

**Acting on the Intent to Vote:
A Voter Turnout Experiment**

Dustin Cho
Adviser: Alan Gerber
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ABSTRACT: Theories of voter turnout have focused almost exclusively on the costs and benefits of voting, even though one potentially important aspect of turnout is what takes place after a citizen has decided she intends to vote but before she arrives at the polls. I test two social psychology theories affecting this process: self-prophecy and implementation intentions. The self-prophecy effect occurs when an individual is asked to predict whether he will vote, and this act of prediction alters his future behavior by making him more likely to vote. Applying the implementation intentions theory to voting, citizens would be more likely to vote if they focused on the concrete actions involved in getting to the polling location rather than simply on the end-state of having voted. The field experiment consisted of phone calls to 5,609 registered voters with scripts intended to test these theories. Neither treatment generates a statistically significant result, which undermines the reliability of very large effects found in prior research on voting self-prophecy effects, and suggests the impact of implementation intentions may be limited with respect to voting.

Why citizens vote is one of the enduring puzzles of democracy. The probability that one's vote will matter is vanishingly small, yet every year, millions of voters diligently cast ballots in elections around the world. Part of the explanation simply may rest in whether citizens expect themselves to vote, and whether they subsequently meet those expectations. From this perspective, voting is not so different from achieving apolitical goals like losing weight or writing a senior essay. A citizen might decide to vote for any number of reasons—instrumental (Downs 1957), social (Gerber, Green, and Larimer 2008), moral (Feddersen and Sandroni 2006), or expressive (Schuessler 2000)—but ultimately her goal-setting and goal achievement skills mediate whether or not she follows through with her decision and actually makes it to the ballot box.

Scholars have often stressed that the costs of voting are high relative to the benefits (Ferejohn and Fiorina 1974; but also Niemi 1976), especially considering that the benefits are usually intangible or improbable. Activities like voting, which require an up-front investment of time and energy without an immediate reward, also require the ability to set goals and then to achieve them. Thus the question: How much can voters' willpower and organizational skills explain turnout? Improved goal-setting and achievement skills have the potential to improve turnout even without manipulating the benefits-versus-costs decision calculus. Prior research has largely focused on how increased incentives, such as the election's importance (Campbell 1960) or social pressure (Gerber, Green, and Larimer 2008), influence turnout. Scholars have also investigated the impact of reducing the costs of voting, including easing voter registration requirements (Rosenstone and Wolfinger 1978; Highton and Wolfinger 1998) and

reducing distance to polling locations (Brady and McNulty 2004). Voters' psychological self-discipline represents a third, underexplored variable that directly affects whether or not they get to the polls on Election Day. Indeed, when viewed from a bottom-up voter's perspective instead of from the top-down aggregate results, the simple act of voting is a mundane chore—the housekeeping of democracy: tedious and possibly irritating, with minimal benefits.

The underlying assumption of voter turnout literature is a model in which *deciding* to vote is synonymous with voting. Citizens balance the costs and incentives then decide whether they will vote—and the model ends there. In this paper, I am interested in the voter's behavior after their decision but prior to the polling booth: their ability to *follow through* with their decision. Specifically, I attempt to exogenously influence two aspects of voter follow-through: first, whether the voter expects she will vote (goal-setting); and second, whether the voter takes the concrete actions necessary to vote (goal achievement). Figure 1 diagrams this process.

[Figure 1]

Most of the voter turnout field experiments in the literature do not distinguish between the multiple steps in this process. Consequently, many studies manipulate both the incentives to vote *and* the likelihood a voter will follow through with the subsequent steps in the voting process, despite trying to measure only the effects of the former. Such studies only estimate composite effects, not the isolated impact of each causal mechanism. For example, mobilization appeals made by door-to-door canvassers (Gerber and Green 2000) might also have the effect of encouraging citizens to consciously decide whether they intend to vote, in addition to the effects of “civic duty” or “close election”

arguments for voting. In contrast, this study focuses solely on voters' ability to follow through with their decision, in order to isolate its importance as distinct from the decision-making process.

One widely cited study by Greenwald, Carnot, Beach, and Young (1987) was the first to examine an aspect of voter follow-through. Greenwald et al. ran a small-scale field experiment and found that voter turnout could be increased by 25 percent by merely asking registered voters whether they expected they would vote in the upcoming election. The authors describe this as a "self-prophecy" effect, where voters bring their behavior in line with their previously stated expectations. This experiment was unique in that it isolated one aspect of the voter's *post-decision* behavior from the pros-versus-cons analysis of voting, which the rest of the turnout literature focused on. Although subsequent field experiments failed to reproduce these dramatic results (Greenwald, Klinger, Vande Kamp, and Kerr 1988; Smith, Gerber, and Orlich 2003), none had a large enough sample size to detect smaller yet still politically significant effects.

Social psychologists' research also suggests another intervention which might affect voters' post-decision behavior. In his path-breaking exposition of "implementation intentions," Gollwitzer (1993) found that fostering *specific plans of action* for goal achievement can increase individuals' success rates. His theory emphasizes that goal achievement can be improved by directing attention away from the desired end-state and toward concrete actions that lead to achieving that end-state. Forming intentions to realize a goal sometimes are not enough; intentions to implement the goal may prove more useful. Experiments have corroborated this theory for a wide variety of goals, from completing mundane tasks in laboratories (Aarts, Dijksterhuis, and Midden 1999) to

regularly practicing breast self-examination in field experiments (Orbell, Hodgkins, and Sheeran 1997).

This study tests the self-prophecy and implementation intentions theories in large-scale a field experiment. Unlike prior studies of the self-prophecy effect, this study's sample size is large enough to detect effects that are smaller but still politically significant. It is the first large-scale study of implementation intentions, and also the first application of the theory to voting turnout. Prior to the February 2008 presidential primary in Connecticut, approximately 6,800 voters were contacted by telephone and surveyed using one of three scripts. One group was merely contacted and reminded about the election date. A second group was reminded about the election date and asked whether they expected to vote, testing the self-prophecy theory. The third group additionally was asked a series of questions about when, how, and from where they would travel to the polls to vote, a treatment intended to foster the development of implementation intentions.

The study finds no statistically significant effect for either of these experimental treatments. The effect of self-prophecy approaches statistical significance and its coefficient suggests it may increase turnout by approximately 2.2 percentage points, but adding controls for demographic underlying differences between the treatment groups actually weakens the result, implying that the effect is likely even smaller. This finding contradicts the very large effect on turnout Greenwald et al. (1987) had previously estimated. The implementation intentions treatment does not even approach statistical significance, and has no noticeable impact on voter turnout. This suggests that the

applicability of the implementation intentions theory may be limited with respect to voting.

In the first section of the paper I review the literature relating to self-prophecy, implementation intentions, voter mobilization, and theories of voter turnout. The second section describes the experiment's design, including the data, study population, and treatments. In section three I describe the field experiment's results, and present robustness checks as well as subgroup analysis. The paper concludes with a discussion of the implications of the findings, and directions for future research.

Review of Existing Literature

Self-Prophecy Effects

When asked directly how they will behave in the future, individuals tend to give responses that overpredict the socially desirable response. For example, in his seminal experiments examining obedience to authority, Milgram (1974; in Sherman 1980) notes that when surveyed, very few people predict that if they were subjects in the experiment they would administer the highest voltage shock to a fellow subject. But as his experiments show, a majority of subjects actually do raise the shock level all the way up when told to by the experiment's administrator. Similarly (but perhaps less disturbingly), before an election, the percentage of survey respondents who say they will vote

consistently exceeds the actual turnout rate by double digits (Field and Connelly 1942; Miller 1952).¹

More importantly, psychologists have found that the act of predicting one's own future actions may actually influence one's subsequent behavior. Sherman (1980) first described this phenomenon and demonstrated it through several experiments. In his first experiment, college students are asked to write essays against open visitation in the dormitories. In one treatment group, students first offer a prediction as to whether or not they will write an essay, then (in a separate incident) are asked to write the essay. In another treatment group, students are not asked to offer a prediction first. Only 29.4 percent of the former group predicts they will write the essay, although 66.7 percent of the latter group writes it. Moreover, only 33.3 percent of the former group writes the essay, suggesting that their behavior is substantially modified by the act of self-prediction. A similar experiment examines student willingness to engage in embarrassing behavior (singing the "Star Spangled Banner" over the phone). A third experiment uses a randomly selected sample from the phone directory, and tests individuals' willingness to volunteer for three hours to collect money for the American Cancer Society. Both of these follow-up studies also confirm Sherman's theory: people tend to mispredict their own actions in a socially desirable direction, and this act of misprediction influences their future behavior.

¹ Note that overprediction is different from overreporting, another common problem in voter turnout studies. Overreporting occurs *after* an election, when respondents falsely claim to have voted, and is usually thought to be the result of social desirability bias and misremembering (Presser 1990). Social desirability bias is likely one of the factors driving overprediction as well, but there may be other equally important causes, and misremembering is certainly not an issue when the election has not yet taken place.

