

Voter Mobilization

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word count: 7645

Civic participation is an essential component of a healthy democracy. Voting allows citizens to communicate preferences to elected officials and influence who holds public office. At the same time, deficiencies and asymmetries of participation in the United States call into question the representativeness of elected officials and public policies.¹ Yet, while political activity is crucial for the equal protection of interests, participation is often seen by individuals as irrational or excessively costly, and it is well known that turnout in the U.S. lags well behind that of other democracies. Scholars have consistently found that participation is linked to socioeconomic variables, psychological orientations, and recruitment. Candidates, parties and organizations thus spend considerable effort mobilizing electoral activity. This chapter highlights contributions made by field experiments to the study of voter mobilization, as well as the problems faced by such work and opportunities for future study.

Observational Studies

Non-experimental studies have primarily relied on survey research to demonstrate correlations between self-reported mobilization and various civic-minded behaviors, while also controlling for various demographic characteristics (for example, age, education, and income) known to be significant predictors of turnout.² The conclusion usually reached is that mobilization efforts are generally effective (for example, Rosenstone and Hansen 1993; Verba, Schlozman and Brady 1995). However, four major empirical hurdles render this conclusion suspect.

¹ For example, California's population has not been majority-Anglo (non-Latino White) for some time, and yet the electorate is over two-thirds Anglo. Thus, elections and ballot measures are decided by an electorate that is not necessarily representative of state opinion.

² Most of the critique that follows is equally applicable to the few studies using selection models. Selection models acknowledge the problem with strategic targeting by campaigns and attempt to model the process; however, such models rely on strong assumptions that may not be warranted in many instances, so the problem of strategic selection is not fully solved (see Sartori 2003).

First, campaigns strategically target individuals likely to vote, donate money or volunteer, thereby creating a strong correlation between the behavior or attitude to be studied and campaign contact. Because contacted individuals are more likely to participate than noncontacted individuals – even in the absence of mobilization – strategic targeting causes researchers to overestimate the effect of mobilization. That is, observational samples use an inappropriate baseline for comparison.

Second, individuals who are easier to contact are also more likely to vote. Arceneaux, Gerber and Green (2006) analyze experimental data as if they were observational by matching contacted individuals in the treatment group to people in the control group with exactly the same background characteristics. They find that matching overestimates the effect of mobilization. Matching fails to account for unobserved differences between treatment and control subjects (for example, residential mobility, health, free time, mortality, social behavior), leading to inflated estimates of the power of the treatment. Thus, the treatment of interest (that is, mobilization) is likely to be correlated with unobserved causes of participation.

A third drawback to survey-based research is that respondents often exhibit selective recall. Politically aware individuals are more likely to report contact from campaigns and organizations because they pay more attention to political outreach and are more likely to place the event into long-term memory (Vavreck 2007). Since politically interested people are more likely to participate, the correlation between mobilization and behavior could be a function of selective memory. Thus, the key independent variable in observational studies relying on self-reported campaign contact is likely to suffer measurement error.

Finally, survey questions used to collect self-reported campaign contact offer categories too coarse to estimate treatment effects. Standard survey questions tend to treat all forms of

campaign contact as identical. For example, the American National Election Study (ANES) item on mobilization asks, “Did anyone from one of the political parties call you up or come around and talk to you about the campaign? (IF YES:) Which party was that?” Yet, experiments using various types of outreach clearly show that the method and quality of mobilization matters, generating turnout effects that range from negligible to double-digits (Green and Gerber 2008). Coarse, catch-all survey measures obscure the object of estimation by lumping heterogeneous forms of campaign contact together.

Controlled experiments directly solve each of these problems associated with observational studies. Random assignment eliminates selection problems and constructs a valid baseline for comparison. By directly manipulating the treatment provided to the subjects, researchers can avoid relying on overbroad survey questions and the vagaries of self-reported behavior. Field experiments using official lists of registered voters can also maximize external validity by including a wide-range of subjects and using official voter turnout records to measure the dependent variable of interest for both the treatment and the control group.

