ROCKING THE VOTE
USING PERSONALIZED MESSAGES TO MOTIVATE VOTING AMONG YOUNG ADULTS

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Abstract We examined a nationwide effort to encourage young adults to vote in the 1996 U.S. presidential election. During the year before the election, individuals were given the chance to sign and self-address one of two kinds of postcards pledging to vote; these cards were mailed back to the individuals within 2 weeks prior to the election. It is important to note that some individuals completed pledge cards that prompted them to provide their own reason for voting by completing the sentence, “I will vote because ______ ,” whereas other individuals completed pledge cards that did not contain this sentence prompt. We conducted a large-scale survey of individuals who filled out pledge cards and determined that receiving a pledge card with the sentence prompt had a positive influence on voting. Moreover, this effect was found above and beyond demographic and psychological predictors of voting. Implications of these findings are discussed.

Introduction

The right to vote is one of the hallmarks of a democracy, yet even as more democracies have emerged in the post–Cold War era, there has been a decline in the frequency with which citizens living in established democracies exercise this fundamental freedom. Voter turnout worldwide has declined over the past 40 years (Dalton 1996), especially in the United States. Much of this decline can be attributed to a generational shift in voting rates, because the current
generation of young people in the United States is much less likely to turn out at the polls than the young people of earlier generations. Likewise, young people in Britain and France are much less inclined to vote compared to older citizens (Dalton 1996). As older generations become a smaller share of the total electorate, reversing the trend of political inactivity among young citizens becomes particularly important to maintaining the viability of participatory democracy.

In the United States, recognition of this problem inspired numerous efforts to increase voter turnout. The 1993 “Motor Voter” bill loosened voter registration requirements by allowing citizens to register to vote when they apply for their driver’s licenses; state laws have been enacted that allow same-day registration and advance voting; organized efforts have been undertaken to transport citizens to the polls on election day; public service announcements extol the benefits of voting. However, despite widespread knowledge that the decline in voting rates is in large part attributable to lowered rates of voting among young people, there have been few organized efforts designed to increase voting among this age group.

One exception has been the campaign sponsored by Rock the Vote, an organization whose primary goal is to encourage young adults to get involved in politics. During the 1992 presidential election, Rock the Vote initiated a mass media campaign on the cable music channel MTV using public service announcements (PSAs) in which celebrities urged young adults to vote. It is important to note that these PSAs were designed to be appealing to younger Americans. In 1996, this media campaign was extended to include radio announcements and was coupled with a nationwide voter registration effort targeted at young adults. Although voter registration drives on college campuses are a common way to reach young adults, Rock the Vote was unusual in that it was committed to reaching out more broadly to young people, including the noncollege population. To this end, Rock the Vote set up voter registration booths at a variety of venues where young adults were likely to congregate, such as music concerts and community festivals, during the 10 months preceding the 1996 presidential election.

One noteworthy aspect of Rock the Vote’s 1996 campaign was the use of “pledge cards” intended to motivate newly registered voters to exercise their right to vote on election day. When registering to vote, campaign workers encouraged individuals to sign, date, and self-address a Rock the Vote postcard. By filling out the card, individuals pledged that they would “rock the system by exercising my right to vote on November 5, 1996.” These pledge cards were then collected by Rock the Vote and were mailed back to the individuals within the 2 weeks prior to election day as a reminder to vote.

Serendipitously, partway through the campaign, Rock the Vote created a new version of the pledge card. The new pledge cards included the following sentence prompt: “I will vote because _____,” with the idea that young people would write their own personalized reasons for voting in the blank space,
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thereby increasing the effectiveness of the pledge card as a means of persuasion. Rather than waste their existing stock of pledge cards, the organization also continued to use the original version of the pledge cards, which did not contain this sentence prompt. In effect, this situation constituted a rare opportunity to systematically examine whether giving individuals the opportunity to provide their own reason for voting would actually lead to increased voter turnout. Our hypothesis that the new version of the pledge cards would increase the cards' effectiveness is grounded in psychological theories of attitudinal advocacy (Chaiken, Wood, and Eagly 1996). If this hypothesis were proved correct, it would have practical as well as theoretical implications. From a practical standpoint, this persuasion technique could provide a fairly simple and efficient means of stimulating a variety of prosocial behaviors. From a theoretical perspective, confirmation of this hypothesis would bolster social psychological theories of attitudinal advocacy—the idea that persuasion is most effective when individuals are induced to espouse the attitude that is the target of the persuasive appeal as if it were their own. Below we discuss several social psychological theories of attitudinal advocacy that generate this hypothesis.

Theories of Attitudinal Advocacy

Psychologists have long recognized the power of written or verbal commitment to guide behavior. Specifically, the act of making a commitment to perform a particular behavior significantly increases the chance that the individual will actually engage in that behavior (e.g., Cialdini 1984; Kiesler 1971). For example, individuals who made a written pledge to wear their safety belts (Nimmer and Geller 1988), to wear safety glasses (Streff et al. 1993), or to participate in a recycling program (Werner et al. 1995) were more likely to do so than individuals who did not make such a written commitment. In the domain of voting behavior, students who were contacted by telephone and asked to predict whether they would vote in an upcoming election (an action that the students may have experienced as committing) were actually more likely to vote than students who were not asked to make such a prediction (Greenwald et al. 1987). Extrapolating from these studies, we would expect that individuals who received the pledge card with the sentence prompt—who not only pledged to vote, but who also had the opportunity to explain why they would vote—should feel a heightened sense of commitment to this behavior, compared to individuals who received the pledge card without the

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1. It should be emphasized that the current research does not investigate whether individuals who receive a pledge card are more likely to vote than those who do not receive a pledge card, but rather whether individuals whose pledge cards contain the sentence prompt are more likely to vote than individuals whose pledge cards do not contain the sentence prompt.
sentence prompt, whose commitment to voting consisted only of signing the pledge card.

