contact rates. In Eldersveld's 1953 experiment, subjects were those who opposed or had no opinion about charter reform. In 1954, subjects were those who had voted in national but not local elections. The Greenwald et al. results are those for which Greenwald and colleagues count as voting those who in a follow up call say they voted outside the jurisdiction of the study. Alternative treatments of these cases has no material effect on the results. Note that this table includes only studies that use random experimental design [or (possibly) near-random, in the case of Gosnell (1927)]. Adapted from Gerber, A.S., Green, D.P., Nickerson, D.W., 2001. Testing for publication bias in political science. *Polit. Anal.* 9, 385–392.

elections in Ann Arbor. Greenwald et al. (1987) investigated the psychological hypothesis that predicting one's behavior had a causal effect on future action. They constructed a brief series of questions that had the effect of inducing treated subjects, a random subset of 32 treated subjects drawn from a collection 60 Ohio State undergraduates, to state that

they intended to vote in the next day's 1984 presidential election. They measured the effect of this treatment on their subsequent turnout and found the intervention produced more than a 20 percentage point boost in turnout. In 1980, Adams and Smith measured the effect of a 30 s phone call on turnout and vote choice in a District of Columbia special election. In the same year, Miller et al. (1981) measured the turnout effects of door-to-door canvassing, phone calls, and direct mail on randomly targeted voters in a Carbondale, Illinois primary election.

Summarizing the early experiment literature, prior to 1998, there were a few studies conducted over many decades and across a range of political contexts. Nevertheless, when the small literature is viewed as a whole, a few conclusions emerge. First, it appears that campaign interventions are highly effective. Short phone calls produce turnout increases of 10 or even 20 percentage points. According to these studies, visits from canvassers or even a single letter also tend to produce effects of this same magnitude. These are very large estimated effects; to put this treatment effect into context, the falloff in turnout between a presidential election and a midterm election is about 10 percentage points. Second, these large treatment effects are observed in both general elections, such as the 1984 presidential election, and less high profile contests. Third, treatment effects show no tendency to decrease over time.

Another important and related line of research employed laboratory experiments to assess the effect of campaign activity. A leading example of this work is the influential study by Ansolabehere and Iyengar (1996), who brought subjects into a laboratory setting designed to mimic a typical living room and measured the effect of political advertisements inserted into mock newscasts. They found that advertisements that attacked the opposing candidate reduced the likelihood that subjects, when interviewed later, said they would vote, an effect that was especially strong among independent voters. Like field experiments, these studies use random assignment to estimate the causal effect of campaign communications. However, it is hard to translate the results from the laboratory experiments into quantitative estimates of the impact of actual campaign activity on actual voter turnout. Despite the researchers' best effort to simulate the typical viewer experience and measure outcomes reliably, the context in which subjects receive the treatment and express outcomes (intention to vote) differs from natural settings in so many ways, both obvious and subtle, that it is unclear how the lab result indicates either the magnitude or even the direction of the campaign effects being studied.⁴

⁴ As Gerber (2011) notes, it is not necessarily the case that estimates obtained in the lab indicate the direction of effects in field settings. There are often plausible arguments for why a lab effect might go in the opposite direction from the real world effect. One major difference between the lab and field is that in the real world individuals have additional choices and exposures. Applying this to laboratory studies of negative campaigning, outside the lab individuals may be inspired by a negative advertisement to seek additional information about the claim or to pay more attention to campaign related stimuli, leading to greater interest and higher participation levels.

In contrast to the occasional experimental study, the vast majority of work on campaigns and turnout was (and is) observational. During the 1990s, the most influential scholarship on the causes of turnout were studies that measured the relationship between voter turnout and voter demographics, attitudes, and reported campaign contacts using survey data. Research by [Rosenstone and Hansen \(1993\)](#) is an exemplar of this line of work. Their book was extremely influential and remains a standard reference (its Google scholar citation count exceeded 3700 as of the start of 2016). The book is cited by all turnout scholars, and the research design they employ is still common in current research. The American National Election Study is a federally funded biennial survey research project that began in 1952 and continues to this day. Questions about voter turnout and campaign contact have been asked since the earliest surveys, and Rosenstone and Hansen use the American National Election Studies (ANES) to measure the effect of reported campaign contacts on various measures of participation. Using estimates from a pooled cross-sectional analysis of ANES data, they calculate the incremental contribution of many different factors, including campaign contacts, on reported participation in presidential and midterm years (see Tables 5.1 and 5.2 in [Rosenstone and Hansen, 1993](#)). They find that the estimated effect of campaign contact on reported voter turnout is approximately a 10 percentage point increase in turnout probability.

The 10 percentage point turnout boost from campaign contact found by Rosenstone and Hansen is similar in magnitude to the effects estimated by many of the early field experiments. However, despite this agreement, there are grounds for skepticism. As we point out in the next section, respondents' exposure to campaign contact is neither randomly assigned nor accurately measured. The move to field experiments in the late 1990s was motivated in part by concern about the potential bias in the dominant survey-based research tradition.

2. HOW DO EXPERIMENTS ADDRESS THE PROBLEMS IN THE PRIOR VOTER TURNOUT RESEARCH?

In this section we present a basic framework for defining causal effects and apply the framework to explain how field experiments eliminate some of the key sources of bias in observational studies. To fix ideas, we will use the classic [Rosenstone and Hansen \(1993\)](#) survey analysis as a running example. In Rosenstone and Hansen, some respondents report that they are “treated” (contacted by a campaign) and some report that they are “untreated” (not contacted by the campaign). The key challenge in estimating the treatment effect of campaign contact on those who are truly contacted is that the analyst must use available data to construct an estimate of a counterfactual quantity, the turnout rate of the contacted in the event they had not been treated. We express this challenge using potential outcomes notation ([Rubin, 1978](#)). For each individual i , let Y_{i0} be the outcome if i does not receive the treatment (in this example, contact by the

mobilization effort), and Y_{i1} be the outcome if i receives the treatment. The treatment effect for individual i is defined as

$$\tau_i = Y_{i1} - Y_{i0}. \quad (1)$$

We define the treatment effect for individual i as the difference between the outcome for i in the two possible, but mutually exclusive, states of the world: one in which i is treated, and another in which i is not. Moving from a single individual, the average treatment effect for the treated (ATT) is defined as

$$ATT = E(\tau_i | T_i = 1) = E(Y_{i1} | T_i = 1) - E(Y_{i0} | T_i = 1), \quad (2)$$

where the $E()$ operator stands for a group average and $T_i = 1$ when a person is treated. The quantity $Y_{i1} | T_i = 1$ is the posttreatment outcome for those who are actually treated, and $Y_{i0} | T_i = 1$ is the outcome that would have been observed for the treated had they, in fact, not been treated.

In Rosenstone and Hansen, as in the rest of the nonexperimental literature, the comparison group for the treated are subjects who are untreated. When covariate adjustment is used, the comparison group is the set of subjects who are untreated but resemble the treated with respect to their background attributes. This approach is susceptible to selection bias when the potential outcomes among the untreated are systematically different from those of the treated. Stated formally, in expectation the observational comparison of the treated and the untreated estimates yields

$$\begin{aligned} E(Y_{i1} | T_i = 1) - E(Y_{i0} | T_i = 0) &= [E(Y_{i1} | T_i = 1) - E(Y_{i0} | T_i = 1)] \\ &\quad + [E(Y_{i0} | T_i = 1) - E(Y_{i0} | T_i = 0)] \\ &= ATT + \text{Selection Bias}. \end{aligned} \quad (3)$$

Under what conditions does the selection bias term disappear? The critical assumption for identification of the average treatment on treated in observational work is that—controlling for covariates (whether through regression or through matching), $E(Y_{i0} | T_i = 1) = E(Y_{i0} | T_i = 0)$, that is, apart from their exposure to the treatment—the treated and untreated group outcomes are on average the same in the untreated state. In the absence of some unusual as-if random circumstance by which some units came to be treated and other remained untreated, this assumption is not credible. Consider the case at hand, estimating the effect of campaigning on voter turnout. Campaigns typically have extensive information available about a jurisdiction's voters based on both administrative records of voter turnout and demographics along with insider information about individuals and neighborhoods. This information, which may not be fully available to the data analyst, is typically used in campaign targeting strategies. Campaigns commonly target those who have shown a tendency to participate, and this characteristic is, from the standpoint of the analyst, an omitted variable. The ANES, for example, does

not record respondents' vote history, although voter files available to campaigns do contain this information. Second, previous turnout records are highly predictive of the outcome variable, turnout. Therefore, $E(Y_{i0}|T_i = 1)$ may be substantially higher than $E(Y_{i0}|T_i = 0)$. Although in this case it is possible to guess the direction of the bias, analysts rarely have a firm basis to speculate about the magnitude of the bias, and so it is not possible to correct the estimates.⁵

Beyond selection bias, field experiments mitigate a variety of other common methodological concerns regarding observational studies of political behavior. In observational studies the researcher controls neither the treatment assignment nor the design of the treatment. At the most basic level, a key feature of field experimentation is that the researcher controls the assignment to treatment and therefore knows which subjects are assigned to treatment and control conditions. Observational studies often attempt to measure whether an individual is treated or not, but survey measures may be unreliable. Commonly, whether a subject is treated or not relies on the subject's self-report (of campaign contact, of advertising exposure, of media usage, etc.). Consider again the example of attempts to measure the effects of campaign mobilization on voter turnout. In this literature, contact is self-reported, and misreporting leads to a treatment group that is a mixture of the treated and untreated. If this misreporting is random misclassification, the estimated average treatment effects will be attenuated, but if those who misreport campaign contact tend to be the more politically engaged, this nonrandom measurement error may exaggerate the effects of campaign contacts. This bias will be heightened when, as is often the case, the subject's turnout is itself based on self-report. There is empirical evidence of both substantial misreporting and a positive correlation between misreporting campaign exposure and misreporting having voted (Vavreck, 2007; Gerber and Doherty, 2009). It should be noted that although from time to time previous observational work has employed validated vote (the ANES used public voting records to add this variable into the survey datasets for the years 1964, 1972, 1974, 1976, 1978, 1980, 1984, 1986, 1988, and 1990⁶), one of the important innovations brought about by the advent of field experimentation in this area is that it has become common for studies of political behavior to use administrative data rather than self-reports.