Pioneering Experiments

The experimental literature on mobilization dates back to Gosnell (1927). Following Gosnell, experiments were used sporadically over the next several decades (Eldersveld 1956; Adams and Smith 1980; Miller, Bositis, and Baer 1981). These small-N studies tested a range of techniques (mail, phone, and door-to-door canvassing) and all reported double-digit increases in voter turnout. Unfortunately, these early studies provided biased estimates of campaign contact by placing uncontacted subjects assigned to the treatment group in the control group. That is, these pioneering studies undercut the analytic benefits of randomization by focusing only on the contacted individuals and turned the experiments into observational studies.

The real flowering of the experimental study of campaign effects came at the turn of the millennium with the 1998 New Haven experiment (Gerber and Green 2000). A large number of subjects were drawn from a list of registered voters and randomly assigned to various nonpartisan treatments (mail, phone, or door) or to a control group. Gerber and Green then had callers and canvassers carefully record whether each subject in the treatment groups were successfully contacted and referenced official records to verify voter turnout for both the treatment and control groups.³ The failure-to-treat problem was addressed by using random assignment as an instrument for contact, thereby providing an unbiased estimate of the effect of contact. The experimental design and analysis disentangled the effect of mobilization from the effects of targeting and selective memory and was very clear about the nature of contact provided to individuals, thereby avoiding measurement error. They concluded that face-to-face contact raised turnout by nine percentage points, mail boosted turnout by half a percentage point, and phone calls did nothing to increase participation.

It is curious that the logic of experimentation took so long to take root in the study of campaigns. Fisher (1925) laid the intellectual groundwork for experiments during the 1930s. There were few technological hurdles to the process since randomization could be performed manually (for example, coin flip) and the analysis through card sorting. Examples of laboratory experiments studying the effects of television advertisements on attitudes and vote intention had been published in leading journals (see Gadarian and Lan, this volume). Regardless of the cause of the delay, the last decade has seen an explosion of interest in the use of field experiments to explore voter mobilization, with increasing attention to other facets of electoral campaigns such as vote choice and campaign contributions. In a meta-analysis of the more than one hundred

³ Ironically, a merging error that did not substantively alter the results cast doubt upon the initial findings with respect to phone calls (Imai 2005).

field experiments replicating their initial study, Green and Gerber (2008) conclude that well-conducted door-to-door visits generally increase turnout by six to ten percentage points, volunteer telephone calls by two to five percentage points, and indirect methods such as mail generally not at all (with some notable exceptions).

Extensions to the first experiments

The initial studies have been extended in a large number of ways. Some studies have examined previously tested techniques for heterogeneity. One notable contribution followed up on initial findings that commercial phonebanks were generally ineffective, while volunteer phonebanks were usually successful. Nickerson (2007) trained volunteer callers to behave like commercial phonebank staff, giving them quotas of numbers of individuals to reach each shift, while paying commercial canvassers to behave like volunteers, urging them to take their time and engage voters in conversation. The result was a reversal of the general trend: commercial phonebankers trained to act like volunteers were able to move voters to the polls, while rushed volunteers were ineffective. Thus, Nickerson concluded that it was the quality of the phonebank that mattered, not the identity of the canvasser or whether or not canvassers were paid.

Other experiments have examined other campaign tactics for contacting voters, such as radio and television advertisements, leaflets, email, and text messaging. In general, the pattern has been that personalized outreach is more effective than indirect outreach. But there are notable exceptions. For example, Dale and Strauss (2007) find that text messages are effective at moving young people to the polls. Whether this is a counter example or evidence that cell phones are considered personal objects is open to debate, and further research is needed to confirm and further explore their findings. Similarly, the effectiveness television advertisements (Vavreck 2007, Green and Vavreck 2008) may be evidence that not all indirect methods of

reaching out to voters are ineffective, or may say something about the power of visual images. Paradoxically, the same rapid growth in field experiments that allowed for precise estimates of the effectiveness of mobilization techniques has also complicated the theoretical picture, necessitating more experiments.

