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

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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 increasing 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; Butler and Broockman 2011; Butler and Nickerson 2011; Broockman 2013, 2014; Grose 2014), 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 to provide 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 re-introduction 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, mail, and mass media) 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

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 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, any correlation between turnout and mobilization activity cannot be taken to demonstrate that there is any causal relationship. 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. XXX)

Gosnell took up this challenge and conducted the earliest field studies of voter mobilization. He investigated the effects of get-out-the-vote (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 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 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, Green, and Green 2003; Gerber, Green, and Nickerson 2003). 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 literatures. 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

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[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.

Election Commission) and candidate vote shares.[2] The pioneering work employing the newly available FEC data was 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 candidate spending levels (Green and Krasno 1998; 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.

Using the results from several of the leading studies, we can calculate the implied cost per vote (the cost of changing the vote margin by 1 vote). 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

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2 There were some exceptions, e.g., Ansolabehere and Gerber (1994).

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 work 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 work in the 1920s was by far the largest. Gosnell measured the effect of a non-partisan 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 elections in Ann Arbor. Greenwald and colleagues (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 second phone call on turnout and vote choice in a District of Columbia special election. In the same year, Miller, Baer and Bositis (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 which attacked the opposing candidates reduced the likelihood that subjects, when interviewed later, said they would vote, an effect 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 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.[3]

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. Work by Rosenstone and Hansen (1993) is an exemplar of this line of work. Their book was extremely influential and it remains a standard reference (its Google scholar citation count exceeded 3,400 as of the start of 2015). 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

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3 As observed by Gerber (2010), although it is often remarked that laboratory results can be taken as indicating the direction of real world effects but not their magnitude, this assertion is not necessarily true, and it may vary across contexts. 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 merely pay more attention to campaign related stimuli, leading to greater interest and higher participation levels.

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 ten 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 estimates 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 were 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:

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

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:

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

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 post-treatment 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 non-experimental 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, the estimand of the observational comparison of the treated and the untreated estimates is:

(3) $E(Y_{i1}/T_i=1) - E(Y_{i0}/T_i=0) =$
$[E(Y_{i1}/T_i=1) - E(Y_{i0}/T_i=1)] + [E(Y_{i0}/T_i=1) - E(Y_{i0}/T_i=0)] = ATT + Selection\ Bias.$

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), $E(Y_{i0}/T_i=1) = E(Y_{i0}/T_i=0)$ – i.e., 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 jurisdictions

voters based on both administrative records of voter turnout and demographics along with insider information about individuals and neighborhoods. This information, which is never fully available to the data analyst, is typically used in campaign targeting strategies. Thus, 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.[4]

Beyond selection bias, field experiments mitigate a variety of other common methodological concerns regarding observational studies of political behavior. In observational studies the researcher does not control either the treatment assignment or the design of the treatment. At the most basic level, a key feature of field experiment is that the researcher controls the assignment to treatment and therefore knows which subjects are assigned to treatment and control conditions. While 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 non-random 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 employed validated

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[4] 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, Green, and Kaplan 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.

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 1990[5]), one of the important innovations brought about by the advent of field experimentation in this area is that it has become standard in 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 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 other than 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.[6]

In experimental analysis it is now standard to account for non-compliance. In the context of voter mobilization non-compliance 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. Non-compliance 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 produce the average treatment effect among compliers (Angrist, Imbens, and Rubin 1996), which is the same as the average effect of the treatment on the treated when experiments encounter one-sided noncompliance.

Properly accounting for non-compliance in voter mobilization experiments is an innovation of the recent work, as experimental studies prior to 1998 either dropped the untreated

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5 See http://www.electionstudies.org/overview/dataqual.htm
6 Perhaps a footnote on lab versus field for voter mobilization?

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). Both 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 turnout rate when untreated than the average turnout rate for 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). 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 enables 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, Cutts, Widdop, and John 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. The next section describes the evolution of the experimental literature, which now encompasses studies conducted in Europe, Asia, and Latin America.

3. Major Directions in Modern Voter Mobilization Experiments

The modern voter mobilization literature can be divided into two main classes of studies. The earliest 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 there was sometimes experimental variation in message content, this was not the major focus of the research. A second type of study 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.

A. 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 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.

a. 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 canvassed using nonpartisan GOTV appeals. In 2001 a multi-site 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 yielded an effect of 7.1

percentage points with a standard error of 2.2 percentage points (Green, Gerber, and Nickerson 2003).