From a slightly different perspective, cognitive dissonance theory (Festinger 1957) posits that people are motivated to avoid holding dissonant or inconsistent cognitions because such inconsistency produces a state of negative arousal. Receiving a pledge card should be perceived as more inconsistent with the decision not to vote when the pledge card prompts the recipient to think about his or her own reasons for voting than when the pledge card simply contains the preprinted message, “I will rock the system by exercising my right to vote on November 5, 1996.” Thus, we expect that pledge cards that contain the sentence prompt would be more effective in motivating voting behavior than pledge cards without the sentence prompt because they should evoke greater levels of dissonance. Subsequent elaborations of the theory of cognitive dissonance have identified additional conditions necessary for dissonance to occur. For example, Aronson (1969; Thibodeau and Aronson 1992) has argued that dissonance is evoked when there is an inconsistency between an individual’s desired self-view and that individual’s behaviors. Pledge cards containing the sentence prompt should be more likely to motivate voting than pledge cards without the sentence prompt, because they are more likely to lead recipients to believe that voting is an important part of their self-concept.

Self-perception theory (Bem 1972) contends that people infer their attitudes from observing their own behaviors. Traditionally, this theory has been invoked as an alternative to the theory of cognitive dissonance (e.g., Cooper and Fazio 1984; Zanna and Cooper 1974), but it also has been used to examine persuasion processes (e.g., Haemmerlie and Montgomery 1984; Miller, Brickman, and Bolen 1975). In the current persuasion context, pledge card recipients should be more likely to infer that they believe voting is important when they receive a pledge card with, rather than without, the sentence prompt because the former is more likely to lead to internal attributions (e.g., “voting is important to me”) and is less likely to lead to external attributions (e.g., “I signed the pledge card because someone handed it to me”) than the latter. This heightened perception of one’s pro-voting attitudes, engendered by the pledge card with the sentence prompt, should then be the crucial determinant of subsequent voting.

In sum, theories of commitment, cognitive dissonance, and self-perception predict that the pledge cards with the sentence prompt, as opposed to those without the sentence prompt, would be more effective in persuading recipients to vote. As such, we would argue that the seemingly simple technique employed by Rock the Vote may engage a whole host of diverse yet compatible psychological mechanisms that fall under the general rubric of attitudinal advocacy (Chaiken, Wood, and Eagly 1996). The notion of attitudinal advocacy, or self-persuasion, represents a set of theories that explain how individuals can be persuaded by their own arguments and actions. Because each of the theories is an example of attitudinal advocacy, it should not be surprising
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that a similar prediction about the effect of the personalized pledge card would be derived from each. Further, because these theories invoke basic psychological processes, we would expect that the predicted effect would emerge above and beyond the influence of a variety of demographic and political-psychological factors (e.g., age, race, education, political interest, political efficacy, political knowledge) that have been demonstrated to predict voting (Verba, Schlozman, and Brady 1995).

Overview of the Current Research

As social scientists, we believed that Rock the Vote’s pledge card program provided an unprecedented opportunity to test the effectiveness of attitudinal advocacy on motivating prosocial behavior. Instead of being exposed to one of two treatment conditions within the confines of the laboratory, where such theories have typically been studied (Eagly 1992), participants in Rock the Vote’s campaign completed one of the two types of voting pledge cards at rallies, concerts, and restaurants, within the context of their usual activities. Additionally, although some of the participants were college students, a sizable proportion came from the noncollege population. Likewise, although most participants were white, sizable percentages were Latino/Latina and African-American. Moreover, because the campaign was conducted in the context of a national election, we were able to examine voting behavior, rather than merely examining attitudes toward voting, or intentions to vote at a later date. These features enabled us to overcome many of the common critiques of experimental psychological research, such as experimental demands for a great deal of cognitive processing, the reliance on college students (who are motivated to engage in information processing), and the measurement of attitudes rather than behavior (see Sears [1986] for a review). Hence, if the new persuasion technique employed by Rock the Vote was found to improve the pledge cards’ effectiveness in an “open” field setting in which participants’ attention was not artificially directed toward the persuasive message, we could be confident that these results would be highly robust and generalizable to other real-world voting campaigns, as well as to other campaigns designed to encourage other types of civic engagement, perhaps even participation in surveys.

Although we were eager to seize this naturally occurring opportunity to test the effectiveness of this novel technique to increase voter turnout, we were also aware that any study conducted to capitalize on this opportunity could include some limitations. Because the intervention was designed and implemented by Rock the Vote prior to our involvement, our research design was constrained. For example, the distribution of the two types of pledge cards was not random. Instead of the random assignment to condition that would be employed in a true experiment, people who participated later in the
campaign were more likely to receive pledge cards with the sentence prompt. If participation in the later stages of the campaign was systematically associated with other factors that predict voting, such as political interest and political knowledge, this would pose a serious threat to the internal validity of our study. Nonetheless, this was a potential confound that could be readily accounted for by including the time at which the pledge card was completed as a statistical control.

In the belief that the potential benefits of this field study outweighed the design's limitations, we decided to conduct a carefully crafted national survey of individuals who completed Rock the Vote pledge cards during the 1996 presidential campaign. Shortly after the election, we mailed questionnaires to a representative sample of individuals who had completed Rock the Vote pledge cards. The main purpose of these questionnaires was to measure the rates at which people who encountered the postcards had voted. The survey instruments included self-reported voting and an additional question designed to correct for overreporting.

In addition, to compensate for the lack of strict random assignment, we included as many known predictors of voting as possible in order to (1) check the extent to which the two samples were comparable and (2) statistically control for any differences between the two groups. Based on a thorough review of the literature on political participation (e.g., Verba, Schlozman, and Brady 1995), we included survey items designed to measure demographic and political-psychological factors that have proven to be significantly related to voting. The demographic items assessed gender, age, race/ethnicity, strength of partisanship, current level of education, and levels of the respondents' parents' education (as proxies for the socioeconomic status of the respondents). The political-psychological items were political interest, personal political efficacy, group political efficacy, subjective political knowledge, objective political knowledge, and prior participation in a variety of political and non-political civic activities.

