JOURNAL OF CONSUMER PSYCHOLOGY, 8(1), 61–89

Social Influence by Requesting Self-Prophecy

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Asking people to predict whether they will perform a target action often increases the probability of their performing that action. This article reviews published and unpublished research evidence for this "self-prophecy" phenomenon and reports 2 new experiments. The studies reviewed demonstrate that the self-prophecy effect occurs in a variety of situations and that it is a moderate-size effect. The new experiments introduce a 1-session procedure that is considerably more efficient in testing theory than the 2-session procedure of previous experiments. In the prior studies, as in the present self-prophecy studies, participants appear to reduce a discrepancy between their principles and their behavior, made salient by prediction, through changing the behavior. Toward the ends of encouraging future investigation and developing theoretical understanding of the effect, the article concludes with discussion of related programs of research that may provide theoretical explanations for the effect.

People overstate their likelihood of performing socially desirable actions for which they are asked to make predictions. Although this is not surprising, it is remarkable that once predicted, the action is more likely to occur. In other words, the prediction becomes a self-fulfilling prophecy. As a compelling example, Sherman (1980) seminally demonstrated this phenomenon's use to increase the rate of students at Indiana University volunteering to do charitable work. Sherman labeled this type of effect the *self-erasing nature of errors of prediction*, because (a) participants predicted that they would perform the action with greater probability than was observed in a no-prediction control group (in this sense, the predictions were in error)

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but (b) increased subsequent performance of the action, making the apparent overprediction of behavior not (or less) erroneous (in this sense the error was self-erasing). We use *self-prophecy effect* as a shorthand label for the phenomenon.

Greenwald, Carnot, Beach, and Young (1987) observed that "the [self-prophecy] influence technique is remarkably simple: It involves asking people to predict whether they will perform the target action" (p. 315). Although exploration of the self-prophecy effect since Sherman's publication in 1980 has been modest, both (a) the effect sizes found in published and unpublished experimental tests and (b) the variety of contexts in which the effect has been observed are compelling. This article provides a meta-analytic review of published and unpublished self-prophecy research and reports new research using a one-session experimental procedure that is considerably more efficient for theory testing than the two-session format used in all previous self-prophecy work. Toward the end of developing parsimonious interpretation, the article concludes with discussion of potential theoretical explanations for the effect.

META-ANALYTIC REVIEW OF EXISTING STUDIES

A summary of each of the self-prophecy tests the authors could locate and relevant meta-analytic statistics are shown in Table 1. All results were drawn from published reports or raw data provided by the original investigators. To confirm reported results for each study included in the meta-analysis, these authors reconducted all analyses of self-prophecy effects.

Sherman (1980) introduced the self-prophecy effect with three experiments. Depending on the context, the prediction request had the ability to decrease (i.e., singing over the phone and writing a counterattitudinal essay) as well as to increase (i.e., volunteering for charity work) the probability of the predicted action. In all three of Sherman's original experiments, having (mis)predicted a given behavior, participants were likely to confirm their predictions in subsequent behavior—the errors were thus self-erasing.

In seeking to determine whether the self-prophecy effect could be consequential in an important nonlaboratory setting, several tests were conducted in relation to public elections. Greenwald et al. (1987) reported a directionally consistent but not statistically significant self-prophecy effect for Ohio State University dormitory residents contacted by telephone with respect to registering to vote, and a significant self-prophecy effect regarding actual voting behavior. Initial contact was made 1 or 2 days prior to the behavioral opportunity; the naturally occurring dependent measures (using official precinct and poll records) regarded the 1984 U.S. presidential election.

Using procedures similar to those of Greenwald et al. (1987), two studies were conducted by Greenwald, Klinger, Vande Kamp, and Kerr (1988) in relation to (a)

voting in a closely contested 1986 U.S. Senate race and (b) voting in a relatively unimportant 1987 State House of Representatives election. Because these studies are unpublished, more detail is included herein than for the published self-prophecy studies.

In the first Greenwald et al. (1988) study, socioeconomically diverse participants selected from registered voters in the city of Seattle, Washington, were randomly assigned to one of four conditions: (a) Prediction only: "Do you predict that you will vote or not vote?"; (b) Preference plus prediction: "Whom do you prefer [then mentioning the Senate candidates' names]?", followed by prediction request; (c) Preference only: The preference question only; and (d) Postelection control: These participants were contacted on the Sunday or Monday 5 or 6 days after the Tuesday election to provide a group of controls who could be contacted by phone, a criterion for participation in the other conditions. Combined turnout rate in the two conditions not requesting predictions was 87.1%, which was not significantly different from the rate of 86.3% in the two conditions that requested predictions. Thus, failure to find a self-prophecy effect in the first of these two studies is convincingly explained by the very high turnout level creating a ceiling effect, leaving little opportunity to observe treatment effects.

In the second unpublished Greenwald et al. (1988) study, participants were randomly selected from registered voters in the state of Washington's 43rd legislative district. Procedures were similar to those of the first experiment, with assignment of participants to one of four conditions: (a) Knowledge only: "Do you know the location of the polling place in your precinct?"; (b) Knowledge plus prediction: After the knowledge question as above, the caller asked, "Do you predict that you will vote or not vote in the primary election tomorrow?"; (c) Knowledge plus prediction plus reason: After the knowledge and prediction questions, the caller continued, "What would you say is the most important single reason for voting in the primary?"; and (d) Contact only: "Can you tell me the outcome of today's football game involving the Seattle Seahawks?" For the comparison of control and prediction conditions, the test of significance for the difference in turnout yielded $\chi^2(1, N = 346) = 2.74$, $p = .09$. As shown in Figure 1, the magnitude of the self-prophecy effect varied as a function of participants' prior voting record, $F(2, 402) = 3.10$, $p < .05$, for the interaction of Treatment × Three Levels of Prior Voting Record. The self-prophecy effect was statistically significant only for the moderate-prior-turnout group, $\chi^2(1, N = 100) = 5.96$, $p = .015$. Thus, in this experiment, the self-prophecy manipulation brought turnout of moderate-prior-turnout voters up to a level indistinguishable from that of high-prior-turnout voters.

Following Sherman's (1980) suggestion that "[the self-prophecy phenomenon] should have intriguing implications for applied work in the areas of consumer behavior, psychotherapy, decision making, and education" (p. 219), Spangenberg and colleagues explored various normative applications of the self-prophecy technique. Spangenberg and Obermiller (1996) demonstrated a statistically significant

TABLE 1
Meta-Analysis of All Known Self-Prophecy Studies

Study	*Prediction Task*	*Study N*[a]	*Summary Statistics*[b]	*Effect Size (r)*	*Effect Size as Fisher's* Z_r[c]	*95% CI*[d] *Lower*	*95% CI*[d] *Upper*
Sherman (1980, Expt. 1)	Agreeing to write a counterattitudinal essay.	36	Control: 67%; Prediction: (29%) 33%	.329	.342	0.00	0.59
Sherman (1980, Expt. 2)	Singing the *Star-Spangled Banner* over the telephone.	52	Control: 68%; Prediction: (44%) 42%	.300	.310	0.03	0.53
Sherman (1980, Expt. 3)	Volunteering to work 3 hr to collect money for the American Cancer Society.	91	Control: 4%; Prediction: (48%) 31%	.352	.367	0.16	0.52
Greenwald, Carnot, Beach, & Young (1987, Expt. 1)	Registering to vote prior to 1984 U.S. presidential election.	46	Control: 9%; Prediction: (71%) 21%	.163	.165	–0.13	0.43
Greenwald, Carnot, Beach, & Young (1987, Expt. 2)	Voting in 1984 U.S. presidential election.	56	Control: 62%; Prediction: (100%) 87%	.289	.298	0.03	0.51
Greenwald, Klinger, Vande Kamp, & Kerr (1988, Expt. 1)	Voting in 1986 Seattle general election—a closely contested U.S. Senate race.	1,139	Control: 87%; Prediction: (100%) 86%	.011	.011	–0.05	0.07
Greenwald, Klinger, Vande Kamp, & Kerr (1988, Expt. 2)[e]	Voting in 1986 Seattle primary election for Democratic seat in the state House of Representatives.	346	Control: 35%; Prediction: (83%) 44%	.089	.089	–0.02	0.19

