

Do You Have a Voting Plan? Implementation Intentions, Voter Turnout, and Organic Plan Making

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Abstract

Phone calls encouraging citizens to vote are staples of modern campaigns. Insights from psychological science can make these calls dramatically more potent while also generating opportunities to expand psychological theory. We present a field experiment conducted during the 2008 presidential election ($N = 287,228$) showing that facilitating the formation of a voting plan (i.e., implementation intentions) can increase turnout by 4.1 percentage points among those contacted, but a standard encouragement call and self-prediction have no significant impact. Among single-eligible-voter households, the formation of a voting plan increased turnout among persons contacted by 9.1 percentage points, whereas those in multiple-eligible-voter households were unaffected by all scripts. Some situational factors may organically facilitate implementation-intentions formation more readily than others; we present data suggesting that this could explain the differential treatment effect that we found. We discuss implications for psychological and political science, and public interventions involving implementation-intentions formation.

Keywords

implementation intentions, self-prediction, voting, nudges

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Helping people to make plans can increase their likelihood of following through on intentions. This finding has powerful implications for interventions directed toward increasing socially important behaviors that people intend to, but often fail to, perform. In this study, we conducted a field experiment during the 2008 U.S. presidential election, designed to increase turnout by assisting voters in forming *implementation intentions*. The results expanded our understanding of implementation intentions in two ways, one as hypothesized and another unexpectedly. First, we found that implementation intentions can be a potent addition to interventions aimed at increasing intention fulfillment for a specific high-salience and socially important behavior: voting. This turnout increase resulted from concrete plan making, not from simply asking people if they intended to vote. In fact, contrary to some past research, self-prediction only marginally increased turnout. Second, an unexpected heterogeneous treatment effect suggested a novel moderator for interventions leveraging implementation intentions. Participants in multiple-eligible-voter households were unaffected by the intervention, whereas those in single-eligible-voter households were strongly affected. We propose that some previously unrecognized situational factors organically

facilitate implementation-intention formation more readily than others and present data supporting this interpretation of our differential treatment effect. Results provide insight into when interventions leveraging implementation intentions will be most potent for increasing voter turnout, and potentially other intended behaviors.

Literature Review

Self-prediction

Predicting that one will follow through on a behavior may increase one's likelihood of doing so. This pattern of self-fulfilling self-prediction has been studied and labeled in multiple different areas of behavioral research (Greenwald, Carnot, Beach, & Young, 1987; Greenwald, Klinger, Vande Kamp, & Kerr, 1988; Morwitz, Johnson, & Schmittlein, 1993; Sherman, 1980). In fact, the original research on what was termed the

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self-prophecy effect was conducted in the context of voter turnout in the 1984 election (Greenwald et al., 1987). That research reported a 23-percentage-point increase in turnout as a result of people being asked if they intended to vote. A more recent experiment failed to replicate those findings using identical procedures and a sample size nearly 10 times as large (Smith, Gerber, & Orlich, 2003). Because the present experiment focused on the novel translation of implementation-intentions formation to voting, our design necessitated that we initially ask respondents if they intended to vote. Therefore, a self-prediction-only condition was included to attempt to replicate the self-prophecy research with the largest sample yet.

Implementation intentions

Even more potent than self-prediction, assisting people in plan making, or forming implementation intentions, can facilitate the fulfillment of goals (Gollwitzer, 1999; Gollwitzer & Sheeran, 2006). Articulating the when, where, and how of following through on an intention creates cognitive links between an anticipated future situation and the intended behavior. These pairs can be thought of as “if situation *Y*, then behavior *X*” (Gollwitzer, Bayer, & McCulloch, 2005; Gollwitzer & Sheeran, 2006). Implementation intentions have been shown to affect dozens of behaviors, including those that are repeated over time, such as exercising (Lippke & Ziegelmann, 2002; Milne, Orbell, & Sheeran, 2002), as well as onetime behaviors executed within a finite window, such as picking up course reading materials at an office at a certain time (Dholakia & Bagozzi, 2003). Nearly every context in which this cognitive process has been studied has involved behaviors that were either new to the participant (e.g., picking up books) or of a low-salience nature, so that participants would be unlikely to organically discuss them in detail with others (i.e., exercising). This article reports the first well-powered experiment showing the impact of implementation intentions on a high-salience, socially important behavior: voting. In the process, we discover a previously unrecognized moderator of the power of implementation-intentions formation to influence behavior.