A further problem that is avoided by field experiments is ambiguity about what intervention is being assessed. Turning again to the case of the voter mobilization research, the ANES item used for campaign contact in the Rosenstone and Hansen study asks

⁵ Further, when "correcting" for bias, this uncertainty about the size of bias is not contained in the reported standard errors and, unlike sampling variability, it remains undiminished as the sample size increases (Gerber et al., 2004). The conventional measures of coefficient uncertainty in observational research thereby underestimate the true level of uncertainty, especially in cases where the sample size is large.

⁶ See <http://www.electionstudies.org/overview/dataqual.htm>.

respondents: “Did anyone from one of the political parties call you up or come around and talk to you about the campaign?” Taken literally, this question asks the respondents about partisan phone or face-to-face contact leading to a conversation about the campaign, which omits all campaign contact through mail, all contact about political issues outside the campaign, and possibly all manner of nonpartisan contact urging turnout. It is unclear whether survey respondents attend to these nuances when answering the question, which only deepens the ambiguity surrounding the treatment effect that survey-based regressions are estimating.

In experimental analysis, it is now standard to account for noncompliance. In the context of voter mobilization, noncompliance most commonly occurs when individuals who were assigned to the treatment group remain untreated. The rate at which failure to treat occurs varies across modes of contact, the intensity of the effort to contact, the difficulty of contact, and attributes of the subjects and context. Noncompliance arises for a variety of reasons, such as the subject relocating, not answering the door or phone when the campaign attempts contact, or the campaign running out of resources before attempting to contact all subjects assigned to be treated. The failure to treat is immediately apparent in field experiments, and the observed difference in average outcomes for the treatment and control groups is adjusted for the proportion of the treatment group contacted to estimate the average treatment effect among compliers (Angrist et al., 1996), which is the same as the average effect of the treatment on the treated when experiments encounter one-sided noncompliance.

Properly accounting for noncompliance in voter mobilization experiments is an innovation of recent work, as experimental studies prior to 1998 either dropped the untreated subjects in the treatment group from the analysis or reclassified them as control group observations (Adams and Smith, 1980; Eldersveld, 1956) or made no mention of the issue (Miller et al., 1981). Such approaches produce biased estimates of the effect of the treatment on the treated if those who cannot be contacted in the treatment group have a different average untreated potential outcome than the entire pool of subjects. Because failure to treat may stem from factors related to propensity to turnout, such as recently relocating, being out of town around election day, being busy or anti-social, or any of a number of other possibilities, noncompliance is unlikely to be ignorable. In studies of GOTV phone calls, those who are hard to contact often prove to be much less likely to vote than the average subject (Gerber and Green, 2005; Arceneaux et al. 2006). In observational studies, those whom the campaign cannot reach will tend to report that they were untreated and will therefore be grouped with those the campaign did not attempt to reach. Thus, in addition to selection bias due to the campaign targeting, there is also bias due to the campaign’s failure to treat some of its targets.

In sum, field experiments have at least three important advantages over survey-based observational studies of voter turnout. Random assignment of the treatment eliminates

the threat of selection bias. Direct manipulation of the treatment also allows researchers to have more control over what the treatment is and to more accurately ascertain whether subjects received it. The use of administrative data to measure outcomes helps ensure symmetry between those assigned to the treatment and control groups. A commonly noted limitation of field experiments is that they seldom encompass a random sample of a national electorate (but see [Fieldhouse et al., 2013](#)), which raises the question of whether experimental results generalize across subjects, treatments, and contexts. One way to address this concern is through extensive replication of experiments, a practice that has become common in voter mobilization research. Indeed, one of the distinctive features of the experimental literature on voter mobilization is the large and ever growing number of studies that replicate and extend existing research. The next section describes the evolution of the experimental literature, which now encompasses studies conducted in Europe, Asia, and Latin America.

3. AN OVERVIEW OF THE EXPERIMENTAL LITERATURE ON VOTER MOBILIZATION⁷

The modern voter mobilization literature can be divided into two main classes of studies. The early work focused on the relative effectiveness of different modes of contact. This focus was in part inspired by a concern that the shift from the more personal campaigning of a previous era to modern campaigns conducted through mailings and television were contributing to a decline in turnout. Although some studies introduced experimental variation in message content, this was not the major focus of the research. A second line of research aimed to measure the effect of alternative messages employed in the communications. Often inspired by psychological theories or political folk wisdom, these studies examined how the impact of the communication changed according to the words and images used in the campaign material. For some influential theories in social psychology, this literature, although published largely in political science journals, provides some of the most telling empirical evidence.

3.1 Modes of contact

The New Haven 1998 study examined the relative effectiveness of three common campaign tactics: door-to-door canvassing, calls from commercial phone banks, and direct mail. The study found that face-to-face canvassing produced an 8 percentage point increase in turnout among those contacted, each piece of mail raised turnout by half a percentage point in households receiving the mail (the number of mailings varied from 0 to 3), and a phone call produced no increase in turnout. A substantial follow

⁷ This section is adapted from [Green and Gerber \(2015\)](#).

up literature measured the effect of each of these three modes of communication across a range of contexts and extended this line of research to include GOTV appeals communicated via television, radio, and social media.

3.1.1 *Canvassing studies*

After the New Haven Study, basic questions of generalizability abounded. Would canvassing work elsewhere? Would it work in competitive as well as uncompetitive municipal races? We first summarize studies that, like the New Haven Study, canvassed using nonpartisan GOTV appeals. In 2001 a multisite evaluation was carried out in six cities: Bridgeport, Columbus, Detroit, Minneapolis, Raleigh, and St. Paul. Baseline turnout rates in the control groups varied considerably across sites, from 8.2% to 43.3%. Despite the varying electoral and demographic contexts, results were no more variable than one would expect by chance. In all six sites, turnout was higher in the assigned treatment group than the control group, although the increase was negligible in one site. Analyzing the data for the six sites with a single regression model raised turnout among those contacted by 7.1 percentage points with a standard error of 2.2 percentage points (Green et al., 2003).

Another mobilization experiment conducted in 2001 extended previous work in three important directions (Michelson, 2003). First, the canvassing effort achieved a remarkable 75%–contact rate. Second, it showed how mobilization works in a rural setting. The study took place in a low-turnout municipal election in a largely Latino California farming community. Third, it varied the campaign message between appeals that stressed either civic duty, ethnic solidarity (for Latino voters), or community solidarity (for non-Latino voters). Regardless of the message used, the team of Latino canvassers proved highly effective at mobilizing Latino voters. For all Latinos, turnout increased from 13.8% (N = 298) to 18.5% (N = 466). For non-Latinos, turnout increased from 25.7% (N = 758) to 28.2% (N = 1243). Canvassers contacted 73% of Latinos and 78% of non-Latinos. The scripts were not significantly different in terms of the effectiveness with which they mobilized voters.

Again examining the effects of alternative messages in addition to the effects of Latino and non-Latino canvassers, Herbert Villa and Melissa Michelson (2003) focused on a sample of voters under the age of 26, encouraging them to vote in the 2002 state and federal elections. Turnout among Latino subjects rose from 7.2% (N = 1384) to 9.3% (N = 1507), and among non-Latino subjects it rose from 8.9% (N = 1438) to 10.0% (N = 1455). The contact rates were 51% and 39%, respectively. Again, Michelson and Villa found no evidence that the content of the canvassing script made an appreciable difference. Michelson returned to Fresno in 2003, using students from her classes to conduct an experiment on the differential effects of partisan and nonpartisan appeals. Like the Bennion study of the 2002 midterm election, which also used students canvassing as part of a course assignment, this study found weak treatment effects (Bennion, 2005).

Overall, the control group ($N = 2672$) turned out at a rate of 15.2%, compared to 14.9% in the treatment group ($N = 3371$), which was contacted at a rate of 34%.

Unlike other studies of door-to-door canvassing, Nickerson (2008) used a placebo control design. Half of those contacted were urged to recycle; the other half, to vote in the 2002 primary elections held in Denver and Minneapolis. Turnout increased from 47.7% ($N = 279$) to 56.3% ($N = 283$) among those urged to vote. Since by design the contact rate was 100%, the study had reasonable statistical power despite the small sample size. Perhaps the most interesting aspect of this experiment was Nickerson's demonstration that turnout among housemates of persons in the treatment group was significantly higher than turnout among housemates of those in the control group, suggesting that the mobilizing effects of a face-to-face conversation with canvassers may have been transmitted to other members of the household.

In 2004, Carrie LeVan (2016) organized a nonpartisan canvassing campaign aimed at mobilizing voters in low-turnout, low-income, and largely Latino precincts in Bakersfield, California. The study comprised 727 voters, 423 of whom lived in households that were assigned to the treatment group. The contact rate among those assigned to the treatment group was 50%. The study found strong canvassing effects. Among voters living in one-person households, for example, turnout was 41.0% in the control group and 54.5% in the treatment group. Gregg Murray and Richard Matland (2012) also conducted a canvassing study in a largely Latino area, Brownsville, Texas. Turnout among the 3844 individuals assigned to the control group was 33.3%, compared to 34.9% among the 7580 assigned to the canvassing group, of whom 22% were actually contacted.

Lisa García Bedolla and Melissa Michelson (2012) collaborated with several nonpartisan groups participating in the California Votes Initiative, which sought to mobilize low-propensity voters in a series of elections from 2006 through 2008. The effort is noteworthy because of the number of organizations that conducted door-to-door outreach, the range of ethnic groups that were targeted, and the range of electoral contexts during which canvassing took place. In all, 117 distinct experiments were conducted. Although the authors note that many of the participating organizations contacted voters primarily to spread the word about the organization's activities or administer issue surveys rather than to engage in voter mobilization (p. 127), the treatment voted at a higher rate than the control group in 77 of these experiments, which would occur by chance with $p < .001$.

By comparison to partisan canvassing, which tends to occur on a vast scale in closely contested states in presidential elections, nonpartisan canvassing is relatively rare. However, since partisan campaigns always have the option of using nonpartisan appeals to mobilize their partisan supporters, experimental evaluations of nonpartisan canvassing are potentially informative even to campaigns that seek to advocate on behalf of a candidate or ballot measure. Nevertheless, the question arises as to whether the results would

differ if canvassers attempted to urge voters to support a particular candidate or cause. Although no experiments have attempted a head-to-head comparison between nonpartisan and advocacy appeals, a series of advocacy experiments suggest that such canvassing may produce widely varying effects.