**Method**

Our research strategy was to draw a random sample of pledge cards from the large number of cards that the Rock the Vote pledge card campaign generated. Figure 1 depicts the chronology and figure 2 schematically depicts the process by which we arrived at the different samples we use in the analyses presented herein. First, in October 1996, under our direction, 3,353 pledge cards (4 percent of the approximately 80,000 completed pledge cards) were selected from large bins in warehouses rented by Rock the Vote. The number of pledge cards in each bin was estimated, and a proportional number of pledge cards was selected from each bin, producing a sample that was as close to being random as possible under the circumstances. As may be seen in figure 2.
<table>
<thead>
<tr>
<th>MONTH</th>
<th>ROCK THE VOTE</th>
<th>CSPP (the authors)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Early to late 1995</td>
<td>Sends out activist kits to set up a network of volunteers who are trained to register voters</td>
<td></td>
</tr>
<tr>
<td>December 1995 to October 1996</td>
<td>Activists staff registration tables at concerts, fairs, and campuses. The reminder postcards are also available at the tables.</td>
<td></td>
</tr>
<tr>
<td>December/January 1995</td>
<td>Rock the Vote starts printing and using the new version of the postcards which contain the sentence prompt &quot;I will vote because ____&quot;</td>
<td></td>
</tr>
<tr>
<td>December 1995 to October 1996</td>
<td>All of the reminder postcards that were filled out by visitors to RTV tables are stored in a warehouse.</td>
<td>CSPP team visits the warehouse and designs a method for drawing a sample of the reminder postcards. RTV staff photocopy these postcards and send them to Minnesota about two weeks before the election.</td>
</tr>
</tbody>
</table>

**Figure 1.** Chronology of Rock the Vote pledge cards
Figure 2. Arrival at sample of survey recipients
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percent of pledge cards \((n = 1,896)\) contained the sentence prompt, "I will vote because _____," and 43 percent of pledge cards \((n = 1,440)\) contained the preprinted message, "I will rock the system by exercising my right to vote on November 5, 1996." Additionally, as figure 2 indicates, 22 percent of pledge cards that contained the sentence prompt were not completed by recipients \((n = 423)\).

On November 7, 2 days after the election, we mailed 3,336 questionnaires, along with a Rock the Vote sticker as an incentive for completing the questionnaire. Recipients were told in a cover letter that the University of Minnesota was conducting an independent survey of participants in Rock the Vote's voter registration campaign. Twenty-six pledge cards were removed from the sample because they were illegible. On December 10, we subtracted from the mailing list both those people who returned a questionnaire to us and those individuals whose questionnaires the U.S. Postal Service returned because they were undeliverable. We sent out a second mailing to the remaining 2,616 people. This time, respondents were promised a free compact disk if they returned the survey.

We received a total of 968 mail surveys, 679 (70 percent) from the first mailing and 289 (30 percent) from the second mailing. Two of these surveys were not used because they had been completed by people other than those who originally completed the pledge card. Nine surveys from the second mailing could not be used because they came from respondents who had already returned surveys from the first mailing. In addition, 18 people refused to give their informed consent, and 345 surveys were returned by the Postal Service as undeliverable. The rate of surveys returned by the Postal Service was greatest among participants who received pledge cards without the sentence prompt (14 percent) compared to participants who completed the sentence prompt (9 percent) and compared to participants who received a pledge card with a sentence prompt but failed to complete the sentence prompt (4 percent), \(X^2(3,334) = 13.52, p = .001\). This left us with a response rate of 32 percent \((n = 957)\) of the people we believe actually received the mail survey.\(^2\) As may be seen in figure 2, the survey return rates differed across condition, \(X^2 (2,856) = 21.41, p < .0005\). Thirty-eight percent of participants who completed the sentence prompt returned their surveys, compared to 30 percent among participants who failed to complete their sentence prompt and 29 percent among participants who received pledge cards that did not contain the sentence prompt. It is difficult to know what factor, or combination of factors, led to this difference in return rates between the different pledge card conditions. However, because we include survey items that measure all of the individual-level variables that are widely acknowledged to influence voting,

\(^2\) Of course, this is an underestimate. We are certain that some unknown number—probably fairly large—of these people did not receive the questionnaire, and yet these surveys were not returned to us by the U.S. Postal Service.
we can control for known preexisting differences between these groups in our statistical analyses.

SURVEY INSTRUMENT

Our survey consisted of 39 questions designed to assess the key variables for our analysis as well as information directly pertinent to Rock the Vote’s efforts (e.g., participants’ reactions to the organization’s campaign, degree of interest in specific political issues). Below we describe the measures we used in our analyses.

Political-psychological variables. We assessed six political-psychological variables that have proven to be significantly related to voting behavior in previous research (Verba, Schlozman, and Brady 1995). Political interest, personal political efficacy, group political efficacy, and subjective political knowledge were all measured on 4-point scales. For each of these, higher values indicate more of the factor (e.g., for subjective political knowledge, 0 = nothing at all, 4 = a great deal). Objective political knowledge was measured by creating a composite index from respondents’ answers to five factual questions about American politics (Delli Carpini and Keeter 1996). Finally, the extent of respondents’ prior civic participation was assessed by creating a composite variable from a checklist of eight different kinds of political and nonpolitical activities. (This checklist did not include prior voting behavior.) Respondents were given a 1 for every kind of activity they had done before, and thus the composite variable ranged from 0 to 8.

Demographics. We assessed several demographic variables, including gender, age, race/ethnicity, partisan strength (1 = Independent/don’t know/other, 2 = mildly Democratic/Republican, 3 = strongly Democratic/Republican), and current level of education (1 = some high school, 5 = postgraduate education). In addition, we measured the levels of the respondents’ parents’ education on the same scale as proxies for the respondents’ socioeconomic status.3

Voting behavior. Our key variable of interest was a measure of the respondents’ voting behavior. To assess voting, we asked the following question on the survey: “For a variety of reasons some people did not vote in the 1996 presidential elections. What about you?” Respondents could indicate that they did vote, that they did not vote, or that they had been too young to vote. If they indicated that they had voted, respondents were asked to provide the specific location of the polling place where they voted. If possible, they were to give an address, to describe the location as best they could, or to indicate that they had voted by absentee ballot. From this information we constructed a “corrected” self-report measure in which individuals were considered to have actually voted only if they were able to give us a specific location where

3. The demographic variables also confirmed that our sample was diverse (e.g., 25 percent of our respondents are nonwhite).
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they had voted; individuals who said they had voted but were not able to provide us with a specific location were considered not to have voted. We use this corrected self-report measure of voting in our primary analyses in order to circumvent many of the problems associated with self-report measures of voting (e.g., Silver, Anderson, and Abramson 1986). A more extensive discussion of our rationale for utilizing this corrected self-report measure is presented in appendix A, and a description of how we validated this correction technique is presented in appendix B.

In addition to assessing voting via self-report, we were able to obtain official voting records from 29 percent of the survey recipients and so were able to conduct a second set of analyses using official voting records as our measure of voting. However, these analyses were limited by the fact that the sample size was severely reduced by our inability to obtain official voting records for all participants.