Subsequent studies also confirm Sherman's empirical results in a variety of contexts. Spangenberg (1997) demonstrates that individuals who make anonymous predictions about whether they will exercise at their health clubs overpredict their attendance, and that their ensuing attendance is also higher than those who are not asked to make a prediction. In an ingenious experiment, Spangenberg and Obermiller (1996) demonstrate that asking students to predict whether or not they will cheat in a low-risk situation lowers the incidence of cheating on a subsequent closed-book assignment.

Sherman interprets this phenomenon as the "self-erasing nature of errors of prediction." According to his theory, in the process of making a prediction, individuals compose a script that becomes a stereotyped response sequence. Then, "when the situation that was imagined is at hand," Sherman describes, "the same script will emerge because of its enhanced availability." Individuals are likely to follow the script they articulated in an earlier prediction because they pre-determined their decision at that point in time, and now that pre-determined decision is easily accessible and is carried out. Sherman hypothesizes that the difference between predicted behavior and unpredicted actual behavior stems from different considerations: when asked merely to predict, individuals place more weight on normative considerations rather than "mundane issues such as availability of time and energy," which are more important to people in an unpredicted response situation. An alternative theory to script evocation has caught more traction recently, however. Cognitive dissonance explains individuals' actions aligning with their predictions as undergoing actual attitude change toward their prediction, in order to reconcile their behavior with their stated views and self-image. This shift in

opinion serves to restore consistency between an individual's self-concept and actions.
(Aronson 1992)

One study by Obermiller, Spangenberg, and Atwood (1992), however, instructively fails to support the self-prophecy theory. They test whether alumni overpredict favorable responses toward a donation request from their university, and whether that overprediction subsequently leads to higher rates of donation. The problem is that no overprediction occurs in the first place. Spangenberg (1997) suggests one possible explanation: "their subjects viewed donation to their alma mater as merely neutral behavior... if no good person script or social norm is called up by prediction, no subsequent bias due to social desirability will result." This study emphasizes that there needs to be a clearly more socially desirable response for misprediction to occur, and only then can it affect succeeding behavior.

Crucial to this paper is the fact that voter turnout behavior has provided a particularly important and repeatedly investigated test of the self-prophecy effect. Table 1 reports the results of the four prior experiments that have attempted to measure self-prophecy's effect on voter turnout, two of which found politically significant effects and two of which did not. Greenwald et al. (1987) first experimented with self-prophecy's impact on voter turnout, using 60 undergraduate registered voters. Thirty-two students were asked over the telephone to predict whether they would vote in the next day's election, and 28 were not asked to make a prediction. All of the 32 students in the prediction condition said they expected to vote, which was much higher than the 64.3 percent turnout rate among subjects in the non-prediction condition. The difference in actual turnout between the two conditions is also statistically significant, with between

24.2 percentage points higher turnout among students who had offered a prediction.² This result may suggest that voting turnout could be substantially increased simply by calling registered voters and asking them to predict whether they will vote. “If one could call 10,000 voters who could be counted on to vote for one's preferred candidate,” Greenwald et al. conclude, “an effect of the strength observed... would increase that candidate's vote total by about 2,500 votes.” An effect of this magnitude would make for a very noteworthy voter mobilization tactic, considering that typical telephone mobilization efforts produce effects of a couple of percentage points at best (Gerber and Green 2000; Gerber and Green 2005).

[Table 1]

However, subsequent attempts to replicate the findings of Greenwald et al. (1987) have failed to repeat such considerable effects. Greenwald, Klinger, Vande Kamp, and Kerr (1988) contacted registered voters in Seattle, Washington the week before a highly contested election and asked half of them to make a prediction as to whether they would vote. The prediction treatment turnout rate was 85.9%, which was not significantly different from the 87.0% turnout among those who were not asked to make a prediction. Spangenberg and Greenwald (1999) interpret this failure to find a self-prophecy effect as the result of a “ceiling effect, leaving little opportunity to observe treatment effects.” In a second, similar experiment by Greenwald et al. (1988), this time conducted during a relatively non-competitive election, the difference between the prediction and non-

² Some students could not be found on the voter rolls, and Greenwald et al. (1987) attempted to recontact them and ask whether they had voted. Whether these results are counted as votes or non-votes or discounted entirely does not affect the significance of the finding. The figure I cite (24.2 percentage points) comes from Greenwald et al.'s Method A of calculating turnout, which codes these subjects as non-voters.

prediction treatments again was not statistically significant. However, breaking the treatment groups down into subgroups based on voter history, Greenwald et al. (1988) found a significant increase in turnout among moderate-prior-turnout voters who offered a prediction ($p = 0.15$). No other subgroup showed a significant difference between treatments.

Hoping to shed light on the conflicting literature, Smith et al. (2003) conducted an experimental replication with 1,160 registered voters. Their experimental treatment groups still does not show a statistically significant difference in turnout, even though the power of their study is large enough to detect a 25 percentage point difference rather easily ($> .99$). After breaking the treatment groups into subgroups based on prior turnout behavior, they still do not find that the treatments had statistically significant effects within subgroups. However, each of the subgroups had a diminished sample size of only about 400 voters, and was therefore unable to rule out single-digit yet politically significant effects. Such small increases actually may be substantively meaningful in election contexts because elections often are decided by only a few percentage points—in fact, and a level of increased turnout comparable with Smith et al.'s study among some subgroups would certainly be considered a success by most telephone mobilization campaigns. The ambiguous and conflicting nature of the prior literature on self-prophecy and voter turnout warrants further investigation, and may help shed light on the effects of self-prophecy in other fields as well.

Implementation Intentions

A separate strand of social psychology research has found that individuals are more likely to achieve their goals by forming intentions to concretely implement those goals. Gollwitzer (1993) was the first to describe an important distinction between “goal intentions (‘I intend to achieve x!’) and implementation intentions (‘I intend to initiate the goal-directed behavior x when situation y is encountered!’).” Implementation intentions, or intentions to take small, specific steps toward goals, can help individuals achieve their long-term and more complicated goals. Gollwitzer suggests that by fostering the formation of implementation intentions, people may become more likely to attain their goals. Forming implementation intentions means determining “*when, where, and how*” implementation of goal-directed behavior begins. This prevents opportunities from slipping away, either because the goal was forgotten at that moment or because it was relegated a lower priority than other competing goal intentions.

Several empirical studies lend their support to the implementation intentions theory. In his first study, Gollwitzer (1993) simply asks students about goals they had for Christmas break and whether or not they had formed an intention on when and where they would get started. He finds that 62 percent of the subjects who formed implementation intentions completed their project, while only 23 percent of those without implementation intentions completed theirs. Since this study is merely correlational, though, it is possible that some other variable is driving both the formation of implementation intentions and project completion. In a follow-up experiment, Gollwitzer asks students to write a report on how they spent their Christmas Eve, to be completed at most two days after the fact. Half of them are also asked in a questionnaire when and where they intend to write their report, in order to foster the formation of implementation

intentions, while the other half are used as the control group. As it turns out, 71 percent of the implementation intentions treatment group and 32 percent of the control group complete the report within the time frame. Both of Gollwitzer's preliminary studies suggest that developing implementation intentions can have very large impacts on rates of goal attainment.

Since then, researchers have explored the impact of implementation intentions in a variety of settings. Orbell et al. (1997) find further evidence for the significance of implementation intentions by surveying 155 women on their attitudes toward breast self-examination (BSE) and their intentions to carry out BSE in the next month. Half the subjects are also asked to form implementation intentions by figuring out exactly when and where they will perform BSE. One month later, all of the subjects fill out a questionnaire reporting whether they performed BSE in the past month, and if so, where and how often. The results indicate that the treatment is highly effective: 64 percent of women in the treatment group carried out BSE, compared with 14 percent in the control group. Among those who initially expressed an above-average intention to practice BSE, 100 percent performed BSE in the treatment group, compared with 53 percent of the control. Moreover, all of the women in the treatment group who had performed BSE had done so in the same location they had initially specified, and nearly all of them had done so at the specified time of day, suggesting that the implementation intentions remained accessible in the subjects' memories as Gollwitzer (1993) theorized. But Orbell et al. suggest that the value of implementation intentions is in helping to fulfill chronic, postponed goals—specifically unlike “the case of voting... [in which] the initiation of behavior following a decision to act is determined by the arrival of election day.” In other

words, they claim that implementation intentions only help to fulfill goals with no hard deadline, unlike voting.

Other experiments in different contexts contradict Orbell et al.'s suggestion by revealing the extraordinarily wide-ranging potential value of implementation intentions. Gollwitzer and Brandstätter (1997) find that asking subjects to *explicitly* state when in a conversation they will speak up increases the likelihood they will speak up at that moment by about one second. Gollwitzer and Brandstätter conclude that “forming implementation intentions is an efficient strategy for promoting immediate action initiation when opportunities present themselves for only a short moment.” They also note that participants who were already interested in the given controversy had higher-quality arguments in the implementation intentions treatment group than in the control group. Aarts, Dijksterhuis, and Midden (1999) demonstrate that implementation intentions also help subjects achieve mundane tasks, such as collecting a coupon from someone, while being distracted by other tasks, such as walking from the lab to the department cafeteria. Bayer and Gollwitzer (2007) demonstrate that subjects taking a scholastic test perform better after forming self-efficacy implementation intentions (asking them to tell themselves before starting each new problem, “I can solve it!”) rather than mere achievement goal intentions (asking them to tell themselves at the onset of the test, “I will solve as many correct problems as possible!”). The results of these studies suggest that the implementation intentions theory has the potential to improve goal achievement in a variety of situations and for a variety of goal types, including for short-term and non-habit-forming tasks, such as voting in a specific election.