Two experiments conducted in 2003 gave early indications that advocacy campaigns could be quite effective in mobilizing voters. In Kansas City, the ACORN organization canvassed extensively in predominantly African American precincts. Its aim was to identify and mobilize those supportive of a ballot measure designed to preserve local bus service. Unlike most other canvassing experiments, this one was randomized at the level of the precinct, with 14 assigned to the treatment group and 14 to the control group. Among voters assigned to control precincts ($N = 4779$), turnout was 29.1%, compared to 33.5% in the treatment group, 62.7% of whom were contacted (Arceneaux, 2005). At roughly the same time, ACORN canvassed in Phoenix on behalf of a ballot measure to determine the future of the county hospital (Villa and Michelson, 2003). ACORN conducted two rounds of canvassing, the first to identify voters sympathetic to the ballot measure and a second to urge supportive voters to vote. The canvassing effort targeted voters with Latino surnames who had voted in at least one of the previous four elections. ACORN made multiple attempts to contact voters (including making a small number of phone calls), the result being that 71% of those living in one-voter households were contacted at least once. This figure rose to 80% among two-voter households. This mobilization campaign had a powerful effect on turnout. Among one-person households, turnout rose from 7.4% in the control group ($N = 473$) to 15.9% in the treatment group ($N = 2666$). Among two-person households, turnout rose from 6.9% in the control group ($N = 72$) to 21.0% in the treatment group ($N = 2550$).

On the other hand, advocacy campaigns have been known to produce disappointing results. Strategic Concepts in Organizing and Policy Education (SCOPE) in Los Angeles canvassed in opposition to the “three-strikes” statewide ballot measure but generated no apparent turnout effect (Arceneaux and Nickerson, 2009). Gray and Potter (2007) found weak mobilization effects in a small canvassing experiment on behalf of a candidate for local magistrate. In their study of canvassing on behalf of a local candidate, Barton et al. (2012) find an unexpectedly negative effect on turnout. Larger candidate advocacy experiments show positive effects, although the treatment-on-treated estimates are smaller than those obtained in the ACORN studies. A sizable experiment on behalf of a Democratic gubernatorial candidate in 2005 generated a treatment-on-treated estimate of 3.5 ($SE = 2.4$), and a series of experiments in 2014 on behalf of state legislative candidates in Republican primary runoff elections generated a treatment-on-treated estimate of 3.1 ($SE = 1.8$).

3.1.2 Commercial phone banks

In 1998 the authors conducted two nonpartisan campaigns using a single commercial phone bank (Gerber and Green, 2000, 2001). The smaller of the two campaigns was conducted in New Haven; a larger study was conducted in neighboring West Haven. In both cities, the elections were rather quiet affairs, with relatively little campaign activity. In both experiments, the group receiving phone calls voted at rates that were no greater than the rates of the control group receiving no calls. None of the three scripts—one stressing civic duty, another, neighborhood solidarity, and a third, the possibility of deciding a close election—had any appreciable impact.

In order to assess whether these results were specific to the context or the calling house, we replicated the 1998 experiments on a grand scale in 2002 (Arceneaux et al., 2006). Congressional districts in Iowa and Michigan were divided into two categories, depending on whether they featured competitive or uncompetitive races. Within each category, 15,000 randomly selected individuals at distinct addresses were called by one of two commercial phone banks, each delivering the same nonpartisan message. Thus, 60,000 people in all were called in the treatment group, and more than 1 million names were placed in the control group. In the 2002 study, the treatment effects were just barely on the positive side of zero, implying that these phone banks mobilized one additional voter for every 280 people they spoke with. Another massive study in Illinois, which called voters before the 2004 November election using a similar nonpartisan script, found somewhat larger effects (Arceneaux et al., 2010). This time one vote was generated per 55 completed calls. However, this study is counterbalanced by a pair of large nonpartisan experiments in North Carolina and Missouri, which found conventional calls to have meager effects, just one vote generated per 500 contacts (Ha and Karlan, 2009).

Calls that advocate on behalf of a candidate or ballot measure have been found to produce similarly weak average treatment effects among compliers. Close to 30,000 calls (about half resulting in successful contact) were made by a commercial phone center on behalf of a ballot measure in a San Francisco municipal election. Consistent with other findings concerning the delivery of brief scripts by commercial phone banks, one vote was produced for every 200 successful contacts (McNulty, 2005). Similar results were found in a relatively small study of a 2002-gubernatorial primary (Cardy, 2005). A much larger experiment conducted by the 2006 general elections also found weak effects, regardless of whether these calls were made using nonpartisan messages or messages advocating support for a minimum wage measure (Mann, 2008). A head-to-head experimental comparison between partisan and nonpartisan scripts indicated that neither had an appreciable effect on turnout (Panagopoulos, 2008).

Several scholars have investigated the hypothesis that the effectiveness of these calls hinges on the manner in which the scripts are delivered. Commercial vendors are paid according to the number of targets they reach, not the number of votes they generate. The callers, who can forge through 50 or so completed calls per hour, behave much

as one would expect given the incentives of piecework and the eagerness of supervisors to move on to the next calling campaign.

In 2002 David Nickerson evaluated a youth-oriented voter mobilization campaign in which a commercial phone bank was paid top dollar to deliver its GOTV appeal in a chatty and unhurried manner. The script required the reader to pause for questions and to invite respondents to visit a website in order to learn more about their polling location. A good deal of coaching ensured that this appeal was read at the proper speed. Between one and four calls were made to randomly selected subgroups of young people over the four-week period leading up to election day. The phone bank kept records of each person they contacted, so that when respondents were contacted a second time, the script took notice of the fact that the previous conversation was being resumed. The calls produced a substantial and statistically significant increase in voter turnout in the target group, but only among those called during the final week of the campaign. In other words, calls made during the first three weeks of a month-long GOTV campaign had no apparent effect on voter turnout. Calls made during the last week produced one vote for every 20 contacts (Nickerson, 2007). This finding set in motion a series of experiments designed to sort out whether the strong effects reflect timing, the use of repeated calls, or the conversational style in which the scripts were delivered.

As to the timing and sequencing of calls from commercial phone banks, a large study conducted across battleground and non-battleground states in the weeks leading up to the 2008 presidential election found that neither first round nor second round calls by themselves boosted turnout but that turnout rose significantly when voters who in round 1 said they planned to vote were later called back and asked whether they could still be counted on to vote. This finding echoes the unusually strong effects found in four follow-up call experiments conducted by volunteer phone banks (Michelson et al., 2009). However, this effect did not replicate in a large commercial phone bank experiment in 2014, which found follow-up calls to have much weak effects (Gerber et al., 2016).

These results tentatively suggest that the active ingredients in a successful call are the scripts and the manner in which they are delivered. This scripts hypothesis was tested in prior to the presidential election of 2004 with calls directed at residents of a battleground and non-battleground state (Ha and Karlan, 2009). A large phone bank deployed three kinds of nonpartisan scripts: a standard script akin to the ones used above; a longer, chattier script in which people were asked whether they knew their polling location, which was provided on request; and a still longer script in which people were encouraged both to vote and to mobilize their friends and neighbors to vote. The results are suggestive, if a bit puzzling. As expected, the standard script had weak effects, raising turnout by just 1.2 percentage points among those contacted. Also as expected, the medium script had a fairly large effect, producing a complier average casual effect (CACE) estimate of 3.4 percentage points. This statistically significant increase

implies that one vote was generated for every 30 completed calls. The puzzling result is the fact that the chatty recruit-your-friends script had an unexpectedly weak effect, as one vote per 69 completed calls.

The call-quality hypothesis was tested in 2010 in a head-to-head competition among different phone banks (Mann and Klostad, 2015). On the high side of the quality spectrum were phone banks that specialized in fundraising or political calls; on the low side were phone banks whose business consisted of a wide array of nonpolitical as well as political clients. Mann and Klostad reason that firms on the low end of the quality spectrum are incentivized to push through a high volume of calls in a mechanical fashion, whereas the focus and reputation of the high-quality firms required them to recruit and retain callers with a knack for political persuasion. Each of the four phone banks called more than 100,000 voters across several states. All the phone banks used the same “chatty” script, which blended several of the ideas discussed in Section 4: gratitude, implementation intentions, and positive descriptive norms. Consistent with the quality hypothesis, the two low-quality phone banks generated weak results, raising turnout among those they spoke with by just 0.2 percentage points. By contrast, the two high-quality phone banks raised turnout among those they reached by 0.9 and 1.4 percentage points. Although the high-quality phone banks proved far less effective than the average volunteer phone bank or the vaunted high-quality phone bank in the Nickerson study, they were significantly more effective than the low-quality phone banks. (Ironically, the lower quality phone banks also reported a higher rate of contacts, which meant that they ended up being more expensive on a cost-per-vote basis.) Given the immense size of this experiment and the tight controls that the authors imposed on the scripts used by the different phone banks, this study offers the most convincing evidence to date about the importance of that intangible ingredient, quality.

3.1.3 Mailings

We begin our summary of the direct mail literature by focusing on “standard” nonpartisan appeals, deferring the discussion of mailings that exert social pressure and other psychological tactics until Section 4. During the four weeks leading up to the 1998 election, we conducted an experiment in which registered voters in New Haven received one, two, or three pieces of nonpartisan direct mail. Each batch of mail reflected one of three themes: the need to do one’s civic duty, the responsibility to stand up for one’s neighborhood so that politicians will take an interest in its problems, or the importance of voting in a close election. Turnout in the control group, which received no mail, phone calls, or door-to-door canvassing, was 42.2% ($N = 11,596$). Turnout was 42.6% ($N = 2550$) among those receiving one mailer, 43.3% ($N = 2699$) among those receiving two, and 44.6% ($N = 2527$) among those receiving three. For the sample as a whole ($N = 31,098$), regression estimates that controlled for the effects of phone and door-to-door canvassing put the effects of each additional mailer at 0.5 percentage point

($SE = 0.3$), which was narrowly significant at the 0.05 level using a one-tailed test. No significant differences were found among the three messages.

In New Haven's 1999 mayoral election, nonpartisan mailings patterned after the civic duty and close election mailings used in the 1998 earlier study were sent to a random sample of the 1998 voter list. The innovation of this study was to send up to eight mailings in order to assess diminishing returns. The close election message had no effect (the election was not remotely close), but the civic duty message performed on par with the 1998 results. The results suggest that returns from mailings begin to diminish after six mailings per household.