Obermiller, Spangenberg, & Atwood (1992)	Making a donation pledge in a college fund-raising campaign.	390	Control: 46%; Prediction: (53%) 46%	−.007	−.007	−0.11	0.09
Spangenberg (1997)							
Short term	Using health club in the next week.	142	Control: 7%; Prediction: (61%) 14%	.089	.089	−0.06	0.26
Long term	Using health club over 6 months.	95	Visits: Control = 5.1; Prediction = 10.25[f]	.198	.200	−0.00	0.38
Spangenberg & Obermiller (1996)	Refraining from cheating on a take-home quiz.	81	Control: 29%; Prediction: (59%) 51%	.232	.236	0.01	0.43
			Overall values[g] (11 studies)	.130	.129	−0.51	0.68
			Overall values (subset of 9 studies)[h]	.205	.208	−0.53	0.77

Note. CI = confidence interval; Expt. = experiment.

[a]Sample sizes in all studies were approximately equal for control and prediction conditions. [b]The data summary lists the percentage of participants performing the target action in a control (no-prediction) condition; then (in parentheses) the percentage of participants in a prediction condition who predicted they would perform the action; and last, the percentage of all prediction-condition participants who performed the target action when they were subsequently given the opportunity. [c]Fisher's (1928) transformation accounts for increasing skewness of the distribution of sampled *r*s as the population value of *r* gets further from zero. This nearly normally distributed transformation addresses complications of comparing and combining *r*s. The relation between *r* and Z_r is given by: $Z_r = \frac{1}{2} \times \ln[(1 + r)/(1 - r)]$. [d]Calculated as an interval around *r* using Fisher's Z_r transformation. [e]Persons who had voted in previous year's (1986) primary election were omitted based on the expectation that they had a high likelihood of voting in the absence of experimental influences. Summary statistics reported are for overall data; however, the magnitude of this self-prophecy effect varied significantly as a function of participants' prior voting record, $F(2, 402) = 3.10$, $p < .05$, for the interaction of treatment by three levels of prior voting record. Effect sizes were $Z_r = .044$, $Z_r = .249$, and $Z_r = .017$, respectively, for low-, moderate-, and high-prior-turnout voters. The self-prophecy effect was statistically significant only for the moderate prior turnout group, $\chi^2(1, N = 100) = 5.96$, $p = .015$. [f]The difference between the average number of visits between the control (5.1) and prediction (10.25) conditions was significant, $F(1, 93) = 3.78$, $p = .05$. [g]Effect sizes were weighted by the square root of *N* to calculate mean values of *r*. [h]Subset of 9 of the 11 total studies omitted Greenwald et al. (1988), Expt. 1, and Obermiller et al. (1992) for theoretical and empirical reasons discussed in text.

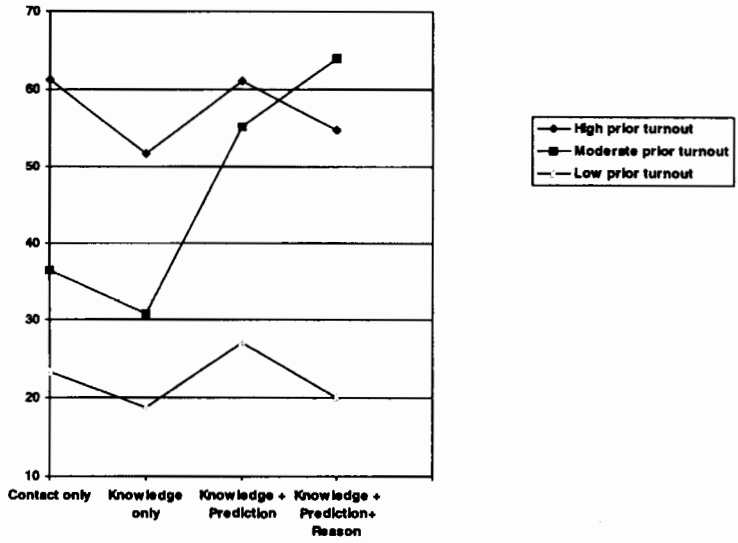

FIGURE 1 Relation between prior voting record and self-prediction manipulation on voter turnout in September 1987 Democratic Party primary contest for seat in Washington State House of Representatives (from Greenwald et al., 1988, Experiment 2).

self-prophecy effect in reducing cheating behavior of college undergraduates. Of students not asked to make a prediction, less than a third avoided the temptation to cheat on a take-home exam. Of those asked to predict, a majority said that they would resist the temptation to cheat; over half of this latter group subsequently did behave honestly when provided the opportunity to cheat. Spangenberg (1997) demonstrated the self-prophecy technique's ability to increase attendance at a health club for members that had not used club facilities for at least a month prior to experimental contact. Although not statistically significant, there was a self-prophecy effect in the expected direction in the 10 days after initial contact. Importantly, however, this study was the first demonstration of the self-prophecy effect over an extended time period. In the 6-month period following initial contact, participants making a prediction used their club at double the rate of control participants.

Obermiller, Spangenberg, and Atwood (1992) examined a direct fundraising appeal using a self-prophecy technique: Would university alumni overpredict their response to a request for a donation pledge, perhaps resulting in increased giving in a subsequent campaign? In their failure to find a statistically significant self-prophecy effect in this context, these authors provided post hoc empirical evidence supporting their suggestion that reactance (Brehm, 1966) may have overpowered the self-prophecy manipulation. Because the person who requested the self-prediction of donation was identified with the university making the pledge

drive, participants appeared to have regarded the prediction request as an attempt to manipulate them into making a donation.

Meta-Analysis Results

As evidenced by the small number of known self-prophecy studies, relatively few investigators have followed Sherman's (1980) lead in conducting research on this novel social-influence phenomenon. However, meta-analysis confirms the consistency of occurrence and size of the self-prophecy effect across existing studies. Chi-square analysis indicated that the set of 11 known studies included in Table 1 was homogeneous with regard to effect size, $\chi^2(10, N = 11) = 2.31, p = .99$. Effect sizes were also homogeneous for a subset of 9 studies after omitting the 2 studies (with relatively large sample sizes) whose authors provided sound methodological and theoretical explanations for their failure to find significant self-prophecy effects, $\chi^2(8, N = 9) = .94, p = .99$ (i.e., Greenwald et al., 1988, Study 1, indicated that a ceiling effect left little opportunity to observe treatment effects; and Obermiller et al., 1992, reported post hoc empirical evidence confirming their reactance explanation for failure to find a treatment effect). A critical ratio (z) test for difference of the weighted mean effect size ($r = .130$) from 0 was significant for the 11 studies, $z = 3.56, p = 10^{-3}$, as well as for the reduced set of 9 studies ($r = .205$), $z = 5.42, p = 10^{-7}$. Overall comparison also indicated that p values were homogeneous for the 11 studies, $\chi^2(10, N = 11) = 3.14, p = .98$, and for the subset of 9 studies, $\chi^2(8, N = 9) = 3.72$, $p = .88$. The combined p obtained for the 11 studies supports the results of the majority of the individual studies; for 11 studies, $z = 5.43, p = 10^{-7}$, and for the 9-study subset, $z = 5.92$, $p = 10^{-8}$. Of importance, the average effect size for the known self-prophecy studies is between Cohen's (1988) small ($r = .1$) and medium ($r = .3$) values, indicating an effect that could be of substantial practical significance in large-scale applications.