“Get out the vote”

Voting is an important social behavior required by a functioning democratic society. In addition to its normative importance, tens of millions of dollars are spent by political campaigns and organizations to increase turnout. Field experimental research in political science has begun studying turnout interventions (e.g., Gerber & Green, 2000), most of which has compared the relative impact of different modes of contact including door to door, mail, e-mail, and phone (see Green & Gerber, 2008, for review). The present experiment used phones to encourage turnout. Research has shown that calls conducted with a conversational tone (like those used in our *standard* condition) can be more effective than calls made with a scripted tone, even though they are not necessarily longer

(Nickerson, 2006, 2007). A meta-analysis of 28 experiments found that a completed phone call from a commercial phone bank increases turnout an average of 1.0 percentage point (Green & Gerber, 2008). That is, for every 100 phone contacts, one person votes who would have otherwise abstained. This experiment introduces the psychological concept of implementation-intentions formation to voter-mobilization research.

Method

Our sample came from a list of registered Pennsylvanians eligible to vote in the 2008 presidential primary, purchased from a consumer data firm. Because only the Democratic primary was competitive, the experiment was limited to registered Democrats ($N = 4,200,109$). We selected the sample on the basis of the following criteria. First, we selected people with verified phone numbers that did not appear on any “do not call” registry ($n = 526,363$). To avoid targeting citizens who were extremely likely to vote, by definition unlikely to be responsive to the experimental procedures, only those who had voted in one or fewer primary elections since 2000 were included in the experiment (excluding 155,669). To expedite requesting a specific person while executing the experiment, only households containing three or fewer registered Democrats were included in the experiment (excluding another 14,578). Within the remaining households, one person was randomly selected as a participant for the experiment (final $n = 287,228$), and households were randomly assigned to one of the experimental conditions or the control condition (in which there was no attempted phone call).

Six scripts were constructed to isolate the effect of self-prediction and implementation intentions on turnout (see Appendix S1 in the Supplemental Material available on-line). The standard scripts encouraged participation by reminding participants about the election and their duty to vote (Scripts A and B). The self-prediction scripts were identical to the standard script but also asked whether the person intended to vote (Scripts C and D). The implementation-intentions scripts were identical to the self-prediction scripts but also asked three follow-up questions designed to facilitate voting plan making: what time they would vote, where they would be coming from, and what they would be doing beforehand (Scripts E and F). Two scripts were constructed for each experimental condition, to test a different hypothesis regarding descriptive social norms, which is not addressed in this article.¹ Because assignment to the scripts was random and our analysis was not affected by which script of the two each participant received, the discussion focuses only on the standard, self-prediction, and implementation-intentions components of the scripts.

Randomization was stratified by household size. The experimental procedure was administered between Saturday and Monday before Election Day by a professional firm that delivered millions of “get out the vote” calls in 2008. Phone numbers were provided to the firm in a random order and were loaded into its computer-aided calling system gradually. At

Table 1. Contact Rate and Impact of Condition on Voter Turnout Across All Household Sizes

Condition	Turnout	Contact rate	ITT analysis			ATT analysis		<i>n</i>
			Difference from the control condition	SE	<i>p</i> _{rep}	ATT	SE	
Control	42.9%	0.0%	—	—	—	—	—	228,995
Standard GOTV	42.8%	26.3%	-0.1%	0.4	.46	-0.2%	1.5	19,411
Self-prediction	43.4%	25.8%	0.5%	0.4	.83	2.0%	1.5	19,411
Implementation intentions	43.8%	23.0%	0.9%	0.4	.95	4.1%	1.7	19,411