Given these encouraging initial results, a series of subsequent experiments tested the effectiveness of nonpartisan mailings as a means of encouraging turnout among ethnic minorities. In a field experiment conducted before the 2002 election, [Janelle Wong \(2005\)](#) classified Los Angeles County voters by last name into one of several Asian American groups: Chinese, Filipino, Indian, Japanese, and Korean. Chinese Americans were sent one piece of bilingual nonpartisan direct mail encouraging them to vote. Other ethnic groups were sent one piece of direct mail in English. Among Chinese Americans, turnout in the control group was 29.0% (2924); the treatment group turned out at a rate of 31.7% (1137). Among other Asian groups, the control group voted at a rate of 38.5% ($N = 5802$), compared with the treatment group rate of 39.4% ($N = 2095$). Also in the 2002 election, a much larger multisite experiment sought to mobilize Latino voters in Los Angeles County, Orange County (California), Houston, New Mexico, and Colorado ([Ramirez, 2005](#)). The content of the bilingual mailers was developed in collaboration with consultants using focus groups. The number of mailers varied across sites from two to four. Despite the high quality of the printing and graphics, they were found to have weak turnout effects.

Several other scholars have attempted to gauge whether ethnic communities can be mobilized using direct mail in 2004. [Trivedi \(2005\)](#) tested alternative nonpartisan messages and graphic themes designed to mobilize Indian American voters living in New York City. Her postcards conveyed ethnic, pan-ethnic, or civic duty appeals, but no message stood out as particularly effective. Richard Matland and Gregg Murray (2012) conducted a nonpartisan mail campaign in largely Latino Brownsville, Texas. Households were randomly assigned a postcard with one of two messages. One emphasized greater power for Latinos if they became more politically active and voted. The other emphasized civic duty and the closeness of the election as the reasons for the recipients should go to the polls and vote in the upcoming presidential election. Both found weak effects.

[Bedolla and Michelson \(2012\)](#) conducted 38 direct mail experiments in California from 2006 through 2008 in an effort to mobilize minority voters using a combination of generic and ethnic appeals. They found weak effects overall, with 19 of the 38 experiments producing positive estimates. Neither voter guides nor hand-written postcards seemed to boost turnout.

Two further strands of the nonpartisan mail literature deserve mention. The first is the simple reminder that an election is imminent. This tactic has repeatedly been shown to have negligible effects and has come to be used as a placebo condition in several experiments (Panagopoulos, 2014, 2013, 2011). Another tactic is to pique voters' interest in an election by calling attention to certain ballot measures. One such experiment sent a single mailing to registered voters both across Florida and specifically in Leon County (Barabas et al., 2010). Each mailing alerted voters to the importance of one ballot measure. The authors compared the mobilization effects of these mailings to those of a generic GOTV mailing and found small differences in effects across different issues or appeals.

The literature gauging the turnout effects of advocacy mailings is essentially a string of null findings. The first large-scale experiments were conducted in 1999 in state legislative and municipal elections on behalf of Democratic candidates (Gerber et al., 2003). The state legislative experiments divided the target population into "prime" Democrats (those with a high propensity to vote), "nonprime" Democrats and Independents, and a random sample of the list of registered voters. The mailings boosted turnout among prime Democrats, but not among other Democrats. Turnout in the random sample rose with the number of mailings, but the effects were small given the number of mailings sent to each household. Combining all of the New Jersey samples suggests that mail did not significantly increase voter turnout. Some slight evidence for demobilization may be found in the negatively toned mayoral campaign, which sent nine mailings to each household.

Another early study evaluated the mobilizing effects of advocacy mail from an abortion-rights interest group, which backed a pro-choice candidate in a gubernatorial primary campaign (Cardy, 2005). The group targeted strongly pro-choice voters whose stances had been previously identified by the phone interviews. The treatment group ($N = 1974$) received five mailings that were printed in full color on glossy paper and mailed between 19 and 6 days before the election. Turnout in the control group ($N = 2008$) was slightly higher than in the treatment group. Other small studies produced results that, on average, suggest little effect on turnout (Cho et al., 2006; Gray and Potter, 2007; Niven, 2006). This conclusion was bolstered by a massive study that sent up to nine pieces of mail on behalf of a Democratic gubernatorial candidate in 2005, as well as a large test on behalf of Republican state legislative candidates in 2014 that sent up to 12 mailers (Cubbison, 2015). The lack of effect is not altogether surprising given that the mailers focused on issues and candidates rather than turnout. Nevertheless, the findings drive home the point that advocacy communications per se do little to stimulate voter turnout.⁸

⁸ Somewhere between nonpartisan mail and advocacy mail are mailers from advocacy groups that target ideologically allied voters but appeal to them using nonpartisan language. See, for example, Mann (2008). These studies tend to produce effects that are somewhere between the noneffects of partisan mail and the weak effects of nonpartisan mail.

3.1.4 Other modes of communication

Compared to the extensive experimental literature on canvassing, phone banks, and direct mail, the literature on other modes of communication looks relatively thin. Some of the most robust findings concern the weak effects of some widely used tactics. For example, [David Nickerson \(2007\)](#) reports the results of 13 experiments in which almost a quarter of a million people—college students, registered voters who did not opt out of email communication, or visitors to websites who agreed to be reminded to vote—were urged via email to vote in an upcoming election. These nonpartisan appeals produced negligible effects, even when 20% or more of the recipients opened the GOTV email on an HTML-compatible browser. [Malhotra et al. \(2012\)](#) found that small but statistically significant effects were found when emails were sent out by the registrar of voters, but identical emails sent by a nonpartisan group has no effect. Alissa Stollwerk collaborated with the Democratic National Committee to assess the effects of three emails encouraging voter turnout in support of the Democratic mayoral candidate in the 2005 New York City general election. The emails were sent in the late afternoon on election eve, on the morning of Election Day, and during the midafternoon of Election Day. The subject lines referred to voting, and the text of the email itself implored Democrats to “vote to ensure that our representatives protect the values and beliefs that we all treasure.” Of the 41,900 people in the treatment group, 13% opened at least one of the emails. The partisan reminders, however, had no effect on voter turnout. Among the 41,900 people in the treatment group, the turnout rate was 58.7%. Among the 10,513 in the control group, turnout was 59.7%. When Stollwerk replicated this study in the days leading up to the 2013 mayoral election, she found positive but insignificant effects ([Stollwerk, 2015](#)). Overall, it appears that GOTV email does little to raise turnout.

Another growing literature evaluates the effects of messages conveyed via social media, such as Facebook. The most well-known study was conducted by a team of academics and Facebook researchers. Prior to the 2010 midterm election, [Bond et al. \(2012\)](#) randomly assigned millions of Facebook users to one of three conditions. The first was a control group that received no encouragement to vote. The second group received an information treatment that consisted of several elements: users were shown a banner at the top of their news feed announcing that “Today is Election Day,” encouraged to indicate whether they voted by clicking an “I Voted” graphic, provided a link to locate their polling place, and presented with a counter tabulating the cumulative vote count among Facebook users. The third group received a social treatment: they received the same encouragement as the information group and were also shown the faces of up to six friends who had voted along with a count of friends who had voted. Data gleaned from Facebook users’ personal profiles allowed the research team to assess actual turnout for about one in 10 subjects, which nevertheless left approximately 60,000 subjects in the control and information conditions and millions

in the social condition. Two key findings emerged. The first is that the information treatment had precisely zero effect on turnout. This finding reaffirms the finding from direct mail experiments suggesting that reminders to vote have little effect on turnout. The second is that the social treatment increased turnout by 0.39 percentage points, which is small but statistically significant. Turnout in the social group is significantly higher than in the control condition or the information condition. Evidently, the active ingredient in social condition is the presentation of friends' turnout, a theme that foreshadows the results discussed in [Section 4.1.1](#).

The idea of putting an “I-voted” widget on Facebook user's news feeds is a creative one, but this intervention is not something that those outside Facebook are at liberty to do, even for a fee. The fallback position is to buy ads on Facebook. In a pair of large-scale experiments, [Collins et al. \(2014\)](#) tested whether “Rock The Vote” ads placed in the news feed in fact raised turnout. In 2012, they assigned approximately 365,000 people to an untreated control group and another 365,000 to a treatment group that received encouragements to vote via sidebar ads and in their news feeds (the latter were actually delivered to 41% of the assigned treatment group). These encouragements showed, for example, the number of days left before the election and a display of friends who “liked” this countdown. Because “Rock The Vote” enjoyed a positive image among the subjects who received its message, having helped many of them register to vote, it was a credible source of information and encouragement. However, voter turnout records later revealed that the treatment and control groups voted at identical rates, 56.5%. The following year, a follow-up experiment using the same design was conducted in 14 states where November elections were being held. Roughly 46,500 voters were assigned to an untreated control and a like number to the treatment group exposed to “Rock The Vote” advertising. This time, a slightly higher proportion of the assigned treatment group, 54%, received ads embedded in their news feeds. Turnout, however, was 14.6% in the control group and 14.0% in the treatment group. In both elections, Facebook ads proved ineffective on increasing turnout.

The experimental literature on email and social media, while disappointing to those who hoped that votes could be mobilized on a grand scale at low marginal cost, is theoretically informative. Evidently, a stream of reminders to vote are ineffective, even when they come from credible sources (e.g., a civic group on whose website one registered to vote or opted in for voting reminders). There is some evidence that more personalized peer-to-peer interaction via social media may foster an interest in politics and increase turnout ([Teresi and Michelson, 2015](#)). Further testing is needed to assess whether new media stimulate turnout to the extent that they mimic direct personal interaction.

3.1.5 Conclusion

We conclude this section by taking note of one of the striking features of the experimental literature on voter turnout—the sheer volume of similar studies employing

each of the modes of contact. To illustrate this, consider the example of GOTV mailings. [Table 3](#), which is adapted from our recent book reviewing the experimental literature ([Green and Gerber, 2015](#)), collects the results of 85 distinct studies conducted between 1998 and 2014.⁹ The table reports the average treatment effect for each study, and includes information about the political context (e.g., general election, primary election, etc.), date, location, and content of the mailing (e.g., mail that supported a candidate or a cause, mail that employed strategies to exert social pressure).