Voter Mobilization Field Experiments

Voter mobilization field experiments recently have enjoyed a renaissance following a Gerber and Green's (2000) field experiment measuring the impact of three typical get-out-the-vote efforts: door-to-door canvassing, direct mail, and telephone calls. They estimate that personal contact increases turnout by about 8 to 10 percentage points,³ and direct mail raises turnout by about 0.6 percentage points per mailing. Phone calls, however, do not significantly boost turnout in their study.

The ineffectiveness of telephone mobilization proved particularly surprising, and many phone-based field experiments have met with mixed results in increasing turnout. This is startling because the conventional wisdom of political campaigns relies heavily on volunteer and commercial phone banking in order to increase turnout among supporters. In an early field experiment, Adams and Smith (1980) use a candidate-specific appeal in a city council race to successfully increase turnout by 5.8 percentage points⁴ (although interestingly, the calls do not appear to affect vote choice). Gerber and Green (2001) test the effect of phone mobilization during a high-turnout election (in 1998, for governor and U.S. congress), finding "little indication that nonpartisan appeals communicated by phone increase turnout." However, Nickerson (2006) points out that while professionalized, commercial phone banks may be useless, volunteer callers show more enthusiasm and forge more of a personal connection with voters, thereby increasing turnout. His experiments suggest that volunteers might boost turnout by around 3.8

³ Their simple calculation of the intent-to-treat effect was 8.7 percentage points, while their multivariate model estimates the effect at 9.8 percentage points.

⁴ Adams and Smith (1980; p. 392) do not report this figure, since their paper simply compares the effect of the treatment-on-treated with the control group's turnout rate. The increase of 5.8 percentage points, from 23.8 percent to 29.6 percent, is the intent-to-treat effect on turnout.

percentage points. More importantly, his research underscores the fact that not all mobilization phone calls are equal.

“Calculus” Versus Psychological Theories of Voting

Traditionally, political scientists have focused on a rational choice model of voting, such as the “calculus of voting” (Downs 1957). According to this model, an individual’s decision to vote is based on the costs and benefits of voting, such that when $pB > C$ she will choose to vote (where p represents the probability of the voter’s ballot being decisive, B is the benefit of such an altered outcome, and C amounts to the cost of voting). However, the near-consensus among political scientists is that p is so vanishingly small that the costs of voting will outweigh the instrumental benefits.

In order to explain this result, Riker and Ordeshook (1968) add a term to the equation representing a sense of civic duty: $pB + D > C$. But as critics point out (Ferejohn and Fiorina 1974; Barry 1970), if the value of pB is negligible due to the infinitesimally low probability of casting a decisive vote, for practical purposes this equation can be reduced to $D > C$. Both “civic duty” and the “costs of voting” remain difficult to pin down conceptually, much less measure or quantify. Fiorina (1976, p. 393) argues, “[I]f citizens vote simply because D outweighs C , why bother with decision theoretic models, symbols, rational choice terminology and other trappings which add little to the standard social-psychological explanation?” Instead of a rational model of decision-making, this equation simply leaves us wondering why some people feel a greater sense of duty than others and what the costs are.

Other fields such as social psychology can offer insight into these questions by offering a framework for understanding individuals' behavior. To delve into these more sophisticated explanations of voting, a second generation of field experiments has moved beyond simply measuring mobilization efforts' effects in favor of testing interdisciplinary theories of political behavior. For example, Gerber, Green and Larimer (2008) fruitfully distinguish the intrinsic benefits associated with voting from the extrinsic social consequences by mailing voters the turnout records for everyone in the neighborhood and threatening to do so again after the next election. Their results provide evidence that increasing social consequences may help overcome the costs of voting. In another study, Gerber, Green and Shachar (2003) offer evidence that voting is a habit-forming activity. One year after a voter mobilization field experiment, they collected turnout data for another, succeeding election and found that being mobilized in the previous election had a significant and strong effect on subsequent turnout. Studies like these indicate that a substantial portion of the decision to vote may be psychological rather than rationally instrumental.

Experiment Design

Setting

The field experiment was conducted in Connecticut during the week prior to the “Super Duper Tuesday”⁵ presidential primary on February 5, 2008. Election turnout benefitted from three remarkable catalysts. First, Connecticut’s 2008 presidential primary

⁵ In 2008, 24 states held primaries on February 5 — the most primaries ever held on a single day (Kaufman 2008).

was unusually competitive, since the Democrats, and to a lesser extent, the Republicans, had highly contested nomination races. Polls from the week prior to the election showed Democratic Senators and frontrunners Barack Obama and Hillary Clinton were neck-and-neck in the Nutmeg State. Senator John McCain was seen as the probable Republican nominee, but he still faced some competition from Governors Mitt Romney and Mike Huckabee. This competitiveness meant that the margins of victory were expected to be slim, and therefore an individual voter might perceive her vote as more likely to be pivotal (the p term was large, if one prescribes to the calculus of voting).⁶ Second, the race was politically important. It was a contest for the nation's highest office during a time of very low approval ratings for the incumbent. The Republican slate of candidates was perceived as covering a wide spectrum of policy positions (suggesting a large B term). Although most people did not see a big policy difference between Obama and Clinton at this point in the campaign, many voters had strong (albeit highly subjective) views about each candidate's character and electability. Finally, the election was heavily hyped, and voters were inundated with information regarding the campaigns. Nationally, the race consistently made the front page, and had done so for weeks. All three frontrunners—Obama, Clinton, and McCain—visited Connecticut in the week prior to the election, and local media prominently covered their events.⁷ Although most previous

⁶ Although convention delegates are awarded proportionally for Democrats, because media coverage was framing primary victories in terms of “winning states” based on their popular vote totals, there was still a distinct advantage to beating one's opponent in the statewide tally.

⁷ McCain held a rally in Fairfield on February 3, Obama held a rally in Hartford on February 4, and Clinton participated in a “roundtable discussion” in New Haven on February 4. Clinton also held a town hall meeting in Hartford on January 28. (Slate 2008)

presidential primaries this late in the campaign have been plagued by relatively low turnout rates, these three factors made this election unusually interesting to voters.

Some states choose to allocate their nomination delegates based on a caucus, but Connecticut's primary is organized much like a general election. The same polling locations and voting machines are used, so a voter's experience of the voting process is nearly identical in both types of elections. This similarity makes the experiment's results more generalizable, and it is not unreasonable to suppose that treatments affecting turnout in this primary might affect general election turnout comparably. The biggest procedural difference between general and primary elections in Connecticut is that the state holds "closed primaries," meaning that each of the state's voters must declare a party affiliation when registering to vote, and is only eligible to vote in the primary for that party.⁸ Of course, a substantive difference between primaries and general elections is that voters do not have partisan differences to choose between, perhaps reducing the B term in the calculus theory of voting or increasing the costs of figuring out which candidate to vote for.

Ultimately in Connecticut's primary, Obama received 50.7 percent of the Democrat vote, compared with Clinton's 46.7 percent. Among Republicans, McCain earned 52.0 percent of the vote, followed by Romney with 32.9 percent.⁹ Voter turnout in

⁸ Voters can change their party affiliations prior to each election, but they may only vote in one party's primary for any given election. This is different than a general election, of course, where every voter can vote in all of her precinct's races, regardless of party.

⁹ One wrinkle on Connecticut's primaries versus general elections is that Democrats' votes are awarded *proportionally* by electing delegates for each district. Obama picked up 26 delegates in Connecticut; Clinton, 22. Republicans use the winner-take-all system, so McCain won all 27 of the state's Republican delegates. (New York Times 2008) It is not clear what the effect of proportional representation is on turnout. One could argue that an individual voter is more likely to be pivotal in changing the delegate count,

Connecticut's 2008 primary was 507,166, or about 45.8 percent of Connecticut's Democrat and Republican registered voters (Connecticut Secretary of State 2008).¹⁰ The turnout rate was 51.1 percent for Democrats and 36.7 percent for Republicans, reflecting a national trend in 2008 of disproportionate interest in the Democratic race. Connecticut is also a predominantly Democratic state, with 62.8 percent of its major-party-affiliated voters belonging to the Democratic party, so a sizeable majority of voters on February 5 were Democrats.

Study Population

This field experiment drew its subjects from five towns in New Haven County (see Table 2).¹¹ The towns were chosen based on the conditions that they all share a 203 area code, thereby simplifying phone calls to each town's registered voters; that they have at least 40,000 residents (to facilitate data collection); and that the towns' officials estimated their turnout data would be available soon after the election. Combined, the towns have a total population of roughly 400,000 (U.S. Census Bureau 2000).¹² They represent a mix of densities, ranging from 6558 people per square mile in New Haven (roughly the density of Dallas, Texas) to 1,103 people per square mile in Wallingford

thereby making it worth voting even noncompetitive states. On the other hand, each delegate is not likely to be pivotal either, so the instrumental importance of each vote is probably roughly the same under either system, since only one candidate gets the nomination in the end.