Note: GOTV = "get out the vote." Turnout was verified using the official Democrat voter file for Pennsylvania. The intent-to-treat (ITT) analysis compared the rate of voter turnout in each experimental condition with the rate in the control condition, independent of contact rates. The average treatment effect among the treated (ATT) estimates the impact on turnout among individuals who were contacted by adjusting the ITT effect for the contact rate (Angrist, Imbens, & Rubin, 1996). Standard errors reflect a fixed-effects estimator to control for strata of randomization. All *p*_{rep}s are based on one-tailed *ps*.

any given moment, the scripts were evenly distributed across callers. Because the implementation-intentions script was longer than the standard and self-prediction scripts, fewer implementation-intentions calls were attempted and completed. The randomized ordering of the phone numbers ensured that the imbalance only reduced efficiency and did not bias the analysis. Appendix S2 in the Supplemental Material available online shows that the covariates did not differ significantly across condition or across contacted participants.

After the election, participants in the experimental and control conditions were matched to official voter-turnout records to determine who voted, so the dependent variable was measured identically for both groups and not subject to self-reporting biases. When not every participant receives the assigned treatment, experiments commonly provide two estimates of treatment effect. The intent-to-treat analysis compared rates of voter turnout across conditions, independent of contact rates. However, most participants were not actually contacted because they were unavailable, and even fewer participants in the implementation-intentions condition were contacted because of the contract with the phone vendor, as described in the previous paragraph. Thus, we also adjusted for the contact rate using the random assignment as an instrument for actual contact (Angrist,

Imbens, & Rubin, 1996; Gerber & Green, 2000), to estimate the average treatment effect among the treated (ATT).

Results

Table 1 contains turnout and contact rates for the four conditions. Although very similar to scripts that have been successful in other settings, the standard script did not have any mobilizing impact (ATT = -0.4). Targets who heard the self-prediction script were 0.5 percentage points (ATT = 2.0) more likely to vote than were targets in the control group, which was marginally significant. Those assigned to the implementation-intentions condition were 0.9 percentage points (ATT = 4.1) more likely to vote compared with the control group, which was highly significant. Although the experiment involved 287,228 targeted voters, the results were not precise enough to disentangle the unique contribution of self-prediction to the overall impact of the implementation-intentions condition.

Analyzing the experimental effect by number of eligible voters in a household helps to explain the differential impact of the self-prediction and implementation-intentions scripts and suggests, post hoc, a moderator for the impact of the implementation-intentions script. Table 2 shows that targets in

Table 2. Contact Rate and Impact on Voter Turnout for Households With One Eligible Voter

Condition	Turnout	Contact rate	ITT analysis			ATT analysis		<i>n</i>
			Difference from the control condition	SE	<i>p</i> _{rep}	ATT	SE	
Control	40.7%	0.0%	—	—	—	—	—	199,131
Standard GOTV	40.5%	25.0%	-0.2%	0.5	.41	-0.7%	2.1	9,487
Self-prediction	40.9%	24.3%	0.3%	0.5	.64	1.1%	2.1	9,474
Implementation intentions	42.7%	22.1%	2.0%	0.5	.997	9.1%	2.3	9,484

Note: GOTV = "get out the vote." Turnout was verified using the official Democrat voter file for Pennsylvania. The intent-to-treat (ITT) analysis compared the rate of voter turnout in each experimental condition with the rate in the control condition, independent of contact rates. The average treatment effect among the treated (ATT) estimates impact on turnout among individuals who were contacted by adjusting the ITT effect for the contact rate (Angrist, Imbens, & Rubin, 1996). Standard errors reflect a fixed-effects estimator to control for strata of randomization. All *p*_{rep}s are based on one-tailed *ps*.

