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## The perseverance effect in the debriefing paradigm: Replication and extension <sup>☆</sup>

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### Abstract

A classic study conducted by Ross, Lepper, and Hubbard (1975) revealed a perseverance effect wherein people who received positive performance feedback on an alleged social perceptiveness test reported more favorable self-perceptions in this domain than those who received negative feedback *despite the fact* that they had received standard outcome debriefing (i.e., been informed about the false, predetermined, and random nature of the feedback) prior to reporting self-assessments. The present studies extend this past research by revealing that (a) there is a form of outcome debriefing (i.e., informing participants about the bogus nature of the test as well as the bogus nature of the feedback) that effectively eliminates the perseverance effect, (b) the perseverance effect that occurs after standard outcome debriefing is limited to perceptions of specific task-relevant skills rather than more global abilities, and (c) affective reactions do not underlie the perseverance effect that occurs in the false feedback paradigm.

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In a study investigating how threats to self-esteem affect social perceptions, Charlotte received feedback indicating that she performed at the 30th percentile on a test that purportedly measured intellectual ability. Shortly thereafter, she evaluated a target person, and was then thoroughly debriefed. The experimenter explained why it was necessary to present a cover story, stressed that her score was randomly determined in advance, and highlighted that her score reflected nothing about her true intellectual ability. Nonetheless, as she headed home, Charlotte found herself wondering about her ability to succeed in university, and seriously contemplated whether she really has what it takes to be a lawyer. This perseverance effect, wherein people cling to newly

formed beliefs even when the evidential basis for those beliefs is completed refuted, was demonstrated convincingly in a classic study conducted by Ross et al. (1975). Their research revealed that participants who were provided with false feedback indicating that they performed well on a test of social perceptiveness ability provided more favorable self-evaluations *after debriefing* than did participants who were told that they performed poorly. In other words, the self-perceptions of task-relevant skills that were elicited by the feedback “persevered” despite extensive debriefing. Presumably, such beliefs persist because subjects attempt to generate explanations for the initial outcome (e.g., I am a pretty outgoing person and that is why I did so well), and the causal factors they identify continue to predict the outcome even when the outcome that prompted the explanation is invalidated (Anderson, Lepper, & Ross, 1980; Ross, Lepper, Strack, & Steinmetz, 1977).

Although conducted over a quarter century ago, Ross et al.’s research continues to have important ethical implications for researchers in social psychology. The false

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feedback paradigm has been used to study many social psychological phenomena, including reactions to social comparisons (e.g., Crocker, Thompson, McGraw, & Ingerman, 1987; McFarland & Buehler, 1995; Tesser, Millar, & Moore, 1988); the impact of threats to self-esteem on self-evaluation (e.g., Brown & Smart, 1991; Dunning, Leuenberger, & Sherman, 1995), social comparison judgments (e.g., Brown & Gallagher, 1992), aggression (Stucke & Sporer, 2002), and prejudice (e.g., Fein & Spencer, 1997); the role of individual differences in emotional, cognitive, and behavioral reactions to performance feedback (e.g., Baumeister, Heatherton, & Tice, 1993; Blaine & Crocker, 1993; Di Paula & Campbell, 2002; McFarlin, 1985; Wood, Giordano-Beech, Taylor, Michela, & Gaus, 1994, 1999); the relation between mood and cognition (e.g., Forgas, 2000; Schwarz & Clore, 1996); and the nature of emotions and emotional regulation in the aftermath of distressing events (Brown & Dutton, 1995; Brown & Marshall, 2001; Dodgson & Wood, 1998; Forgas & Ciarrochi, 2002; McFarland & Buehler, 1998; Nummenmaa & Niemi, 2004). Given the continued and widespread use of the false feedback paradigm, it is essential that researchers have at their disposal debriefing techniques that can effectively eliminate the perseverance phenomenon. The goal of our research was to critically examine the debriefing process and further clarify the precise features that make for an effective debriefing in this paradigm.