These studies can be used to explore how treatment effects vary with differences in subject pools, election contexts, messages scripts, and other details of the experiment. [Table 4](#) shows the results of several meta-analyses performed using [Table 3](#) data and presents the results for the overall effect of mailings and for some subsets of studies formed by partitioning the studies by the message scripts that appeared on the mailing. Pooling all mail studies together shows that sending a piece of mail to a voter increases the subject's turnout rate by about $\frac{3}{4}$ of a percentage point. Further, there is some evidence that the content of the mailings influences the size of the treatment effect. Messages that exert social pressure are substantially more effective than the typical nonpartisan GOTV message, for example, and pooling across the social pressure studies shown in [Table 3](#) produces a treatment effect estimate of a 2.3 percentage point increase in voting rates. Two things should be kept in mind when interpreting the results from these meta-analyses. First, there is substantial heterogeneity in both the estimated treatment effects and the experimental conditions within the groupings used to form the sets of studies included in each of the rows of [Table 4](#). Second, because there are relatively few studies that conduct a "horserace" between messages in which scripts vary but the other experimental conditions are held constant, it is possible that some of the observed differences in message effectiveness are due to variation in conditions other than the message, a possibility highlighted by the variability of the treatment effects across studies that use similar messaging approaches.

4. THE EFFECT OF MESSAGING

We next review studies that assess the effectiveness of alternative messages. The New Haven study varied message as well as mode of contact. The study tested the effect of including three different messages based on the calculus of voting and folk theories about campaign messaging: pictures and text that urged voting on the grounds that it is a civic duty, that one's vote might be pivotal in deciding a close race, and that one's neighborhood benefits from higher turnout and the attention that attracts among elected officials. There were some differences in the estimated effects, but these fell short of statistical

⁹ These 85 table entries are produced from an analysis of 220 distinct treatment and control comparisons. See [Green and Gerber 2015](#), Table B-1 for details of how study results were condensed for this table.

Table 3 Results of direct mailing experiments in the United States from 1998 to 2014

Context	Study	Estimated turnout effect per mailer	SE	Advocacy used	Social pressure (S) or gratitude (G)
1998G	Gerber & Green—New Haven	0.51	0.3		
1999G	Gerber & Green—New Haven	0.30	0.18		
1999G	Gerber et al.—Connecticut and New Jersey	−0.01	0.09	X	
2000G	Green—NAACP	−0.02	0.46	X	
2002G	Ramirez—NALEO	0.05	0.07		
2002G	Wong—Los Angeles county	1.3	1		
2002M	Gillespie—Newark	−1.1	2.5	X	
2002P	Cardy—Pennsylvania	−0.23	0.50	X	
2002P	Gerber—Pennsylvania	−0.05	0.31	X	
2002S	Gillespie—Newark	−1.6	2	X	
2003M	Niven—West Palm Beach	1.42	2.07		
2004G	Anonymous—Minnesota	−0.86	0.74		
2004G	Matland & Murray—Brownsville	2.94	1.09		
2004G	Trivedi—Queens county	1.13	1.67		
2005G	Anonymous—Virginia	0.2	0.05	X	
2006G	Barabas et al.—Florida	0.25	0.62		
2006G	Bedolla & Michelson—APALC	1.15	0.53		
2006G	Bedolla & Michelson—OCAPICA	−0.45	0.79		
2006G	Bedolla & Michelson—PICO	−3.17	0.97		
2006G	Gray & Potter—Franklin county	−2.92	2.73	X	
2006G	Mann—Missouri	−0.06	0.04	X	
2006G	Anonymous—Maryland	−0.41	0.32	X	
2006P	Bedolla & Michelson—APALC	0.01	0.34		
2006P	Bedolla & Michelson—PICO	1.09	0.82		
2006P	Gerber et al.—Michigan	1.8	0.3		S*
2006P	Gerber et al.—Michigan	5.23	0.17		S
2007G	Gerber et al.—Michigan	1.78	0.87		S*
2007G	Gerber et al.—Michigan	5.15	0.46		S
2007G	Mann—Kentucky	2.73	0.20		S
2007G	Panagopoulos—Gilroy	−0.3	1.4		
2007G	Panagopoulos—Iowa and Michigan	2.20	0.84		S
2008G	Keane & Nickerson—Colorado	−0.67	0.29		

Table 3 Results of direct mailing experiments in the United States from 1998 to 2014—cont'd

Context	Study	Estimated turnout effect per mailer	SE	Advocacy used	Social pressure (S) or gratitude (G)
2008G	Nickerson—APIA Vote	−1.2	0.6		
2008G	Nickerson—FRESC	−0.2	0.7		
2008G	Nickerson—Latina Initiative	0.23	0.26		
2008G	Nickerson—NCL	1.47	0.63		
2008G	Nickerson—Voto Latino	−0.59	0.33		
2008G	Rogers & Middleton—Oregon	−0.03	0.48	X	
2008P	Enos—Los Angeles county	2.05	1.13		
2008 PP	Barabas et al.—Florida	−2.73	0.62		
2008 PP	Nickerson & White—North Carolina	0.8	0.7		
2008 PP	Nickerson & White—North Carolina	0.96	0.26		S
2009G	Larimer & Condon—Cedar falls	0.74	2.38		S
2009G	Mann—Houston	1.2	0.6		G
2009G	Panagopoulos—New Jersey	2.5	0.5		G
2009G	Panagopoulos—New Jersey	2	0.5		S
2009S	Abrajano & Panagopoulos—Queens	1.10	0.40		S
2009S	Mann—Houston	1.1	0.5		G
2009S	Panagopoulos—Staten Island	2	0.98		G
2009S	Sinclair et al.—Chicago	4.4	0.6		S
2010G	Anonymous—Nevada	0.15	0.45		S
2010G	Barton et al.—unknown state	−2.23	1.65	X	
2010G	Bryant—San Francisco	1.75	1.99		
2010G	Gerber et al.—Connecticut	2.00	0.53	X	
2010G	Gerber et al.—Connecticut	0.39	0.64		
2010G	Mann & Mayhew—Idaho, Md., N.C., and Ohio	2.00	0.42		
2010G	Murray & Matland—Texas and Wisconsin	1.75	0.66		
2010G	Murray & Matland—Texas and Wisconsin	1.46	0.66		S
2010M	Panagopoulos—Lancaster	−1.08	0.96		
2010P	Binder et al.—San Bernardino county	−0.11	0.50	X	
2010P	Panagopoulos—Georgia	2.5	0.5		G
2011G	Mann & Kalla—Maine	2.40	0.58		
2011G	Panagopoulos—Lexington	0.97	0.75		
2011G	Panagopoulos et al.—Hawthorne	−0.40	0.71		
2011G	Panagopoulos et al.—Hawthorne	2.17	0.58		S

Continued

Table 3 Results of direct mailing experiments in the United States from 1998 to 2014—cont'd

Context	Study	Estimated turnout effect per mailer	SE	Advocacy used	Social pressure (S) or gratitude (G)
2011M	Panagopoulos—Key West	1.1	0.5		S
2011M	Panagopoulos—Key West	−0.05	0.35		
2011S	Mann—Nevada	0.85	0.28		G
2011S	Panagopoulos—Charlestown	−0.30	0.53		
2012G	Citrin et al.—Virginia and Tennessee	0.74	0.41		
2012G	Doherty & Adler—battleground state	0.05	0.20	X	
2012G	Levine & Mann—Georgia and Ohio	0.24	0.30		G
2012M	Panagopoulos—Virginia	0.03	0.62		
2012P	Condon et al.—Iowa	2.85	0.64		S
2012P	Condon et al.—Iowa	0.4	0.9	X	
2012P	Condon et al.—Iowa	2.7	0.9		
2012R	Gerber et al.—Wisconsin	1.1	0.7		
2012R	Rogers et al.—Wisconsin	1.05	0.27		S
2013G	Biggers—Virginia	0.11	0.18		
2013G	Matland and Murray—Minn., Ohio, Tex., and Va.	0.41	0.32		
2013M	Matland & Murray—El Paso	0.12	0.39		
2014G	Broockman & Green—California	0.35	0.13	X	
2014G	Cubbison—North Carolina	−0.12	0.07	X	
2014P	Green et al.—Texas	0.12	0.53	X	
2014P	Hill & Kousser—California	0.49	0.08		

Context refers to the election year and type, where *G*, general; *M*, municipal; *P*, primary; *PP*, presidential primary; *R*, runoff; *S*, special election; *SE*, standard error. Advocacy refers to appeals that urge support for candidates or causes. Social pressure refers to appeals that emphasize compliance with the social norm of civic participation. Social pressure entries marked with an asterisk forcefully assert the norm of voting but do not tell recipients that whether they vote is a matter of public record. Gratitude mailers thank recipients for voting in a prior election or for their past involvement in elections.

When a given study involved multiple pieces of mail or varying quantities of mail, regression was used to estimate the per-mailer turnout effect. The number of significant digits in the table may vary depending on how the studies' authors reported their results. When a given author or authors report multiple mail tests by the same organization in the same election, we calculated the overall estimated effect and standard error by taking the precision-weighted average, which is equivalent to a fixed effects meta-analysis.

APALC, Asian Pacific American Legal Center; *APIA*, Asian and Pacific Islander American Vote; *FRESC*, Front Range Economic Strategy Center; *NAACP*, National Association for the Advancement of Colored People; *NALEO*, National Association of Latino Elected Officials; *NCL*, National Council of La Raza; *OCAPICA*, Orange County Asian and Pacific Islander Community Alliance; *PICO*, People Improving Communities through Organizing. This table uses information from Green, D.P., Gerber, A.S., 2015. *Get Out The Vote: How to Increase Voter Turnout*, third ed. Brookings Institution Press (Table B-1).

Table 4 Meta-analysis of direct mail experiments in the United States, 1998–2014

Type of direct mail	Estimate	95% Confident interval	No. of studies
Advocacy (excludes social pressure)	0.010	(−0.101, 0.120)	19
Nonadvocacy (excludes social pressure)	0.523	(0.299, 0.748)	51
Social pressure	2.280	(1.259, 3.301)	15
All	0.759	(0.530, 0.988)	85

Results obtained using the metan command in Stata 12, with the random effects option. Estimates are in percentage points. Advocacy includes mailings that urge support for ballot issues or candidates. Social pressure refers to mailings that emphasize compliance with the social norm of civic participation and present recipients with information about their record of voting in past elections. The nonadvocacy category includes mailings the express gratitude for past turnout or stress the norm of voting but do not present or refer to past or future turnout records. Excluding these studies reduces the estimate to 0.366, with a confidence interval ranging from 0.136 to 0.596.

significance, and it appeared that messaging effects were, if present, relatively modest. A large number of subsequent field experiments investigated message effects and some approaches, especially those that employ a treatment that is designed to induce social pressure to participate, have shown large and reproducible increases in turnout.