Results

Statistical Controls

We recognized that the design of our investigation included a potential threat to internal validity because the distribution of the different kinds of postcards was not entirely random. As mentioned earlier, individuals who participated in the early stages of the campaign were significantly more likely to receive pledge cards without the sentence prompt ($t(807) = 19.40, p < .0001$). Moreover, some survey respondents ($n = 67$) were part of a special campaign targeted at urban youth, the “Hip Hop Coalition” campaign. These respondents completed different pledge cards that did not contain the sentence prompt. Compared to all others, Hip Hop Coalition respondents were significantly more likely to be persons of color ($t(801) = 10.23, p < .0001$) and were lower in objective ($t(810) = 3.53, p < .001$) and subjective ($t(810) = 2.16, p < .03$) political knowledge. They did not differ from the rest of the sample in gender, partisan strength, education level, parents’ education level, political interest, and individual and group political efficacy. To guard against these confounds, we coded whether participants received Hip Hop Coalition pledge cards and the month in which participants filled out their pledge cards so that we would be able to control for these factors statistically in our primary analyses.

Additionally, we compared individuals who received a pledge card with the sentence prompt to those who received a pledge card without the sentence prompt on a number of previously mentioned demographic and political-

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4. We removed 144 respondents from the sample because they claimed to have voted via absentee ballot, and we could not use our corrected self-report measure to determine if they had voted or not. In addition, four individuals were too young to vote, and three more respondents did not report whether they had voted. In sum, we removed 151 respondents from our analyses.
psychological variables so that we could ascertain whether these groups differed in any systematic way that would threaten the validity of our conclusions. There were no differences between the two groups on gender, age, political interest, subjective political knowledge, and whether they responded to the first or the second mailing. However, nonwhites were more likely to receive pledge cards without the sentence prompt, \( t(801) = 3.26, p = .001 \). In addition, individuals who received pledge cards without the sentence prompt had more education \( (t(810) = 3.44, p = .001) \), and had parents who attained higher levels of education (mother’s education, \( t(810) = 2.50, p = .01 \), father’s education, \( t(810) = 2.17, p = .03 \)). Individuals who received pledge cards without the sentence prompt were also higher in partisanship strength \( (t(810) = 3.02, p = .003) \), political knowledge \( (t(810) = 3.37, p = .003) \), personal political efficacy \( (t(810) = 2.91, p = .006) \), and group political efficacy \( (t(810) = 2.43, p = .02) \), and were more likely to participate in politics \( (t(810) = 2.67, p = .008) \). It is interesting that, with the possible exception of race, the characteristics of individuals who received pledge cards without the sentence prompt have all been demonstrated to be positively associated with voting (Verba, Schlozman, and Brady 1995). In other words, these differences actually work against our prediction of finding increased rates of voting among individuals whose pledge cards contained the sentence prompt. At any rate, we include controls for all of these demographic and political-psychological variables in our statistical analyses.

Some individuals who received the pledge card with the sentence prompt did not complete it, posing an additional threat to internal validity. A comparison of individuals who completed the sentence prompt to those who left the sentence prompt blank revealed only three differences. Those who did not complete the sentence prompt were lower in subjective political knowledge, \( (t(513) = 2.01, p = .05) \), were less likely to have participated in the past, \( (t(513) = 2.22, p = .03) \), and filled out their pledge card closer to the election \( (t(510) = 3.55, p < .0004) \).

**THE INFLUENCE OF TYPE OF PLEDGE CARD ON VOTING**

To test our primary hypothesis concerning the effect of the type of pledge card on participants’ voting behavior, we estimated two binary logit models to examine the effects of the different pledge cards on voting.  

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5. It could be argued that the dependent variable should have three categories instead of two. For example, one way to look at the dependent variable is to notice that the correction index is nested within the response to whether the respondent voted. All of the cases that were sorted by the correction index were respondents who claimed that they had voted. Thus there is the risk that the results of our equation are biased by a selection process that we have not estimated (i.e., a separate model that determines whether people claim to have voted, in addition to our model of interest). In an effort to assess whether our results are biased by a selection process, we estimated a bivariate probit model. Bivariate probits estimate the correlation between the errors of the selection equation and the equation of interest. Under a wide variety of specifications for the selection model, the correlation between the errors (rho) was never far from zero and
### Table 1. Logit Model for Variables Predicting Voting Using Corrected Measure of Voting ($N = 791$)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Model 1: New vs. Old Postcard</th>
<th>Model 2: Effects of Filling in a Reason</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>$-2.299^{***}$ (.782)</td>
<td>$-2.276^{**}$ (.783)</td>
</tr>
<tr>
<td>Gender (high = woman)</td>
<td>.464** (.167)</td>
<td>.463 * (.168)</td>
</tr>
<tr>
<td>Age</td>
<td>-.001 (.014)</td>
<td>-.002 (.014)</td>
</tr>
<tr>
<td>Race/ethnicity (0 = white, 1 = other)</td>
<td>-.321 (.196)</td>
<td>-.321 (.196)</td>
</tr>
<tr>
<td>Strength of partisanship</td>
<td>.193 (.110)</td>
<td>.194 (.110)</td>
</tr>
<tr>
<td>Level of education</td>
<td>.109 (.116)</td>
<td>.112 (.116)</td>
</tr>
<tr>
<td>Mother's education</td>
<td>-.069 (.085)</td>
<td>-.068 (.085)</td>
</tr>
<tr>
<td>Father's education</td>
<td>.030 (.081)</td>
<td>.027 (.082)</td>
</tr>
<tr>
<td>Objective political knowledge</td>
<td>.040 (.075)</td>
<td>.040 (.075)</td>
</tr>
<tr>
<td>Subjective political knowledge</td>
<td>.133 (.164)</td>
<td>.137 (.164)</td>
</tr>
<tr>
<td>Political interest</td>
<td>.218 (.147)</td>
<td>.214 (.147)</td>
</tr>
<tr>
<td>Personal political efficacy</td>
<td>.187 (.141)</td>
<td>.187 (.141)</td>
</tr>
<tr>
<td>Group political efficacy</td>
<td>-.073 (.122)</td>
<td>-.075 (.122)</td>
</tr>
<tr>
<td>Prior civic participation</td>
<td>.160 *** (.046)</td>
<td>.161 *** (.046)</td>
</tr>
<tr>
<td>Survey from the second mailing</td>
<td>-.813 *** (.172)</td>
<td>-.818 *** (.172)</td>
</tr>
<tr>
<td>Rap the Vote pledge card</td>
<td>.449 (.476)</td>
<td>.472 (.479)</td>
</tr>
<tr>
<td>Month signed postcard</td>
<td>-.027 (.072)</td>
<td>-.031 (.072)</td>
</tr>
<tr>
<td>No prompt versus prompt</td>
<td>.682 * (.310)</td>
<td></td>
</tr>
<tr>
<td>Uncompleted prompt versus all others</td>
<td></td>
<td>.811 * (.404)</td>
</tr>
<tr>
<td>Completed prompt versus all others</td>
<td></td>
<td>.676 * (.311)</td>
</tr>
</tbody>
</table>