A third line of extensions from the initial New Haven experiment has focused on subpopulations with below average rates of voter turnout. To the extent that low rates of participation bias the electorate, focusing on groups with the lowest rates of voter turnout is a priority. Research in other areas of political science suggests that civic engagement strategies that are effective with Anglos (nonLatino whites) will not necessarily work for African Americans, Latinos and Asians. However, a lengthy series of recent experiments demonstrate that these subgroups generally respond to requests to vote in a similar manner as do high-propensity voters (Michelson, García Bedolla and Green 2007, 2008, 2009). Each population faces its unique challenges, however. The residential mobility of young and poor voters makes them harder to contact (Nickerson 2006). Campaigns targeting Latinos need to be bilingual in most instances and efforts aimed at Asian-Americans need to be multilingual. Despite these challenges, field experiments have proved that all of these groups can be effectively moved to the polls

A fourth set of analyses extend the experimental project by considering the dynamics of voter mobilization. Contact has been found to be more effective as Election Day approaches, and yet thirty to fifty percent of the mobilization effect on turnout in one election is carried into future elections (Gerber, Green, and Shachar 2003). That is, blandishments to vote are less effective when made earlier in an election campaign, suggesting that contact has a limited shelf life, but those individuals who are effectively moved to the polls continue to be more likely to

vote in future elections, suggesting the act of voting is transformative. Using data from fourteen experiments targeting low-propensity communities of color conducted previous to the November 2008 election, Michelson, García Bedolla and Green (2009) find a similar habit effect in low-propensity communities of color. Across fourteen separate mobilization experiments conducted during 2008, one third of the mobilization effect generated earlier in the year was transferred to turnout in the general election. Gerber et al. hypothesize that individuals successfully encouraged to vote may in the future feel more self-confident about their ability to negotiate the voting process, or may have shifted their self-identity to include civic participation rather than abstention.

Mobilization experiments have also explored the effect of social networks. Nickerson (2008) examines two canvassing efforts that spoke with one individual in two-voter households, allowing for measurement of both the effect on contacted voters and their housemates. The study utilized a unique placebo design, wherein individuals assigned to the control group were contacted but received a message encouraging them to recycle. Both experiments found that sixty percent of the propensity to vote was passed along to the other member of the household. Yet, other experiments using social networks have produced mixed results (see Nickerson, this volume).

Electoral context is also an important factor: even if all individuals in a treatment group are successfully contacted, not all will be moved to vote. This is a reflection of the ongoing real-world context from which experimental subjects are taken (see Gaines, Kuklinski and Quirk 2007). Arceneaux and Nickerson (2009) argue that mobilization has the strongest effect on voters who are indifferent about turning out, but these indifferent voters are not the same from one election to the next. Only low-propensity voters can be mobilized in high-salience elections,

while high-propensity voters are more likely to respond in low-salience elections and occasional voters are best targeted during mid-level salience elections.

Since the baseline effectiveness of various treatments has been established, voter mobilization experiments provide an excellent real-world setting by which to test social psychological theories. Researchers know how much turnout is elicited using various techniques. By embedding psychological theories into messages encouraging turnout, the strength of the role the psychological constructs play in voter mobilization can be measured. One of the first efforts to link social psychology to voting behavior through field experiments was conducted by a team of researchers at Ohio State University prior to the 1984 presidential election. Students predicting that they would vote were in fact more likely to do so (Greenwald et al. 1987). Efforts to replicate the finding on a larger scale, however, have failed to uncover reliable treatment effects on representative samples of voters (for example, Smith, Gerber and Orlich 2003). Gollwitzer's theory of implementation intentions (Gollwitzer 1999), which holds that articulating explicit plans for action increases follow through, has been found to more than double the effect of mobilization phone calls by simply asking subjects about when they will vote, where they will be coming from, and how they will get to the polling place (Gerber and Rogers 2009; Nickerson and Rogers 2009).