Here we focus on studies in which the messaging is closely related to or explicitly inspired by leading social psychological theories and for which there is a sufficiently large literature to get a sense for the robustness of the findings.¹⁰ It is useful to compare the mechanisms that might be at work in these psychological approaches to the more standard elements emphasized in the classic accounts of rational participation.

When voting is analyzed from the standpoint of rational decision theory, an individual votes if $pB > C$, where p is the probability the vote changes the outcome (one vote makes or breaks a tie), B is the private benefit to the individual from the preferred candidate winning, and C is the cost of voting. This is the decision theoretic account, since in this account the “pivot probability” is a belief and there is no effort to justify it as the endogenous outcome of game among voters. Because the empirical probability of being pivotal in a large election is miniscule, elections that attract tens of millions of voters represent an anomaly. As long as there is even a modest amount of noise regarding turnout, for any symmetric rule mapping voter costs and benefits into voting, as a theoretical matter the chances that the election in a large electorate will be an exact tie (or within one vote) is essentially zero, which leads to a zero expected return for participation. To account for substantial turnout rates, the basic theory was expanded to include an explicit term for the benefits from voting: $pB + D > C$, where D stands for a sense of civic duty (Riker and Ordeshook, 1968). Some of the messaging strategies can be

¹⁰ Rogers et al. (2013) provide an argument for why voter mobilization field experiments are an excellent environment to test social psychology theories and provide a description of some early findings.

relatively easily incorporated into the standard decision theoretic framework for rational participation or modest extensions of it. Messages employed might affect citizens' beliefs about the components of the formula or the weight should be placed on them.

An alternative source of theoretical inspiration is social psychology, which emphasizes the ways in which behavior may be induced by raising the salience of certain ideas and norms. For example, it has been argued that behaviors such as obtaining an immunization become more likely when people think about how where and when they would be immunized (Milkman et al., 2011). As described below, similar approaches have been used to mobilize voters. One might express this hypothesis using the language of $pB + D > C$ by arguing that rehearsing the steps by which one will cast a ballot reduces the cognitive costs encompassed by C . Similarly, it may be argued that one of the benefits of voting (D) is that it raises one's esteem in the eyes of others, who look down on those who do not perform this civic obligation. We next consider experimental tests of these propositions.

4.1 Self-prophecy and implementation intentions

Both the theory of "self-prophecy" (Greenwald et al., 1987) and the theory of "implementation intentions" (Gollwitzer, 1999) hypothesize that the trajectory of an individual's behavior can be altered by inducing the individual to state that he or she will take a certain action. We will discuss each of these theories and their application to voter turnout.

The notion of self-prophecy is inspired by the idea that some kinds of prediction errors may be self-correcting (Sherman, 1980). There are many things that a person feels he or she ought to do but, for some reason, the individual's actions do not match his or her putative goals. Contributing something to charity, getting more exercise in the coming year, and voting in the next election would be examples of such aspirations. When asked to predict whether they expect to undertake the desirable action, people frequently say they will. According to Sherman (1980) and subsequent authors, inducing individuals to predict their behavior produces "self-erasing error" or an example of "self-prophecy," as the prediction itself induces a sense of obligation to follow through, which then leads to a higher level of adherence to the predicted course of action. Applying this argument to voter mobilization suggests that merely by asking individuals if they expected to vote, a question that is overwhelmingly answered in the affirmative, one can raise turnout.¹¹

The "self-prophecy effect" was first applied to voting behavior by Greenwald and colleagues. Prior to the 1984 presidential election, several dozen college students were phoned and asked some questions about the upcoming election. They found that the

¹¹ It is possible that such a question could also serve as a reminder to vote, but, as noted in our earlier discussion of reminder phone calls and email, there is ample evidence that reminding people that an election is coming has negligible effects on turnout. Simple reminders are often used as the placebo condition in messaging studies involving direct mail.

incremental effect adding an item that asked subjects to predict their participation was a stunning 23 percentage point increase in the voting rate (Greenwald et al., 1987).¹² Subsequent studies were much less supportive. When the same setup was repeated by the original authors in a 1986 senate election and a 1987 state primary, they found no effect. Studies of self-prophecy by other scholars have found treatment effects similar to those produced by a typical commercial turnout phone call (on the order of a 1% turnout increase). In a replication study approximately 10 times the size of the original Greenwald study, Smith et al. (2003) organized a phone bank to call registered voters in advance of the 2000 presidential primary. They compared the turnout of subjects asked if they knew where and when to vote with those asked these questions and whether they expected to vote on Tuesday; the incremental effect of the self-prophecy treatment was -0.1 percentage points. Dustin Cho (2008) replicated this experiment at a larger scale during the 2008 presidential primary and found a 2.2 percentage point turnout increase from the self-prophecy treatment. A large study by Nickerson and Rogers (2010), also conducted during the 2008 presidential primary, found a 2 percentage point effect. Although the effect of self-prophecy each of these three follow up studies was not significant, pooling these findings together suggests that self-prophecy might produce a small boost in turnout, although nothing close to the finding reported in the sentinel study.

In addition to the studies that directly test self-prophecy, several studies provide indirect evidence about self-prophecy's effectiveness. In some studies the question about vote prediction is just one component of the treatment. Christopher Mann (2005) studied the effect of being administered a multiquestion preelection survey that included questions about turnout intention. He found that those registered voters assigned to be asked about their vote intentions and other political attitudes by major news media pollsters voted at the same rate as the randomly selected control group who were not called for the survey. A recent study by Green et al. (2015) found that canvassing door to door with a script that merely asked residents how they intend to vote produced no increase in turnout. Commercial phone banks often conclude their GOTV appeals with the query "can I count on you to vote?" As noted above, the overall turnout effect of these calls is small, typically finding less than a 1 percentage point increase in turnout.

A theory closely related to self-prophecy is the "implementation-intentions" hypothesis, which posits that there is a weak but consequential cognitive barrier between an individual's goals and taking the actions needed to accomplish those goals. According to this theory, getting a person to state the goal and then elaborate the steps necessary to achieve the goal makes accomplishing the goal more likely. The exercise of elaboration makes the steps required more salient and illuminates the path for successful goal-oriented

¹² There are some studies of self-prophecy in other domains. For example, Morwitz et al. (1993) detect an effect of asking people about their plans to buy a car on subsequent car purchases.

action. A messaging strategy based on this theory has been applied to voter turnout by supplementing the self-prophecy item (do you expect to vote?) with follow-up questions about what subjects need to do to achieve their (now stated) intention to vote.

An early effort to test implementation intentions in a field setting was [Nickerson and Rogers \(2010\)](#). Their study asked subjects if they intended to vote and then, for a subset of those subjects, proceeded to a series of questions about actions related to casting a ballot. Those who stated they planned to vote were asked: Around what time do you expect you will head to the polls on Tuesday? Where do you expect to be coming from when you head to the polls on Tuesday? What do you think you will be doing before you head out to the polls?¹³ Nickerson and Rogers report that the implementation intentions script (which combines a standard GOTV message, an inquiry about intention to vote, and the implementation intentions questions) boosted turnout by 4.1 percentage points, and that the incremental effect of the three questions implementation battery was 2.1 percentage points.¹⁴

Several other studies have investigated the effect of elaborating a voting plan. These include [Dustin Cho \(2008\)](#) and [Gerber et al. \(2015\)](#), who found an implementation intentions phone call script to have negligible effects, and [Rogers and Ternovski \(2015\)](#), who tested a version of implementation intentions using a mailing and found a statistically significant 0.5 percentage-point effect from a single mailer.¹⁵ Overall, it appears that scripts that evoke self-prophecy and implementation intentions may nudge turnout upward, but the effects tend to be much smaller than suggested by the sentinel studies.

4.1.1 Social pressure, pride, and shame

If we restrict ourselves to a theoretical model that focuses exclusively on the pivot probability, the benefits from being decisive, and the costs of participation, it is impossible to produce a robust explanation for the observed high levels of turnout in mass elections.

One response to this gap between prediction and model is to extend the set of considerations used by the voter to evaluate whether to vote. An example of this approach is [Coate and Conlin \(2004\)](#), in which two groups of strategic voters incorporate a group identity and then adhere (for unmodeled psychological or social reasons) to a behavioral rule that maximizes group welfare. The particular voting rule (the cut point for the cost of

¹³ Slightly over 85% of subjects said that they planned to vote in the upcoming election.

¹⁴ Further exploration of treatment effect by Nickerson and Rogers revealed that, unexpectedly, the effect of implementation was concentrated among those who resided in households with one eligible voter, for whom the overall effect of the implementation script was 9.1 percentage points and the incremental effect of the implementation questions in particular was 8 percentage points. The authors speculated that this finding was consistent with the idea that implementation planning was unnecessary for those living in multiple voter households because, due to the relative centrality of politics in these households and other aspects of the multiresident social context, these individuals were more likely to already have a voting plan.

¹⁵ The Rogers and Ternovski mailing also included a “gratitude treatment,” a message strategy described below.

voting that separates voters from nonvoters) that is considered normatively desirable for each group emerges as an equilibrium outcome. How these implied norms of proper voter behavior for members of each group—rules that make sense for the group but are not rational for the individual—are enforced is either a psychological or social matter.

A complementary but alternative strategy has been to examine the norms that support voting directly and study how these norms are enforced. Survey evidence indicates that voting behavior appears to be embedded in a set of social norms that support voting. Simple reflection suggests that there is some social dimension to voting, but how important is this consideration? It may be that people merely pay lip service to ideas of voting and civic duty, but perhaps the norms regarding voting are more deeply rooted. If individuals are susceptible to feelings of pride and shame regarding their voting behavior, interventions that heighten these feelings may affect turnout. Conversely, if heightening social pressure leads to a large change in turnout, this lends plausibility to social pressure as a mechanism that is working to produce the observed levels of mass participation.

Experiments have explored the effectiveness of “social pressure,” that is, strategies crafted to tap into the basic human drive to win praise or avoid scolding. Social pressure is exerted by praising those who uphold a social norm or by chastising those who violate them. The level of social pressure exerted can be varied through variation in the intensity of the message or through disclosure of the individual’s level of compliance with the norm. In the voter mobilization literature, social pressure messages typically involve three components: exhorting the receiver to comply with the social norm, stating that the receiver’s behavior will be monitored, and warning that the receiver’s compliance may be disclosed to others.