¹⁰ Note that this does not include unaffiliated or third-party registered voters, which together comprise a sizeable portion of the Connecticut electorate. Such voters cannot cast ballots in major-party primaries.

¹¹ The experiment was also conducted in Milford, Connecticut, but the Milford Registrar of Voters was unable to provide turnout data in time for this paper, so subjects who were registered in Milford are not included in this paper's analysis.

¹² In 2000, their total population was 383,196, but since then it is likely the population has changed, so a more exact figure would connote a false sense of accuracy.

(still moderately dense, but not urban), roughly spanning the range of urban, suburban, and small-town settings. Their demographics vary as well, from more than 94 percent white in Wallingford to New Haven's diverse population, 43.5 percent of which is white, 37.4 percent African American, and 21.4 percent self-identifying as ethnically Hispanic. Wallingford is the wealthiest, with a median income of \$57,308, while New Haven has the lowest median income, at \$29,604. (Hamden, Waterbury, and West Haven fall somewhere in between these extremes for each of these attributes.)

[Table 2]

The official lists of registered voters in each town served as the basis for the survey population. Voters whose phone numbers were missing or did not have a 203 area code were removed, as were repeat listings and obvious data-entry errors. Entries with missing voter histories for 2004 and 2006 were also eliminated from the study. The remaining study population consisted of 54,903 registered voters. Each of these voters was randomly assigned to one of three experimental treatments, with each treatment group consisting of about 18,300 individuals.

Over the course of the four days prior to the Tuesday election, a professional phone bank specializing in political campaigns attempted to call each of the voters in the study population.¹³ The order of the list of voters in the study population was randomized after assigning treatments, and calls were made following this random order.¹⁴ Calls were attempted between 5 p.m. and 9 p.m., when voters were more likely to answer their

¹³ Calls were made each day, starting on Thursday, January 31 and ending on Monday, February 4, the day before the election.

¹⁴ Callers were not assigned to specific treatment scripts; after a caller completed a call, a centralized computer system selected the next voter record on the list, whom the caller would attempt contact next.

phones. Callers asked to speak with the specific voter on the contact list. If a voter did not pick up her phone or was not available, a caller would try to contact the voter again on subsequent days.¹⁵ Callers did not leave voicemail messages.

For the purposes of this study, a phone call was considered a “contact” if the caller completed the prefatory portion of the script, asked the initial question, and received some sort of codeable response from the respondent. According to this definition, contact was established with 5,609 voters, or 10.2 percent of the study population—not an unusual rate of contact for professional phone banks. Telephone survey contact rates are consistently low for many reasons, including outdated voter records, incorrect phone numbers, language barriers, respondents not answering the phone, and a high rate of respondents simply declining to participate.

[Table 3]

Table 3 shows descriptive statistics for the treatment groups, comparing the covariates in each group. The chi-square tests of independence show that only one covariate had a statistically significant difference between treatment groups: whether the voter had voted in 2002.¹⁶ The other voter history variables, as well as the voters’ genders, were not significantly different between the two treatment groups based on these chi-square tests. Voter age and number of years registered to vote were continuous variables and therefore more properly tested with a t-test of difference of means, and only

¹⁵ The phone bank cycled through the list several times over the four days of calls.

¹⁶ Note that the low reported turnout for elections prior to 2004 is not necessarily accurate; the data for many of these elections was missing, but these cases were not dropped from the dataset.

one of the differences was significant at the 5 percent level.¹⁷ The randomized assignment of treatments also prevents significant differences among unobserved characteristics.

Treatments

A telephone script was assigned to each of the three treatment groups: Mere Contact, Self-Prophecy, and Implementation Intentions. The Appendix contains the full scripts, along with summary statistics of the responses. All three scripts begin by briefly explaining that this is a short university research survey of voters. Then all three confirm contact with an initial question, asking whether the voter is aware of the presidential primary on February 5th. This is the only question in the Mere Contact treatment, and its purpose is fourfold. First, if the voter replies with any codeable response, it confirms that contact has been established. Voters who are reachable by phone are not a representative sample of all registered voters, so it is important to establish a baseline of turnout among voters who can be reached by phone. This question allows such voters to be identified. Second, the script notifies voters who may not be aware of the election date that the primary is being held on February 5, thereby controlling for this aspect of the calls between all three treatments. This notification also serves to control for any priming effect of the call. Otherwise, a call regarding the election might increase a voter's perception of the election's importance ("university researchers care about this!")—or even make voters concerned they're being studied—and thereby increase turnout.

¹⁷ The significant difference was between the mean ages for the Self-Prophecy and Implementation Intentions treatments ($p = 0.034$). Two significant differences in treatment group characteristics is somewhat concerning, but since the treatments were randomly assigned, the only explanation is that the somewhat improbable occurred. In order to guard against these differences influencing the estimates of treatment effects, these variables are controlled for during data analysis.

Thirdly, the caller introduces the first question by remarking, “High turnout is expected for this Tuesday’s election.” This statement is intended to make a social norm of voting salient, as well as to impress upon voters that others consider this election important. These social cues were intended to reinforce individuals’ tendency to provide socially desirable responses to survey questions. A higher social desirability bias rate increases the number of subjects who have the opportunity to alter their behavior based on the self-prophecy intervention. After all, the self-prophecy effect only influences the behavior of those whose responses were biased. Finally, the voter’s response to the question gives a minimal indicator of the voter’s level of political interest or awareness, which can be used as a control variable for underlying differences between the treatment groups. After the voter responds to this initial question, the Mere Contact script ends, whereupon callers thank the voter and hang up.

The Self-Prophecy and Implementation Intentions treatments continue with a second question, asking whether the voter expects she will vote in the primary.¹⁸ This question is similar to the ones posed by Greenwald et al. (1987) and Smith et al. (2003). Since the socially desirable response is to vote, this question is intended to overpredict the Mere Contact group’s response rate. According to the self-prophecy theory, the question will increase turnout, as voters bring their subsequent behavior in line with their previously stated (and upwardly biased) expectations. The Self-Prophecy treatment group also serves as a control for the Implementation Intentions treatment, because turnout in the Self-Prophecy group is a baseline for people who express an expectation of voting without any of the additional implementation intentions framework. Differences between

¹⁸ If the voter does not answer with a yes or no, she is prodded once (“If you had to guess...?”).

the two groups' turnout levels can be isolated to the effect of implementation intentions, as distinct from any self-prophecy effect. For the Self-Prophecy treatment group, this was the final question of the survey.

The Implementation Intentions treatment included all of the previously stated questions, and added another section intended to foster implementation intentions in voters. These questions were asked *only* if the voter responded yes, she expects she will vote in the primary. A voter who affirmatively answered the prior question, indicating she expected to vote, is probably also expressing an intention, or goal, of voting.¹⁹ Implementation intentions only help individuals to achieve pre-existing goals; they do not foster the formation of goals themselves. If the voter did not express any intention of voting, implementation intentions were not relevant, so the survey ended there. However, 81.4 percent of respondents who were asked the self-prophecy question answered that they did expect to vote,²⁰ which meant the overwhelming majority of respondents assigned to the Implementation Intentions treatment group received the full battery of questions. The high figure also suggests that the respondent's bias toward answering the caller with the socially desirable response is very strong, which is a crucial component of the study. Without this bias, neither the self-prophecy nor the implementation intentions theory would act to improve turnout. If the proportion of people who responded that they expected to vote were no different from overall turnout, there would be no errors in

¹⁹ Although the word “expect” is different than “intend,” and has a significantly different meaning, it seems unlikely that voters would interpret the two differently in the context of a survey a few days prior to the election. Slim indeed would be the margin of people who would respond differently to these two questions.

²⁰ 83.7 percent of the Self-Prophecy group and 78.9 percent of the Implementation Intentions group responded affirmatively. The Mere Contact group was not asked this question. See Table 9 in the Appendix for further survey response results.

voters' self-prophesying for them to correct with subsequent behavior. And if everyone who had an intention of voting actually did, then voters would not need any help from implementation intentions to improve their goal achievement.

In order to help them meet their goal, the theory of implementation intentions suggests that voters should focus on the direct actions they need to take to achieve the goal, rather than focusing solely on the goal itself. To foster the formation of such implementation intentions, this treatment group is asked a series of questions about *when* on Tuesday they will go to their polling location, *where* they will be just before leaving for the polls, and *how* they will get there. Finally, each voter is asked to repeat a statement affirming the time, prior location, and mode of transportation for her trip to the polling booth. These questions force the respondent to consider concretely when she will vote and how she will get there, allowing her to plan out her election day schedule in advance. By focusing on specific actions (“I’ll leave my house at 6 p.m. in my car”) rather than merely the desired end-state (“I’ll vote”), voters put together a definite plan. By asking about their point of departure for the polls, voters associate going to vote with a particular time and place, making that plan of action more accessible when the voter encounters the predetermined situation. Although mundane daily tasks or other distractions might divert the voter from going to the polls, by forming a specific plan ahead of time, voting is harder to forget about and less easily dismissed when faced with the prearranged context. Repeating the affirmation of the implementation intentions forces a voter to explicitly commit herself to this plan, further making it difficult to dismiss on Election Day.