Psychological theories have also been used to explain apparent paradoxes in the literature. For instance, contacting people more than once, either by phone or in person, does not increase turnout significantly more than a single phone contact. However, an important caveat to this finding is that follow-up calls made to individuals who indicate in an initial contact that they intend to vote has a powerful and large effect on turnout (Michelson, García Bedolla and McConnell 2009). In a series of experiments, Michelson et al. asked youth, Latinos, and Asian

Americans that were contacted during an initial round of telephone calls whether or not they intended to vote. Restricting follow-up calls to so-called “yes” voters resulted in double-digit treatment effects, most of which can be attributed to the second call. To explain this finding, Michelson et al. turn to Sherman’s (1980) theory of the self-erasing nature of errors of prediction, which posits that asking people to predict their future behavior increases the likelihood of them engaging in the predicted behavior, and Fishbein and Ajzen’s (1957) theory of reasoned action, which holds that subjects respond to treatment if a social norm is cued and subjects care what others think.

Monitoring has been found to enhance compliance to social norms in the laboratory (for example, Rind and Benjamin 1994). Consistent with those findings, field experiments have found that the mobilization effect is enhanced by messages that signal to voters that their behavior is being observed. Green, Gerber and Larimer (2008) sent mailers to targeted individuals that indicated to varying degrees that they were being monitored. Some mailers noted only that researchers were watching the election, while others included a list of the voting history of the recipient alone or along with that of their neighbors, some with a promise to send an updated chart after the election. The more intrusive and public the information provided, the larger the effect on turnout. The final treatment arm in the experiment raised turnout by 8.1 percentage points, exceeding the effect of many door-to-door efforts.

Without existing benchmarks to compare the results, the 1998 New Haven experiment simply constituted proof that campaigns can mobilize voters and overcome the collective action problem inherent in political participation. It also provided an invaluable example of how campaigns could be studied experimentally. That template has been expanded on to answer questions of increasing nuance and detail about who can be mobilized, the dynamics of

mobilization, and the psychology of mobilization. Despite the wealth of insights gained from the last decade of experiments, the experimental study of mobilization behavior faces a large number of potential problems. The next section discusses the practical problems of carrying out experiments, concerns about the external validity of the findings, and ethical concerns about studying campaign activities.

Problems Facing Field Experiments: Implementation

The major attraction of experiments as a methodology is that randomization assures the treatment and control groups are comparable. By gathering theoretically “ideal” data sets, researchers can offer transparent and straightforward analysis without the need for control variables or complicated modeling. However, constructing these data is difficult. Not only is the process time consuming, but working in the field removes control from the researcher, and problems can arise. In particular, treating the correct people and documenting the contact can be difficult.

While the heads of organizations may agree to participate in experiments, faithful execution of the protocol is not always a given. Mistakes can be made by managers when providing lists. Volunteers may make mistakes when knocking on doors, speak with everyone encountered on a block in enthusiasm for the campaign or avoid blocks entirely because they don’t think the campaign will be well received. Treating members of the control group is mathematically equivalent to failing to apply the treatment to members of the treatment group and does not necessarily invalidate the experiment. The assignment can still be used as an instrument for actual contact to purge the estimate of the nonrandom determinants of contact, but statistical power will suffer dramatically (Nickerson 2005).

Carefully training managers and canvassers can help to mitigate these problems, as can active involvement by the researcher in providing lists and monitoring the campaign. Randomizing at the precinct-level, rather than at the household level, can prevent many errors by managers and volunteers. The serious downside of this strategy is that the power of the experiment decreases. The power of an experiment comes, in large part, from the number of random decisions made. Randomly assigning 10 precincts to treatment and control groups rather than 5,000 households yields vastly fewer possible outcomes of the process. Statistical power is decreased when subjects in a group share characteristics and tendencies (that is, intraclass correlation is high). Whether this decrease in power offsets problems in implementation depends on the extent of anticipated problems and the degree of homogeneity in precincts among subjects. Last minute changes in strategies have caused a number of experiments to go in the dumpster as the control group is mobilized just like the treatment group. Nickerson (2005) offers various scalable protocols to conserve statistical efficiency in the face of problems implementing a treatment regime.