Tentative Generalizations About the Self-Prophecy Effect

Virtually all demonstrations of the self-prophecy effect have been in field settings for which alternative explanations in terms of laboratory social artifacts do not arise. Most of the demonstrations have involved behaviors that occur naturally (i.e., voting, charitable contribution, cheating, health club use), but some involved quite unusual behavior (i.e., singing over the telephone and writing a counterattitudinal essay). The experiments that were clearly unsuccessful in obtaining self-prophecy effects are, unfortunately, only modestly informative about the effect's limiting conditions.

The wide variety of situations that have produced the self-prophecy effect and the relatively few failures to find it create some confidence that the effect is robust enough to be of practical significance. Nevertheless, the research conducted to date has neither established empirical conditions on which the self-prophecy effect depends nor converged on a theoretical interpretation of the effect. Theory-relevant generalizations that appear to be supported by characteristics of situations in which the effect has been demonstrated are that (a) the target action for the self-prophecy has most often been a socially desirable action, and (b) the self-prophecy effect has been observed chiefly in situations for which participants cannot make confident self-predictions based on past experience.

Social desirability of target actions. Registering to vote and voting (Greenwald et al., 1987; Greenwald et al., 1988), volunteering to do charitable work (Sherman, 1980), resisting the temptation to cheat (Spangenberg & Obermiller, 1996), and exercising at a health club (Spangenberg, 1997) all appear to be socially desirable behaviors for which one can expect overprediction of own performance. Two other demonstrations of the self-prophecy effect—Sherman's (1980) finding of the effect with target actions of declining to sing over the telephone and declining to write a counterattitudinal essay—on reflection also appear to have this same underlying component. The target actions are unconventional behaviors unlikely to be encountered in everyday life: Choosing to write an essay in favor of an opinion you do not espouse is cognitively incongruous and socially undesirable; likewise, making a fool of yourself by singing over the phone is socially undesirable and embarrassing. Thus, existing evidence suggests that social desirability is a necessary condition for the effect.

Nonconfident self-predictions. In most studies in which the self-prophecy effect occurred, the target action was either completely unfamiliar to participants (e.g., Sherman, 1980, singing on the phone and writing a counterattitudinal essay; Greenwald et al., 1987, registering to vote for the first time and first-time voting) or was one for which participants' past performance could not provide a basis for confident self-prediction (e.g., Spangenberg, 1997, exercising at a health club for infrequent club users; Greenwald et al., 1988, voting in a primary election by moderate-prior-turnout voters). The null findings appear to be in situations for which participants had enough past experience to predict their future performance more confidently. For example, Obermiller et al.'s (1992) participants had all previously experienced requests for donation from the annual business school fund-raising campaign and accurately predicted their response to an upcoming request to contribute. Indeed, of respondents predicting that they would not give, 93% did not contribute in response to the later request. Perhaps the most informative failure to produce the self-prophecy effect was Greenwald et al.'s (1988) null finding for low-prior-turnout voters in the study conducted in a low-turnout primary election.

When asked to predict their intentions regarding the upcoming election, these voters may well have known from past experience that they would not vote. Interestingly, a majority of these low-prior-turnout voters predicted that they would vote (75%), even though only a small proportion of the 75% did vote. A substantial fraction of these participants apparently succumbed to the social desirability pressure to declare that they would vote, while apparently knowing that this was, indeed, unlikely. Thus, existing empirical evidence suggests a potential role of past experience in making confident behavioral self-predictions.

Need for an Efficient Research Model

One obvious reason for slow progress in establishing empirical limiting conditions or theoretical interpretations regarding the effect is the difficulty of conducting self-prophecy experiments. All demonstrations of the effect have involved either a two-session laboratory procedure or effortful field-experimental procedures in which the experimental induction and observation of its effects are separated in time. The election experiments are seemingly efficient because large numbers of participants can be called easily, and the induction can precede the opportunity to vote by only a few days. However, it typically requires a few months before electronic voting records can be accessed to assess the dependent measure, and elections necessarily occur infrequently. Spangenberg's (1997) health club study collected the dependent measure over a 6-month interval. These procedures are obviously inefficient for testing theory. Therefore, a major aim of the present research was to develop an efficient research model of the self-prophecy effect. The aims were to demonstrate the effect in a single-session group-administered laboratory experiment and to illustrate the usefulness of the one-session procedure for testing theoretical interpretations.

PRELIMINARY EXPERIMENT

An initial experiment was conducted as a laboratory project in an undergraduate research methods course at the University of Washington. The experiment used a method for observing implicit gender stereotypes based on Banaji and Greenwald's (1995) demonstration that errors in judging fame of male and female names reveal an implicit stereotype that associates male (more than female) gender with fame-deserving achievement. The task used in the preliminary experiment was for participants to complete the first names of 48 stimuli, each consisting of the first initial and complete last name of a famous person. Participants were instructed to guess the first name when they could not confidently retrieve it from memory. Data were scored by classifying erroneous guesses as male or female first names. A pre-

vious use of this task showed that both male and female participants typically produce a preponderance of male-name errors. The self-prophecy manipulation in the preliminary experiment consisted of asking participants to predict how they would respond when they had to guess: Would they be more likely to guess a male name, a female name, or the two equally?

Name-Generation Task

All participants received a list of 48 names on a single sheet, each presented in the form of a first initial followed by blank underline, then a last name. The names were selected from a larger group that had been pretested for recognizability in a sample of University of Washington students. The selected names were ones for which the nature of fame-deserving achievement (politics, sports, entertainment, or literature) was correctly identifiable by approximately a quarter of the pretest sample. Half of the names used in the preliminary experiment were female and half were male, although this aspect of the list's structure was not mentioned in the instructions. The instructions did explain that when the name did not seem recognizable, often a guess as to the first name would nevertheless be correct. Instructions also requested participants to provide a first name for all 48 stimuli, explaining to participants that their data would not be usable if any names were omitted.

Procedure

Participants were 44 female and 39 male undergraduates who volunteered in exchange for extra credit in their introductory psychology course. They were randomly assigned to control and self-prophecy treatments. Control participants performed the task as described in the preceding paragraph. The self-prophecy condition included two variations from the control procedure: (a) After the instructions for the name-generation task, participants were asked to predict, by choosing among three alternatives, how they would respond when they did not know the correct name—would they be more likely to guess male names, female names, or both equally? and (b) they received an additional instruction to circle the letter *m* or *f* to the left of each name as they responded to it, to indicate the gender of the name they had guessed. This second component of the experimental treatment meant that any observed self-prophecy effect would be confounded with a possible effect of the added gender-identification task.

