When Do Individuals Help Close Others Improve? The Role of Information Diagnosticity

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On the basis of the self-evaluation maintenance model (SEM; Tesser, 1988), it was hypothesized that individuals give less improving information to relationally close (rather than distant) others, out of concern for being outperformed by close others in the future. Further, this effect only occurs if diagnostic and valid criteria for success are present. Three studies confirmed the hypotheses. In Studies 1 and 2, participants gave less improving information to familiar than to unfamiliar others in a domain (academics) in which diagnostic assessment criteria (grades) were available. This pattern was not found in a domain (social life) without diagnostic criteria. These results were replicated in Study 3, in which relative performance and diagnosticity of assessment criteria were manipulated and amount of improving information given to friends and strangers was measured. Diagnosticity of comparison information is an important addition to the SEM model.

The process of self-evaluation, in which information relevant to the self is sought out or scrutinized, is motivated (Sedikides & Strube, 1995, 1997). Three motives have received the bulk of the theoretical and empirical attention: self-assessment, or the desire for accurate information about the self (Strube, Lott, Le-Xuan-Hy, Oxenberg, & Deichmann, 1986; Trope, 1975); self-enhancement, or the desire for positive information about the self (Campbell & Sedikides, 1999; Sedikides, 1993); and self-verification, or the desire for consistency between beliefs about the self, whether positive or negative, and new information about the self (Swann, 1983; Swann, Stein-Seroussi, & Giesler, 1992). Self-evaluation researchers have also recently acknowledged a fourth motive, self-improvement (Collins, 1996; Sedikides & Strube, 1997; Taylor, Neter, & Wayment, 1995): Individuals seek out information that has the potential to improve the self even when that information is inconsistent or negative.

The main focus of self-improvement research has been the types of information that individuals solicit. As an illustrative example, we note that Wayment and Taylor (1995) instructed participants to rate the usefulness for self-improvement of three types of information: objective information, which is characterized by clear assessment criteria such as grades or weight; personal standards information, which includes autobiographical memory or performance expectancies; and social comparison information. Participants regarded objective and personal standards information as more useful than social comparison information in the academic domain, whereas they regarded personal standards information as more useful than objective or social comparison information in the social domain.

One type of information that has escaped the attention of self-improvement researchers is information provided directly from other persons. Indeed, the role of others in the self-evaluation process has been largely limited to social comparison. Besides acting as comparison targets, however, individuals are often motivated to improve others, and they behave accordingly. For example, teachers may set a high comparison standard for their students, but they will also strive to improve students' knowledge of the subject matter. Likewise, parents may set a high standard of socially approved behavioral conduct, but they will also strive to improve their children's social skills.

On other occasions, though, individuals are motivated not to improve others. To begin, individuals have a general preference for downward social comparisons (Wills, 1981). Specifically, they not only opt to make downward rather than upward social comparisons after failure but also avoid social comparisons that they believe will be unfavorable to them (Brickman & Bulman, 1977; Gibbons, Persson Benbow, & Gerrard, 1994). However, in certain circumstances, upward social comparison is unavoidable. In such circumstances, we argue, individuals will cope with the inevita-

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bility and threat of an upward comparison in the future by withholding information that has the potential to improve the relative position of the other person. We test this general proposition in the current research.

The Self-Evaluation Maintenance Model

One variable that is relevant to the aforementioned proposition is the relationship between the individuals who give and those who receive the improving information. The self-evaluation maintenance (SEM) model (Tesser, 1988) serves as a springboard for theorizing about this relationship. According to the SEM model, individuals alter their reactions to social comparison in ways that maintain their positive self-regard. These reactions vary depending on the perceived quality of the individual's performance, the personal relevance of the comparison domain, and the closeness of the individual's relationship to the comparison target. When outperformed by others, individuals may respond positively (i.e., *reflection*) or negatively (i.e., *comparison*). Individuals who are outperformed by a close other in domains that they consider personally irrelevant do not perceive the upward comparison as a threat, and thus reflection occurs. Individuals do feel threatened when they are outperformed by a close other in personally relevant domains, however, and thus comparison occurs. In support of these postulates, Wheeler and Miyake (1992) reported that individuals make more upward comparisons with strangers and acquaintances than with friends in everyday social interactions, presumably because of the impending threat in upward comparisons with close others.

The majority of research on the SEM model has focused on reactions to comparisons that are made in the context of past behavior. For example, in a study by Tesser and Smith (1980; see also Tesser & Campbell, 1986) participants provided both a friend and a stranger with clues to help them solve a word identification task. When participants believed that they had not performed well on the task and if they considered the task to be personally relevant, they gave harder clues to friends than to strangers. Individuals will undermine the performance of a friend when they believe that their own past performance does not compare favorably with the friend's present performance.

It seems likely, however, that current thoughts and behavior are motivated not only by past comparisons but also by the anticipation of future comparisons. If correct, this assertion would invite an important addition to the SEM model: Individuals will withhold improving information on personally relevant domains from close (but not distant) others in an attempt to minimize the likelihood of being outperformed by close others in the future.