**Note.**—For the first column, $\chi^2(17) = 90.3$, $p > .0001$; pseudo $R^2 = .09$. For the second column, $\chi^2(18) = 90.6$, $p > .0001$; pseudo $R^2 = .09$.

* $p < .05$.  
** $p < .01$.  
*** $p < .0005$.

we used the corrected self-report index of voting, in which participants were only considered to have voted if they were able to provide a specific location of where they voted. To maximize the number of cases, we replaced missing values on continuous variables with the sample mean. The first model (table was always well within a 95 percent confidence interval around zero. (See Lawrence, Maltzman, and Wahlbeck [1999] for a similar interpretation of the value of rho; see also Berinsky [1999] for an application with a significant rho and Greene [1997] for discussions of bivariate probits in general. Details of these analyses are available from the authors.) Additionally, our findings of the effect of the different postcards do not vary when the analysis is run as an ordered probit or as a multinomial logit.
1, col. 1) includes a dummy variable that compares recipients who received the pledge card with the sentence prompt \((n = 515)\) to those who received a pledge card without the sentence prompt \((n = 297)\). This dummy variable was coded so that 0 represented having a pledge card without the sentence prompt and 1 represented having a pledge card with the sentence prompt.

The second model (table 1, col. 2) checks whether the effect depended upon whether the recipient completed their sentence prompt \((n = 418)\) or failed to complete the sentence \((n = 97)\). This model includes two dummy variables. The first (uncompleted prompt vs. all others) was coded so that 1 represented respondents who received the new version of the postcard, but had not filled out a reason why they would vote, and 0 represented all others. In the second dummy (completed prompt vs. all others), 1 represented having a pledge card with the completed sentence prompt and 0 represented either having a pledge card with the sentence prompt uncompleted, or having a pledge card without the sentence prompt.

In both analyses, we controlled for demographic variables (gender, age, race, education, mother’s education, and father’s education) as well as political-psychological variables (political interest, personal political efficacy, group political efficacy, partisan strength, objective political knowledge, subjective political knowledge, and prior political/civic participation). We also included a dummy variable for whether the respondent sent the survey back after the first or second mailings (0 = first mailing, 1 = second mailing) and a dummy variable for whether recipients received a Hip Hop Coalition pledge card (0 = no, 1 = yes). Finally, we controlled for the month in which recipients completed their pledge card.

As expected, we found that the type of pledge cards people received predicted their likelihood of voting on election day (see table 1). Consistent with our hypothesis, participants whose pledge cards contained the sentence prompt were more likely to vote than participants whose pledge cards did not contain the sentence prompt \((p = .03)\). Moreover, the second column of table 1 indicates that the effect was significant regardless of whether the participant filled out the sentence \((p = .030)\) or left it blank \((p = .045)\).

To illustrate the magnitude of the effect of the two types of pledge cards on voting, we computed the predicted probabilities of voting based on whether recipients with various characteristics received a pledge card with or without the sentence prompt and whether recipients had completed the sentence prompt.\(^6\) The effect of the different types of pledge cards on voting was dramatic. For example, these coefficients indicate that a typical respondent had a 70 percent chance of voting when they received a pledge card without

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6. To generate these probabilities, we set continuous variables to their mean value and set categorical values to their modal value. That is, the probabilities indicate the effect of the new style of postcard as if each version were sent to a female respondent who did not receive a Rap the Vote postcard and who returned her survey in response to the first mailing and who was average in other respects. The coefficients were drawn from table 1, model 1.
the sentence prompt, but had an 83 percent chance of voting when they received a pledge card with the sentence prompt.

It is important to point out that receiving a particular type of pledge card affected probability of voting above and beyond demographic variables such as educational level and race, and political-psychological variables. Once all of the variables were entered into the equation, gender was also a significant predictor such that women were more likely to vote than men, $p = .006$. Of the political-psychological variables, only previous participation in political or civic activities ($p = .001$) was a significant predictor of voting. Finally, those who responded to our second mailing were less likely to vote than those who responded to our first mailing, $p < .001$. It is important to note that neither race ($p = .10$), receiving a Hip Hop Coalition pledge card ($p = .35$), nor the month in which participants completed their pledge card ($p = .72$) had a significant influence on voting, ruling out these factors as potential confounds.

**OFFICIAL VOTING RECORDS**

We then repeated our logit model on the sample of 228 survey respondents for whom we had official voting records, using the official voting records as the dependent measure ($0 =$ did not vote, $1 =$ voted). In this analysis, neither having a pledge card with the sentence prompt ($p = .84$) nor the act of completing the sentence prompt ($p = .32$) were significant predictors of voting. However, the pattern of voting rates among the three pledge card groups was generally consistent with the results found using the corrected self-report measure. Specifically, the proportion of voters among those who received the pledge card that did not contain the sentence prompt was .54, the proportion of those who received the pledge card with the sentence prompt but who failed to complete the prompt was .50, and the proportion of voters among those who both received the pledge card with the sentence prompt and completed the prompt was .62. It may have been the case that the small sample size in this analysis prevented us from finding significant effects of the type of pledge card.7 Additionally, in this analysis, education was a significant predictor of voting, such that less well-educated individuals were less likely to vote ($p = .05$). Finally, as in our previous analysis, those who responded to our second mailing were less likely to vote than those who responded to our first mailing ($p = .007$).