Another mobilization experiment conducted in 2001 extended previous work in three important directions (Michelson 2005). First, the canvassing effort achieved a remarkable 75 percent 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 studied the effect of different messages by varying the campaign message between civic duty and ethnic solidarity. 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 percent (N = 298) to 18.5 percent (N = 466). For non-Latinos, turnout increased from 25.7 percent (N = 758) to 28.2 percent (N = 1,243). Canvassers contacted 73 percent of Latinos and 78 percent 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, Melissa Michelson and Herbert Villa focused on a sample of voters under the age of twenty-six, encouraging them to vote in the 2002 state and federal elections (Michelson 2005). Turnout among Latino subjects rose from 7.2 percent (N = 1,384) to 9.3 percent (N = 1,507), and among non-Latino subjects it rose from 8.9 percent (N = 1,438) to 10.0 percent (N = 1,455). The contact rates were 51 and 39 percent, 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 2003). Overall, the control group (N = 2,672) turned out at a rate of 15.2 percent, compared to 14.9 percent in the treatment group (N = 3,371), which was contacted at a rate of 34 percent.

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 percent (N = 279) to 56.3 percent (N = 283) among those urged to vote. Since by design the contact rate was 100 percent, 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 organized a nonpartisan canvassing campaign aimed at mobilizing voters in low-turnout, low-income, and largely Latino precincts in Bakersfield, California.[16] 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 percent. The study found strong canvassing effects. Among voters living in one-person households, for example, turnout was 41.0 percent in the control group and 54.5 percent in the treatment group. Gregg Murray and Richard Matland also conducted a canvassing study in a largely Latino area, Brownsville, Texas.[17] Turnout among the 3,844 individuals assigned to the control group was 33.3 percent, compared to 34.9 percent among the 7,580 assigned to the canvassing group, of whom 22 percent 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 the experiments, which would occur by chance with $p < 0.001$.

Nonpartisan canvassing appears to mobilize those who are reachable at their doorsteps. Since partisan campaigns always have the option of using nonpartisan appeals to mobilize their partisan supporters, the nonpartisan canvassing results 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 vote for 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 fourteen assigned to the treatment group and fourteen to the control group. Among voters assigned to control precincts (N = 4,779), turnout was 29.1 percent, compared to 33.5 percent in the treatment group, 62.7 percent 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 (Michelson 2005). 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 percent of those living in one-voter households were contacted at least once. This figure rose to 80 percent among two-voter households. This mobilization campaign had a powerful effect on turnout. Among one-person households, turnout rose from 7.4 percent in the control group (N = 473) to 15.9 percent in the treatment group (N = 2,666). Among two-person households, turnout rose from 6.9 percent in the control group (N = 72) to 21.0 percent in the treatment group (N = 2,550).

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 2007). Potter and Gray (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 on behalf of state legislative

candidates in Republican primary runoff elections generated a treatment-on-treated estimate of 3.1 (SE = 1.8).

Table 3 collects the results of canvassing studies from 1998 to present and provides information on the political context (the control group turnout and the election in which the experiment was conducted) and the treatment effect estimates. We distinguish between non-partisan efforts, which employed messages encouraging participation, and advocacy, which involved messages of support for an issue or candidate. Table 4 reports the results of a meta-analysis of the studies in Table 3. Interesting, the treatment effect from canvassing found in New Haven in 1998 is roughly similar to the magnitude of the effects found in the meta-analysis for elections with similar turnout levels of background turnout (the New Haven point estimate of 8.4 percentage versus 6.2 percent for the meta-analysis). More generally, canvassing produces a sizable turnout boost in election with low and moderate turnout. The incremental effect of canvassing declines markedly with higher levels of background turnout, and the estimated effect cannot be distinguished from zero for high turnout elections.

b.   Phone studies.

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 our initial results were specific to the context of the calling house, we replicated the 1998 experiments on a grand scale in 2002.[10] 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 assigned to be 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.[11] This time one vote was generated per fifty-five 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). Head-to-head experimental comparisons between partisan and nonpartisan scripts indicate that neither have 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 fifty 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 twenty 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. However, this effect did not replicate in another large experiment in 2010, which found no effect of first round, second round, or follow-up calls (Gerber, Green, and Ha 2011).

These results 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 2007). 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, raising turnout by 3.4 percentage points. This statistically significant increase implies that one vote was generated for every thirty completed calls. The puzzling result is the fact that the chatty recruit-your-friends script had an unexpectedly weak effect, one vote per sixty-nine completed calls.