Even if an organization makes a good faith effort to adhere to the prescribed protocol, simply applying the treatment to assigned subjects can be objectively difficult. When working door-to-door, canvassers must negotiate unfamiliar streets, and in rural neighborhoods may find themselves in areas without street signs or house numbers. Physical barriers such as locked gates and apartment buildings, the presence of dogs or the lack of sidewalks may prevent canvasses from accessing doors. Even when canvassers have access, targeted voters are often not at home. Young people and low-income individuals are likely to have moved since registering; older individuals are often at work or otherwise away when canvassers are available. All of these factors will cause contact rates to be less than 100 percent (in fact, contact rates in the high single

digits or low teens are not uncommon for a single pass through a neighborhood or call sheet). Low contact rates reduce statistical power and the primary solution is to revisit the neighborhood or phone list repeatedly. This added labor can decrease the number of subjects covered. Thus, researchers should structure their randomizations in such a way that unattempted people can be placed into the control group or omitted from the analysis (Nickerson 2005).

Experiments in some minority areas pose special challenges because of naming conventions. Latino families often use the same first names but with suffixes (for example, Junior, Senior), or with different middle names (for example, Maria A. Garcia and Maria E. Garcia). Many all share the same twenty last (clan) names, and have very similar first names as well. Canvassers working in these communities must be particularly attentive to the details of the names (and perhaps ages or other identifying information) in order to ensure that they are contacting the targeted individual. Those preparing walk lists or call sheets must be attentive to these issues as well; for example, by not deleting the middle or suffix name columns to save space, and by drawing attention to these problems during canvasser training. Furthermore, matching names to voter files after the election can be complicated as multiple matches will be likely. Once again, collecting and retaining as much identifying information as possible will mitigate these problems.

A final logistical problem (and a challenge for internal validity) is defining what constitutes contact from the campaign. Contact is not a problematic definition for impersonal forms of outreach such as mail, leaflets, and email. Incorrect addresses and spam filters may prevent some materials from reaching their intended targets, but most mailed and emailed GOTV messages can safely be assumed to have been delivered. However, it is difficult to know how much of a script must be completed on the phone or in person to consider a subject treated. This

coding decision makes no difference for intent-to-treat analysis that relies solely on assignment to treatment conditions (and is most useful for program evaluation), but it poses a large problem for attempts to measure the effect of a campaign on individuals (the quantity political scientists are typically interested in). If treatment is defined as a respondent listening to the entire script, but there is an effect of listening to half of the script and hanging up, then estimates of the treatment effect will be biased. An alternative is to define treatment more loosely, including any individual with whom any contact is made. This allows for more reasonable adoption of the assumption that noncontact has zero effect, but may also dilute the measured effect of the intended treatment.

A related problem is heterogeneity in the treatment applied by canvassers and callers. Again, variance in the treatment provided is not a problem for indirect tactics, but it is a concern when campaign workers are interacting with subjects. In laboratory settings, variance in treatment is typically solved by limiting oversight and implementation of the experiment to one or two people. This solution is not practical in large voter mobilization experiments where hundreds of thousands of households can be included in the experiment. Conversations that are rushed and impersonal are less effective than those that are measured and conversational (Nickerson 2007, Michelson et al. 2009, Ha and Karlan 2009). The talent and charisma of individual volunteers will vary in large campaigns and subjects may be given qualitatively different treatments depending on their canvasser/caller.

Researchers can work to minimize variance in treatment by carefully training workers and crafting scripts that anticipate deviations and questions, thereby equipping canvassers to provide consistent answers. However, the researcher should keep in mind that the quantity to be estimated is always an *average* treatment effect. This average conceals variation in how subjects

respond and variation in the treatment provided. Researchers can take two steps to capture this variation. Canvassers and callers can be randomly assigned phone numbers or canvassing areas, and researchers can record which canvasser contacts each targeted subject. Combined, these two design principles allow researchers to measure the extent of the variation across canvassers.

Problems Facing Field Experiments: External Validity

The chief reason to study campaign effects in the field rather than in the laboratory is to more accurately capture the experience of typical registered voters receiving contact in real-world settings with the associated distractions and outside forces acting on the interaction. That is, the whole point of field experiments is external validity. However, field experiments themselves can only draw inferences about compliers, campaigns subjecting themselves to experimentation, and the techniques campaigns are willing to execute.