**Discussion**

In line with theories of attitudinal advocacy, our study demonstrated that people who completed and received pledge cards that prompted them to pro-

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7. The sample size was 228. Since there were 17 coefficients and a constant being estimated, there were only about 12 cases per parameter.
vide their own personalized reason to vote were more likely to have actually voted than those who completed and received pledge cards that did not contain the sentence prompt, "I will vote because ___." The success of the personalized message pledge card technique is especially interesting theoretically because it reveals how voting behaviors can be influenced by a feature in the environment, in contrast to other determinants of voting that are features of individuals, such as demographic characteristics (i.e., gender and race) and political/psychological features (i.e., partisanship, previous political participation, and political interest). Arguably the latter characteristics are less mutable because they have been instilled in the individual over time and are themselves largely influenced by factors that are relatively fixed, such as one's socioeconomic status (see Verba, Schlozman, and Brady 1995).

The existence of a group of recipients who received pledge cards with the sentence prompt but who failed to provide a personalized reason for voting provides some insight into the mechanisms underlying the differential effectiveness of the two types of pledge cards. Recall that participants who received pledge cards that contained the sentence prompt, "I will vote because ___." were more likely to vote than participants whose pledge cards did not contain the sentence prompt, even when they failed to complete the prompt. At first glance, these findings seem counterintuitive. Self-perception theory (Bem 1972) might predict that receiving a pledge card in which the sentence prompt is left uncompleted would lead individuals to infer that voting is not all that important to them; consequently, such individuals should be least likely to vote. Similarly, theories of commitment would not predict that individuals who did not bother to complete the sentence prompt on their pledge card would be any more likely to vote than individuals whose pledge cards did not contain the sentence prompt; the former group cannot be said to have made a stronger commitment to voting than the latter.

One theory of attitudinal advocacy that does support this finding is the theory of cognitive dissonance (Festinger 1957). According to this theory, receiving a card with a preprinted message pledging one to vote should arouse less dissonance in individuals who plan to forgo voting than receiving a pledge card with the sentence prompt, even when that pledge card is left uncompleted because the former message can more readily be discounted as the sentiments of someone else.

Our inference that processes of cognitive dissonance may be responsible for the superiority of the pledge card that contained the sentence prompt should be interpreted cautiously, however. First, the failure to complete the sentence prompt was under the control of the participant rather than being the outcome of random assignment. Second, the group of participants who did not complete the sentence prompt was quite small relative to the other two groups. For these reasons, future research should be conducted that attempts to replicate this phenomenon in the context of a true experiment.

Our investigation represents a rare and noteworthy research strategy in that
we teamed up with a national nonprofit organization to examine the effect of a “natural experiment” that they had inadvertently created. Rock the Vote’s 1996 voter registration campaign was an extensive and wide-reaching attempt to increase turnout among younger Americans, an age group that has been shown to vote at significantly lower levels than their elders (Dalton 1996). It is significant that Rock the Vote was able to mobilize more than 1,000 volunteers from college campuses and communities across the country to register and distribute pledge cards to over 80,000 young voters throughout the election year. When we learned of the efforts of Rock the Vote, and specifically of the two types of pledge cards in circulation, we recognized that this was an opportunity to conduct a test of psychological theory in the context of a socially meaningful behavior. Consequently, we capitalized on their large-scale, nationwide intervention by conducting research that was high in external validity. By surveying a relatively large and diverse national sample of pledge card recipients, we made our investigation ecologically valid and were able to demonstrate that self-persuasion processes engendered by the prompt, “I will vote because ____,” could motivate young adults to exercise their right to vote.

Because the intervention was designed and implemented by Rock the Vote prior to our involvement, our research design was necessarily constrained. For example, the distribution of the two types of pledge cards was not random, as evidenced by a comparison of the two groups. However, with the exception of the fact that individuals who received pledge cards with the sentence prompt were more likely to be white, the attributes on which this group differed were negatively associated with voting. Thus, this failure of random assignment should have worked against our hypothesis that receiving a pledge card with the sentence prompt would increase the likelihood of voting. For that reason, despite the threats to internal validity, we feel confident that the effects we found were not due to preexisting differences between the two groups.

We believe that this field investigation, in addition to its relevance to voting behavior, also makes a valuable contribution to persuasion research. First, the use of a range of diverse theories as a rationale for our predictions is in keeping with social psychology’s recent focus on the search for organizing themes among theories (see Higgins and Kruglanski 1996). By demonstrating the effectiveness of self-persuasion processes in an ecologically valid field setting, such as the one provided by Rock the Vote’s get-out-the-vote campaign, we need not worry that these findings are artifacts of particular experimental paradigms or of the laboratory context. As such, this study takes into account a prominent critique of psychological theories that have been tested primarily through laboratory investigations, namely, that the evidence for a particular theory may rest entirely upon the particular experimental paradigm employed by the theory’s proponents (e.g., Eagly 1992; Tedeschi, Schlenker, and Bonoma 1971). Also, we were able to discern our predicted effects despite the presence of the substantial “noise” that is inherent in the
field, such as the effects of the actual campaigns. Thus, we are confident that these theories and findings are robust.

Although our investigation focused on the relative superiority of the pledge cards with personalized reasons for voting, it is also likely that all participants in the Rock the Vote campaign who completed a pledge card voted at higher rates than individuals who did not participate in the campaign. Because we only surveyed individuals who actually participated in the pledge card campaign, our methodology did not allow us to test the overall influence of the pledge cards. However, past research examining the influences on voting can inform this hypothesis. For example, the National Election Studies have documented that the very act of being contacted and interviewed about one’s political views prior to an election stimulates participation in that election (Clausen 1968; Traugott and Katosh 1979). This “interviewer effect” may reflect a more general phenomenon and indicate that any personal contact with a potential voter prior to an election will increase the likelihood that the individual will participate in the election. In fact, political campaigns often attempt to capitalize on this phenomenon by contacting likely voters in the weeks and days before an election. Interestingly, and more closely related to the effect of Rock the Vote’s efforts, about 70 years ago, Gosnell (1927) conducted an experiment to determine whether mailing reminder postcards to likely voters would increase the likelihood that they would register and vote in the election. This is exactly what he found. Finding an effect for providing a personalized reason for voting, above and beyond the effect of simply receiving a pledge card in the mail, is all the more impressive given that the voting rates of these individuals may have already been “boosted.”