### The current research

We conducted two studies that were designed to extend Ross et al.'s (1975) findings regarding the nature of an effective debriefing. Before presenting our precise goals, it is important to consider their research in greater detail. In their main study, participants were first informed that the research was examining "physiological responses during decision making." Next, while attached to a recording device, they completed a decision-making task (i.e., distinguishing real from fake suicide notes) that purportedly assessed social perceptiveness ability.<sup>1</sup> After receiving either success or failure feedback, they were assigned to one of three groups. In the *outcome debriefing* condition, participants were informed of the "true" purpose of the study ("to examine physiological reactions to feedback"), and that their feedback was false, randomly assigned, predetermined, and non-reflective of their actual ability. In the *process debriefing* condition, participants received the same information as that provided to outcome debriefing participants, with one important addition: they were informed about the perseverance phenomenon and encouraged to avoid engaging in this cognitive process. In the *no debriefing condition*, participants did not receive a debriefing at this point in the session. Participants then completed three assessments of belief perseverance: (1) estimates

<sup>1</sup> Dr. Lepper (personal communication, August, 1997) has confirmed that participants were told that the decision-making task assessed general social perceptiveness ability. The original (1975) report did not mention this point explicitly.

of current performance and predictions regarding future performance on an equally difficult set of notes, (2) evaluations of ability on the specific task (i.e., identifying real suicide notes), and (3) ratings of abilities presumably related to social perceptiveness (i.e., recognizing falsehood, sensitivity to others' feelings, and test-taking skills). A perseverance effect would be revealed if post-debriefing self-perceptions in the success group were more favorable than those in the failure group. The results indicated that although process debriefing was effective in eliminating perseverance on all measures, outcome debriefing yielded a perseverance effect on both performance estimates and ratings of ability on the specific task.<sup>2</sup>

Our research had four objectives. First, we assessed the possibility that there is a form of outcome debriefing that can be effective in eliminating perseverance. One noteworthy feature of the original Ross et al. (1975) research is that although debriefed participants were told that their score on the test was bogus, they were led to believe that the test itself was a valid test of an important underlying ability, and that therefore, a "real" score on the test actually existed. It seems possible that this feature of the debriefing might make the perseverance effect more likely. Presumably, participants who have received outcome debriefing might find themselves wondering what their real score on the test is, and might even use the fake score as an anchor with which to estimate their real score (Cervone & Palmer, 1990; Mussweiler, Strack, & Pfeiffer, 2000; Wegner, Coulton, & Wenzlaff, 1985).<sup>3</sup> In essence, curiosity about one's real score could engender a train of thought that leads participants to construct a scenario or image of their actual performance that is consistent with their randomly assigned performance. This, image, in turn, might engender further self-relevant thoughts (e.g., attributions to stable qualities, self-praise or criticism) that ultimately create perseverance in self-perceptions. In our research, we developed a revised form of outcome debriefing that included information indicating that the test itself was not a real validated test of social perceptiveness ability. We expected that the addition of this feature would eliminate the perseverance effect that is normally obtained with outcome debriefing.

Second, we explored the generality of the perseverance effect that occurs after standard outcome debriefing. In Ross et al. (1975), perseverance was strongest on self-perceptions that were specific to the test-domain (i.e., performance estimates and ratings of the ability to identify real suicide notes). However, although they assessed perceptions of a few "related

<sup>2</sup> We focused our discussion on Study 2 of the Ross et al. (1975) report because it included a "no debriefing" control group and a process debriefing group. The results of their Study 1, which included only the outcome debriefing condition, were comparable to those of Study 2, except that perseverance was obtained on the "related abilities" measure.