Researchers can attempt to include all registered voters in an experiment and make assignments to treatment and control groups. However, as discussed above, the treatment will not be applied to all subjects. Subjects can be usefully divided into those who are successfully treated (compliers and always-takers) and those who are not (noncompliers) (Angrist, Imbens and Rubin 1996). As an epistemological matter, it is impossible to know the effect of the treatment on noncompliers because they do not accept the assigned treatment by definition. Thus, conditioning on contact provides researchers only with the average treatment effect on those contacted. People who cannot be contacted are likely to be different from people who can be contacted (Arceneaux, Gerber and Green 2006), so the extent to which the results apply to the uncontacted is an open question. Raising contact rates can address some concerns about external validity, but without 100 percent compliance it is impossible to know what would happen if all targeted individuals were successfully contacted.

Researchers are also limited by the types of campaigns that agree to cooperate with them. Specifically, campaigns are likely to agree to randomize their contacts only when they have limited resources or if they do not believe the experiment will influence the outcome of the election. Given the high level of uncertainty of most political candidates, as well as the contradictory and expensive advice of campaign consultants, this generally has meant that political parties and candidates have declined to participate in field experiments.⁴ To date, only one high profile campaign, that of Rick Perry in the 2006 Texas gubernatorial race, has agreed to participate in a nonproprietary experimental study (Gerber et al. 2007). The bulk of experiments have been conducted by nonpartisan 501(c)3 civic organizations, many of whom have a strong incentive to cooperate as funders increasingly want such efforts to include experimental evaluation components. If well funded and highly salient campaigns behave differently and/or voters respond differently to outreach from “brand name” organizations, then external validity is a real concern for much of the mobilization literature.

A primary tension in the experimental mobilization literature is between theory and authenticity. Working with an actual campaign or organization can expand the scope of an experiment and add verisimilitude, but organizations have competing goals that compromise research design. Because of objections from the organization being studied, theories are rarely tested cleanly. Experiments can be designed to minimize the direct and indirect cost to campaigns, but the tradeoff is nearly unavoidable. For example, groups regularly resist removing a control group from their target pool of potential voters, either because they overestimate their ability to gather enough volunteers and reach all voters in a particular community or because they believe it will hurt their reputation if they do not reach out to all individuals that would expect to be contacted. Organizations also resist trying new techniques

⁴ Most partisan experiments conducted to date have been proprietary in nature.

proposed by researchers and prefer to use familiar techniques used by the group in past campaigns. Some of these objections can be overcome by offering additional resources in exchange for cooperation, encouraging groups to provide an honest estimate of organizational capacity, and designing experiments to minimize the bureaucratic burden on managers. Still, cutting edge research is difficult to orchestrate with existing campaigns and organizations.

Researchers constructing their own campaign have more freedom, although they are still limited by internal institutional review board (IRB) requirements and federal law, but their efforts may not mimic actual campaign behavior. For example, researchers are likely to be constrained by tax laws preventing research dollars from pursuing partisan aims, thereby limiting much of their research to nonpartisan appeals. Conducting free-standing campaigns also opens researchers to a host of ethical considerations that are largely not present when working with an organization already intervening in the community.

Problems Facing Field Experiments: Ethical Concerns

Voter mobilization scholars interacting with “real-world” politics and political campaigns have the potential to change real-world outcomes. Thus, they face ethical obligations that likely exceed limits that might be imposed by internal IRBs. The first ethical concern is that conducting experiments in actual electoral environments can present a situation where a researcher could swing a close election. Most high profile elections are decided by large margins, but even here there are well-known exceptions, such as the narrow victories of George W. Bush in Florida in 2000, Christine Gregoire in the Washington gubernatorial election of 2004, and Al Franken in the 2008 Senate race in Minnesota. Local elections are much more frequently decided by small margins, many by only a few dozen votes. Thus even a nonpartisan voter mobilization campaign could swing an election by increasing voter turnout in one

neighborhood but not another. Avoiding experiments that could potentially alter electoral outcomes may well reduce allegations of tampering; however, such a strategy may limit the external validity and usefulness of GOTV research.⁵ Without research in tightly contested partisan settings, scholars are limited in the conclusions they can draw about when mobilization works and which types of messages are most persuasive.