Results

[Table 4]

Following the February 5 primary, I obtained turnout data for the contacted voters from the Registrar of Voters in each of the towns. Table 4 reports overall turnout rates for each of the treatment groups, broken down by town. When pooled, the turnout rate for the Mere Contact control group is 59.1 percent. The turnout rate for the Self-Prophecy group, in comparison, is 61.4 percent, suggesting that asking whether one expects to vote may increase turnout by about 2.2 percentage points.²¹ However, the Implementation Intentions group only had a turnout rate of 58.9 percent, which is actually lower than the control group. This is strange because its script contained all of the same appeals that the Mere Contact and Self-Prophecy scripts included. Either the addition of the implementation intentions-specific questions actually hurt turnout, or the result should not be considered significant. The town-by-town breakdown reveals that these differences were not consistent across towns. Waterbury experienced incremental improvements in turnout for each of the treatments. Hamden, New Haven, and Wallingford showed an increase in the Self-Prophecy group but a decrease in the Implementation Intentions group. West Haven's results were the reverse. Based on the results in Table 4, the effect of each of the treatments remains ambiguous.

[Table 5]

²¹ The reason these figures do not add up perfectly is due to rounding errors. The Mere Contact treatment turnout was more precisely 59.13 percent, and the Self-Prophecy turnout was 61.36 percent, so the difference was 2.23 percentage points.

Table 5 examines the statistical significance of the treatment effects. The first specification (Column 1) simply runs an ordinary least squares regression of turnout on dummy variables for the Self-Prophecy and Implementation Intentions treatments. This model may be written as

$$Y_i = B_0 + B_{SP}T_{SPi} + B_{II}T_{IIi} + u_i, \quad (1)$$

where Y_i represents voter turnout for individual i , and is regressed on two dummy variables (T_{SPi} , T_{IIi}), one for each treatment (Self-Prophecy and Implementation Intentions). Mere Contact is the control group and is dropped to prevent perfect multicollinearity; u_i represents unobserved noise. The parameters of interest for these regressions are B_{SP} and B_{II} , which measure the effects of the treatments.

As Column 1 of Table 5 reports, the effect of the Implementation Intentions treatment is nowhere near significant, and has a slightly negative coefficient. The Self-Prophecy coefficient is 2.2 (as mentioned above) and is not statistically significant by any conventional standard. However, with a p-value of 0.164, some might argue that it is approaching significance, and might be significant if only the study had greater statistical power. To explore this possibility, I use three statistical techniques to more carefully parse the results: controlling for covariates, grouping the two substantive treatments into one variable, and examining the treatments' effects on certain specific subgroups.

The second model specification (Column 2, Table 5) adds fixed effects variables for towns to control for covariance between the towns and treatment assignments. The model can be written as

$$Y_i = B_0 + B_{SP}T_{SPi} + B_{II}T_{IIi} + B_1C_{1i} + B_2C_{2i} + B_3C_{3i} + B_4C_{4i} + u_i, \quad (2)$$

where B_1C_{1i} , B_2C_{2i} , B_3C_{3i} , and B_4C_{4i} represent four of the towns (the fifth town being dropped to prevent multicollinearity). The coefficients for both treatments actually weaken and become less statistically significant in this model. Column 3 shows a model that adds controls for prior voter turnout:

$$Y_i = B_0 + B_{SP}T_{SPi} + B_{II}T_{IIi} + B_1C_{1i} + B_2C_{2i} + B_3C_{3i} + B_4C_{4i} + B_{2006}V_{2006i} + B_{2004}V_{2004i} + B_{2002}V_{2002i} + B_{2000}V_{2000i} + B_{1998}V_{1998i} + B_{1996}V_{1996i} + u_i \quad (3)$$

The additional dummy variables represent whether the individual voted in the elections of 2006, 2004, 2002, 2000, 1998, and 1996. Although there were some differences in prior vote history among the treatment groups (see Table 3), including a statistically significant difference for 2002 turnout, adding these covariates to the model does not improve the significance of the treatment effects. Column 4 adds further controls for age, sex, and number of years registered:

$$Y_i = B_0 + B_{SP}T_{SPi} + B_{II}T_{IIi} + B_1C_{1i} + B_2C_{2i} + B_3C_{3i} + B_4C_{4i} + B_{2006}V_{2006i} + B_{2004}V_{2004i} + B_{2002}V_{2002i} + B_{2000}V_{2000i} + B_{1998}V_{1998i} + B_{1996}V_{1996i} + B_{AGE}D_{AGEi} + B_{FEMALE}D_{FEMALEi} + B_{YRSREG}D_{YRSREGi} + u_i \quad (4)$$

Sex is a dummy variable, while the variables for age and years registered are continuous variables measured in years. Despite adding these demographic controls, the coefficients barely move and do not become significant.

Taking advantage of voters' responses to the first question in the survey (whether they knew the primary was being held on February 5), the fifth model (Column 5) adds a dummy for a "yes" response:

$$Y_i = B_0 + B_{SP}T_{SPi} + B_{II}T_{IIi} + B_1C_{1i} + B_2C_{2i} + B_3C_{3i} + B_4C_{4i} + B_{2006}V_{2006i} + B_{2004}V_{2004i} + B_{2002}V_{2002i} + B_{2000}V_{2000i} + B_{1998}V_{1998i} + B_{1996}V_{1996i} + B_{AGE}D_{AGEi} + B_{FEMALE}D_{FEMALEi} + B_{YRSREG}D_{YRSREGi} + B_{AWARE}D_{AWAREi} + u_i \quad (5)$$

This was a very low bar for voters to clear: 93.2 percent of respondents answered affirmatively. And in fact, a slightly lower percentage (92.6 percent) responded “yes” in the Implementation Intentions group, suggesting this may be a useful variable to control for. The coefficients become slightly more significant, but not enough to be convincing. After all of these various covariates are added to the model, the coefficient for the Self-Prophecy treatment has fallen slightly to 1.8 percentage points (and remains insignificant). The coefficient for Implementation Intentions is indistinguishable from zero.²²

[Table 6]

One possibility is that the treatments are having an effect, but that the sample size is too small to detect a significant difference. Each of the treatment groups only has about 1850 voters, after all. By combining the Self-Prophecy and Implementation Intentions groups into a large “treated” group, and simply comparing this combined group with the Mere Contact control group, it is possible to measure for the effect of *any* treatment. Table 6 employs this technique, using the same model specifications explained for Table 5. The general treatment effect is statistically insignificant in all five specifications, and its coefficient stays fairly steady at about 1 percentage point increased turnout.

[Table 7]

Tables 7 and 8 break down this study’s population into subgroups for further analysis. Table 7 runs model specifications one and five (minus the town dummy variables) for each town. Implementation Intentions does not produce a statistically

²² Another pair of control variables I tested were the date and time of the phone calls, but these had infinitesimally small coefficients and did not affect the significance levels or coefficients of the treatments.

significant effect in any of the towns. The Self-Prophecy effects vary quite a bit, from negative in West Haven to a 7.2 percentage point estimated increase using model one in Hamden. Although this latter case might be said to be approaching significance, none of the coefficients are significant at any conventional level. It is possible that this variation in the coefficients represents a meaningful difference between towns—perhaps the citizens of Hamden are simply more susceptible to the Self-Prophecy treatment—but it seems more plausible that these differences only represent random variation.

[Table 8]

It is important to note that these estimated differences represent intent-to-treat effects, rather than the average effect of the treatments on the treated. The intent-to-treat effects include all of the subjects randomized and contacted by phone. However, only 83.7 percent of respondents replied that they expected to vote, and 8.1 percent of respondents said they did *not* expect to vote—according to the self-prophecy theory, the likelihood these citizens vote should actually *decrease*. In other words, only 83.7 percent of the Self-Prophecy group received the proper treatment. This interpretation implies a different estimate of the effect of self-prophecy, as calculated by the following equation:

$$0.837(B_{SP}) - 0.081(B_{SP}) = 2.23,$$

where B_{SP} represents the effect of self-prophecy (the percentage point increase in a voter's likelihood to act the way she predicted she would act), 0.837 is the percentage of respondents who expected they would vote, 0.081 is the percentage who did not expect to vote, and 2.23 is the net percentage point increase for the Self-Prophecy treatment group over the Mere Contact control group. According to this equation, $B_{SP} = 2.95$ percentage points, which is the average effect of the self-prophecy treatment on the treated. Similarly

for implementation intentions, the intended treatment is only applied to those respondents who initially answer yes, they expect to vote, and then answer all of the remaining questions, concluding with a vocal affirmation of when, how, and from where they will vote. As Table 9 indicates, only 51.3 percent of the Implementation Intentions treatment group met all of these criteria. However, the estimated effect of implementation intentions in all of the regressions was negligible, so there is not much of an effect of the treatment even when restricted to the treated.