The call quality hypothesis was tested in 2010 in a head-to-head competition among different phone banks (Mann and Klofstad 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 non-political as well as political clients. Mann and

Klofstad 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 blends 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.

c. 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 was not assigned to receive mail, phone calls, or door-to-door canvassing, was 42.2 percent (N = 11,596). Turnout was 42.6 percent (N = 2,550) among those receiving one mailer, 43.3 percent (N = 2,699) among those receiving two, and 44.6 percent (N = 2,527) 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 percent (2,924); the treatment group turned out at a rate of 31.7 percent (1,137). Among other Asian groups, the control group voted at a rate of 38.5 percent (N = 5,802), compared with the treatment group rate of 39.4 percent (N = 2,095). 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 (Ramírez 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 conducted a nonpartisan mail campaign in largely Latino Brownsville, Texas.[14] 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 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 generate positive effects.

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 (Matland and Murray forthcoming; 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, Barrilleaux, and Scheller 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, Green, and Green 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 who stances had been previously identified by phone interviews. The treatment group (N=1,974) received five mailings that were printed in full color on glossy paper and mailed between nineteen and six days before the election. Turnout in the control group (N=2,008) was slightly higher than in the treatment group. Other small studies produced results that, on average, suggest little effect on turnout (Cho et al.

2005; 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 2014 multisite mail test in Republican primary elections in Texas (Green, Zelizer, and Kirby 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.[7]

d. Radio, Television, and Social Media
<mark>TO BE FILLED IN</mark>

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 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

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7 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 (DATES). These studies tend to produce effects that are somewhere between the non-effects of partisan mail and the weak effects of nonpartisan mail.

literature to get a sense for the robustness of the findings.[8] 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 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 (CITE). 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.

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8 Rogers, Fox, and Gerber (200X) 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.

A. 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 notation 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 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.[9]

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 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).[10] 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

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9 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 turnout. Simple reminders are often used as the placebo condition in messaging studies involving direct mail.

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

the order of a 1% turnout increase). In a replication study approximately 10 times the size of the original Greenwald study, Smith, Gerber and Orlich 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 (2009) 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 studied the effect of being administered a multi-question pre-election that included questions about turnout intention. He found that those registered voters assigned to be asked about their vote intentions and other political attitudes voted at the same rate as the randomly selected control group who were not called for the survey. A recent study by Green, Zelizer, and Kirby 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 action.[11] A messaging strategy based on this theory has been applied to voter turnout by supplementing the self-prophecy item (do you

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11 Cite to papers testing this theory outside of political behavior

expect to vote?) with follow up questions about the details 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?[12] 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 question implementation battery was 2.1 percentage points.[13]

Several other studies have investigated the effect of elaborating a voting plan. These include Dustin Cho (2009), who found a slight turnout decline from an implementation phone call 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.[14] [Insert Gerber, Huber, Hill here when the numbers are crunched].

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

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12 Slightly over 85% of subjects said that they planned to vote in the upcoming election.
13 Further exploration of treatment effect by Nickerson and Rogers revealed that, unexpectedly, the effect of I.I. was concentrated among those who resided in households with one eligible voter, for whom the overall effect of the I.I. script was 9.1 percentage points and the incremental effect of the I.I. battery was 8 percentage points. The authors speculated that this finding was consistent with the idea that the I.I. battery was redundant for those living in multiple voter households because, due to the relative centrality of politics in these households and other aspects of the multi-resident social context, these individuals were more likely to already have a voting plan.
14 The Rogers and Ternovski mailing also included a "gratitude treatment," a message strategy described later in this paper.

Coate et al, 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 (Coate et al). The particular voting rule (what is the cut point for the cost of 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 voter are more deeply rooted. If individuals are susceptible to feelings of pride and shame regarding their voting behavior, interventions that heighten these feelings may work to change 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, researchers employed 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 non-partisan mailing raises turnout by less than 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.[15] The main effort has been to replicate and extend the "Self" mailing. Many of the academic field experiments are performed in partnership with non-profit organizations, campaigns, and governments. The social pressure mailings, especially those that confronted voters with their voting record and those of their neighbors (the highly effective "Neighbors" mailing), produced some backlash (Matland and Murray 2013), 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 3 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

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15 Although the neighbors mailing has been used from time to time in campaigns, there is one academic follow up to the Michigan neighbors mailing. In the very high turnout 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 percent, a subgroup with participation levels more similar to the Michigan subjects than the overall Wisconsin subject pool.

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.[16]

A second strategy is to employ social norms to praise rather than scold. Panagopolous used this approach and encouraged subjects to join an "honor roll" of perfect voters. A collection of voters identified as African American, Hispanic, and 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 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.

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 subject 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

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16 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 Independent's to vote because the Republicans in power where having a negative impact did not increase turnout (Settle and Schwenzfeier 2014). This may be related to the mechanism thought to be at work. A partisan message is typical 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).

adding this to the mailing was a statistically significant quarter percentage point increase in turnout.17

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, Kimeldorf, and Cohen 2008, Bernstein and Simmons 1974, Clark, Northrop, and Barkshire 1988, Rind and Bordia (1995), Panagopolous 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 is being discussed.