We have provided evidence that the positive effect of personal contact on encouraging a behavior may be enhanced if individuals are simply asked to generate meaningful reasons for that behavior. The effectiveness of a relatively simple mobilization technique to influence behavior such as voting is all the more impressive, given that this strategy is short-term and consists essentially of a “two-shot” deal—pledging to vote while providing a personal reason to do so, and being reminded of that pledge and personalized reason at a later date. Given the importance of mobilization for political participation more generally, and given that younger Americans are voting at lower rates than previous generations despite possessing higher levels of education, it is quite remarkable that such a minimal mobilization effort could result in effects strong enough to add significantly to the effects of the variables studied in traditional voting research.
Appendix A

A Correction Technique Designed to Circumvent Problems with Self-Report Measures of Voting

Because of the many difficulties in assessing voting behaviors through self-report, our survey also included an additional question designed to correct for the likelihood of overreporting voting behavior. Specifically, a great deal of research has found that simply asking people whether they voted often results in an overestimation of voting, compared to official voting records (Abelson, Loftus, and Greenwald 1992; Presser 1990; Silver, Anderson, and Abramson 1986). Validation studies conducted by the University of Michigan's Survey Research Center from 1964 to 1980 have revealed that 20–30 percent of citizens falsely report that they voted when they actually had not (Silver, Anderson, and Abramson 1986). This well-documented overreporting bias is problematic in two respects. First, this bias obviously inflates the reported levels of voting. More important for our purposes, this bias may influence which psychological and demographic factors are found to predict voting in statistical models. Specifically, there is evidence that predictors of voting are different when voting is operationalized by self-report as opposed to official voting records (e.g., Presser 1984; Presser and Traugott 1992). This may be because factors positively associated with voting have also been found to be positively associated with overreporting. Specifically, the factors that predict voting, such as higher levels of education and support for the norm of voting, also predict overreporting (e.g., Silver, Anderson, and Abramson 1986).

It is interesting that respondents' race is an exception to the finding. Although some research has shown that blacks are less likely to vote than whites, blacks appear to be more likely to overreport voting than whites (Abramson and Claggett 1984, 1986, 1992; Traugott and Katosh 1979). Finally, of particular relevance to our project, there is some evidence that younger voters are more likely to overreport than older voters (Traugott and Katosh 1979), although this evidence is equivocal (Silver, Anderson, and Abramson 1986).

Although efforts to prevent overreporting when the survey is administered have been unsuccessful, to our knowledge attempts to correct for erroneous self-reporting after the fact have remained unexamined. In an attempt to correct for overreporting, Lavine and Snyder (1996) asked survey respondents not only whether they voted, but where they voted. They reasoned that, among individuals who claimed to have voted, those who were able to provide the specific location of their polling place would be more likely to have actually voted than participants who were not able to do so. Since we anticipated an overreporting bias with our national sample, we decided to use Lavine and Snyder's (1996) screening question. This technique was designed to allow us to correct for overreporting. Instead of relying solely on respondents' self-reports or on the cooperation of hundreds of public officials, we coded respondents as voters only if they could provide us with the specific location of their polling place. This technique is similar to the question wording proposed by Belli et al. (1999) in that it uses respondents' memories to improve the accuracy of self-reported voting behavior. However, it differs in that it is meant as a correction of self-reports, whereas Belli et al. seek to improve the self-report itself.

8. This technique is similar to the question wording proposed by Belli et al. (1999) in that it uses respondents' memories to improve the accuracy of self-reported voting behavior. However, it differs in that it is meant as a correction of self-reports, whereas Belli et al. seek to improve the self-report itself.
correction may be particularly potent for younger, first-time voters. Whereas older voters may recall the location of their polling place from past experience, younger voters who report a specific polling place are more likely to be speaking from direct experience. Validation of this technique is presented in appendix B.

Appendix B

Validation of the Corrected Measure of Self-Reported Voting Behaviors

Voting Records Verification

To validate our proposed correction technique, we contacted a sample of secretaries of state and boards of elections across the country to assess survey respondents' actual voting behaviors. We were able to obtain official voting records for 24 percent \((n = 234)\) of the 957 survey respondents. During this process, we realized that we would not be able to verify whether 144 respondents had voted because they claimed to have done so via absentee ballot. The address we had for them was the one they had written on their pledge cards, but not necessarily the county or city in which they voted, and thus it could not be determined whether these respondents were actual voters or nonvoters. Consequently, we removed these respondents from the sample. In addition, four individuals were too young to vote, and three more respondents did not report whether they had voted. In sum, we removed 151 respondents from our validation analyses.

Test of the Correction Technique for Overreporting

Consistent with previous literature, we expected that survey respondents would overreport their voting behavior. For this reason, we began by comparing respondents' self-reports to the official voting records for the 234 survey respondents for whom we had this information. In response to the question, "For a variety of reasons some people did not vote in the 1996 presidential election. What about you?" 83 percent \((n = 195)\) of these survey respondents indicated that they had voted. However, only 58 percent \((n = 136)\) of these respondents actually voted according to official records; the official voting records showed that 72 percent of the respondents voted. Thus, it appeared that, consistent with the literature on overreporting, more people in our sample reported having voted than the official voting records suggested.\(^9\) In fact, only two \((.9\) percent\) respondents who claimed that they had not voted appeared on official records as voters.

As stated earlier, this overreporting bias could cause researchers to find different predictors of voting, depending on whether voting is operationalized via official voting records.

\(^9\) This overreporting may be the result of a number of different factors. For example, as noted earlier, younger voters are more likely to overreport than older voters (Traugott and Katosh 1979). It is also possible that these voters were more likely to overreport because they had participated in a get-out-the-vote campaign and were aware that Rock the Vote, the sponsor of the campaign, would see the results of the survey.
Personalized Messages and Voting

records or via self-report. Using self-report data was preferable to using official voting records because the sample of respondents for whom we had official voting records ($n = 203$) constituted only 29 percent of the sample ($n = 806$). However, we also wanted to use a measure of voting that was as similar to the official voting records as possible. Thus, we constructed a “corrected” self-report measure using the correction technique described earlier. Specifically, individuals were considered to have actually voted only if they were able to give us a specific location of where they had voted; individuals who said they had voted but were not able to provide us with a specific location were considered not to have voted. According to this corrected self-report measure, 67 percent ($n = 542$) of the 806 survey respondents voted.