<sup>3</sup> Some anecdotal support for this assumption can be found in the spontaneous comments made by participants from other studies in our lab. We regularly use false feedback, and have found that it is not uncommon for participants to ask for information about their "real" score after being debriefed. Indeed, it is these comments that sparked the current research.

skills," they did not ask participants to rate themselves on the precise global dimension allegedly assessed by the suicide note task—social perceptiveness ability. In our research, we included several items to assess participants' perceptions of their global social perceptiveness skills. Based on Ross et al.'s findings, we expected that perseverance would likely be limited to specific ratings. However, it seemed worthwhile to assess the generality of the perseverance effect because their preliminary study (see Footnote 2) did reveal an effect on the "related skills" measure. Moreover, given that the test is portrayed as assessing a general ability, it seems plausible that perseverance could occur on more global perceptions. The generality of the perseverance effect is an important issue. If the phenomenon extends to perceptions of global abilities then the prescription that researchers conduct debriefings that completely eliminate perseverance is rendered ever more pressing.

Finally, we examined the possibility that the perseverance effect occurring after standard outcome debriefing might be due to "affective perseverance" (e.g., Sherman & Kim, 2002). Ross et al. discussed, but were not able to test, the possibility that perseverance in self-perceptions could derive from mood reactions that are not completely eliminated through debriefing. Presumably, participants' post-debriefing moods could influence self-perceptions via mood-congruent processing (e.g., Brown & Mankowski, 1993; Sedikides & Green, 2001). We included a post-debriefing mood assessment to evaluate this notion.

## Study 1

Using a procedure closely modeled after that used by Ross et al. (1975), participants were exposed to either success or failure feedback on an alleged test of social perceptiveness that involved distinguishing real from fake suicide notes. They then received one of four debriefing inductions: (1) standard outcome debriefing (i.e., participants were told about the "true" purposes, and that their score was false, predetermined, and randomly assigned), (2) revised outcome debriefing (i.e., participants received standard outcome debriefing and learned that the test was bogus), (3) process debriefing (i.e., participants received standard outcome debriefing and information regarding perseverance), or (4) no debriefing. After the debriefing variation, participants provided current and future performance estimates, ratings of their specific and global abilities, and mood ratings. We predicted that perseverance would occur in only the standard outcome debriefing condition, and that it would be limited to ratings of performance and specific task-relevant skills. We also evaluated whether affective reactions mediated the perseverance effect.

## Method

### Participants and design

The participants were 67 female and 61 male SFU undergraduates who took part individually and were pro-

vided with course credit for participating. They were randomly assigned to the conditions of a 2 (feedback: success vs. failure)  $\times$  4 (debriefing condition: no debriefing vs. standard outcome debriefing vs. revised outcome debriefing vs. process debriefing) between-subjects factorial design. Males and females were distributed approximately equally across the conditions. Preliminary analyses indicated that there were no interactions involving gender; thus, the primary analyses do not include gender as a factor.

### Procedure

Participants were first provided with a cover story indicating that the researchers were exploring "personality traits and physiological responses during decision-making." Accordingly, they first completed a personality survey, after which they were attached to physiological recording equipment. During a "baseline assessment of arousal," they read an information sheet indicating that the decision-making task involved reading 15 pairs of suicide notes, and selecting the one "real" note from each pair. The task was described as a widely used measure of social perceptiveness—"the ability to make accurate judgments about other people's behaviors and motives." This ability was depicted as an important attribute that is linked to a wide variety of positive outcomes. Participants then completed the test, after which they were told to sit still while a "post-task reading" was obtained. At this time, the experimenter left the room briefly to "score the test."

### Feedback manipulation

The experimenter returned and presented the participant with a feedback sheet tucked inside the test booklet. The feedback sheets were prepared in advance, ensuring that the experimenter remained blind to condition. Participants were told that their score was either 14/15 (success) or 4/15 (failure), and that the average was 9/15.