Table 3. Contact Rate and Impact on Voter Turnout for Households With Two or Three Eligible Voters

Condition	Turnout	Contact rate	ITT analysis			ATT analysis		n
			Difference from the control condition	SE	p_{rep}	ATT	SE	
Control	44.2%	—	—	—	—	—	—	29,864
Standard GOTV	44.3%	27.4%	0.1%	0.6	.26	0.3%	2.1	9,924
Self-prediction	45.0%	27.2%	0.8%	0.6	.84	3.0%	2.1	9,927
Implementation intentions	43.8%	23.8%	-0.4%	0.6	.21	-1.5%	2.4	9,927

Note: GOTV = “get out the vote.” Turnout was verified using the official Democrat voter file for Pennsylvania. The intent-to-treat (ITT) analysis compared the rate of voter turnout in each experimental condition with the rate in the control condition, independent of contact rates. The average treatment effect among the treated (ATT) estimates impact on turnout among individuals who were contacted by adjusting the ITT effect for the contact rate (Angrist, Imbens, & Rubin, 1996). Standard errors reflect a fixed-effects estimator to control for strata of randomization. All p_{rep} s are based on one-tailed p s.

one-eligible-voter households were unaffected by the standard and self-prediction scripts. However, targets in these households in the implementation-intentions condition were 2.0 percentage points (ATT = 9.1) more likely to vote than those in the control group.² Table 3 shows that in multiple-eligible-voter households, none of the scripts significantly affected turnout relative to the control group. A significant interaction between household size and condition (implementation intentions vs. control) confirmed that turnout was increased only among targets in the implementation-intentions condition who came from one-eligible-voter households ($p_{rep} = .98$; see Fig. 1).

Why would targets in the one-eligible-voter households respond to the implementation-intentions intervention while those in multiple-eligible-voter households remained unaffected? One possible explanation is that citizens in the two types of households differed in their responsiveness. Those in multiple-eligible-voter households voted at the same rates in the 2006 general election as those in one-eligible-voter households (31.0%), were marginally younger (mean age = 42.6 years vs. 43.1 years, $p_{rep} > .99$), were more likely to be men (52.6% vs. 56.7%, $p_{rep} > .99$), were more likely to be African American (26.9% vs. 22.6%, $p_{rep} > .99$), and were slightly more likely have been in the implementation-intentions group (23.8% vs. 26.2%, $p_{rep} > .99$). The heterogeneous effect of the implementation-intentions intervention was unaffected by whether we controlled for these attributes. To our knowledge, no research has determined that certain attributes systematically affect people’s responsiveness to an implementation-intentions intervention. Nonetheless, the previous comparisons show that eligible voters who live together are different from those who live alone. Although there is no supportive evidence, it is conceivable that these differences could explain why eligible voters who live together are impervious to the implementation-intentions intervention.

However, we found supportive evidence for a plausible explanation that could partly explain our results. Eligible voters who live together may be more likely to organically make voting plans than those who live alone. Casting a vote can

be time-consuming, personally important, and deliberative and can require transportation and child care—characteristics that might make a topic more likely to be the subject of intrahousehold conversation. Perhaps targets in one-eligible-voter households were less likely to have made voting plans before our intervention than were those in multiple-eligible-voter households. This hypothesis was confirmed in the data. Targets in one-eligible-voter households were more likely to answer “do not know” than were those in multiple-eligible-voter households to each of the three plan-making questions (see Table 4).

Discussion

Phone calls encouraging citizens to vote are staples of modern campaigns. Insights from psychological science can make these

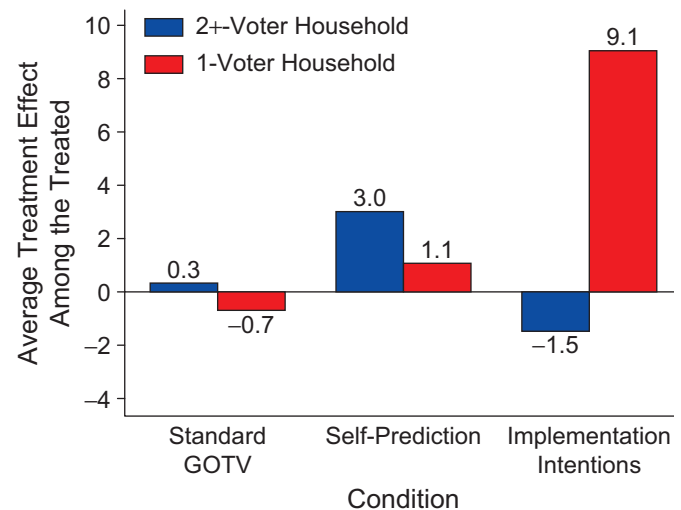


Fig. 1. Average treatment effect among the treated as a function of condition and number of eligible voters in the household. In all three conditions, participants were reminded by phone about the election and their duty to vote. Targets in the standard “get out the vote” (GOTV) condition received this reminder only. Those in the self-prediction condition were also asked if they intended to vote, and those in the implementation-intentions condition were asked three follow-up questions designed to facilitate making plans for voting.