Greenwald et al. (1988) claim that self-prophecy effect may be stronger for voters who have an inconsistent turnout history, as opposed to a very consistent voter or nonvoter. Smith et al. (2003) tested this theory and found some politically significant differences in turnout,²³ but had much too small a sample to determine if the differences were statistically significant.²⁴ Table 8 investigates the possibility that the self-prophecy effect varies depending on vote history by dividing the study population into four groups based on prior turnout. The high turnout history group consisted of 3,042 voters who cast ballots in both 2004 and 2006. Moderate turnout was coded as voters who cast ballots *either* in the 2006 election *or* in the 2004 election, but not both; this was a group with 950 voters. The low turnout group had not voted in either of the previous elections. Although none of the coefficients are statistically significant, the largest (about 5.5 percentage points) and most nearly significant ($p = 0.14$) are for the Self-Prophecy treatment on

²³ For example, among voters who had rarely voted in prior elections, the difference between the control and prediction treatments is 8.6 percentage points—an extremely large effect.

²⁴ Each treatment group for each level of prior turnout behavior had about 100 individuals. There were 12 groups total, the cross-product of four treatment groups and three levels of prior turnout.

moderate turnout voters, consistent with the claim by Greenwald et al. However, there are not enough moderate turnout voters in this study to establish significance.

Neither of the treatments is statistically significant, and the large size of the study rules out effects larger than 5 percentage points.²⁵ The small (if any) baseline effect is also cost-inefficient. With prices running around \$2 per call, assuming a 2.2 percentage point effect suggested by the (insignificant) coefficient in the first model in Table 5, the price per additional vote of \$90 is fairly expensive. In contrast, door-to-door campaigns generally cost around \$20 per vote, volunteer phone banks cost \$35 per vote, and professional phone banks generate an additional vote for about \$200 (Green and Gerber 2004).²⁶ Nevertheless, it is possible that a study with a larger sample size could establish the Self-Prophecy treatment effect is in the range of 1 to 4 percentage points—an arguably politically significant result. Mobilization campaigns are a product of coinciding and mutually reinforcing efforts, typically using a wide variety of techniques ranging from candidate appearances to television ads to door-to-door canvassing. Not infrequently, elections come down to extremely slim margins, and increasing turnout among supporters by a couple of percentage points could be decisive.

However, there are two reasons to be skeptical that the effect is even in the range of a 3 percentage point increase. First, the Implementation Intentions treatment has

²⁵ At a 10 percent confidence level, this study has a statistical power of 92.3 percent for detecting a 5 percentage point difference—in other words, the probability of a Type II error (falsely failing to rejecting the null hypothesis) is 7.7 percent.

²⁶ These calls were more expensive than typical phone bank calls due to the interactive “survey” nature of the scripts. That said, the per-call price probably also was driven up by the much longer and more involved Implementation Intentions script, so it is not unlikely that a campaign based solely around the Self-Prophecy script could cost much less. But even if it were half the price, the cost of each additional voter still would be \$45—considerably more expensive than door-to-door mobilization and somewhat less cost-effective than traditional volunteer phone banks.

absolutely no noticeable effect. This is strange because the Self-Prophecy script is wholly contained within the Implementation Intentions script—nothing that is said to the Self-Prophecy group is not said to the Implementation Intentions group as well. For a significant effect of around 3 percentage points to exist for Self-Prophecy, it would require that the additional Implementation Intentions questions negate the positive effects of the first half of the survey—essentially, a significant *negative* effect of about the same magnitude. While this is possible, I can think of no theoretical reason that this would be the case, especially when several experiments in other fields have found the effects of implementation intentions to be so strongly positive (e.g. Orbell et al. 1997).

Secondly, there may be some small, unobserved covariance with the treatments, despite the randomization, such that the Self-Prophecy group would have had a slightly higher turnout rate even if no phone calls had been administered at all. As mentioned earlier and indicated by Table 3, there is a statistically significant difference between the treatment groups for turnout in 2002, with the Self-Prophecy group leading the pack in turnout. And as Table 5 shows, adding dummy variables and controls for prior voter history actually reduces the significance and magnitude of the coefficient. If this heterogeneity were just noise interfering with the true effect of the treatment, the significance should increase; a diminished p-value after adding control variables suggests that these controls were actually driving some of the results in the first model through omitted variable bias. While I cannot think of an explanation for the differences between the treatment groups (other than its purely random outcome), my data analysis suggests that this possibility should not be ignored.

Discussion

The most salient conclusion of this study is the relative weakness of self-prophecy and implementation intentions effects. These treatments failed to increase turnout substantially, despite the frequency of large magnitude effects in prior research.

This study conclusively rules out a self-prophecy effect of the 25 percentage point magnitude first estimated by Greenwald et al. (1987). It is not clear what explains the huge discrepancy, but three key factors distinguished this study's methodology from Greenwald et al.'s. First, this field experiment targeted a random cross-section of Connecticut voters in five towns, with a broad and representative range of ages, races, education levels, and other demographic features. Greenwald et al. restricted their study population to college students living in the dormitories at one university. It is possible that college students, immersed in a research-focused environment by choice, are more receptive to scholarly studies and surveys and therefore gave the phone call greater weight. Second, Greenwald et al. also asked respondents who said they planned to vote one additional question: what they considered the most important reason for voting. As Smith et al. (2003) point out, these two interventions may have entirely independent effects and need to be distinguished from each other. This experiment demonstrates that self-prophecy, when properly isolated, does not have a very large effect. (Smith et al. show that the "important reason for voting" question does not have a large effect either.)

The third difference is that Greenwald et al. used a volunteer-based phone bank of graduate students and others affiliated with the university conducting the research, while this study relied on a commercial phone bank. As Green and Gerber (2004) emphasize,

volunteer phone banks are typically much more effective because for-profit callers often rush through the scripts, mumble, have a thick accent, or seem otherwise uninterested in the work and fail to connect with the voter. (Indeed, while monitoring calls during the study, I overheard one voter ask why the caller wanted to know when he was going to vote. The caller responded nonchalantly, “I don’t know; it just says here I’m supposed to ask you that.” Needless to say, the voter did not answer the question.)

Of course, the most conspicuous difference between this study and prior attempts to investigate self-prophecy effects is its sheer size, which enables statistical recognition of much smaller effects. However, since the treatment failed to produce even a 5 percentage point increase in turnout, this study still does not offer authoritative evidence on whether self-prophecy has an effect of less than that.

This study is the first application of the implementation intentions theory to voting, and the null finding stands in stark contrast to prior experiments in other contexts. Where other studies have found noteworthy improvements in goal achievement, this experiment did not find a significant result, despite being the largest-scale test of implementation intentions to date. One possible reason for this is the difference between “chronic or postponed goals,” which may benefit from the formation of a plan of concrete actions, and goals “in which the context for the behavior is certain,” such as voting (Orbell et al. 1997). Goals with clear deadlines and specific contexts come pre-packaged with plans for when and how to achieve them. Voting occurs only on Election Day by traveling to the polls and casting a ballot, so Orbell et al. suggest that implementation intentions have little to add in such a situation. But other studies of context specific tasks with concrete deadlines do benefit from implementation intentions (Gollwitzer 1997;

Aarts et al. 1999), so the crucial difference between voting and these cases is probably something else.

There are three relevant distinctions that could explain the discrepancy. First, as mentioned previously, commercial phone banks are simply not good at establishing a personal connection with voters. Other implementation intentions studies have relied on volunteers and university researchers, who likely connected with subjects much more effectively. Second, unlike other experiments on implementation intentions, this was a large-scale field experiment. All of the prior studies have relied on university-affiliated subjects or recruited volunteers. All but one of the other studies relied on laboratory settings, and the one “field experiment” (Orbell et al. 1997) took place on a university campus, where the 155 subjects either studied or worked. It is plausible that these campus-centric populations are not representative, and therefore the external validity of such studies is limited to similar populations or contexts. University-affiliated people might approach such experimental treatments with more of an open mind and interest in the results.

Third, though, it is the important possibility that the 81.4 percent of respondents who said they expected they would vote actually were not expressing an intention to vote at all. Perhaps they gave “the benefit of the doubt” that they might vote, although they already knew that the probability was slim. Perhaps they were simply lying. If such respondents were not expressing an honest intention to vote, but instead were just telling the caller what they thought the caller wanted to hear, then fostering the formation of implementation intentions would be putting the cart before the horse. And if this is the case, mobilization efforts should focus once again on increasing the incentives and

decreasing the costs of voting, since voters have to establish the goal of voting before implementation intentions can help them achieve it. Nevertheless, it remains difficult to know exactly how to interpret the high proportion of people who say they expect they will vote and then don't.

Finally, although these interventions failed to produce meaningful impacts on turnout, these results should not be interpreted as reinforcing the unimportance of the period after the decision to vote but before the voter goes to the ballot box. This study tests only two theories of the link between action and behavior, and many more possible treatments can be found in the social psychology literature. A good starting point would be experimenting with interventions increasing voter motivation, so that a lack of willpower does not prevent them from going to the polls on Election Day. Just as it can be difficult to write an essay even after deciding you want to write it, voters may decide to vote but fail to follow through with that decision due to psychological, irrational factors. The next step in voter turnout research is to identify those psychological factors and offer ways to overcome them.