### Debriefing manipulation

After a few minutes, the experimenter removed the electrodes, and delivered one of the debriefing variable inductions. Participants in the *no debriefing* condition were told that while their credit form was being prepared they were to complete a final "thoughts and reactions" questionnaire (see below). In the debriefing conditions, the experimenter explained that the study was actually examining "the effects of feedback on physiological responses," and that it had therefore been necessary to provide false test feedback. Participants in the *standard outcome debriefing* condition were informed that their score was a fake score that had been randomly assigned to them prior to their arrival. Additionally, they were shown a "random assignment schedule," and the experimenter emphasized that the score contained absolutely no information about the participant's actual performance or underlying abilities.

Participants in the *revised outcome debriefing* condition were provided with the same information as that provided to those in the standard outcome debriefing group.

Importantly, however, they were also told that the suicide-note test was a *fake* test that had been made to look like a real test. The experimenter explained that all of the suicide notes in the test were fake notes, and that the test did not measure any underlying abilities.

Participants in the *process debriefing* condition were also provided with the same information as that provided to standard outcome debriefing participants. Additionally, however, they were informed about the nature of the perseverance effect and how it might have personal relevance for them in this context. The experimenter highlighted that people's beliefs sometimes persist even after debriefing because they generate independent evidence that explains the feedback, and urged participants to avoid thinking in this way.

#### Dependent measures

Next, the experimenter asked participants to complete a "thoughts and reactions" questionnaire. They were assured of anonymity and asked to place the completed questionnaire into a sealed unmarked envelope and place it amongst other unmarked envelopes. The first section consisted of filler questions (e.g., clarity of instructions) that validated the cover story. Participants then completed two items assessing their *perceptions of performance* on the suicide note task. One item requested that they estimate their score on the suicide note test out of 15. The wording of this question was carefully tailored to each debriefing condition to ensure that the question made sense in light of the details of each condition. Participants in the *no-debriefing* condition were asked to recall the score they had just been assigned. Participants in the *standard outcome debriefing* and *process debriefing* conditions (who were told that their score was fake) were asked to estimate their "actual score on the suicide note test (/15)." Participants in the *revised outcome debriefing condition*, who were told that the test was not real, were asked the following: "Even though you now know that the task was not actually a real test of your ability to distinguish genuine from fictitious suicide notes, imagine for a moment right now that it had been a real test (i.e., one including real and fake notes). If you took this test now, what would you estimate to be your score on the test?." Participants also estimated their future performance on a different, but equally difficult, set of genuine and fake notes. As well, they evaluated the *specific ability* to distinguish real from fake suicide notes (1 = *much lower ability than average*; 11 = *much higher ability than average*).

Next, participants responded to 8 scales (range 1–11) assessing their *perceptions of their global abilities*: good at detecting another's distress (*extremely poor-extremely good*); good at understanding why people behave in certain ways (*extremely poor-extremely good*); likelihood of pursuing a job requiring social perceptiveness skill (*extremely unlikely-extremely likely*); good at being a psychologist (*extremely poor-extremely good*); socially perceptive (*extremely low-extremely high*); sensitive to others' feelings (*extremely insensitive-extremely sensitive*); good at taking tests under pressure (*extremely poor-extremely*

*good*); ability to recognize falsehood (*extremely poor-extremely good*). The latter three items constitute Ross et al.'s (1975) "related skills" measure. Finally, participants rated their current moods (happy, satisfied, pleased, disappointed, sad, proud, and competent; 1 = *not at all*; 9 = *extremely*).

#### Final debriefing

Participants in all conditions received a final "process" debriefing. The experimenter explained the exact hypotheses, the necessity for the elaborate deception, and the fake nature of the test.

#### Results and discussion

##### Creation of indexes

We first constructed several indexes: (1) performance estimate index (i.e., the average of current and future score estimates,  $\alpha=.81$ ), (2) global abilities index (i.e., the average of the 8 global ratings,  $\alpha=.80$ ), and (3) mood (i.e., the average of the 7 mood ratings with the 2 negative items reverse scored,  $\alpha=.85$ ).