On the other hand, working in cooperation with “real” campaigns does mitigate some ethical concerns. Working with campaigns means that an experiment simply systematizes an activity that would take place in any case. A control group or ineffective experimental treatment to be tested could swing an election, but the decision is ultimately made by the candidate or civic group studied, not by the researcher. Working with organizations engaged in campaigns immunizes researchers to some extent from ethical concerns about election outcomes.

Yet, much as doctors and psychologists face dilemmas on whether to monitor government torture, researchers must consider carefully whether or not they want to be involved in and lend validity to campaigns that pursue illiberal ends, violate privacy, or cause psychological distress. For instance, flyers announcing that elections are held on Wednesday may be an effective campaign tactic, but testing such a tactic violates the democratic norm of broad participation. Similarly, voter files make accessible to scholars massive amounts of personal information. As with all research that involves human subjects, the privacy of individuals must be respected. Scholars should take steps to anonymize data as thoroughly as possible when sharing with other academics and research assistants.

Even when information is used legally, it can cause private citizens to feel that their privacy has been violated and generate adverse consequences, as illustrated by a recent set of

⁵ Ideally, researchers could work with groups on both sides of the partisan divide to avoid appearances of bias. In practice, partisan organizations are generally suspicious, and researchers are likely to be forced to specialize on one side or the other.

experiments conducted by Gerber et al. (2008) and Panagopoulos (2009b). In both cases, researchers indicated to treatment group individuals that their voting history was public knowledge and that they would be broadcasting their election behavior – either via mailings or newspaper advertisements – to their neighbors. No laws were broken, yet individuals in the treatment groups were horrified to learn that their private voting behavior might be made public, to the extent that in the latter case they contacted their local District Attorneys and the researcher was contacted by law enforcement. Regardless of the legality of such experiments, scholars might think twice about trying to replicate or build upon this sort of work. As data about people becomes increasingly available for purchase or harvest from the Web, researchers should limit what may be considered violations of privacy, even if they are using public data.

Future Directions

This chapter has focused on voter turnout in particular because it is the best developed experimental literature with regards to mobilization, yet much remains to be explored. New technologies will need to be tested, such as interactive text messaging, nano-targeting advertisements, and the numerous peer-to-peer activities pioneered by MoveOn.org (see Middleton and Green 2008). More theories from related fields such as psychology (for example, cognitive load), economics (for example, prospect theory), and sociology (for example, social cohesion) can be applied to the voter mobilization setting. More can be learned about the dynamics of information flow in campaigns. The availability of inexpensive mobile computing platforms (for example, Palm Pilots, Blackberries, and iPhones) will afford researchers the luxury of better data and the ability to execute more sophisticated experiments. It is also likely that the effectiveness of tactics will vary over time, and these shifts should be documented.

Moving beyond turnout in general to vote choice is another area where future research is likely to make major inroads. Some nonproprietary research has been done on partisan or persuasive campaigns, but the results from these experiments differ wildly. For example, Gerber (2004) examines the result of several field experiments conducted in cooperation with actual candidates to estimate the effect of mailings. Preferences are measured by examining ward-level returns for two experiments randomized at the ward level, and with postelection surveys for three experiments randomized at the household level. For the ward-level experiments, mailings sent by the incumbent had a significant effect on vote choice in the primary but not in the general election. By contrast, for the three household-level experiments incumbent mail did not affect vote choice, while challenger mail had statistically significant and politically meaningful effects. Similarly intriguing results are reported by Arceneaux (2007), who found that canvassing by a candidate or her supporters increased support for the candidate (as measured by a postelection survey), but did not alter voters' beliefs about the candidate. It is not even clear whether partisan or nonpartisan campaigns are better at mobilizing voters. To convincingly answer the question, partisan and nonpartisan messages must be tested head-to-head; such experiments are rare and inconclusive (Michelson 2005, Panagopoulos 2009a). In short, the field is wide open for ambitious scholars to understand what factors influence individual vote choice and whether partisan appeals are more or less effective at stimulating turnout than are nonpartisan appeals.