Results

The name-generation task was scored to indicate the relative likelihood of generating male and female names when the correct famous name was not guessed. The index used was the number of female-name errors minus the number of male-name

errors, divided by the total number of errors that were unambiguously classifiable by gender. On this measure, a positive score indicates a preponderance of female-name errors.[1] This error/gender measure was analyzed in a design with experimental treatment and participant gender as between-participants factors, and gender of famous names as a within-participants factor. Six participants (3 in each condition; 4 female and 2 male) who had more than 75% correct name completions (compared to under 30% as a mean for the entire sample) were dropped from this analysis, because a priori they had little opportunity to reveal any treatment effects. The analysis showed effects of name gender (relatively more erroneous female names generated when the famous name was actually that of a female), $F(1, 73) = 33.60$, $p < .0005$, and participant gender (relatively more erroneous female names produced by female than male participants), $F(1, 73) = 8.31$, $p = .005$. Neither of these effects (name gender or participant gender) interacted with the effect of experimental treatment, which resulted in relatively more erroneous female names in the experimental condition than in the control condition, $F(1,73) = 9.64$, $p = .003$. The findings are shown in Figure 2.

Discussion

Results of the preliminary experiment are encouraging in indicating that a one-session procedure can be used to produce a self-prophecy effect. Further, the effect was a socially significant one, of reducing the expression of a stereotype. The major limitation of the preliminary study was the confounding of the self-prophecy manipulation with a procedure that obliged participants to attend to the gender of names as they were generating them. In Experiment 1, the two procedures are examined as independent factors.

OVERVIEW OF MAIN EXPERIMENTS

Experiment 1, as the follow-up of the preliminary experiment, virtually designed itself and was required to determine whether the preliminary experiment's result could be attributed to an effect of the self-prophecy manipulation. The two confounding procedures of the preliminary experiment were separated into orthogonal design factors. The factors were labeled *self-prophecy* and *gender monitoring* (regarding participants' classifying name gender while generating names).

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[1]As an illustration: A participant making 5 female-name errors (a female-name error occurs, for example, when the line with J. Carson, which would accurately be completed as "Johnny," is erroneously completed as "Judy") and 10 male-name errors would have a gender/error index of –0.33. This index is calculated by taking the difference between female- and male-name errors and dividing by the total number of errors (i.e., [5-10]/15 = –0.33).

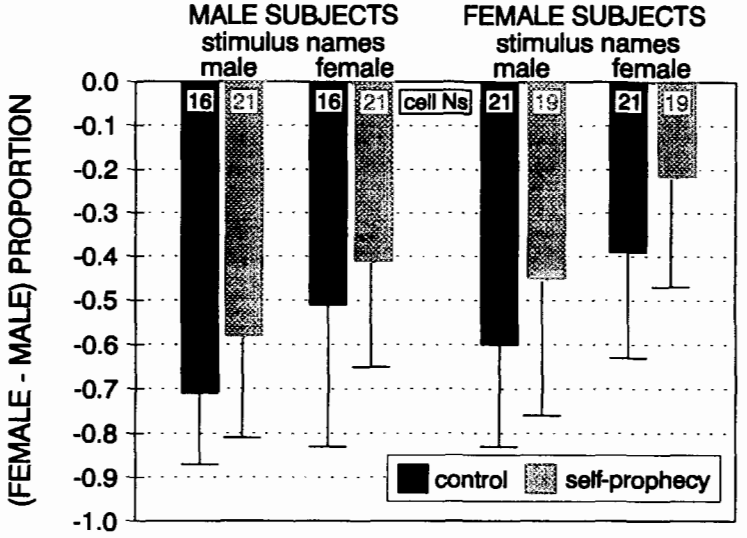

FIGURE 2 Preliminary test of the self-prophecy effect with famous-name-completion-task. The dependent variable is the proportion of unambiguously gender-classifiable erroneous famous names that were female minus the proportion that were male. Error bars show +1 *SD*. Magnitude of the self-prophecy effect (in *d* units) can be read from this and later graphs by comparing the height difference between adjacent bars to the height of the error bars. (If the two were the same height the effect size *d* would be 1.0.)

After Experiment 1 succeeded in establishing an independent effect of the self-prophecy procedure, Experiment 2 used a Solomon four-group design (Campbell & Stanley, 1963) to test the hypothesis that prior experience with the task might reduce the magnitude of the self-prophecy effect. (The Solomon four-group design includes two 2-group experiments, one testing the treatment effect in a pretest-posttest design, the other in a posttest-only design.) Experiment 2 showed that pretest experience did not at all impair the self-prophecy effect and established that the effect could be demonstrated in a repeated measures, or pretest–posttest, design.

EXPERIMENT 1

Overview

Experiment 1 employed a two-factor design (Self-Prophecy [vs. Control] x Gender Monitoring [vs. Control]) in a single laboratory session. The target behavior was the name-generation task used in the preliminary experiment. Participants generated complete first names in response to a series of stimuli consisting of initial letters and last names of moderately famous persons from the domains of art, entertainment, literature, science, politics, and sports. The preliminary experiment

established that the first-name generation task reliably produces a preponderance of male-name guesses when the correct name was not known. The self-prophecy and gender-monitoring treatments of Experiment 1 were targeted at reducing the strength of this tendency, which arguably models the operation of a gender stereotype, one that associates male gender more than female gender with fame-deserving achievement.

Method

Participants and Procedure

Participants were 331 undergraduate students from introductory psychology and marketing courses at the University of Washington and Washington State University randomly assigned to one of four experimental conditions. The experimental tasks were completed during class time for course credit. The name-generation task required 10 min or less and consisted of a list of 48 (24 male, 24 female) stimuli consisting of first initials followed by full last names. Because the names were preselected as being recognizable to approximately one quarter of students from these populations, it was assured that most participants would be obliged to guess frequently to produce all the requested first names. The list of 48 names was provided in two versions, counterbalanced within treatments, which reversed the order of the first 24 and last 24 names.

Introductory Instructions

The following introductory instructions, which were the same for all conditions, appeared on the front of the single-sheet questionnaire.

> On the other side of this sheet are names of people who have appeared in news and mass media—politicians, writers, entertainers, athletes, etc. For each name, we give you the full last name, but only the first letter of the first name. Your task is to complete the missing letters of the first name.
>
> Work rapidly. Don't take more than 5–10 seconds for any name. To give you an idea of the difficulty of this task: From previous research with similar names, we know that, on average, students know about 25% of the names immediately, can guess about another 25%, and will find about 50% unfamiliar.
>
> VERY IMPORTANT: For us to be able to analyze your data it is essential that you respond for each name, even when you can't think of the correct response. Because half or more of the names may be unfamiliar, you will have to guess often. (Such guesses, surprisingly, are often correct.) It is actually

> MORE IMPORTANT to us that you respond to each name than that you give correct responses! WE WILL NOT BE ABLE TO USE YOUR DATA IF YOU OMIT RESPONSES TO ANY NAMES.
>
> The task is completed when you have given responses for ALL of the names on the other side of this sheet.

The four conditions were distinguished by the following instructions.

Control. Participants completed the name-generation task without further preliminary instructions.

Self-prediction only. Before participants were asked to complete the name-generation task, the instructions continued:

> Before you begin this task, we ask you to try to predict one aspect of your performance. Please try to predict the proportions of female and male first names that you will guess when you are uncertain of the correct name. That is, predict what you will guess when you can't think of the first name of a famous person with the given initial and last name.
>
> Check the space below that best indicates your prediction about your own guessing behavior. *Please give careful thought to what you expect you will do before answering this question.*
>
> When I don't know the famous person's first name, I predict that I will distribute my guesses among male and female first names as follows (check one): I predict that I will guess [__mostly male; __mostly female; __male and female names equally] when uncertain.

Gender monitoring only. After the introductory instructions, participants were asked to turn to the other side of the sheet and complete the name-generation task. At the top of the page containing the initial-plus-name stimuli participants were instructed:

> To help us score the data, please also indicate whether each name is male or female by circling the *m* or *f* to the left of each.