##### Performance estimates

We predicted that a perseverance effect on performance estimates would occur in only the standard outcome debriefing condition. Revised outcome and process debriefing were expected to eliminate perseverance. A 2 (feedback)  $\times$  4 (debriefing) ANOVA performed on this index revealed two main effects (debriefing:  $F(3,120)=8.90, p<.0001$ ; feedback:  $F(1,120)=48.90, p<.0001$ ) that were qualified by a significant interaction effect ( $F(3,120)=57.76, p<.0001$ ) that supported the prediction (see Table 1). Not surprisingly, among those who were not debriefed, participants who received success feedback reported higher estimates ( $M=13.18$ ) than those who received failure feedback ( $M=4.46$ ),  $t(120)=15.03, p<.001$ . Importantly, though, even among those who received standard outcome debriefing, recipients of success feedback reported higher estimates ( $M=9.33$ ) than recipients of failure feedback ( $M=7.96$ ),  $t(120)=2.49, p<.05$  (i.e., a perseverance effect). The success–failure difference was not significant ( $t<1$ ) in the revised and process debriefing groups, indicating that perseverance was eliminated in these conditions.<sup>4</sup>

<sup>4</sup> On the performance estimate measure, the variability in the control groups was lower than that found in the debriefing groups, probably because control participants found it relatively easy to recall the score they received a short while before. Consequently, the overall error term may be somewhat lower than it would be if these control groups were not included in the analysis. To assess whether the perseverance effect obtained in the standard outcome debriefing group would remain significant if an alternative error term was used, we conducted an independent-groups *t* test that used the two pertinent cell variances to represent error variability. The perseverance effect was maintained,  $t(30)=2.40, p<.05$ . As well, we assessed whether the significant interaction effect would be preserved if a more stringent critical *F* value were used. We conducted the conservative Box test for heterogeneous variances outlined in Howell (2002), and the interaction effect remained significant,  $p<.05$ . In sum, the findings on the performance estimate measure are not attributable to the lower variances in the control conditions.

Table 1

Dependent variables as a function of type of performance feedback and debriefing technique

Measure and type of feedback	Type of debriefing			
	No debriefing	Standard outcome debriefing	Revised outcome debriefing	Process debriefing
<i>Performance estimates</i>				
Failure				
M	4.46 <sub>a</sub>	7.96 <sub>b</sub>	10.38 <sub>c</sub>	8.85 <sub>b</sub>
N	14	17	17	17
SD	.49	1.40	1.81	2.11
Success				
M	13.18 <sub>d</sub>	9.33 <sub>c</sub>	10.08 <sub>c</sub>	8.03 <sub>b</sub>
N	14	15	17	17
SD	.99	1.79	1.66	1.50
<i>Specific ability rating</i>				
Failure				
M	3.07 <sub>a</sub>	6.47 <sub>c</sub>	7.53 <sub>d</sub>	7.12 <sub>c,d</sub>
N	14	17	17	17
SD	.83	1.17	1.55	1.49
Success				
M	9.36 <sub>b</sub>	7.20 <sub>d</sub>	7.24 <sub>d</sub>	6.65 <sub>d</sub>
N	14	15	17	17
SD	1.08	1.08	1.30	.93
<i>Global ability ratings</i>				
Failure				
M	6.82 <sub>a</sub>	7.69 <sub>c</sub>	7.48 <sub>c,a</sub>	7.47 <sub>c,a</sub>
N	14	17	17	17
SD	1.45	1.07	1.23	1.15
Success				
M	7.84 <sub>c</sub>	7.45 <sub>c</sub>	7.86 <sub>c</sub>	7.01 <sub>c</sub>
N	14	15	17	17
SD	1.10	1.09	1.08	.85

Note. Higher scores on the performance estimate index reflect higher performance (/15). Higher scores on the ability measures indicate greater ability (1–11). Within rows and columns for each measure, means *not* sharing a common subscript letter differ at the .05 level (two-tailed). There is one exception: on the specific ability rating, the success vs. failure difference is marginally significant within the standard debriefing condition ( $p < .08$ ).