In a large experiment conducted in a primary election in Michigan in 2006, [Gerber et al. \(2008\)](#) evaluated a set of four mailers that conveyed varying doses of social pressure. The first mailer employed a hectoring tone to encourage citizens to do their civic duty and vote. The second mailer added to this message an element of surveillance by telling people that they were part of an academic study and that their turnout in the upcoming election would be monitored. The third mailing, labeled the “self” mailer, included information from the voter files listing the voting behavior of household members in recent elections and contained a promise to send an updated mailing after the election reporting whether the listed individuals voted or not. Finally, the “Neighbors” mailing increased the social pressure by including the turnout history of the household as well as that of the neighbors on the recipient’s block. Thus, the four mailings represented a (steep) gradation in social pressure.

The results show a very strong effect of social pressure on voter turnout. Bear in mind that a typical nonpartisan GOTV mailing raises turnout by half a percentage point. The first mailing, a forceful civic duty appeal, raised turnout by 1.8 percentage points, while the “self” mailing raised turnout by 4.9 percentage points. The “neighbors” mailing produced a remarkable 8.1 percentage point boost in turnout. These quantities are all

distinguishable from zero and each other, since the treatment groups each comprised 20,000 households and the control group comprised 100,000 households.

Follow up studies have confirmed the basic contours of these results.¹⁶ The main effort has been to replicate and extend the “self” mailing. The social pressure mailings, especially those that confronted voters with their voting record and those of their neighbors (the highly effective “Neighbors” mailing), provoked outrage among some recipients (Murray and Matland, 2014), prompting a search for messaging strategies that produced the turnout effect without as much agitation. These efforts led to a few different approaches. First, there was an attempt to build on the “self” treatment, a strong message that produced an outsized increase in vote but only a modest level of resistance. Table 5 shows the results of several studies testing messages employing the “self” approach. Pooling the results of these studies, which were conducted across a variety of political contexts, shows that the “self” mailing is a powerful treatment, with especially strong effects in low-to-medium salience elections. The results across these studies are similar to the 16% boost (+4.9 from a base of 29.7 in the control group) observed in the 2006 Michigan primary election study. In addition to the higher base rate of voting in a general election, the cases in which the mailing had weaker effects may also be related to the mechanism of the social pressure treatment; the Texas and Wisconsin studies by Matland and Murray used a version of the mailer that did not scold voters for failing to vote.¹⁷

A second strategy is to employ social norms to praise rather than scold. Panagopoulos (2013) used this approach and encouraged subjects to join an “honor roll” of perfect voters. A collection of voters identified as African American, Hispanic, or unmarried women were randomly assigned to receive a mailing that presented the perfect voting history of 10 neighbors. The text included this language:

There is no action more important to our democracy than going to the polls to vote. That's why Our Community Votes, a non-profit organization that encourages voting, is recognizing citizens in your neighborhood who have perfect voting records in general elections over the past four years. These neighbors deserve our recognition and congratulations for doing their civic duty and making

¹⁶ Although the neighbors' mailing has been used from time to time in campaigns, to date, there is only one academic follow-up to the Michigan neighbors' mailing. In the very high turnout of Wisconsin governor election, a neighbors' mailing produced a one-percentage point increase overall and a 3 percentage point increase among those whose base turnout rate was 30%, a subgroup with participation levels more similar to the Michigan subjects than the overall Wisconsin subject pool (Rogers et al. 2015).

¹⁷ No turnout gains were produced by a partisan version of the self mailing, in which subjects were presented with their turnout record and told that it was important for Democrats and Independents to vote because of the negative effects of having Republicans in power (Schwenzfeier, 2014). This may be related to the mechanism thought to be at work. A partisan message is typical in politics and does not cause the subject to reflect on civic duty and the social implications of participation. Nonpartisan mailers that threaten to shame/praise nonvoters/voters by putting their names in a local newspaper seem to produce large effects, although these experiments are somewhat underpowered (Panagopoulos, 2010).

Table 5 The effects of the self mailer on voter turnout across multiple studies

Study	Election type	Setting	Control	Self	Percentage increase in turnout
1 ^b	2006 August primary	Michigan	29.7 (191,243)	34.5 (38,218)	16% ^a
2 ^c	2007 Municipal	Michigan	27.7 (772,479)	32.4 (27,609)	17% ^a
3 ^d	2007 Gubernatorial general (previous nonvoters)	Kentucky	6.8 (19,561)	8.9 (13,689)	31% ^a
3 ^d	2007 Gubernatorial general (previous voters)	Kentucky	13.2 (25,037)	16.3 (17,731)	23% ^a
4 ^e	2009 Municipal special	New York city	3.2 (3445)	4.2 (3486)	36% ^a
5 ^f	2010 General	Texas	40.5 (63,531)	43.1 (1200)	6%
5 ^f	2010 General	Wisconsin	49.0 (43,797)	50.8 (801)	4%
6 ^g	2011 Municipal	California	10.6 (13,482)	12.0 (1000)	13%

^aStatistically significant at $p < .01$, one-tailed test. This Table is adapted from Table 11-1, Get out the Vote, [Green and Gerber \(2015\)](#).

^bGerber, A.S., Green, D.P., Larimer, C.W., 2008. Social pressure and voter turnout: evidence from a large-scale field experiment. *Am. Polit. Sci. Rev.* 102 (1), 33–48.

^cGerber, A.S., Green, D.P., Larimer, C.W., 2010. An experiment testing the relative effectiveness of encouraging voter participation by inducing feelings of pride or shame. *Polit. Behav.* 32, 409–422.

^dMann, C.B., 2010. Is there backlash to social pressure? A large-scale field experiment on voter mobilization. *Polit. Behav.* 32, 387–407.

^eAbrajano, M., Panagopoulos, C., July 2011. Does language matter? The impact of Spanish versus English-language GOTV efforts on latino turnout. *Am. Polit. Res.* 39, 643–663.

^fMurray, G.R., Matland, R.E., 2014. Mobilization effects using mail: social pressure, descriptive norms, and timing. *Polit. Res. Q.* 67, 304–319. The table reports only the results of the Self mailer with no additional information about the voting rate of the community.

^gPanagopoulos, C., Larimer, C.W., Condon, M., 2014. Social pressure, descriptive norms, and voter mobilization. *Polit. Behav.* 36, 451–469.

their voices heard. And with New Jersey's election for governor taking place on November 3rd, we hope that you will go to the polls and join your neighborhood's Civic Honor Roll of perfect voters. Voting records show that you voted in the presidential election of 2008 but not in the 2005 election for governor. Voting records are public information, so people know when you voted, but never how you voted. By voting on November 3rd, you will join the following voters as perfect voters.

[Panagopoulos \(2013, p. 275\)](#)

This approach, an extensive modification of the self-message, raised turnout significantly, albeit less than the original self-mailer; turnout rose by 2.3 percentage points among African American and Hispanic subjects and by 1.3 percentage points among women.

A third variation includes language that hints at the possibility that the subjects might be contacted after the election and asked to explain their participation or failure to participate. An example of this approach is [Rogers and Ternovski's \(2015\)](#) large-scale study of turnout on the 2010 midterm election, which included a box in the corner stating that "You may be called after the election to discuss your experience at the polls." They find that the incremental effect of adding this to the mailing was a statistically significant quarter-percentage point increase in turnout.¹⁸

4.1.2 Gratitude

Gratitude is thought by some to have evolutionary roots and to have developed in a manner to facilitate social exchange and reciprocity ([Trivers, 1971](#)). Drawing on the extensive and growing literature on the power of gratitude and the reciprocity caused by expressions of gratitude ([McCullough et al. \(2008\)](#), [Bernstein and Simmons \(1974\)](#), [Clark et al. \(1988\)](#), [Rind and Bordia \(1995\)](#)), [Panagopolous \(2011\)](#) proposed a voter mobilization message in which the subject is thanked for prior participation. Part of the motivation for the gratitude mailing was to explore a method of making the self mailing, which has the subject's vote history as a centerpiece, more palatable. Thanking the voter provides an explanation for why the subject's vote history has been looked up and presented.

Panagopolous tested the gratitude mailing in three very different elections: a 2009 special election in Staten Island, New York, the November 2009 Governor's Election in New Jersey, and a 2010 Georgia Primary. He found sizeable effects for the gratitude mailings, with a turnout boost of 2.4 percentage points in Staten Island, a 2.5 percentage point increase in New Jersey, and a 2.4% increase in Georgia. The effects of the gratitude mailing were approximately two-thirds as large as the self mailer. An unexpected feature of this trio of studies emerged in the Georgia study, where Panagopolous included two additional treatment arms: (1) a mailing in which the vote history was discussed but there was no mention of official records of voter turnout and (2) a mailing that included just a generic expression of gratitude for the subjects attention to politics, but did not mention anything about the individual or their voting record. The key portion of the basic gratitude message was:

THANK YOU FOR VOTING!

We realize voting takes time and effort.

Official voter records indicate that you voted in the last midterm election in November 2006, and we just wanted to say "thank you."

¹⁸ Another field experiment that reports the results of an intervention that includes a similar message ("researchers will contact you within three weeks of the Election to conduct a survey on your voter participation") is [DellaVigna et al. \(2014\)](#).

Our democracy depends on people like you exercising their right to vote. We appreciate the fact that you made it a priority to cast a ballot.

We also remind you that the primary elections in Georgia will take place on

Tuesday, July 20, 2010. *You are eligible to vote.*

The version that makes no mention of official records is identical except the sentence about official records is excluded. The text for the generic gratitude treatment is:

THANK YOU!

Our democracy depends on people like you paying attention to politics and getting involved in the political process. We appreciate the fact that you make this a priority.

We also remind you that the primary elections in Georgia will take place on

Tuesday, July 20, 2010. *You are eligible to vote.*

These three arms fielded in Georgia were approximately equally effective, producing turnout increases of over 2 percentage points. Remarkably, the point estimate for the generic expression of gratitude was a 3.1 percentage point turnout increase, implying that the gratitude mailer is not simply a veiled self mailer but rather taps into a distinct set of psychological mechanisms. More research is needed to verify this potentially important discovery and to assess whether GOTV messages delivered in person or by phone are enhanced by expressions of gratitude.

4.1.3 Descriptive norms

In contrast to prescriptive norms, which assert that people ought to vote, descriptive norms center on what others do, with the implication you should do likewise. For example, the statement “Everyone else is voting, and you should, too” suggests that you should conform to others’ example, either because others know best or because there are personal advantages to going along with the crowd. Conversely, a statement of the form “Turnout is low, so we hope that you will vote” sends a mixed message; voting is encouraged, but the descriptive norm seems to militate in favor of not voting.