(The *m* and *f* codes did not appear in conditions that lacked the gender-monitoring instructions.)

Self-Prediction Plus Gender Monitoring

Both the self-prediction and gender-monitoring instructions were added to the introductory instructions.

Results

As in the preliminary experiment, the name-generation task was scored to indicate the relative likelihood of generating male and female names when the correct famous name was not guessed. The dependent measure was the number of female-name errors minus the number of male-name errors, divided by the total number of errors that were unambiguously classifiable by gender, calculated separately for stimuli based on actual, famous females and males. On each of these error/gender indexes, positive scores indicated a preponderance of female-name errors. This measure was analyzed in a design with the experimental treatments of self-prophecy and gender monitoring in addition to participant gender as three between-participants factors, and with the gender of famous names as a within-participants factor.

The results appear in Figure 3. Greatest interest was centered on the test for the main effect of the self-prophecy manipulation. As expected, the female–male proportion index was higher for the self-prophecy conditions ($M = -.39$) than for the control conditions ($M = -.42$). However, this difference was not statistically significant, $F(1, 323) = 1.94$, $p = .165$. As can be seen in Figure 3, the effect of the self-prophecy manipulation was stronger for male than for female participants, a difference reflected in the significant interaction of participant gender and the self-prophecy factor, $F(1, 323) = 5.25$, $p = .023$. Also apparent in Figure

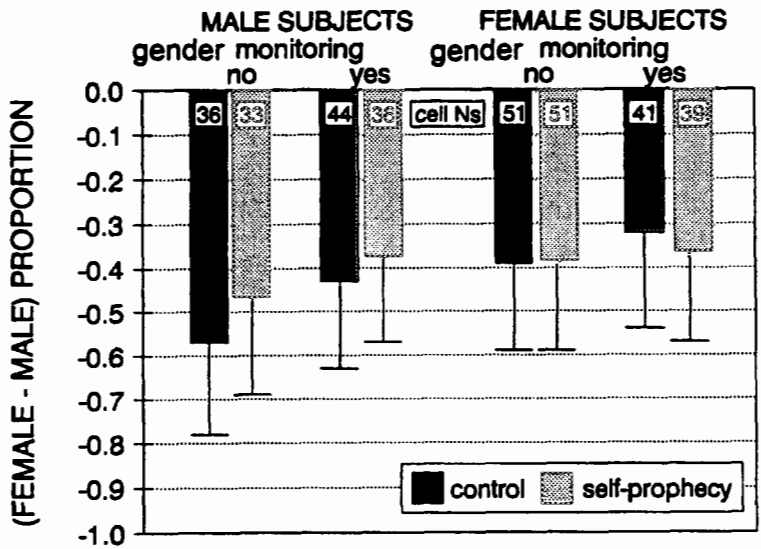

FIGURE 3 Results of Experiment 1—Self-Prediction × Gender Monitoring. (See also Figure 2 caption regarding dependent variable and error bar interpretation.)

3 is a main effect of gender monitoring, with relatively more erroneous female than male names when participants were obliged to attend to the gender of names as they were generating them ($M = -.37$) than when not ($M = -.44$), $F(1, 323) = 13.18$, $p < .001$; and a main effect of participant gender, with relatively more erroneous female names produced by female ($M = -.37$) than by male ($M = -.45$) participants, $F(1, 323) = 18.59$, $p < .001$. Not shown in Figure 3 is an expected effect of stimulus name gender, with relatively more erroneous female names generated when the famous name was actually that of a female ($M = -.33$) than of a male ($M = -.49$), $F(1, 323) = 63.20$, $p < .0001$; and an unexpected interaction of participant sex with name gender, such that males' erroneous guesses showed a greater effect of stimulus name gender than did females' erroneous guesses, $F(1, 323) = 6.68$, $p = .01$.

Discussion

In an orthogonal factorial design, Experiment 1 established effects of both the self-prophecy and gender-monitoring manipulations that were confounded in the preliminary experiment. Interaction of the self-prophecy manipulation with participant gender demonstrated the efficacy of the self-prophecy manipulation to reduce the expression of a gender stereotype for male participants. Interestingly, but also unexpectedly, the same effect was not obtained for female participants. The gender-monitoring procedure was also found to reduce the stereotype expression, for both male and female participants, confirming that it was an active ingredient of the successful influence procedure of the preliminary experiment. Experiment 1 therefore confirmed the effectiveness of the one-session procedure for the self-prophecy effect, although it did not find the effect to have operated as generally as hoped (i.e., it worked clearly only for male participants).

EXPERIMENT 2

Overview

Experiment 2 was designed to demonstrate use of the one-session procedure to test hypotheses regarding theoretical interpretation of the self-prophecy effect. The experiment used a Solomon four-group design (Campbell & Stanley, 1963) manipulating two factors (Self-Prophecy × Prior Experience) in a single laboratory session to test the hypothesis that prior experience with a task would reduce the magnitude of the self-prophecy effect. This hypothesis follows from the script evocation hypothesis offered by Sherman (1980) and the previous discussion regarding nonconfident self-predictions. Script evocation appears consistent with previous findings of the self-prophecy effect in situations for which, because of limited past experience, participants may have little confident basis for predicting their behav-

ior. Making a prediction consistent with the social norm perhaps elicits a "good person" script that is followed on behavioral opportunity. Presumably, the participant who does have a basis for confident self-prediction (i.e., past experience with the behavior for which prediction is requested) already has a personal script that is likely to be invoked in the target situation.

The target behavior was again the name-generation task used in the preliminary experiment and Experiment 1. The Solomon four-group design in effect combined two 2-group self-prophecy experiments: one with the posttest-only design used in the preceding experiments and one with a pretest–posttest design that gave participants experience with the name-generation task before the self-prophecy manipulation. The prior-experience (pretest) task was a parallel form of the posttest dependent measure constructed with a different set of stimulus names.

Method

Participants and Procedure

Participants were 406 students in an introductory undergraduate marketing course at Washington State University participating for course credit.[2] The experiment was conducted with groups of 10 to 100 participants over a 5-day period. At each experimental session participants received an envelope containing three color-coded tasks, each on a single sheet of paper. The three tasks were two name-generation tasks, each taking 10 min or less, and an unrelated filler task of approximately the same duration. All participants were instructed to complete the tasks sequentially following the color coding (Time 1, white; Time 2, gold; Time 3, tan); participants were instructed to replace each instrument in their envelope on completion of each task.

Depending on assigned condition (see following section), participants completed the two name-generation tasks at either Times 1 and 2, 1 and 3, or 2 and 3. All conditions were represented in each session, and random assignment of colors to the three tasks in each envelope determined the condition to which the participant was assigned. Only one colored instrument was out of the envelope at any point in the procedure; three participants were discarded who were observed not to follow these instructions. Four other participants were discarded for having omitted more than a few items of the name-generation task, and 6 participants were dropped from some analyses because they did not answer a concluding question asking if they were male or female. The 393 participants for whom participant sex data were available included 224 males and 169 females. (Group sizes vary slightly in some instances due to a few participants missing data required to conduct specific analyses.)

[2]All studies reported in this article used separate groups of participants. No participants participated in more than one experiment.

The name-generation tasks consisted of either the one used in Experiment 1 or a parallel version with different stimulus names.[3] Counterbalancing the two parallel versions of the name-generation task comprised an internal replication within the four-group design; thus, there were eight different orders of tasks in envelopes representing the four experimental conditions. The task title and introductory statements were identical to those of Experiment 1. The various conditions were implemented with the following postintroductory instructions.