### Specific ability rating

Again, a perseverance effect was anticipated in only the standard outcome debriefing condition. A  $2 \times 4$  ANOVA performed on the specific ability rating revealed two main effects (debriefing:  $F(3,120)=4.72$ ,  $p < .01$ ; feedback:  $F(1,120)=39.62$ ,  $p < .0001$ ) that were qualified by a significant interaction effect ( $F(3,120)=50.59$ ,  $p < .0001$ ) that supported the prediction (see Table 1). As expected, non-debriefed participants who received success feedback reported a greater ability to distinguish real from fake suicide notes ( $M=9.36$ ) than those who received failure feedback ( $M=3.07$ ),  $t(120)=13.97$ ,  $p < .001$ . Consistent with predictions, recipients of standard outcome debriefing revealed a marginally significant perseverance effect (success:  $M=7.20$ ; failure  $M=6.47$ ),  $t(120)=1.78$ ,  $p < .08$ . Perseverance did not occur in the revised or process debriefing groups (success vs. failure  $t < 1$ ,  $ns$ ).

### Global ability ratings

One goal of our research was to examine whether perseverance occurs on the more global dimension purportedly assessed by the suicide note task. A  $2 \times 4$  ANOVA performed on the global ability index revealed that the perse-

verance effect was not obtained on evaluations of general social perceptiveness skills. Although a significant interaction was obtained ( $F(3,120)=2.63$ ,  $p < .05$ ), it occurred solely because non-debriefed “success” participants reported greater global ability ( $M=7.84$ ) than non-debriefed “failure” participants ( $M=6.82$ ),  $t(120)=2.62$ ,  $p < .01$ . No evidence for perseverance was obtained within any debriefing condition (i.e., all  $ps > .20$ ). Separate analyses of the individual items that were included in the overall global index revealed a similar pattern of effects—no evidence of perseverance in any debriefing group (again, all relevant  $ps > .20$ ). Thus, it appears that when the perseverance effect occurs, it is limited to perceptions of specific task-relevant features (i.e., score estimates, and ratings of task specific ability). It is worth noting as well that although failure feedback decreased global ability ratings (relative to the combined failure debriefing condition means) it does not appear that positive feedback increased global ratings. The debriefing condition averages can be interpreted as representing a “baseline” global ability rating, and the mean in the no debriefing/success group did not differ from the mean of the combined success (or failure) debriefing groups. It seems, then, that participants had rather

favorable preexisting views of their global social perceptiveness abilities, and that positive feedback did not further elevate these self-views.

#### Mood ratings

To test whether post-debriefing mood reactions engender perseverance in self-perceptions through mood-congruent processing, we first conducted a  $2 \times 4$  ANOVA on the mood index. This analysis revealed a marginally significant interaction effect ( $F(3,120)=2.41, p<.07$ ) wherein the difference between the success and failure conditions was significant only among non-debriefed participants (success  $M=6.77$ ; failure  $M=5.47$ ;  $t(120)=3.25, p<.001$ ). If affective reactions underlie the perseverance effect obtained in the standard outcome debriefing condition, participants in this group who received success feedback should have reported more positive moods ( $M=6.33$ ) than those who received failure feedback ( $M=6.42$ ), and they did not,  $t<1, ns$ . As an additional test, we reran the ANOVAs conducted on the performance estimate and specific ability measures (i.e., the measures that revealed perseverance) using mood as a covariate. Both interaction effects were maintained when mood was controlled (estimates ( $F(3,119)=53.23, p<.0001$ ); specific ability rating ( $F(3,119)=46.83, p<.0001$ )). Thus, the perseverance effect occurring on these measures does not appear to be due to affective perseverance.