Table 4. Percentage of Targets Who Responded With “Do Not Know” to Each Question in the Implementation-Intentions Battery and Tests of the Difference Between 1-Voter and 2+-Voter Households

Question	Household size		Difference between types of households			
	1 voter	2+ voters	$\chi^2(1)$	<i>n</i>	<i>p</i>	<i>p_{rep}</i>
What time will you vote?	22.4%	18.0%	9.70	3,252	< .01	.98
Where will you be coming from?	10.4%	7.9%	5.66	2,941	.02	.93
What will you be doing beforehand?	10.8%	7.9%	7.55	2,885	< .01	.96

calls dramatically more potent while also generating opportunities to expand psychological theory. In our experiment, a standard phone call had no impact on overall turnout, a call eliciting a vote-intention self-prediction produced a marginally significant turnout increase, and a script that incorporated both self-prediction and implementation intentions resulted in a 4.1-percentage-point increase in turnout among those in the experimental conditions. The weak self-prediction impact supports recent contentions (Smith et al., 2003) that the much-cited large self-prediction effect detected in Greenwald et al. (1987) is no longer replicable. Unexpectedly, we found that among single-eligible-voter households, targets were 9.1 percentage points more likely to vote when they received a script guiding them to make a plan, whereas targets in multiple-eligible-voter households were unaffected by the same script. We propose that one source of this heterogeneity could be the social context in which targets reside. Targets living in multiple-eligible-voter households were much less likely to have a preexisting voting plan than were targets living in single-eligible-voter households. This suggests that targets living with others who might share an interest in the focal behavior are more likely to engage in plan making on their own, which might explain the impotence of the plan-making intervention when directed at them. This rationale seems most plausible for behaviors about which intrahousehold conversation is greatest (i.e., high-salience, personally important behaviors). In addition to studying the conditions under which implementation intentions naturally arise in the world, future research should examine the types of behaviors for which these intentions occur on their own.

By introducing the psychological construct of implementation intentions to this political topic, we broaden traditional conceptualizations of the cost of voting, which usually relate to the cost of the time required to go to one’s polling place (Blais, 2000). Guiding the formation of implementation intentions does not reduce the time required to cast a vote; rather, it increases the likelihood that one will plan time for it.

This research contributes to a growing body of work using behavioral science to facilitate socially important behaviors (Thaler & Sunstein, 2008). Campaign professionals can use psychological science more widely to help citizens follow through on their intentions to vote. In addition to showing that implementation intentions can be a valuable addition to “get out the vote” efforts, we provide prescriptive guidance for other public

interventions. For prosocial behaviors that are highly salient and widely discussed, our results suggest interventions will have the greatest impact on individuals least likely to have engaged in those discussions. For example, after television reporter Tim Russert died of a heart attack, many health organizations used the high-salience event to encourage people to get medical examinations (Johnson, 2008). This was likely a topic of conversation within many households, especially those with multiple adults. Under these conditions, our results suggest that single-adult households might have benefited most from an implementation-intentions intervention for the goal of getting an exam.

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Declaration of Conflicting Interests

The authors declared that they had no conflicts of interests with respect to their authorship and/or the publication of this article.

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Supplemental Material

Additional supporting information may be found at <http://pss.sagepub.com/content/by/supplemental-data>

Notes

1. We also studied whether a script emphasizing high turnout would be more effective than one emphasizing low turnout. The results are to be reported elsewhere.
2. Contrasts showed that this increase was significantly greater than in the standard “get out the vote” condition ($SE = .007, p = .002, p_{rep} > .98$) and in the self-prediction condition ($SE = .007, p < .007, p_{rep} > .96$).

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