Testing an Addition to the SEM Model

An implicit assumption that the SEM model makes is that individuals are well aware of how their performance compares with the performance of others. However, there are situations in which relative standing is difficult to gauge. One factor that influences the accurate knowledge of relative standing is the *diagnosticity* of the comparison information. If the assessment criteria for a given comparison domain are diagnostic (e.g., grades as a measure of academic success), individuals will be able to pinpoint their relative standing easily. If, on the other hand, the assessment criteria are nondiagnostic, individuals will have trouble gauging their relative standing.

The issue of information diagnosticity has implications for the SEM model. We postulate that when highly diagnostic criteria for the assessment of the comparison domains are available, individuals will be concerned with their relative standing to close (but not distant) others. One way individuals will manifest this concern is by reducing the amount of improving information that they offer to close others. If the assessment criteria are nondiagnostic, relative performance will be obscured, and hence there will be no difference in the amount of improving information that individuals provide to close and distant others. We test this addition to the SEM model by examining the amount of improving information that individuals give to close and distant others in domains that differ in the diagnosticity of the assessment criteria.

Wayment and Taylor (1995) differentiated between two comparison domains: academic achievement and social life. They claimed that the assessment criteria in the academic domain (e.g., grades or grade point average) are objective and quantifiable, whereas the assessment criteria in the social life domain are nebulous. Given that objective and quantifiable assessment criteria are by definition more diagnostic than are nebulous assessment criteria, we hypothesize that individuals will be more knowledgeable in their evaluations of success in the academic than in the social life domain.

A second difference between the academic and social life domains lies in the *ambiguity* of the assessment criteria. Dunning, Meyerowitz, and Holzberg (1989) defined ambiguity as the number of behaviors or characteristics that are associated with a given performance domain. When the domain is unambiguous, different individuals will evaluate success on the basis of the same criteria; hence, there will be consensus for a small number of characteristics that constitute valid indicators of success. However, if the domain is ambiguous, each individual will evaluate success on the basis of his or her own idiosyncratic criteria; hence, there will be little agreement about valid indicators of success. Dunning and his colleagues reported that individuals are more accurate in their evaluations of ability when the assessment criteria are unambiguous.

The assessment criteria for academic success are high in diagnosticity, both because of the objective and quantifiable character of grades as an indicator of academic success (Wayment & Taylor, 1995) and because of the consensus that grades constitute a valid criterion of academic success (Dunning et al., 1989). In contrast, the assessment criteria for success in the social life domain are low in diagnosticity, both because of idiosyncratic indicators of success in this domain and because of the lack of consensus regarding a single, valid success criterion. On the basis of the proposed addition to the SEM model, we predict that individuals will give less improving information to close (i.e., familiar) than to distant (i.e., unfamiliar) others within the academic domain but not within the social life domain.

Pilot Study

We have claimed that there is consensus for grades being a diagnostic indicator of academic success, whereas no consensus exists regarding a single diagnostic indicator of success in the social life domain. We examined this assumption in a pilot study

in which participants reported the criteria that they would use to determine how successful others are in the academic and social life domains.

We tested 22 female and 11 male participants. As in all of the reported research, participants were introductory psychology students at the University of North Carolina at Chapel Hill. They generated questions, as many or as few as they wanted, that they believed would be useful in evaluating the quality of the academic and social life of another student. We defined *academic life* as those experiences that relate directly to a person's academic education. Participants generated questions that would be useful in determining the level of the student's academic success (i.e., how good a student this person is). We defined *social life* as a person's social experiences. Participants generated questions that would be useful in determining how successful a person's social interactions were (i.e., how good the person's social life is). The questions about the academic and social lives were generated separately, and the order of question generation was counterbalanced. The sessions lasted 15–20 min.

In Table 1 we display each question that was generated by 2 or more participants, along with the proportion of participants who generated each question. A majority of participants (.81) reported that they would ask about grades or grade point average. Thus, there was consensus that grades or grade point average, as well as effort, are diagnostic indicators of academic success. It is notable that there was no consensus regarding diagnostic indicators of social success, as no single question was generated by a majority of participants. Beyond the issue of reliability, the social questions that participants generated most often were not valid, standardized measures of social success. These results validate the use of the academic and social life domains for testing the addition to the SEM model.

Table 1
Proportion of Participants Who Generated Each Academic and Social Life Question in the Pilot Study