#### Study 2

Study 1 revealed that a slightly revised form of outcome debriefing can be as effective as process debriefing in eliminating perseverance. When participants were informed not only that their score was false, but also that the test was not a valid measurement tool, their post-debriefing self-perceptions were uninfluenced by false feedback. In Study 2, we explored the possibility that revised outcome debriefing may be more effective than standard outcome debriefing because it preempts a pattern of thought that occurs in the latter form of debriefing. When participants receive standard outcome debriefing, they probably find themselves contemplating or ruminating about their “real score” on what they have been led to believe is a valid test. This curiosity about their real score may lead them to use their assigned score as a subjective anchor point with which to estimate their real score (Wegener et al., 1985), and the constructed score representation could lead to further thoughts that reflect explanations of the hypothetical score (Fleming & Arrowood, 1979; Ross et al., 1975). These types of thoughts would be expected to yield perseverance in self-perceptions. In contrast, in revised outcome debriefing, participants learn that the test is bogus, and thus they should be much less likely to ruminate about what their real score might be. Study 2 explored this reasoning. Recipients of failure feedback received one of three forms of debriefing: standard outcome, revised outcome, or process debriefing. Immediately after debriefing, we obtained several measures

reflecting the degree of contemplation regarding actual performance levels. We expected that recipients of revised outcome debriefing would ruminate less about their real performance than participants in the other conditions.

#### Method

##### Participants

Participants were 6 male and 24 female SFU undergraduates who received course credit.

##### Procedure

The procedure closely paralleled that used in the failure\debriefing conditions of Study 1 up to the point at which the initial debriefings were completed. At this point, the experimenter left the room for 4 min, and returned with the “thoughts and reactions questionnaire.” After a couple of filler questions, participants were informed that we were interested in the thoughts that people have after learning about our true purposes, and that they should indicate the degree to which they had certain thoughts during the past few minutes. On a scale ranging from 1 (*not at all*) to 11 (*a great deal*), they indicated the degree to which they found themselves (a) wondering about or contemplating what their real score on a test of social perceptiveness might be, (b) wondering about what their actual ability level is in the domain of social perceptiveness, and (c) thinking that the score they were given on the test might be a useful starting point for estimating their actual performance/ability on a test of social perceptiveness. They were then fully debriefed.<sup>5</sup>

#### Results

We predicted that participants in the revised outcome debriefing condition would think less about their real score or ability level than participants in the other debriefing conditions. A one-way ANOVA performed on an index reflecting the average of the three contemplation items ( $\alpha=.76$ ) revealed support for this prediction,  $F(2,27)=5.56, p<.01$ . Immediately after debriefing, recipients of revised outcome debriefing reported thinking about their score and ability level significantly less ( $M=5.16$ ) than did recipients of standard outcome debriefing ( $M=7.56$ ;  $t(27)=2.75, p<.01$ ) or process debriefing ( $M=7.76$ ;  $t(27)=2.98, p<.01$ ).

<sup>5</sup> We did not include the perseverance measures because we reasoned that they would be tainted by the previous act of completing the contemplation measures, and thus would not yield the typical perseverance effect (i.e., lower ratings in the standard outcome debriefing condition than the other conditions). We reasoned that even participants who reported thinking little about their real score during the previous few minutes (i.e., those in the revised outcome debriefing group) would be prompted to think about it after completing items that inquired explicitly about this score. Thus, the contemplation measures are reactive in the sense that completing them would likely create perseverance in most participants.

## General discussion

Prior to the publication of Ross et al.'s research in 1975, it seems likely that most social psychologists simply assumed that their debriefings were effective. After all, why would research participants who are told that they lack an important intellectual or social skill not embrace information conveying that the feedback is invalid? By demonstrating the perseverance effect, and revealing a strategy to eliminate this phenomenon, Ross and colleagues provided the field with both a "wake-up call" and a solution. In the past quarter century, process debriefing has become the most widely accepted protocol for the proper conduct of debriefings (e.g., Aronson, Ellsworth, Carlsmith, & Gonzales, 1990) and authors often mention explicitly that this is the form of debriefing that they delivered.