The reason for the dearth of studies in this area is the difficulty in measuring the dependent variable. Whereas voter turnout is a public record in the U.S., vote choice is private. Thus, researchers must either randomize precincts and measure precinct-level vote choice or they must survey individuals after the election. The precinct-level strategy has two primary downsides. First, treating a sufficient number of precincts to draw valid inference requires very

large experiments that are often beyond the budget of experimenters. Second, randomizing at the precinct level precludes the analysis of subgroups of interest because precinct-level vote totals cannot be disaggregated. Surveying subjects after an election solves the subgroup problem, but introduces problems of its own. Such surveys are expensive, and nonresponse rates are often high, leading to problems with external validity and concerns that subject attrition may not be equal across treatment and control groups.

Civic participation is much broader than the act of voting. Citizens (and noncitizens) attend meetings, volunteer for organizations, donate to campaigns, lobby elected officials, and engage in a host of activities. In principle, all of these topics are amenable to experimental study. For example, several experiments have explored charitable giving. Han (2009) randomly changed an appeal to buy a one-dollar bracelet to support Clean Water Action, a national environmental group, to add two sentences including personal information about the requester and meant to trigger a liking heuristic. Individuals asked to donate and who received the appeal that added the personal information were twice as likely to donate. Miller and Krosnick (2004) randomly varied the text of a letter soliciting donations to the National Abortion and Reproductive Rights Action League (NARAL) of Ohio. The control letter included the same sort of language usually found in such fundraising letters, a “policy change threat” letter warned that powerful members of Congress were working hard to make abortions more difficult to obtain, and a “policy change opportunity” letter claimed powerful members of Congress were working hard to make abortions easier to obtain. Recipients of the letters, all Democratic women, were asked to make a donation to NARAL Ohio and to sign and return a postcard addressed to President Clinton. Only the threat letter had a significant effect on financial

contributions, while only the opportunity letter had a significant effect on returned postcards. Future experiments could expand on these results to study campaign donations.

Another emerging area of field experiments explores how citizen lobbying affects roll call votes in state legislatures. Bergan (2007) conducted an experiment in cooperation with two public health-related groups aiming to win passage of smoke-free workplace legislation in the lower house of the New Hampshire legislature. Group members were sent an email asking them to send an email to their legislators; emails intended for legislators selected for the control group were blocked, while emails intended for legislators selected for the treatment group were sent as intended. Controlling for past votes on tobacco-related legislation, the emails had a statistically significant effect on two pivotal votes. This form of political mobilization is increasingly common among grassroots organizations and worthy of further study.

Nearly every civic behavior could be studied using experiments if enterprising researchers were to partner with civic organizations. In exchange for randomly manipulating the appeals to members of the group (or the broader public) and measurement of the outcome of interest (for example, meeting attendance), organizations could learn how to maximize the persuasiveness of their appeals to attract the largest possible set of volunteers, donors, or activists. The work on voter turnout can serve as a useful template for these types of studies.

Conclusion

Since the modern launch of the subfield less than a decade ago, hundreds of field experiments have expanded our understanding of when and how voter mobilization campaigns work to move individuals to the polls. Despite real-world hazards such as threatening dogs, contaminated control groups, and uneven canvasser quality, hundreds of efforts have replicated and extended the initial findings offered by Gerber and Green (2000). Experiments have been

conducted in a variety of electoral contexts, with a variety of targeted communities, and exploring a variety of psychological theories. Several chapters in this volume offer additional details about experiments in voter mobilization, including one by Chong on work with minority voters and one by Sinclair on the power of interpersonal communication. Yet, much work remains to be done. We look forward to the next generation of experiments, which in addition to refining existing results will include more new technologies, richer theoretical underpinnings, more work on partisan and persuasive campaigns, and behaviors beyond turnout.

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