Question	Proportion
Diagnostic of a successful academic life	
What are your grades?	.81
How much effort do you put into your schoolwork?	.81
What is your major?	.41
What classes are you taking this term?	.38
Do you like your classes?	.25
How well do you know the campus libraries?	.22
How often do you miss class?	.19
Are you involved in any extracurricular activities?	.19
How many credit hours are you taking this semester?	.16
How well do you know your professors and TAs?	.13
How often do you go out partying on school nights?	.13
Do you ever take classes that are not required?	.13
Have you ever been tutored?	.13
Where do you normally study?	.13
Do you understand how your classes relate to the "real" world?	.09
Do you tend to procrastinate about things?	.06
Do you have any nonneed based scholarships?	.06
Diagnostic of a successful social life	
Are you in a fraternity or sorority?	.47
What do you enjoy doing in your free time?	.35
How many close friends do you have?	.35
Do you have a boyfriend or a girlfriend?	.35
How often do you go out with friends?	.31
What do you do on weekends?	.28
Do you consider yourself to be shy or to be outgoing?	.28
Are you happy?	.19
How often do you start conversations with strangers?	.19
Do you drink or smoke?	.19
Would you rather be in a small group or a large group?	.19
How many casual friends and acquaintances do you have?	.16
How often do you talk on the telephone?	.16
Would you rather talk to someone or send them an e-mail?	.13
How often do you instigate social events?	.09
How easy is it for you to make friends?	.09
How often do you feel lonely?	.09
Where are you from?	.09

Note. TA = teaching assistant.

Study 1

Participants in Study 1 recalled one of four types of interaction: interactions with familiar or unfamiliar others related to their academic lives, and interactions with familiar or unfamiliar others related to their social lives. They also rated the amount of improving information that they had given to others in these interactions.

We predicted a Familiarity × Domain interaction. Within the academic life domain, we expected that participants would give less improving information to familiar than to unfamiliar others. Within the social life domain, we expected that participants would not differ significantly in the amount of improving information that they give to familiar and unfamiliar others.

Method

Participants, Procedure, and Materials

We tested 80 female and 40 male participants. The number of participants per experimental session ranged from 4 to 20. The typical session lasted approximately 30 min.

The procedure involved two separate stages. In the first stage, participants recalled and summarized one of four types of interactions from their past, crossing two independent variables. One independent variable was the domain of the interaction to be recalled. In the academic life domain, participants recalled interactions that were related to their education. Academic life was defined as any experiences that the participant or the other person had that were related directly to school. Examples include studying with others and discussions about which classes to take in the future. In the social life domain, participants recalled social interactions with others. Social life was defined as any experiences that the participant or the other person had in social situations. Examples include discussions about the personalities of the individuals and discussions of social plans for the upcoming weekend.

The second independent variable was familiarity with the other person. In the familiar condition, participants recalled interactions with persons whom they knew very well, such as close friends, family, or romantic partners. In the unfamiliar condition, participants recalled interactions with

persons whom they did not know well, such as acquaintances or casual friends.

Participants were given 10 min to recall and summarize as many interactions as possible. Participants were asked to provide brief, general descriptions of the nature and location of each interaction. Next, participants added the following information for each interaction: gender of the other person, relationship with the other person (stranger, acquaintance, casual friend, close friend, romantic partner, spouse, parent, or other relative), and the other person's experience in college (current college student, former college student, never attended college, or unknown). Subsequently, participants provided ratings for the following questions, all on a scale ranging from 1 (*low*) to 7 (*high*): familiarity with the other person, importance of the interaction to participant, and amount of improving information given to the other person. Information was defined as improving when the participant regarded it as likely to better one or more aspects of the other person's academic or social life.

Results

Description of the Recalled Interactions

Participants in the familiar/social condition summarized an average of 11.53 interactions. Examples are "Talking with my girlfriend about our trip" and "Talking with my Mom about her temper tantrums when we go out." Participants in the familiar/academic condition summarized an average of 7.70 interactions. Examples are "Studying with my roommate" and "My sister telling me what classes we should take next semester." Participants in the unfamiliar/social condition summarized an average of 8.83 interactions. Examples include "This boy from class asked me to go out with him on Friday" and "We had breakfast with another girl in my sorority." Finally, participants in the unfamiliar/academic condition summarized an average of 6.30 interactions, with examples including "A bunch of us from Astronomy class complaining about our professor" and "Some guy borrowed my psychology notes."

The proportion of interactions that involved college students was similar across conditions. Within the academic life domain, an average of 85.7% of the interactions with familiar others and 84.6% of the interactions with unfamiliar others were with current or former college students. Within the social life domain, 84.2% of the interactions with familiar others and 85.2% of the interactions with unfamiliar others were with current or former college students.

Data Analysis Strategy

For each participant, we computed the mean amount of improving information given to the other person. This was the principal dependent measure. We also computed the mean level of importance of the interaction to the participant.

College students naturally have interactions with professors, teaching assistants, or tutors. Given the status differential, it is rather unlikely for participants to contribute academic improving information in these situations. Similarly, interactions in which participants acted as tutors very likely involve giving academic improving information. To guard against the possibility that these types of interactions are responsible for the relative differences between the academic and social life domains, we removed such interactions from the data set. This practice resulted in the removal of 3.1% of interactions in the academic domain and 0.3% of interactions in the social domain.

Manipulation Check

To test the effectiveness of the familiarity manipulation, we conducted a 2 (familiarity) × 2 (domain) × 2 (gender) analysis of variance (ANOVA) with the mean familiarity rating as the dependent measure. The familiarity main effect was significant, $F(1, 112) = 324.31$, $p < .001$. The reported level of familiarity was indeed higher in the familiar ($M = 6.34$) than in the unfamiliar ($M = 3.57$) condition. No other effects reached significance.