The current studies build upon this classic early work in several ways. First, our findings revealed that there is a form of outcome debriefing that is as effective as process debriefing in eliminating perseverance. It appears that the addition of one piece of information (i.e., information that the test is invalid or bogus), is sufficient to prevent perseverance in performance-related self-perceptions. Based on our findings, one might be tempted to suggest that researchers use revised outcome debriefing *in lieu* of process debriefing. This position is not without merit. Revised outcome debriefing appears to work because it preempts the ruminative processing that normally serves to solidify feedback-based self-perceptions. Successful process debriefing, in contrast, probably requires that participants engage in a more effortful or controlled corrective process (e.g., Wegener & Petty, 1997) wherein they adjust rumination-inspired self-perceptions in a direction that opposes the feedback. Study 2 offered some indirect support for this possibility: Participants in the process debriefing condition were as likely as those in the standard outcome debriefing condition to report ruminating about their performance; nonetheless, they managed to avoid perseverance (see Study 1). If this general reasoning is correct, it could be argued that revised outcome debriefing will be more effective than process debriefing among participants who are less motivated or able to engage in an effortful correction process. Despite this advantage, however, we believe that a "combined" debriefing would be most efficacious [i.e., one that incorporates the best features of both revised outcome (i.e., information about test invalidity) and process debriefing (i.e., information about perseverance)]. This form of debriefing would not only preempt perseverance among participants who are less inclined to engage in correction, but also provide a long-term strategy for all participants to use in the event that they experience feedback-consistent self-thoughts after leaving the laboratory. Combined debriefing does require that researchers use test items that do not actually represent valid measures of underlying ability (e.g., that they use word completions rather than GRE test items as measures of "intellectual aptitude"); however, this should not gen-

erally pose a problem because studies incorporating the false feedback paradigm rarely call for the use of validated tests of underlying abilities. Participants need only *believe* that a test measures important abilities for this paradigm to be effective.

Our research also extends Ross et al.'s research by confirming that the perseverance effect is restricted to people's perceptions of specific performances and abilities. This lack of perseverance on evaluations of global ability cannot be easily attributed to methodological factors. The global ability index included multiple items, was internally consistent, and was sensitive to negative feedback. Moreover, the cover story delivered to participants stressed that the test measured important general abilities. We can only speculate, but the absence of perseverance on this index may reflect the operation of self-enhancement or self-verification motives. Much past research has revealed that the average person processes information in a manner that confirms a generally positive stable self-view (e.g., Swann, Pelham, & Krull, 1989; Taylor & Brown, 1988). The results on the global ability index are consistent with this general finding. "Failing" participants appear to have been quite willing to relinquish negative global perceptions that were formed temporarily in reaction to the feedback. Additionally, "succeeding" participants appear to have been unwilling to modify (in response to either feedback or debriefing) the highly positive self-views that they "brought with them" to the experiment. Overall, the findings on the global ability index can be taken as "good news" for researchers who use false feedback. In an attempt to capture the strong motivational forces and threatening emotions that affect people in everyday life, researchers often portray their tests as measuring highly important and global abilities. Our results imply that when researchers use this strategy, their participants are unlikely to suffer long-term negative consequences.

Finally, our results revealed that the perseverance effect obtained in the debriefing paradigm is not due to "affective perseverance." Participants' affective reactions revealed no evidence of perseverance, and perseverance in self-perceptions occurred even when mood reactions were controlled. Thus, debriefing of any kind appears to be effective in eliminating the mood changes produced by false feedback. Although some researchers have obtained evidence for affective perseverance in other paradigms (Sherman & Kim, 2002), perseverance in the debriefing paradigm appears to be due to more cognitive (e.g., ruminative) processes.

In conclusion, our research highlights that there are several critical features of an effective debriefing in the false feedback paradigm. In particular, the distinction between "debriefing about the false nature of the feedback" and "debriefing about the false or unvalidated nature of the test itself" appears to be an important one, and we hope that future researchers will construct debriefings that reflect both of these components.

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