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 percent 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 is that in Georgia Panagopolous included two additional treatment arms, a mailing in which the vote history was discuss but there was no mention of official records of voter turnout and 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. To get a feel for this message approach, consider the text of the Georgia mailings. The key portion of the basic gratitude message was:

THANK YOU FOR VOTING!

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17 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, List, Malmendier and Rao (2014).

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."

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 excludes the 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 expressional of gratitude was a 3.1 percentage point turnout boost, 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.

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 characterizing voting rates as high or low on subjects' actual turnout. Panagopoulos, Larimer, and Condon (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.

Discussion.

A. COMMENT ON THE EXTENSIVE LITERATURE OUTSIDE THE US THAT HAS TESTED SOME OF THE SAME PROPOSITIONS

B. Downstream effects

One of the most interesting findings to emerge from GOTV research 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.[9] This type of persistent effect has since been replicated many times over.[10] For example, the 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 1,850 votes in August 2006, plus an additional 900 votes over the next three August primaries.[18] This pattern of over time persistence

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18 Coppock, Alexander, and Donald P. Green. Forthcoming. Is Voting Habit Forming? New Evidence from Experiments and Regression Discontinuities. *American Journal of Political Science*.

holds for other large social pressure studies and for the array of experiments that Lisa Garcia Bedolla and Melissa Michelson report in their landmark study of minority mobilization.[19]

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. 2014). 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.

C. Future directions

Structural models

Optimization of messaging and targeting

Understanding the economics of firms that sell suboptimal GOTV services

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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

NOTE: 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 3,800 votes. Adapted from Gerber (2004).

Table 2: Voter Mobilization Experiments Prior to 1998 New Haven Experiment

Study	Date	Election	Place	N of subjects (including control group)	Treatment	Effects on Turnout *
Gosnell (1927)	1924	Presidential	Chicago	3,969 registered voters	Mail	+1%
Gosnell (1927)	1925	Mayoral	Chicago	3,676 registered voters	Mail	+9%
Eldersveld (1956)	1953	Municipal	Ann Arbor	41 registered voters	Canvass	+42%
				43 registered voters	Mail	+26%
Eldersveld (1956)	1954	Municipal	Ann Arbor	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	2,650 registered voters	Phone	+9%
Greenwald et al (1987)	1984	Presidential	Columbus, Ohio	60 registered voters	Phone	+23%

* These 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, Green, and Nickerson (2001).