Gender Effects

We tested for gender effects by conducting a 2 (familiarity) × 2 (domain) × 2 (gender) ANOVA for both dependent measures. The gender main effect was significant, $F(1, 112) = 4.78$, $p < .032$. Women ($M = 3.78$) gave more improving information than did men ($M = 3.39$). However, no interactions involving gender reached significance. Thus, we excluded gender from subsequent analyses.

Importance of the Interactions

The importance of the interaction to the participant emerged as a significant predictor of the amount of improving information given, $F(1, 118) = 28.84$, $p < .001$. However, we obtained no interactive effects between interaction importance and the two principal independent variables (domain and familiarity). Thus, we did not include interaction importance as a factor in subsequent analyses. Because of its theoretical relevance in the SEM model, however, we included interaction importance as a covariate.

Giving Improving Information

We conducted an analysis of covariance with domain and familiarity as factors and interaction importance as a covariate. We display in Table 2 both the adjusted means and the results of simple-effects tests. The domain main effect was significant, $F(1, 115) = 23.36$, $p < .001$. Participants gave more social than academic improving information. The familiarity main effect was not significant.

The predicted Familiarity × Domain interaction was significant, $F(1, 115) = 5.82$, $p < .018$ (Table 2). Within the academic domain, participants gave significantly less improving information to familiar than to unfamiliar others. Within the social domain,

Table 2
Adjusted Means for the Amount of Improving Information Given by Participants in Study 1

	Improving information	
Familiarity with other	Academic	Social
Familiar	3.03_a	4.22_c
Unfamiliar	3.48_b	3.87_{bc}

Note. Means that do not share a subscript are significantly different ($p < .05$).

however, there was not a significant difference in the amount of improving information that participants offered to familiar and unfamiliar others.

We also tested a second set of contrasts comparing differences between the two domains. As predicted by the addition to the SEM model, participants provided significantly less improving information to familiar others in the academic than in the social domain. However, participants did not differ in the amount of academic and social improving information that they gave to unfamiliar others.

Discussion

The results of Study 1 are consistent with the hypotheses. Within a domain that contains a diagnostic indicator of success (academic life), participants reported giving less improving information to familiar than to unfamiliar others. However, within a domain that does not contain a diagnostic indicator of success (social life), there were no significant differences in the amount of improving information that participants provided to familiar and unfamiliar others. Also, participants offered less academic than social improving information to familiar others, whereas no such difference was evident in the case of unfamiliar others.

Nevertheless, there are alternative explanations for the findings. One possibility is that individuals may not know enough specific details about the academic life of familiar others or the social life of unfamiliar others to offer improving information. Academic interactions with casual friends and acquaintances such as classmates may involve the details of academic work, resulting in a higher level of knowledge of the person's specific academic needs. Similarly, social interactions with close friends, family, and romantic partners may involve details of a person's social life and a better understanding of this person's social needs. It may be that individuals only give improving information if they are aware of the specific needs of the other person. We examine the relation between improving information offered and domain-specific knowledge in Study 2.

Another criticism of Study 1 is that it relies on the recall of interactions. Delayed recall of everyday interactions can differ from recall that is based on on-line impressions of interactions and follows shortly thereafter (Pemberton, Insko, & Schopler, 1996). Finally, a selection bias in the recalled interactions may also have been present. To avoid these potential pitfalls, we had participants in Study 2 record information about their daily interactions in an on-line fashion.

Study 2

In Study 2, participants kept records, for 6 days, of the amount of academic and social improving information that they gave to others in daily interactions. This methodology allows for a within-subject replication of the results of Study 1.

Method

Participants and Materials

We tested 13 male and 10 female participants. They completed a modified version of the Rochester Interaction Record (RIR; Reis & Wheeler, 1991) for every eligible interaction during the 6 days of the study (Appendix A). Participants completed one record sheet for each interaction. The record sheets were bound into a booklet, with each booklet containing 20 sheets.

Each record sheet began with information about the date, time, and length of the interaction. Participants then judged whether the interaction was related to the academic lives of either themselves or the other person. If an interaction was related to the academic domain, participants answered the two questions that followed. In the first of these questions, participants reported the amount of improving information that they had given to the other person in that interaction. In the second question, participants rated their knowledge of the other person's academic life. Next, participants responded to the same questions about the social life domain. In the initial training session, the experimenter explained that a given interaction could be considered relevant to one domain, both domains, or neither domain. The presentation order of the questions was counterbalanced.

Participants then reported the following information about the interaction: gender of the other person, relationship to the other person, the other person's experience in college, the participants' familiarity with the other person, the participants' liking of the other person, and personal importance of the interaction. The record sheet ended with a brief description of the interaction.

Procedure

Participants attended a 45-min training session on the day before recording began. The experimenter explained that the participants' role was to record information about (a) each interaction that they had over the next 6 days that lasted at least 8–10 min and (b) shorter interactions that they judged to be important. He then distributed the interaction record booklets and explained each step of the recording process. He asked participants to complete one interaction record sheet as soon as possible after each eligible interaction. Detailed instructions for the recording of the interactions were included on the inner covers of the booklet. Giving improving information was defined as in Study 1. The experimenter stressed the importance of accurate and prompt interaction recording, and he attempted to motivate participants (by highlighting the expertise that participants would gain about human interaction) and make them feel like active contributors to the project (by soliciting feedback and encouraging them to contact him with questions or suggestions).