Before using this corrected self-report measure in our analyses, we needed to ensure that it was more similar to the official voting records than the self-report measure alone. We then conducted the following analyses to examine the reliability of the corrected self-report measure. We reasoned that to the extent that the predictors of overreporting using the corrected self-report measure were the same as the predictors of overreporting using official voting records, we could be confident that our corrected measure was an accurate proxy for official voting records. In order to examine whether these predictors were similar, we conducted two sets of $t$-tests on each of our demographic and political-psychological variables. In one set we used official voting records as the criterion variable to assess who had overreported. In another set we used the corrected self-report measure as the criterion variable to assess who had overreported. In both instances, we compared overreporters (i.e., incorrect nonvoters) to individuals who correctly reported not voting (i.e., correct nonvoters). 10

We conducted the first set of $t$-tests using the official voting records as the criterion variable. As may be seen in the first two columns of table B1, compared to correct nonvoters ($n = 37$), overreporters ($n = 61$) tended to have higher levels of education, were more likely to have participated in political activities in the past, and were more likely to be interested in politics. Overreporters were also more likely to be higher in personal political efficacy, group political efficacy, and subjective political knowledge. These predictors are consistent with the literature, which has shown that the characteristics that predict voting, such as education, political efficacy, stronger partisan strength, and more interest in politics, are also more likely to predict overreporting of voting (Silver, Anderson, and Abramson 1986). Moreover, also consistent with the literature (e.g., Abravam and Claggett 1986), overreporters in our study were more likely to be nonwhite.

We then repeated the analyses using the corrected self-report measure as the criterion variable, and essentially found the same results for education, past participation in civic activities, interest in politics, personal political efficacy, and subjective political knowledge. The only difference was in partisan strength, which showed slightly larger

10. The reader may wonder why we compared overreporters only to correct nonvoters, rather than to all individuals who correctly reported their voting behavior (i.e., the population of correct nonvoters and correct voters). The literature, and our official voting records, reveal that virtually all misreporting consists of individuals claiming that they had voted when they had not (Silver, Anderson, and Abramson 1986). Comparing overreporters to the population of correct nonvoters and correct voters would then confound the predictors of voting with the predictors of overreporting. For this reason, the proper comparison for assessing the predictors of misreporting is between individuals who correctly reported not voting and individuals who incorrectly reported voting (Silver, Anderson, and Abramson 1986).
### Table B1. Mean Predictors of Overreporting Using Corrected Self-Report Measure and Official Voting Records to Assess Voting

<table>
<thead>
<tr>
<th>Variable</th>
<th>Official Records Correct</th>
<th>Official Records Overreport</th>
<th>Correction Index Correct</th>
<th>Correction Index Overreport</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>b (SE)</td>
<td>b (SE)</td>
<td>b (SE)</td>
<td>b (SE)</td>
</tr>
<tr>
<td>Race/ethnicity</td>
<td>.19 (.40)</td>
<td>.47** (.50)</td>
<td>.21 (1.16)</td>
<td>.38** (1.29)</td>
</tr>
<tr>
<td>Level of education</td>
<td>2.97 (.58)</td>
<td>3.41** (.69)</td>
<td>3.00 (.65)</td>
<td>3.25** (.81)</td>
</tr>
<tr>
<td>Mother’s education</td>
<td>3.12 (1.07)</td>
<td>3.16 (1.22)</td>
<td>3.18 (1.14)</td>
<td>3.02 (1.21)</td>
</tr>
<tr>
<td>Father’s education</td>
<td>3.39 (1.20)</td>
<td>3.39 (1.36)</td>
<td>3.37 (1.25)</td>
<td>3.26 (1.44)</td>
</tr>
<tr>
<td>Prior civic participation</td>
<td>1.62 (1.62)</td>
<td>3.28*** (1.92)</td>
<td>2.34 (2.02)</td>
<td>2.94** (1.88)</td>
</tr>
<tr>
<td>Political interest</td>
<td>2.59 (.64)</td>
<td>3.07** (.66)</td>
<td>2.75 (.69)</td>
<td>3.07*** (.67)</td>
</tr>
<tr>
<td>Personal political efficacy</td>
<td>2.17 (.75)</td>
<td>2.57** (.65)</td>
<td>2.19 (.75)</td>
<td>2.42* (.71)</td>
</tr>
<tr>
<td>Group political efficacy</td>
<td>2.50 (.77)</td>
<td>2.85* (.80)</td>
<td>2.42 (.81)</td>
<td>2.59 (.85)</td>
</tr>
<tr>
<td>Objective political knowledge</td>
<td>3.70 (1.41)</td>
<td>3.85 (1.25)</td>
<td>3.80 (1.32)</td>
<td>3.82 (1.38)</td>
</tr>
<tr>
<td>Subjective political knowledge</td>
<td>2.49 (.65)</td>
<td>2.97*** (.58)</td>
<td>2.73 (.67)</td>
<td>2.92* (.57)</td>
</tr>
<tr>
<td>Strength of partisanship</td>
<td>.86 (.80)</td>
<td>1.11 (.78)</td>
<td>.72 (.72)</td>
<td>1.04** (.73)</td>
</tr>
</tbody>
</table>

**NOTE.** - *P*-values represent *t*-tests between nonvoters who accurately reported not voting and nonvoters who reported voting, using the correction index of voting and official voting records. Standard deviations are in parentheses.

* *p < .05.
** *p < .01.
*** *p < .0005.

Differences among those for whom we could only use the correction index (the difference was .25 among those with official voting records and .31 among those classified only by self-report and the correction index). Finally, the correction also showed that overreporters were more likely to be nonwhite than white. Because the corrected self-report measure predicted the same characteristics of overreporters as official voting data, and because these are the same general characteristics that have been found in the literature to predict overreporting, we felt confident in the validity of our corrected self-report measure as a proxy for official voting records. In the case of the present research, using the corrected self-report measure rather than the official voting records enabled us to use a larger number of respondents in our analyses while circumventing problems associated with the uncorrected self-report measure.

Survey researchers may be interested in our demonstration of how a correction index of behavior can be effectively designed to cope with the problem of overreporting in a survey design when people are motivated to inaccurately portray themselves as having engaged in a behavior and when it is not economically, ethically, or practically feasible to collect behavioral measures. This technique of administering a question...
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designed to reduce false positives after the fact and then validating the effectiveness of the corrected self-report measure could potentially be used in improving the validity of self-report measures of behaviors within a variety of domains.

References