Appendix

Mere Contact Script

Hi. My name is [interviewer's first name], and I'm conducting a university research survey of registered voters. You can help us a lot by answering just a few questions about voter knowledge. I'm not selling anything, and the entire questionnaire will take about one minute to complete.

- Q1 High voter turnout is expected for this Tuesday's election. Were you aware that Connecticut's presidential primary elections will be held this Tuesday, February 5th?**
- a) Yes
 - b) No
 - c) Other
 - d) Refused

Thank you for your help.

Self-Prophecy Script

Hi. My name is [interviewer's first name], and I'm conducting a university research survey of registered voters. You can help us a lot by answering just a few questions about voter knowledge. I'm not selling anything, and the entire questionnaire will take about one minute to complete.

- Q1 High voter turnout is expected for this Tuesday's election. Were you aware that Connecticut's presidential primary elections will be held this Tuesday, February 5th?**
- a) Yes
 - b) No
 - c) Other
 - d) Refused

- Q2 Do you expect you will vote in Tuesday's election?**
[IF RESPONSE IS "MAYBE" OR "DON'T KNOW," ASK:
"If you had to guess, do you expect you will vote on Tuesday, or not?"]
- a) Yes
 - b) No
 - c) Don't know (volunteered twice)
 - d) Already voted
 - e) Refused

Thank you for your help.

Implementation Intentions Script

Hi. My name is [interviewer's first name], and I'm conducting a university research survey of registered voters. You can help us a lot by answering just a few questions about voter knowledge. I'm not selling anything, and the entire questionnaire will take about one minute to complete.

- Q1 High voter turnout is expected for this Tuesday's election. Were you aware that Connecticut's presidential primary elections will be held this Tuesday, February 5th?**
- a) Yes (was aware)
 - b) No
 - c) Other
 - d) Refused
- Q2 Do you expect you will vote in Tuesday's election?**
[IF RESPONSE IS "MAYBE" OR "DON'T KNOW," ASK:
"If you had to guess, do you expect you will vote on Tuesday, or not?"]
- a) Yes
 - b) No
 - c) Don't know (volunteered twice)
 - d) Already voted
 - e) Refused
- Q3 [ASK ONLY IF ANSWER TO Q2 WAS "YES"]**
It sometimes helps citizens remember to vote if they think about when and where they will be before they plan to cast their ballots. What time on Tuesday do you plan to go to your polling location?
[IF RESPONSE IS "DON'T KNOW," ASK:
"If you had to guess, what time do you think you will go to your polling location?"]
[OPEN-ENDED. RECORD NEAREST CATEGORY OF RESPONSE.]
- a) Time: [HOUR] [AM/PM]
 - b) Morning
 - c) Afternoon
 - d) Evening
 - e) Don't know (volunteered twice)
 - f) Refused
 - g) Not applicable
- Q4 [ASK ONLY IF ANSWER TO Q2 WAS "YES"]**
Where will you be just before you go vote?
[IF RESPONSE IS "DON'T KNOW," ASK:
"If you had to guess, where do you think you will be before you go vote?"]
[OPEN-ENDED. RECORD NEAREST CATEGORY OF RESPONSE.]
- a) Home
 - b) Work

- c) Errands/shopping
- d) Other
- e) Don't know (volunteered twice)
- f) Refused
- g) Not applicable

Q5 [ASK ONLY IF ANSWER TO Q2 WAS "YES"]

How will you get to your polling location?

[IF RESPONSE IS "DON'T KNOW," ASK:

"If you had to guess, how do you think you will get to your polling location?"]

[OPEN-ENDED. RECORD NEAREST CATEGORY OF RESPONSE.]

- a) Car (driving or unspecified)
- b) Car (carpooling or getting a ride)
- c) Walking
- d) Public transportation (e.g. bus, subway)
- e) Other
- f) Don't know (volunteered twice)
- g) Refused
- h) Not applicable

Q6 [ASK ONLY IF ANSWER TO Q2 WAS "YES"]

Please commit yourself to voting by saying the following statement aloud. Can you please repeat after me? "On Tuesday [TIME, e.g. "at 4 p.m." or "in the morning"], when I am at [PLACE, e.g. "home"], I will [MODE OF TRANSPORTATION, e.g. "drive"] to the polling place and cast my vote!"

- a) Repeated statement
- b) Refused

Thank you for your help.

[Table 9]

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Figure 1. The Voting Process, from the Voter's Perspective

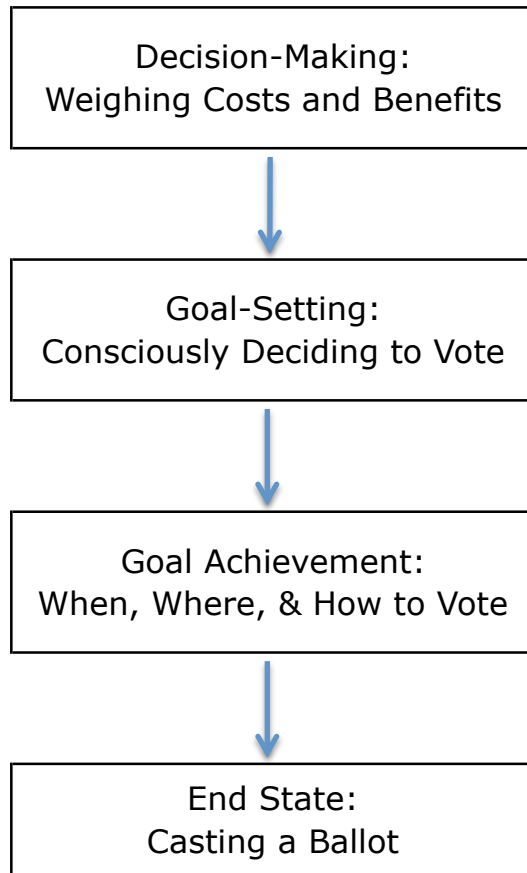


Table 1. Results from Previous Experiments on the Effects of Self-Prophecy on Voter Turnout

Study	Election	Study Population	Control	Treatment	Difference	Sample Size
Greenwald, Carnot, Beach, and Young (1987) ¹	1984 Presidential Election	OSU student registered voters	57.1%	81.3%	24.2%	60
Greenwald, Klingler, Vande Kamp, and Kerr (1988), Experiment 1 ²	1986 U.S. Senate Election	Registered voters in Seattle	87.0%	85.9%	-1.1%	627
Greenwald, Klingler, Vande Kamp, and Kerr (1988), Experiment 2 ³	1987 State Primary	Low-turnout registered voters in Seattle	31.0%	40.0%	9.0%	226
Smith, Gerber, and Orlich (2003) ⁴	2000 Presidential Primary	Registered voters in New England town	43.4%	43.3%	-0.1%	588

Notes:

¹ Taken from Experiment 2 using Method A for coding out-of-state voters.

² Figures compare control versus prediction-only treatment groups.

³ Figures compare knowledge control versus knowledge-plus-prediction treatment groups.

⁴ Figures compare control versus prediction-only treatment groups.

Table 2. Descriptive Statistics of Towns in Study Population

Town	Population	Density (pop./mi. ²)	Percent Caucasian	Median HH Income
Hamden	56,913	1,736.1	77.3%	\$52,351
New Haven	123,626	6,558.4	43.5%	\$29,604
Wallingford	43,026	1,102.7	94.8%	\$57,308
Waterbury	107,271	3,754.7	67.1%	\$34,285
West Haven	52,360	4,832.0	74.2%	\$42,393
Total (pooled)	383,196	2,945.4	65.1%	\$39,151

Source: U.S. Census Bureau 2000

Table 3. Relationship between Treatment Group Assignment and Covariates (Individual-Level Data)

	Implementation			Pooled	Chi-Square Test of Independence	P-Value
	Mere Contact	Self-Prophecy	Intentions			
	Mean	Mean	Mean	Mean		
Voted 2006	61.5%	61.4%	61.0%	61.3%	0.957	
Voted 2004	64.6%	63.8%	64.0%	64.1%	0.868	
Voted 2002	16.3%	17.7%	13.9%	16.0%	0.007	
Voted 2000	16.3%	16.7%	15.0%	16.0%	0.353	
Voted 1998	11.3%	10.7%	9.9%	10.6%	0.368	
Voted 1996	12.0%	11.6%	10.9%	11.5%	0.619	
Female	46.8%	47.5%	46.0%	46.8%	0.635	
Age (years)	58.9	58.5	59.8	59.1		
Yrs Registered	17.0	16.0	16.9	16.6		
N	1906	1858	1845	5609		

Note: A chi-squared test of independence cannot be applied to continuous variables such as age and years registered; only to dichotomous dummy variables. Difference of means t-tests for these two variables were insignificant between each pairing of groups, with one exception: the difference between the mean ages for the Self-Prophecy and Implementation Intentions groups had a p-value of 0.034.