Every 2nd day, participants returned completed booklets. The experimenter examined these booklets and contacted participants as needed to clarify ambiguities. He also sent electronic mail messages to participants each day inquiring about outstanding issues and reminding them of the experimental task. At the end of the 6-day recording period, participants attended a 30-min debriefing session in which they completed a postexperimental questionnaire designed to assess the accuracy of record keeping. To ensure the accuracy of these measures, we asked participants to complete them after they had received their credit for study participation.

Results

Description of the Recorded Interactions

Participants recorded an average of 37.7 interactions (range = 18–82) over the course of the study. Of those interactions, participants judged that 35.3% were relevant to the academic domain and 76.3% were relevant to the social domain. Participants judged 23.2% of the total number of interactions to be relevant to both domains. Finally, participants judged that 11.6% of interactions were not relevant to either domain; we excluded these interactions from the analyses.

In the postexperimental questionnaires, participants estimated that they recorded an average of 79.5% of the eligible interactions over the course of the study and that the average length of time

between the completion and the recording of the interactions was 62 min. All participants reported that they believed the data in their record books to be an accurate summary of their interactions.

Data Analysis Strategy

In an effort to create two familiarity groups analogous to the experimental conditions of Study 1, we divided the interactions on the basis of familiarity with the other person. The familiar group included interactions with familiarity ratings of 6 or 7, whereas the unfamiliar group included interactions with ratings of 5 or below. The familiar group consisted of 59.4% of the total interactions recorded, with a mean rating of 6.70. The unfamiliar group comprised the remaining 40.6% of the interactions, with a mean rating of 3.89.

To create two groups for the interaction domain, we used participants' judgments of whether each interaction was related to the other person's or to their own academic and social lives. Within the academic domain, 92.6% of the familiar interactions were with a current or former college student, as were 96.2% of the unfamiliar interactions. Within the social domain, 92.2% of the familiar interactions were with a current or former college student, as were 90.8% of unfamiliar interactions. As in Study 1, we removed interactions that involved instructors (professors, teaching assistants, or tutors) from the data set. This practice led to the elimination of 1.4% of the interactions. One participant did not record any academic interactions with familiar others and thus was not included in the analyses.

For each participant, we computed the mean amount of improving information that the participant gave to the other persons within both the academic and the social domain. This rating was the main dependent measure. We also computed (separately for interactions with familiar and unfamiliar others) the mean rating of each participant's domain-specific knowledge of the other persons, the importance of the interaction to the participant, and the participant's liking of the other persons.

Gender and Order Effects

We assessed gender and order effects by conducting a 2 (familiarity) × 2 (domain) × 2 (gender) × 2 (order) mixed ANOVA with familiarity and domain as within-subject factors. The mean amount of improving information given served as the dependent measure. No significant main effects or interactions involving gender or order of presentation emerged. Consequently, we omitted gender and order from further consideration.

Potential Covariates

To determine whether the participants' ratings of interaction importance and their liking of the other persons should be included as covariates, we tested the relations between importance and amount of improving information given and between liking and amount of improving information given within each of the four conditions created by crossing the domain and familiarity variables. No statistically significant relations emerged between improving information given and either importance or liking. Thus, we excluded these variables from subsequent analyses.

Giving Improving Information

We conducted a 2 (domain) × 2 (familiarity) repeated measures ANOVA on amount of information given (top panel of Table 3). The main effects were not significant. The predicted interaction effect, however, was significant, $F(1, 21) = 10.28$, $p < .005$.[1] As in Study 1, participants gave less improving information to familiar than to unfamiliar others in the academic domain. Contrary to our Study 1 findings, participants gave more improving information to familiar than to unfamiliar others in the social domain. We revisit this contradictory finding shortly.

When interacting with familiar others, participants offered significantly less academic improving information than social improving information. There was no significant difference in the amount of academic and social improving information given to unfamiliar others.

Amount of Domain-Specific Knowledge

The amount of knowledge that participants had of the other person's academic life was a significant predictor of the amount of academic improving information given to both familiar others, $F(1, 17) = 5.51$, $p < .032$, and unfamiliar others, $F(1, 17) = 9.09$, $p < .008$. To test whether the reported findings for amount of improving information given are accounted for by the amount of domain-specific (i.e., academic) knowledge for the other person, we repeated the main analyses using the amount of domain-specific knowledge as a covariate. We conducted these analyses using hierarchical linear modeling.

The familiarity main effect was significant, $F(1, 62) = 4.75$, $p < .034$ (bottom panel of Table 3). Participants gave less improving information to familiar than to unfamiliar others. The domain main effect was not significant. The predicted interaction effect remained significant, $F(1, 62) = 11.36$, $p < .002$. Participants gave more improving information to familiar than to unfamiliar others in the academic but not the social domain. As a reminder, we note that we had obtained a statistically significant difference in the social life domain (i.e., participants appeared to give more improving information to familiar than to unfamiliar others) before controlling for the amount of domain-specific knowledge.