In comparison to the literature on prescriptive norms, the literature on descriptive norms rests on fewer studies, and the experiments tend to be smaller in size. An early study by [Gerber and Rogers \(2009\)](#) showed that voting intentions are affected by information about whether turnout is likely to be high or low. Subsequent studies have gauged the effects of such information on subjects’ actual turnout. [Panagopoulos et al. \(2014\)](#) presented voters in a 2011 municipal election with either a standard mailer or a self mailer. Each type of mailer was distributed with different variants. In the high turnout condition, the mailer included the wording “THE MAJORITY OF YOUR NEIGHBORS DO THEIR CIVIC DUTY. DO YOURS TOO.” Following this statement, individuals were told “TURNOUT IN YOUR COMMUNITY: 70%” in

reference to turnout in the 2008 general election. In the low turnout condition, the wording was reversed: “THE MAJORITY OF YOUR NEIGHBORS DO NOT DO THEIR CIVIC DUTY. BUT YOU SHOULD DO YOURS.” Following this statement, individuals were told “TURNOUT IN YOUR COMMUNITY: 35%” in reference to turnout in the 2006 election. In the self condition, wording with either the high or low norm boosted turnout slightly but not significantly; estimated effects were essentially zero in the standard condition. Another study by [Murray and Matland \(2014\)](#) presented parallel experiments conducted in Lubbock, Texas and Kenosha, Wisconsin. Standard or self-mailers sent to subjects in the low descriptive norm condition included the following passage:

In the Lubbock city elections earlier this year, voter turnout was around 10%, among the lowest levels recorded in the past twenty years. While there are many opportunities to participate, millions of people in Texas never take advantage of these opportunities. Many experts are discouraged by how few voters they expect for the upcoming election. We encourage you to buck this trend among your fellow Lubbock citizens and vote on Tuesday, November 2nd.

By contrast, the high descriptive norm language expressed optimism:

In the General Election in Lubbock in 2008, voter turnout was over 70% of registered voters and among the highest levels recorded in the past twenty years. Throughout the country there has been a surge in voter participation. Many experts are encouraged by this trend and are expecting another large turnout in the upcoming election. We encourage you to join your fellow Lubbock citizens and vote on Tuesday, November 2nd.

Again, the results were ambiguous. In Lubbock, the mailers were equally effective regardless of whether they conveyed high or low norms or none at all. In Kenosha, the high-norm language boosted turnout significantly, whereas the low-norm language had no effect. Although larger replication studies are needed in order to estimate these effects with more precision, it appears that descriptive norms exert weaker effects than prescriptive norms.

4.1.4 Discussion

Our summary of the literature has highlighted a number of empirical regularities. One is that encouragements to vote tend to be more effective when delivered in person than via direct mail or email. Another is that advocacy messages that give voters reasons to support or oppose a given candidate or cause tend not to increase turnout. Yet another is that messages that forcefully assert the social norm of civic participation are often highly effective at stimulating turnout, especially in low salience elections.

4.2 Voter mobilization outside the US

Although these conclusions emerge from a robust experimental literature, the studies described above were all conducted in the context of American elections, which leaves

open the question of whether the results hold outside the United States. The last decade has seen a steady increase in the number of GOTV experiments conducted in other countries. One of the earliest large-scale experiments assessed the effects of nonpartisan phone calls and canvassing in the United Kingdom (John and Brannan, 2008), and several follow-up studies have extended this experimental work to nationally representative samples (Fieldhouse et al., 2013) and to partisan campaigns (Foos and John, 2016). Within the domain of nonpartisan campaigns, these studies confirm the effectiveness of personal GOTV tactics and, if anything, suggest that volunteer phone banks work especially well in the UK, where landlines are less overburdened by telemarketing calls. On the other hand, impersonal tactics such as direct mail have been found to be effective, too, both in the UK (Fieldhouse et al., 2013) and Ireland (Regan, 2013), again perhaps due to the lower volume of commercial junk mail in those countries. Interestingly, partisan canvassing and phone calls have produced mixed results, with studies in the UK, France, and Spain finding no increase in turnout (Foos and John, 2016; Pons, 2014; Ramiro et al., 2012) or heterogeneous effects that are positive only among supporters (Foos and de Rooij, 2013).

Although studies conducted outside the United States have the potential to shed light on the interaction between interventions and electoral context, the lack of individual-level administrative data on voter turnout often presents an impediment to field experimental research. One response has been to conduct an experiment in a single precinct, stationing poll workers to observe who votes in that location, as Guan and Green (2006) did when studying door-to-door canvassing among students in a Chinese university. Occasionally, research collaboration with government agencies gives scholars access to extraordinarily rich data on both turnout outcomes and the social attributes of the study participants. For example, in their study of voter mobilization in Denmark, Bhatti et al. (2016) had access to detailed family data linking parents and offspring, enabling the research team to assess whether the text messages they sent to young voters affected turnout among family members and housemates. Another approach is to randomize at the polling station or city level, a research strategy that has been used in Brazil (De Figueiredo et al., 2011), Italy (Kendall et al., 2013), and Mexico (Chong et al., 2015). Although this type of experimental design tends to be less powerful than one based on individual assignment, it allows the researcher to estimate treatment effects on both turnout and vote share. Some of the most interesting studies are those that show how persuasive campaign messages affect vote share even when they do not affect turnout (Pons and Liegey, 2013), a finding reminiscent of analogous experiments in high-salience US elections (Rogers and Middleton, 2015).

4.3 Downstream effects

One of the most interesting findings to emerge from GOTV research in the US and UK is that voter mobilization campaigns have enduring effects. The New Haven residents

who were randomly assigned to receive direct mail or face-to-face canvassing in 1998 were more likely to vote in both the election held in November 1998 and the mayoral election held in November 1999. This type of persistent effect has since been replicated repeatedly (Coppock and Green 2016). For example, voters assigned to receive mailings in the Michigan social pressure experiment not only voted at higher rates in the 2006 August primary; they were also significantly more likely to vote in August primaries in 2008, 2010, and 2012. The self mailing generated approximately 1850 votes in August 2006, plus an additional 900 votes over the next three August primaries. This pattern of over time-persistence holds for other large social pressure studies (Davenport et al., 2010; Rogers et al., 2015), for nonpartisan efforts to mobilize ethnic minority voters in California (Bedolla and Michelson, 2012), and for GOTV efforts in Britain (Cutts et al., 2009).

The enduring impact of voter mobilization is subject to multiple interpretations. One interpretation is that voting is a habit-forming activity. Someone who votes in this election is more likely to vote in the next election. Someone who skips an election is less likely to vote in the future. America's low turnout rates may reflect the fact that we have the most frequent elections on earth. One might liken sleepy municipal elections to gateway drugs; by enticing so many people to abstain from voting, they weaken voting habits. Another interpretation is that voting in the initial election attracts the attention of political campaigns, which direct extra attention to recent voters, thereby promoting their continuing participation. The one study to track campaign activity—using contact records from the campaigns themselves—found that those assigned to the treatment group prior to a spring election were more likely to receive mail but no more likely to receive phone calls or personal visits prior to the fall general election (Rogers et al., 2015). Still another interpretation is that mobilization effects endure because subjects continue to remember the communication that mobilized them initially, a hypothesis that has some plausibility when the initial mobilization takes the form of strongly worded social pressure mailers.

4.4 Future directions

Much of the existing experimental work on stimulating voter turnout is inspired by theoretical accounts of why people vote or psychological theories of how individuals might be persuaded to take an action. These frameworks make directional predictions about the effects of interventions, but there is rarely any effort to estimate parameters of the subjects' utility function. An important avenue for future research is to use field experiments to estimate parameters in explicit structural models. An example of this work is Della Vigna et al. (2014), which incorporates a "social-image" motivation for voting into the subject's utility function and designs a set of experiments that identifies the monetary value of voting in order to avoid having to say you failed to vote (lying is costly). Based on

their experimental results and some assumptions (including an evidence-based assumed cost to subjects of lying), Della Vigna et al. estimate that the monetary cost of admitting failure to vote is between \$5 and \$15 for the 2010 congressional election, a plausible estimate given the observed level of turnout and the modest time and effort cost of voting.

A noteworthy feature of Della Vigna et al. is that, as a side benefit to their search for interventions designed to estimate model parameters, the authors conduct novel experiments that are interesting in their own rights. Misreporting of voting is a common source of measurement error in surveys. Della Vigna et al. examine the effect of providing the subject an incentive to tell the interviewer she did not vote; a random subset of survey respondents are told after 2 min of a 10-min survey that if they answer that they did not vote in the recent congressional election, the survey will end rather than continue for eight more minutes. Della Vigna et al. find that providing the incentive to say you did not vote has a small, statistically insignificant effect on reported voting by respondents who had voted, but nonvoters are substantially more likely to admit (that is, report truthfully) having not voted. Thus, very little misreporting of turnout is induced by the incentive, while the net degree of misreporting is substantially reduced in the incentive condition.

Another path for further research is to explicitly consider the implications of the accumulating corpus of academic work on voter mobilization for the “applied work” done by campaigns and elections. Most directly, there is the question of how campaign activity might be optimized given the experimental evidence on the relative effectiveness of alternative communications’ strategies or the differences in treatment response across individuals. Imai and Strauss (2011), continuing a line of work pioneered by Kramer (1966), consider the question of crafting the optimal GOTV campaign. They use data from existing experiments to estimate treatment effect heterogeneity and then compare the relative effectiveness of strategies that begin by targeting the individuals who are expected to have the largest turnout response versus a strategy that assumes zero-treatment effect heterogeneity. They find that there are often large expected gains from incorporating treatment-effect heterogeneity into the prioritization of mobilization targets.

A second question (perhaps better labeled a puzzle) regarding the relationship between the experimental work and real world campaign activity is to understand how accumulating experimental evidence affects industry practice. What accounts for the continued reliance by candidates and parties on methods with little evidence to support their use, despite the fact that elections have important stakes and are often sharply competitive? Some scholars have argued that the continued ubiquity of techniques experimentally demonstrated to produce small returns for large expenditures (early TV advertising, for example) stems from the financial windfalls the spending produces for campaign consultants (Sheingate, 2016). This explanation, while somewhat persuasive, seems at best incomplete, because all marketers, political or otherwise, would be

interested in selling worthless things at a high price, but this is probably not often a sustainable business model.

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