Control. Participants completed the unrelated filler task at Time 1 in the experimental session. At Time 2, participants read the name-generation task introductory information and were then asked to turn to the other side of the sheet and complete the task. Time 3 for this condition consisted of completing the alternate version of the name-generation task.

Self-prediction only. Participants completed the unrelated filler task at Time 1 in the experimental session. At Time 2, before being asked to complete the name-generation task, participants followed the same procedure as that described previously for the self-prediction-only condition in Experiment 1: Participants read the name-generation task introductory information and directions for making a prediction, followed by the request for prediction. As in the control condition, participants in this condition also completed an alternate version of the name-generation task at Time 3.

Prior-experience only. At Time 1, participants completed the name-generation task following the instructions and procedure associated with the aforementioned control condition. The unrelated filler task was then completed at Time 2, followed at Time 3 by completion of a version of the name-generation task counterbalanced with that completed by the participant at Time 1.

Prior experience plus self-prediction. At Time 1, participants completed the name-generation task following the instructions and procedure associated with the aforementioned control condition. Participants then completed the unrelated

[3]The name-generation task differed from that of Experiment 1 in that there were only 23 female names in this study (as opposed to 24 in the first). After data were collected, it was concluded that one of the female stimulus names could be also be completed with a famous male name and was therefore dropped from the measure for analysis purposes. We have no reason to believe this slight difference affected the outcome of the study in any way.

filler task at Time 2. At Time 3, participants completed a counterbalanced version of the name-generation task following the self-prediction-only instructions and procedure outlined previously.

Results

Again, the name-generation task was scored by calculating the number of female names given as errors minus the number of male names given as errors, and then dividing by the total number of errors that were unambiguously classifiable by gender. As before, a positive score on this measure indicates a preponderance of female-name errors. This error/gender measure was analyzed in a design with three between-participants factors (self-prophecy, prior experience, and participant gender) and one within-participants factor (gender of famous names).

Primary interest was in the self-prophecy effect. This effect was observed (see Figure 4). Overall, the error/gender index was higher for the two prediction conditions ($M = -.45$, $SD = .23$) than for the two no-prediction conditions ($M = -.55$, $SD = .22$), $F(1, 381) = 17.42$, $p < .0005$. Additionally, there was a main effect of prior experience with the name-generation task: A higher proportion of female names was generated after prior experience with the task ($M = -.46$, $SD = .21$) than with no prior experience ($M = -.54$, $SD = .23$), $F(1, 381) = 14.27$, $p < .0005$. Some other

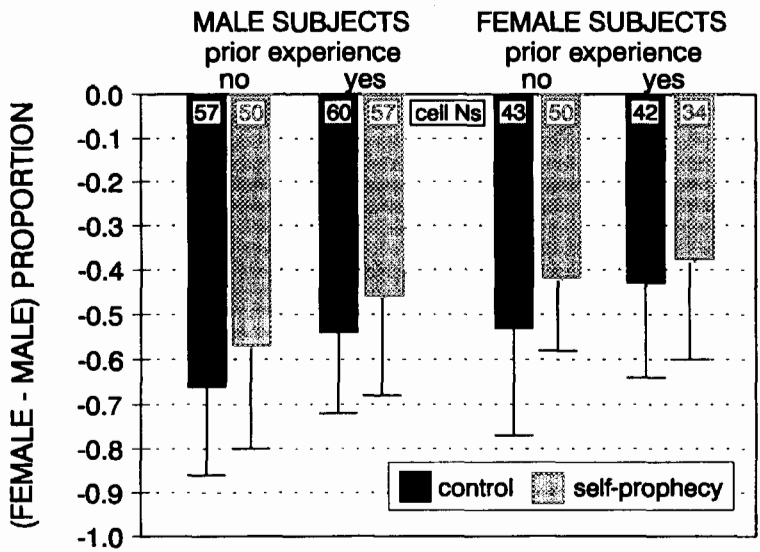

FIGURE 4 Results of Experiment 2—Self-Prediction × Prior Experience With Task. (See also Figure 2 caption regarding dependent variable and error bar interpretation.)

effects that repeated previous findings were a main effect of participant sex, with females generating a higher proportion of female names ($M = -.44$, $SD = .22$) than did males ($M = -.55$, $SD = .22$), $F(1, 381) = 37.04$, $p < .0005$; and a main effect of stimulus name gender, with higher proportion of erroneous female names generated in response to female ($M = -.42$, $SD = .30$) than to male ($M = -.58$, $SD = .28$) stimulus names, $F(1, 381) = 80.50$, $p < .0005$.

Importantly, the limitation of the self-prophecy effect to male participants that was observed in Experiment 1 was not again observed in Experiment 2—for the interaction of prediction with participant sex, $F(1, 381) = 0.10$, $p = .75$. The only other significant effect in the four-factor analysis of variance was an interaction of participant gender with prior experience (not shown in Figure 4): The effect of prior experience in producing an increased proportion of female-name errors was stronger for male than female participants, $F(1, 381) = 8.31$, $p = .004$. The interaction of self-prophecy with prior experience, though in the predicted direction, was not statistically significant ($F < 1$).

Ancillary analyses proving informative included a within-participant comparison across time (participants' first and second completion of the name-generation task). The predicted result here was an interaction of the "repeat" factor (pretest and posttest) with self-prophecy. This interaction was in the predicted direction (i.e., posttest scores were greater than pretest scores for the self-prophecy group only) and statistically significant, $F(1, 189) = 5.57$, $p = .019$. This result was also significant when participant gender was included as a between-participants factor, $F(1, 187) = 5.53$, $p = .02$.

Arguably, participants exhibiting optimistic self-prediction bias should be more prone to the self-prophecy effect. Thus, self-prophecy as a function of the direction of participants' predictions was analyzed in a design with three between-participants factors (prior experience, prediction direction, and participant gender) using only the posttest measure as the dependent variable (i.e., participants completing the name-generation task at Time 3). There were very few participants of either gender (5 females, 2 males) predicting that they would be more likely to guess female names when in doubt; these 7 participants were dropped from the following analyses. There was a main effect of prediction direction, $F(1, 174) = 5.89$, $p = .016$ (i.e., participants posttest scores corresponded to their predictions) and a main effect of prior experience, $F(1, 174) = 5.71$, $p = .018$ (i.e., more female guesses for pretested participants), with no interaction. The same pattern of significant results ($p < .04$) held when participant gender was not included in these comparisons. Omitting non-pretested participants and comparing pretest or posttest error/gender indexes for self-prophecy participants (i.e., participants in the self-prophecy condition at any one time in the procedure) indicated that the self-prophecy effect was essentially the same in magnitude whether participants predicted that they would guess more male names or male and female equally, $F(2, 187) = 3.83$, $p = .02$.

Discussion

Experiment 2 demonstrated that the self-prophecy effect can be successfully produced in a single-session procedure, both in an after-only design and in a repeated measures (pretest–posttest) design. Both two-group experiments contained in the Solomon four-group design independently demonstrated the efficacy of the self-prophecy manipulation to reduce expression of the stereotype of associating fame more with male than with female gender. Considered as an effect size using the r metric, the self-prophecy effect was similar in magnitude in the two subdesigns (r = .18 and .26, respectively, for the prior-experience and no-prior-experience conditions) and was statistically significant in both subdesigns, $F(1, 192) = 6.39, p = .01$ and $F(1, 205) = 11.76, p = .001$, respectively. It is apparent that the self-prophecy effect was smaller with prior experience of the task than without. However, this difference was not significant ($F < 1$), and accordingly there was no more than slight support for the hypothesis that the self-prophecy effect would occur with lack of prior experience with the task about which a prediction was requested.