Sources: Official voter registration and turnout records from each of the five towns.

Table 4. Effects of Three Phone Call Treatments on Voter Turnout in the February 2008 Presidential Primary, by Town

Town	Experimental Treatment Group			<i>N</i>
	Mere Contact	Self-Prophecy	Implementation Intentions	
Hamden	65.9%	73.1%	65.5%	499
New Haven	69.5%	72.6%	66.0%	1640
Wallingford	61.7%	63.5%	60.0%	936
Waterbury	48.6%	50.7%	51.6%	1449
West Haven	52.2%	49.9%	54.9%	1085
Pooled	59.1%	61.4%	58.9%	5609
Pooled <i>N</i>	1906	1858	1845	

Sources: Official voter turnout records from each of the five towns.

Table 5. OLS Regression Estimates of the Effects of Phone Call Treatments on Voter Turnout in the February 2008 Presidential Primary

	Model Specifications				
	(1)	(2)	(3)	(4)	(5)
Self-Prophecy	0.0223	0.0197	0.0174	0.0175	0.0181
(Standard Error)	(0.016)	(0.016)	(0.015)	(0.015)	-0.015
P-Value	0.164	0.211	0.244	0.236	0.217
Implementation Intentions	-0.0027	0.0004	0.0026	0.0024	0.0040
(Standard Error)	(0.016)	(0.016)	(0.015)	(0.015)	(0.015)
P-Value	0.867	0.98	0.863	0.871	0.786
Constant	0.591	0.516	0.512	0.368	0.148
<i>N</i>	5609	5609	5609	5609	5609
<u>Covariates</u>					
Town Dummies	N	Y	Y	Y	Y
Vote History	N	N	Y	Y	Y
Demographics	N	N	N	Y	Y
Aware of Election	N	N	N	N	Y

Note: Voter History includes variables for casting ballots in 2006, 2004, 2002, 2000, 1998, and 1996 (as indicated on official voter registration records). Demographics includes variables for voter age, sex, and number of years registered (as indicated on official voter registration records). Aware of Election is a dummy variable coded as 1 if the respondent said yes to the first question on all three treatments' phone surveys, which asked whether she was aware the presidential primary was on Tuesday, February 5.

Table 6. OLS Regression Estimates of the Effects of Any Phone Call Treatment on Voter Turnout in the February 2008 Presidential Primary

	Model Specifications				
	(1)	(2)	(3)	(4)	(5)
Either Treatment (Standard Error)	0.0098 (0.014)	0.0101 (0.014)	0.0100 (0.013)	0.0100 (0.013)	0.0111 (0.013)
P-Value	0.476	0.459	0.438	0.434	0.382
Constant	0.591	0.516	0.512	0.369	0.148
<i>N</i>	5609	5609	5609	5609	5609
<u>Covariates</u>					
Town Dummies	N	Y	Y	Y	Y
Vote History	N	N	Y	Y	Y
Demographics	N	N	N	Y	Y
Aware of Election	N	N	N	N	Y

Note: Voter History includes variables for casting ballots in 2006, 2004, 2002, 2000, 1998, and 1996 (as indicated on official voter registration records). Demographics includes variables for voter age, sex, and number of years registered (as indicated on official voter registration records). Aware of Election is a dummy variable coded as 1 if the respondent said yes to the first question on all three treatments' phone surveys, which asked whether she was aware the presidential primary was on Tuesday, February 5.

Table 7. OLS Regression Estimates of the Effects of Phone Call Treatments on Voter Turnout in the February 2008 Presidential Primary, by Town

	Hamden		New Haven		Wallingford	
	(1)	(5)	(1)	(5)	(1)	(5)
Self-Prophesy (Standard Error) P-Value	0.0723 (0.05) 0.146	0.0422 (0.046) 0.355	0.0310 (0.028) 0.265	0.0299 (0.025) 0.233	0.0179 (0.038) 0.641	0.0170 (0.036) 0.634
Implementation Intentions (Standard Error) P-Value	-0.0037 (0.052) 0.943	-0.0130 (0.048) 0.787	-0.0350 (0.028) 0.211	-0.0157 (0.025) 0.534	-0.0168 (0.039) 0.668	-0.0011 (0.036) 0.975
Constant	0.659	-0.014	0.695	-0.025	0.617	-0.056
N	499	499	1640	1640	936	936
<u>Covariates</u>						
Vote History	N	Y	N	Y	N	Y
Demographics	N	Y	N	Y	N	Y
Aware of Election	N	Y	N	Y	N	Y

Note: Voter History includes variables for casting ballots in 2006, 2004, 2002, 2000, 1998, and 1996 (as indicated on official voter registration records). Demographics includes variables for voter age, sex, and number of years registered (as indicated on official voter registration records). Aware of Election is a dummy variable coded as 1 if the respondent said yes to the first question on all three treatments' phone surveys, which asked whether she was aware the presidential primary was on Tuesday, February 5.

		Waterbury		West Haven	
	(1)	(5)	(1)	(5)	
	0.0211	0.0232	-0.0234	-0.0179	
	(0.033)	(0.03)	(0.037)	(0.036)	
	0.521	0.442	0.525	0.623	
	0.0306	0.0241	0.0268	0.0266	
	(0.031)	(0.029)	(0.038)	(0.037)	
	0.331	0.404	0.476	0.471	
	0.486	-0.076	0.522	0.079	
	1449	1449	1085	1085	
N	Y	Y	N	Y	
N	Y	Y	N	Y	
N	Y	Y	N	Y	

Table 8. OLS Regression Estimates of the Effects of Phone Call Treatments on Voter Turnout in the February 2008 Presidential Primary, by Prior Turnout History

	Both 2004 and 2006 Voters		Either 2006 or 2004 Voters		Neither 2006 nor 2004	
	(1)	(5)	(1)	(5)	(1)	(5)
Self-Prophesy (Standard Error)	0.0219 (0.02)	0.0179 (0.019)	0.0589 (0.04)	0.0550 (0.037)	0.0039 (0.03)	-0.0038 (0.029)
P-Value	0.269	0.349	0.139	0.14	0.896	0.895
Implementation Intentions (Standard Error)	-0.0085 (0.02)	-0.0012 (0.019)	0.0082 (0.039)	0.0026 (0.037)	0.0073 (0.03)	0.0135 (0.029)
P-Value	0.67	0.949	0.834	0.943	0.809	0.638
Constant	0.718	0.398	0.453	0.381	0.433	0.154
N	3041	3041	950	950	1615	1615
<u>Covariates</u>						
Town Dummies	N	Y	N	Y	N	Y
Vote History	N	Y	N	Y	N	Y
Demographics	N	Y	N	Y	N	Y
Aware of Election	N	Y	N	Y	N	Y

Note: Voter History includes variables for casting ballots in 2006, 2004, 2002, 2000, 1998, and 1996 (as indicated on official voter registration records). Demographics includes variables for voter age, sex, and number of years registered (as indicated on official voter registration records). Aware of Election is a dummy variable coded as 1 if the respondent said yes to the first question on all three treatments' phone surveys, which asked whether she was aware the presidential primary was on Tuesday, February 5.

Table 9. Descriptive Statistics of Voter Responses to Callers' Questions

Town	Experimental Treatment Group			Pooled
	Mere Contact	Self-Prophecy	Implementation Intentions	
<i>Q1. Were you aware that Connecticut's presidential primary elections will be held this Tuesday, February 5th?</i>				
Yes	93.5%	93.4%	92.6%	93.2%
No	5.9%	6.1%	6.7%	6.2%
<i>Q2. Do you expect you will vote in Tuesday's election?</i>				
Yes		83.7%	78.9%	81.4%
No		8.1%	10.4%	9.4%
Don't know (volunteered twice)		5.1%	5.7%	5.4%
Already voted		1.5%	1.8%	1.7%
Refused		0.2%	0.7%	0.6%
<i>Q3. What time on Tuesday do you plan to go to your polling location?</i>				
[Specified hour]			11.6%	
Morning			28.0%	
Afternoon			14.0%	
Evening			7.4%	
Don't know (volunteered twice)			13.8%	
Refused			0.7%	
<i>Q4. Where will you be just before you go vote?</i>				
Home			46.8%	
Work			14.4%	
Errands/shopping			2.4%	
Other			3.6%	
Don't know (volunteered twice)			6.0%	
Refused			0.7%	
<i>Q5. How will you get to your polling location?</i>				
Car (driving or unspecified)			52.8%	
Car (carpooling or finding a ride)			5.8%	
Walking			11.2%	
Public transportation			1.0%	
Other			1.2%	
Don't know (volunteered twice)			1.1%	
Refused			0.4%	

Q6. Please commit yourself to voting by saying the following statement aloud...

Repeated statement			51.3%	
Refused			22.3%	
<i>N</i>	1906	1858	1845	5609

Note: Missing values signify questions not asked in that treatment's script. Questions Q3 through Q6 were only asked of those respondents who said yes to Q2, but percentages are expressed in terms of the entire treatment group's size. Percentages that do not add up to the correct total represent respondents who declined to complete the survey at some point after answering Q1.