These results suggest that the amount of domain-specific knowledge does not mediate the effects within the academic domain. In fact, the mean differences in the amount of academic improving information given to familiar and unfamiliar others increased after we controlled for domain-specific knowledge. However, the amount of domain-specific knowledge did mediate the effect within the social domain. It may be that in domains in which diagnostic assessment criteria are available, individuals give improving information on the basis of their general familiarity with other persons. When diagnostic criteria for success are unavailable, however, individuals give improving information on the basis of specific knowledge of other persons.

[1] We also tested this interaction effect using hierarchical linear modeling. The results were very similar to those of the ANOVA, $F(1, 63) = 10.77$, $p < .002$.

Table 3
The Unadjusted and Adjusted Mean Amount of Improving Information Given by Participants in Study 2

Familiarity with other	Improving information	
	Academic	Social
Unadjusted means		
Familiar	2.90_a	3.68_c
Unfamiliar	3.41_b	3.08_{ab}
Adjusted means[a]		
Familiar	2.64_a	3.22_b
Unfamiliar	3.79_b	3.42_b

Note. Within each set of means (unadjusted and adjusted), means that do not share a subscript are significantly different ($p < .05$).
[a] Means from hierarchical linear modeling analyses, adjusted for the amount of domain-specific information.

Discussion

The results of Studies 1 and 2 are generally consistent with the addition to the SEM model. Furthermore, the results of Study 2 suggest that the amount of domain-specific information about familiar and unfamiliar others does not qualify as a rival hypothesis.

Nevertheless, other rival hypotheses remain. One such hypothesis is that norms dictate what individuals should discuss with familiar and unfamiliar others. When individuals are interacting with familiar others, social lives may be the prevalent discussion topic, and perhaps people view helping interactants improve socially as normative. In contrast, when individuals from our targeted population (college students) interact with unfamiliar others, talk about academic progress may be a prevalent discussion topic, and perhaps they view helping interactants improve academically as normative. Another rival hypothesis involves the history of interactions with familiar others. Participants might have exhausted the improving information that they had to offer to familiar others in previous interactions. One objective of Study 3 is to test the addition to the SEM model in a way that rules out the above hypotheses.

The SEM model focuses on situations in which individuals believe that other persons have outperformed them. Consequently, the processes that we propose in the addition to the SEM model may only occur when individuals interact with a person who they believe has outperformed them. We tested this possibility by manipulating the relevant variables (i.e., familiarity, diagnosticity of assessment information, comparison information) in a laboratory setting.

Study 3

Participants in Study 3 completed a bogus test that ostensibly measured their verbal creativity. We manipulated the perceived diagnosticity of the test and participants' relative performance compared with that of another person. Then we gave participants the opportunity to share improving information with this person, who was either a friend or a stranger.

Method

Participants, Procedure, and Materials

We tested 96 female participants. All participants reported to the laboratory with a female student whom they considered to be a good friend. We tested either two or three sets of friends in each session.

Participants reported to a suite with three rooms on each side, divided by a common area. Each participant sat in a different room. The experimenter asked participants to print their first and last name on the outside of a large manila envelope and collected the envelopes.

The experimenter told participants that the study was on verbal creativity, "the combination of vocabulary skills and the capacity to seek out novel and creative solutions to verbal problems." Verbal creativity was purported to be a recently discovered skill under intense empirical scrutiny by psychologists and educators. In an effort to increase the personal relevance of verbal creativity for participants, the experimenter stated that (a) verbal creativity is a good predictor of some types of academic and nonacademic performance, (b) graduate admissions tests such as the Law School Admission Test, Graduate Record Examination, and Graduate Management Admission Test are likely to include a measure of verbal creativity in the near future, and (c) many large companies are considering the addition of a measure of verbal creativity to standard job-screening and worker placement tests. The experimenter explained that the reason participants reported to the laboratory with a friend was that the ongoing evaluation of verbal creativity tests requires the comparison of scores of test takers who know each other well.

Next, the experimenter informed participants that they would take the (fabricated) Briggs–Alston Verbal Creativity Test. At this point, we introduced the diagnosticity manipulation. The experimenter instructed participants in the high-diagnosticity condition that this test had been pretested on a sample of 15,000 college students and had been shown to be a valid and reliable indicator of verbal creativity in all previous studies. He told participants in the low-diagnosticity condition that the test was new and had not been pretested or standardized, so it was unclear whether it was a good indicator of verbal creativity.

The experimenter then stated that following test completion, participants would be involved in a training session designed to help them improve their scores on verbal creativity tests. Participants would then take a second version of the test. Each participant would also share information about the training session with another participant. The experimenter would determine randomly whether that person would be the friend or a stranger.