The main effect of prior experience apart from a significant interaction with self-prophecy suggests some mechanism (possibly practice or awareness) by which prior exposure alone significantly affects responses. However, the ancillary analysis showing a significant interaction, in which posttest scores were greater than pretest scores for the self-prophecy group only, indicates that practice or awareness are not plausible explanations for the main effect of prior experience. Something about the combination of self-prophecy and prior experience appears to reduce stereotyping behavior; perhaps past actions for which participants see a need for correction are made salient upon prediction request.

The main effect of prediction direction in the ancillary analysis of Time 3 responses suggests that the mechanism underlying self-prophecy occurs at the prediction request, not at the time of the behavioral opportunity. This result, however, is slightly complicated by the finding for non-pretested participants, for whom the self-prophecy effect was essentially the same in magnitude whether participants predicted that they would guess all males or males and females equally at any of the times in the procedure. Perhaps prediction request (regardless of the nature of the prediction) elicits a script previously used by some participants that they do not like. Increased awareness regarding participants' past behavior may lead to cognitive dissonance, and future actions subsequently fall in line with the socially desirable norm to reduce dissonance.

GENERAL DISCUSSION

This article's meta-analytic review of prior research—most of which is confirmatory of the self-prophecy phenomenon—established that there is enough existing

support for the self-prophecy effect to regard it as worthy of further investigation. Further, the potential for application has been amply demonstrated by virtue of most of the previous research on the self-prophecy effect having been conducted in nonlaboratory settings (Greenwald et al., 1987; Greenwald et al., 1988; Sherman, 1980; Spangenberg, 1997; Spangenberg & Obermiller, 1996).

The new research reported in this article develops and demonstrates a single-session (either pretest–posttest or posttest only) procedure that provides a substantial gain in the efficiency with which research on the self-prophecy effect can be conducted in the laboratory. Effect sizes for the present studies can be determined approximately from Figures 2 through 4, in which mean differences that have half the height of error bars represent effect sizes of $d = .5$ ($r = .24$). Previous research and the present experiments indicate that the effect is large enough both to be effectively studied in the laboratory and to be worthy of application outside the laboratory (the average approximates the conventional designation of a "moderate" effect size[4]).

Theoretical Interpretation of the Self-Prophecy Effect

How do the present findings advance theoretical interpretation? Existing empirical evidence and the varying contexts in which the effect has been observed suggest that self-prophecy is potentially mediated, or driven by, one or more different processes that have been considered in other programs of research. These separate, albeit arguably interrelated, constructs include script evocation (e.g., Sherman, 1980), cognitive dissonance (e.g., Festinger & Carlsmith, 1959), induced hypocrisy (e.g., Aronson, Fried, & Stone, 1991), self-awareness (Duval & Wicklund, 1972), planning fallacy (Buehler, Griffin, & Ross, 1994), and intention measurement (e.g., Morwitz, Johnson, & Schmittlein, 1993).

Script evocation. Sherman (1980) proposed that the act of making a prediction about one's own behavior produces a cognitive representation of oneself performing the action—a script that is likely to be retrieved and invoked to guide performance when the target situation is later encountered. This interpretation is consistent with the previous suggestion that the self-prophecy effect may occur only when participants have no basis for confident self-prediction. That is, the participant who does have a basis for confident self-prediction presumably already has a script that is likely to be invoked in the target situation. Even though this participant may mentally rehearse a different script in response to the self-prediction request, that rehearsal may be insufficient to override the preexisting script when the target situation is later encountered. In this script-evocation interpretation, social

[4]Cohen (1988) defined effect sizes of "small" and "moderate" as $r = .1$ and $r = .3$, respectively.

desirability of the target action is responsible for leading participants to predict performance of the more socially desirable option when they lack a preexisting script. Although inconclusive, existing evidence suggests that social desirability of the target action is a necessary condition for the effect to hold—perhaps creating a script that overrides preexisting scripts held by participants.

Cognitive dissonance. The self-prophecy procedure shares procedural elements with some well-established cognitive dissonance experimental procedures. For example, a critical ingredient of the induced-compliance experiment (e.g., Festinger & Carlsmith, 1959) is to use the implicit authority of the experimenter to elicit the participant's agreement to perform a counterattitudinal action. The participant is then likely to show attitude change toward a position consistent with the induced action. The social influence on response to an initial request and subsequent change consistent with that response are elements shared with the self-prophecy effect. Why either of these effects consistently occur is yet to be agreed on. There is a long history of controversy over theoretical interpretation of the cognitive changes induced by such dissonance experiments. One generally successful theoretical view of these results is Aronson's (1992) proposal that these effects rest on the experiment's inducing a discrepancy between the participant's self-concept and the induced action; the opinion change serves to restore consistency by providing a self-concept-preserving justification for the induced action. This desire to remain consistent may also underlie self-prophecy participants' actions lining up with their predictions.

Induced hypocrisy. Aronson's (1992) theoretical interpretation of cognitive dissonance is especially relevant to the self-prophecy effect because it was used recently (e.g., Aronson, Fried, & Stone, 1991; Stone, Aronson, Crain, Winslow, & Fried, 1994) to develop a new class of findings described as an "induced-hypocrisy" effect. Participants advocated the socially desirable action of advocating condom use for AIDS prevention. When participants were later made mindful of their past failures in this regard and given an opportunity, these participants purchased more condoms than did participants in various control conditions. Stone et al. (1994) interpreted the effect as serving to reduce dissonance evoked by eliciting pro-attitudinal advocacy while making participants "mindful of their own past failures to use condoms" (p. 116). This induced-hypocrisy manipulation was also used to increase water conservation (Dickerson, Thibodeau, Aronson, & Miller, 1992) and to increase recycling behavior (Fried & Aronson, 1995). Specifically, in all of these experiments, participants made pro-attitudinal speeches that they were led to believe would have a positive impact on others. Participants were also reminded of their history of failing to live up to the standards they advocated in their speeches. This line of research showed that the combination of these two factors appears to arouse dissonance, but either factor in isolation does not.

Perhaps the self-prophecy effect also works by motivating participants to reduce a values–action discrepancy made salient by the self-prediction procedure accompanied by reminder of their past failures to act in the predicted direction. For example, moderate-prior-turnout voters in Greenwald et al.'s (1988) Experiment 2 may have been motivated, by confrontation with the self-prediction request, to recognize and then reduce the discrepancy between their value on electoral participation and their inconsistent past conformity to this ideal.

In support of the assertion that self-prophecy and induced hypocrisy are closely related programs of research, a few important similarities between them are readily seen. First, requests for self-prophecy regarding socially desirable behaviors are analogous to pro-attitudinal speeches that participants in induced-hypocrisy experiments are led to believe will have a positive impact on others. Second, the request for self-prediction also likely reminds participants of their history of failing to live up to their own normative standard, which is an important component of the hypocrisy research. (Recall that successful demonstrations of the self-prophecy effect are primarily associated with making predictions regarding socially desirable actions.) And third, both lines of research have relied primarily (until this article) on "real-world" tasks as dependent measures—behaviors for which participants are likely to have some familiarity with, are likely unaware they are being measured, and would not see as unusual to undertake or avoid. Several studies of induced hypocrisy include conditions much like those of self-prophecy experiments: *hypocrisy*, *reminded*, *public commitment*, and *control* in hypocrisy studies are analogous to *self-prediction*, *knowledge-only*, *preference-only*, and *control*, respectively, in self-prophecy work. A strong argument can therefore be made for the interpretation of the self-prophecy phenomenon comprising a new method of inducing cognitive dissonance: Prediction makes participants mindful of the discrepancy between actual and ideal states, and subsequent action consistent with the prediction reduces that discrepancy.