Table 3. Results of Door-to-Door Canvassing Experiments

	Context	Study	CACE	SE	Control Turnout	advocacy?
1.	1998G Gerber & Green -- New Haven		8.4	2.6	30% to 50%	
2.	2000G Green & Gerber -- Oregon		8.4	4.5	50% to 70%	
3.	2001G Green et al. -- Bridgeport		14.4	5.3	Under 30%	
4.	2001G Green et al. -- Columbus		9.7	7.9	Under 30%	
5.	2001G Green et al. -- Detroit		7.8	4.5	30% to 50%	
6.	2001G Green et al. -- Minneapolis		10.1	8.7	Under 30%	
7.	2001G Green et al. -- Raleigh		.2	3.2	Under 30%	
8.	2001G Green et al. -- St. Paul		14.4	6.4	30% to 50%	
9.	2001G Michelson -- Dos Palos		4.1	2.2	Under 30%	
10.	2002G Bennion -- Indiana		.6	5.1	30% to 50%	
11.	2002G Gillespie -- St. Louis		.8	1	Under 30%	
12.	2002G Michelson -- Fresno		3.5	1.6	Under 30%	
13.	2002G Nickerson et al. -- Michigan		16.8	15.9	30% to 50%	X
14.	2002M Gillespie -- Newark		-7.9	27.9	30% to 50%	X
15.	2002P Nickerson -- Denver		8.6	4.2	30% to 50%	
16.	2002P Nickerson -- Minneapolis		10.9	4.1	Under 30%	
17.	2002R Gillespie -- Newark		1.2	7.3	Under 30%	X
18.	2003G Arceneaux -- Kansas City		7	3.9	Under 30%	X
19.	2003G Michelson -- Phoenix		12.9	1.8	Under 30%	X
20.	2004G LeVan -- Bakersfield		24.2	7.5	30% to 50%	
21.	2004G Matland & Murray -- Texas		7.4	4.3	30% to 50%	
22.	2005G Anonymous -- Virginia		3.5	2.4	30% to 50%	X
23.	2005G Nickerson -- Virginia		27	15.4	Under 30%	X
24.	2006G Bedolla & Michelson -- AACU		-3.4	8.1	30% to 50%	
25.	2006G Bedolla & Michelson -- CARECEN		-.5	2.9	50% to 70%	
26.	2006G Bedolla & Michelson -- CCAEJ		4.4	5.9	50% to 70%	
27.	2006G Bedolla & Michelson -- PICO		3.1	3.9	30% to 50%	
28.	2006G Bedolla & Michelson -- SCOPE		6.6	2.1	30% to 50%	
29.	2006G Nickerson -- Dearborn		8.7	3.8	30% to 50%	
30.	2006G Nickerson -- Grand Rapids		-.4	4.3	Under 30%	X
31.	2006P Bedolla & Michelson -- CARECEN		2.2	1.8	Under 30%	
32.	2006P Bedolla & Michelson -- CCAEJ		43.1	12.5	Under 30%	
33.	2006P Bedolla & Michelson -- SCOPE		2.6	3.3	Under 30%	
34.	2007G Davenport -- Boston		13.4	7.02	Under 30%	
35.	2007P Bedolla & Michelson -- AACU		-1.4	2	Under 30%	
36.	2008G Arceneaux et al. -- California		10.7	10.2	Above 70%	
37.	2008G Bedolla & Michelson -- CARECEN		.7	6	50% to 70%	
38.	2008G Bedolla & Michelson -- CCAEJ		.3	4.5	50% to 70%	
39.	2008G Bedolla & Michelson -- PICO		1.2	1.7	50% to 70%	
40.	2008G Bedolla & Michelson -- SCOPE		.5	1.1	Above 70%	
41.	2008P Bedolla & Michelson -- CARECEN		4	2.6	Under 30%	
42.	2008P Bedolla & Michelson -- CCAEJ		3.9	2.8	Under 30%	
43.	2008P Bedolla & Michelson -- PICO		1	1.3	Under 30%	
44.	2008PP Bedolla & Michelson -- CARECEN		.9	3.2	50% to 70%	
45.	2008PP Bedolla & Michelson -- PICO		9	3.4	30% to 50%	

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     |-------------------------------------------------------------------------|
46.  |   2008PP Bedolla & Michelson -- SCOPE       3.4      2.3   50% to 70%      |
47.  |    2010G Barton et al. --   Midwest        -7.7      3.8   50% to 70%    X |
48.  |      2010G Bryant -- San Francisco        -32.9     21.6   50% to 70%      |
49.  |          2010G Cann et al. -- Utah          8.2      4.6   50% to 70%      |
50.  |   2010G Hill & Lachelier -- Florida         1.8      9.3    Under 30%      |
     |-------------------------------------------------------------------------|
51.  |          2014R Green et al. -- Texas        3.1      1.8   30% to 50%      |
     +-------------------------------------------------------------------------+
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Notes: CACE = average treatment effect among compliers. SE = standard error of the estimated CACE.

Table 4. Meta-Analysis Results, by Base Rate of Turnout

Turnout Rate in the Control Group	All Canvassing Average CACE	Non-Advocacy Canvassing Only Average CACE
Under 30%	4.6 [2.4, 6.9]	3.3 [1.4, 5.2]
30% to 50%	6.2 [4.2, 8.2]	6.7 [4.4, 9.0]
50% to 70%	1.4 [-1.2, 4.0]	2.1 [0.1, 4.1]
70% and Higher	0.6 [-1.5, 2.8]	0.6 [-1.5, 2.8]

Notes: Estimates obtained through random effects meta-analysis. 95% confidence intervals in brackets.

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	2006 August Primary	Michigan	29.7 (191,243)	34.5 (38,218)	16%*
2	2007 Municipal	Michigan	27.7 (772,479)	32.4 (27,609)	17%*
3	2007 Gubernatorial General (Previous Nonvoters)	Kentucky	6.8 (19,561)	8.9 (13,689)	31%*
3	2007 Gubernatorial General (Previous Voters)	Kentucky	13.2 (25,037)	16.3 (17,731)	23%*
4	2009 Municipal Special	New York City	3.2 (3,445)	4.2 (3,486)	36%*
5	2010 General	Texas	40.5 (63,531)	43.1 (1,200)	6%
5	2010 General	Wisconsin	49.0 (43,797)	50.8 (801)	4%
6	2011 Municipal	California	10.6 (13,482)	12.0 (1,000)	13%

* Statistically significant at $p < 0.01$, one-tailed test. This Table is adapted from Table 11-1, Get out the Vote, Green and Gerber (2015).

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