The experimenter then distributed the test, which consisted of a six-page packet. As was the case with all handouts distributed to participants, the top of these pages contained the letterhead of the "Academic Testing Institute: A Division of the National Education Council." The first page consisted of instructions, and each of the following five pages had a target word at the top. The target words were *subterranean, discriminatory, parenthetical, prestidigitator,* and *interpolation.* The participants' task was to generate and write down other words that could be spelled from the letters of the target word. Each generated word had to have at least three letters, and no single letter could be used more times than it appeared in the target word. The participants were given 3 min to generate and write these words for each of the target words.

Following test completion, there was a 10-min break for scoring, during which the experimenter entered the words that had been generated by the participants into a computer. This practice was intended to increase the believability of the test scores that we gave to participants. Next the experimenter distributed a two-page handout containing the test results. The first page, which the experimenter read aloud, contained a restatement of the diagnosticity manipulation as well as instructions for interpretation of the score or scores on the following page. The second page contained the participant's score and, in some conditions, the score of another person. In the latter case, half of the participants received the friend's score, whereas the other half received a stranger's score. The scores were identified on the

sheet by the participants' first and last name, as written on the manila envelope at the beginning of the session.

These scores constitute the comparison information manipulation. All participants read that their score was in the 76th percentile. One group of participants read that the other person scored in the 91st percentile, whereas a second group read that the other person scored in the 61st percentile. A third group of participants received only their own score.

The experimenter then reminded participants that they would complete a second verbal creativity test, which would be preceded by a brief training session designed to improve scores. He stated that there were two types of training sessions; half of the participants would learn how to identify as many words as possible, and half would learn how to identify individual words that scored high on the test. Participants would then share some of this information with another person who had received the different training session. Participants who had received the score of another person on the first test would share information with that same person. The experimenter told the rest of the participants that he would randomly determine whether they would share information with the friend or a stranger.

In actuality, all participants received the training session that was supposedly designed to help them identify words that score high on the test. This session took the form of a two-page packet. First, participants read that they would be presented with 10 pieces of information that could help them improve their scores on the second test. They read the following text:

> You will notice that the pieces of information are rank-ordered. Previous research has found that reading the piece of information ranked #1 was the most helpful in improving scores on this test, whereas reading the piece of information ranked #10 was the least helpful. In other words, all of these contain information that can help to improve your score, but the piece of information ranked #1 can affect it the most, whereas the piece of information ranked #10 can affect it the least.

Following the instructions were the 10 pieces of rank-ordered information (Appendix B). A target word was given as well, and each item included several additional words that could stem from that target word; the additional words helped to illustrate the information given in each item. The pieces of information that were ranked low were those that pilot participants judged to be plausible suggestions that they had not considered when completing the verbal creativity test. The participants studied these pieces of information for 5 min.

At the end of this period, the experimenter went into each participant's room and distributed an 11-page packet. The 1st page contained the target word, and the following 10 pages contained the 10 pieces of information in the proper rank order, excluding the numbers that indicated the rank ordering. The experimenter also distributed to each participant one of the manila envelopes that contained the name of another person. Half of the participants received the envelope with their friend's name, and half received the envelope with a stranger's name. Those participants who had received the score of another person after the first test received the envelope containing the name of that same person.

The experimenter then reminded participants that there had been two different types of training sessions and that they would now share some of the information from their training session with another person who had received the alternative session. Participants were to go through the 10 pieces of information again, pick the 3 that they wished to give to the other person, and find those 3 pieces of information in the packet that they had received. They would need to put those 3 pieces of information in the envelope that contained the name of the other person. The experimenter would collect the pieces of information and give them to the other person whose name was written on the front of the envelope. Participants would study this information before the second test began. Next the experimenter mentioned to participants, with the exception of those in the no-comparison information condition, that they would receive their own score and the score of the other person after the second test. Finally, he told all participants that they would not meet with the other person at any time to discuss their scores or the information from the training sessions. The participants had 5 min to select the 3 pieces of information and place them in the envelope.

Subsequently, the experimenter gathered the envelopes and explained that he would like the participants to answer some questions about the test and the training session before they began the second test. The participants completed three questionnaires. Items on the first questionnaire included ratings of the importance of verbal creativity as an intellectual trait, the accuracy of the test as a measure of verbal creativity, their certainty that their score on the first test was an accurate reflection of their verbal creativity, their satisfaction with their score on the first test, and the likelihood that they would take a verbal creativity test in the future. Items on the second questionnaire included ratings of how much they thought the training session would improve their score on the second test, how much they thought the information that they shared with the other person would improve their score on the second test, and an open-ended question about how they decided on the three pieces of information to give to the other person. Items on the third questionnaire included an open-ended question asking how long participants had known the other person as well as ratings of how well they knew the other person and the probability that they would interact with this person in the future. All of the close-ended questions used a scale from 1 (*low*) to 11 (*high*). Detailed and individual debriefing followed.

Independent and Dependent Variables

We used three independent variables: familiarity (friend, stranger), diagnosticity of assessment information (low, high), and comparison information (participant outperformed other, other outperformed participant, no comparison information). The dependent measure was the mean rank order of the pieces of information that participants shared with the other person.