Self-awareness. Also arguably related to dissonance theory, the theory of self-awareness (Duval & Wicklund, 1972) suggests that the presence of self-focusing stimuli (often cameras or mirrors in laboratory studies) heighten self-focused attention, producing a state of objective self-awareness that involves attention to discrepancies between actual and ideal selves. The theory further supposes that negative affect results from perception of such discrepancy (Gibbons, 1990), in turn leading to attempts to reduce the discrepancy. Direct comparison suggests that self-awareness theory is probably not distinguishable from induced-hypocrisy work; the two are highly similar theories, experimentally accounting for approximately the same dissonance-related conclusions. Correspondence between dissonance and self-awareness research can also be seen in the observation that both lines of research show individuals to be strongly influenced

by norms—heightened self-awareness may either increase or decrease pro- or antisocial behavior depending on what is considered more normatively desirable (Carver, 1974; Rule, Nesdale, & Dyck, 1977; Wegner & Schaefer, 1978). Thus, if the relation between self-hypocrisy and self-prophecy is accepted, the relation between self-prophecy and self-awareness is straightforward. Interpreting the request for self-prediction as a (private) self-focusing manipulation (like a mirror) is not very different from the adjustment in the induced-hypocrisy version of dissonance theory that is needed to accommodate the self-prophecy effect (i.e., interpreting the prediction request as a means of making participants mindful of an actual–ideal discrepancy).

Planning fallacy. In general, people have a systematic tendency to optimistically predict that they will do something sooner than it actually (if ever) gets done; this tendency is called the planning fallacy. The planning fallacy does not extend to the prediction of others' behaviors; people make greater use of previous experience for social than for self-predictions. Demonstrating this effect, Buehler, Griffin, and Ross (1994) showed that people use a simulation heuristic rather than more diagnostic base-rate information when making subjective predictions about their own behavior. Buehler, Griffin, and MacDonald (1997) conducted further experiments in this program of research showing that mental simulation and planning exert a greater impact on prediction than on actual behavior; this result continued to hold given external task-completion incentives. In fact, optimistic bias was most pronounced when individuals had an external incentive to complete the task in question. Although elaboration of thought patterns was elicited in some planning fallacy studies, participants were normally first asked to make a prediction regarding the time required to complete a future behavior—a condition much like the prediction request in self-prophecy experiments. The pattern of cognition supported in planning fallacy research seems to be a focus on singular information (i.e., optimistic, plan-based future scenarios) justifying participants' optimism at the expense of relevant distributional information (i.e., past experiences). Participants tended to attribute past prediction failures to relatively external, transient, and specific factors. Individuals do not use recollections of own past performances to improve the accuracy of their time estimates, but they do use this information to overestimate others' completion times. When planning fallacy occurs, it is an obvious situation in which self-prophecy does not work—an inaccurate self-prediction is made and not subsequently fulfilled to self-correct the error. If one had a theory to explain when the planning fallacy is obtained and when not, that same theory could likely explain when self-prophecy holds and when it does not. Indeed, apart from the cognitive elaboration associated with some planning-fallacy manipulations, these programs of research may turn out to be two problems that have the same theoretical solution. Unfortunately, research on the planning fallacy is as devoid of empirically supported theoretical explanation as is earlier work on self-prophecy.

Intention measurement. Another recent program of research implicitly related to self-prophecy regards the effect of measuring intention on category and brand-level purchase behavior (Fitzsimons & Morwitz, 1996; Morwitz, Johnson, & Schmittlein, 1993). Based on research showing that the process of survey measurement changes consumer attitudes, intentions, and behaviors (e.g., Feldman & Lynch, 1988), Morwitz and colleagues found correlational support for the impact of measurement of purchase intention on actual purchase incidence. Unfortunately, this line of inquiry has not provided compelling theoretical explanations for its findings. Because these authors conducted correlational analyses on longitudinal panel data, they wisely stated that their results must be interpreted acknowledging the possibility of systematic differences between their groups. Further, interpretation difficulties given the data they collected eliminate the possibility of clearly identifying the mechanism through which their "mere-measurement" effect operates. The most compelling explanation for their results is based on the premise that stating intentions increases accessibility of cognition concerning a product category or a brand within a category. The issue of interest to this article is whether the measurement of purchase intention is an instance of self-prophecy. Recall the business school fund-raising telephone study discussed before (Obermiller et al., 1992), in which the prediction request was similar to a purchase-intention measure: The circumstances of requesting prediction of purchase (in Morwitz & colleagues' work) may have been more like this study than they were similar to studies for which the self-prophecy effect held. To disentangle these two lines of research, future work is needed in which purchase intention and the self-prophecy technique are compared under identical circumstances. Such a study would allow one to determine whether the former is a special case of the latter or something different altogether.

It is these authors' opinion that both the reviewed research and the new experiments reported in this article appear to best fit with theoretical interpretations in terms of self-hypocrisy (dissonance) and self-awareness theorization. Dissonance (or objective self-awareness) is likely to be aroused by the prediction request, causing participants to focus on a discrepancy between their behavior and their ideals. Subsequent actions that are more consistent with ideals serve to reduce the aversive properties of the dissonance or objective self-awareness states. Future research must systematically test this and alternative explanations in well-controlled experimental work to clearly develop theoretical interpretation of this compelling effect.

CONCLUSION

This article shows that the self-prophecy effect occurs in a variety of situations, is a moderate-size (not small) effect, and is reproducible in a single-session procedure—either posttest-only or pretest–posttest design. Further, the name-generation task presented herein is a useful tool for the study of gender stereotyping and could

be adapted to explore other behaviors. Perhaps more important, this article effectively demonstrates the substantive importance and theoretical intrigue of the effect: The self-prophecy effect has been subterranean for too long. Some of the most powerful and enduring persuasion techniques from social psychology have been drawn from theory and research on cognitive dissonance (Festinger, 1957). Research and discussion presented earlier suggests that the effects of self-prophecy on subsequent behavior may be a unique dissonance-evoking technique that few investigators have explored since Sherman's (1980) initial finding. The existing research, albeit sparse, supports Sherman's statement: "[the self-prophecy phenomenon] should have intriguing implications for applied work in the areas of consumer behavior, psychotherapy, decision making, and education" (p. 219). A wide range of moral or societal concerns could be addressed using this simple technique: Drunk driving, public utility conservation, healthy eating habits, recycling behavior, disease control, and environmental sensitivity are but a few examples. This work brings an important novel social-influence effect to the attention of other investigators, suggests some plausible theoretical interpretations, and provides research tools that can be used for needed further investigation.

Finally, behavior modification using the self-prophecy technique is noncoercive and ethically unobjectionable. For example, consider that people are unlikely to predict that they will cheat if cheating is against their own moral standards or general societal standards. Rather, people can be expected to make predictions that present themselves in a favorable light when asked to make self-predictions. Therefore, a personally undesired or antisocial behavior should not be elicited by a self-prophecy manipulation. The worst (and perhaps best) that a self-prophecy modification technique can achieve is to induce people to become "better," or less hypocritical, by their own standards.

ACKNOWLEDGMENTS

We thank Carl Obermiller, Dave Sprott, Bob Fisher, Frank Kardes, and anonymous reviewers for suggestions improving this article.

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Accepted by Frank Kardes.