Predictions

We predicted that the mean rank order of the information that participants gave to the other person would be higher for friends than for strangers in the high-diagnosticity condition but not in the low-diagnosticity condition. Furthermore, we predicted that this difference would only be present when the other person outperformed the participant on the first test.

For participants who believed that they had outperformed the other person on the first test, we expected no differences in the rank order of the information that participants gave to friends and strangers in either the high- or the low-diagnosticity conditions. According to the SEM model and the proposed addition, participants should be concerned about the results of comparisons with a close other more than comparisons with a distant other only when they believe that they have been outperformed by the other in the past. Furthermore, the reflexive effect reported by Beach et al. (1998)—that individuals experience negative affective reactions when they outperform close others—has been found only with married heterosexual couples. Given that the participants were same-sex friends, there was no basis for predicting that participants would give more improving information to a friend than to a stranger when they believed that they had outperformed the other.

Finally, we expected no differences in the rank order of the information given to friends and strangers in the case in which participants received no comparison information. The addition to the SEM model, like the SEM model itself, is dependent on individuals comparing themselves with others. When participants received only their own score on the first test and expected to receive only their own score on the second test, they would have no basis for drawing comparisons to others.

Results and Discussion

Data Analysis Strategy

In Studies 1 and 2, the main analysis was a comparison of (a) the amount of improving information given to close and distant others in a domain in which there was a theoretical reason to expect a difference (i.e., academic life) with (b) the amount of improving information given to close and distant others in a domain in which there was no theoretical reason to expect a difference (i.e., social life). We followed the same strategy in Study 3.

However, in Study 3 there were five conditions in which we expected no differences between friends and strangers. For this reason, the crucial test was an interaction effect in which the difference between friends and strangers in the other-outperformed-participant/high-diagnosticity condition was compared with the average difference between friends and strangers in the other five conditions. In effect, this analysis treats as controls the five conditions in which no differences were expected, and it compares them with the one condition in which there was a theoretical reason to expect a difference between friends and strangers. (For justification of such planned comparisons, see Hays, 1973, p. 582; Keppel, 1973, p. 90; Winer, 1971, p. 384.) We also conducted follow-up analyses to test for differences in the amount of improving information given to friends and strangers within these five control conditions.

Manipulation Checks and Covariates

Diagnosticity. We examined the effectiveness of the diagnosticity manipulation in two ways. Participants rated the accuracy of the test as a measure of verbal creativity as well as their certainty that their score on the first test was an accurate reflection of their verbal creativity. We conducted a multivariate analysis of variance (MANOVA) on these two measures, using all three independent variables (familiarity, diagnosticity, and comparison information) as factors. The only significant multivariate effect was for diagnosticity, multivariate $F(2, 82) = 7.53$, $p < .001$. The univariate tests were significant for both accuracy, $F(1, 84) = 9.32$, $p < .003$, and certainty, $F(1, 84) = 11.61$, $p < .001$. Participants in the high-diagnosticity condition rated the accuracy of the test higher ($M = 7.04$) than did those in the low-diagnosticity condition ($M = 5.90$). Participants also reported more certainty in the high-diagnosticity condition ($M = 6.63$) than in the low-diagnosticity condition ($M = 5.29$).

Familiarity. We conducted three checks for familiarity. Participants reported the length of time that they had known the other person, their familiarity with the other person, and the probability that they would interact with the other person in the future. The multivariate effect for familiarity was significant, multivariate $F(3, 81) = 525.79$, $p < .001$. The univariate test for length of time was significant, $F(1, 83) = 16.18$, $p < .001$. Participants in the friends condition had known the other person for an average of 25.92 months (range = 1–180 months, $Mdn = 9.5$ months), compared with 0.07 months (range = 0–1 month, $Mdn = 0$ months) for those in the strangers condition. The univariate test for familiarity was also significant, $F(1, 83) = 715.86$, $p < .001$. Participants in the friends condition were more familiar with the other person ($M = 8.50$) than were those in the strangers condition ($M = 1.23$). Likewise, the univariate test for likelihood of future interactions was significant, $F(1, 83) = 1{,}363.49$, $p < .001$. Participants were more likely to interact with the other person in the future in the friends condition ($M = 10.31$) than in the strangers condition ($M = 1.72$).

Personal relevance. One goal of the procedure was for participants across all experimental conditions to view verbal creativity as having high personal relevance. We tested this notion using participants' ratings of the importance of verbal creativity and the likelihood that they would take a verbal creativity test in the future. We conducted a MANOVA on these two measures using all three independent variables as factors and found no significant effects. Additionally, we obtained no significant univariate effects for either measure. It is notable that the mean for the personal importance measure was 7.96, which is significantly higher than the scale midpoint, $t(95) = 12.71$, $p < .001$. The mean for the likelihood measure was 6.11, which is also significantly higher than the scale midpoint, $t(94) = 2.27$, $p < .025$. These results suggest that the test was personally relevant to participants and that personal relevance was uniform across experimental conditions.

To assess whether the questions regarding personal importance and likelihood of taking future tests should be used as covariates in subsequent analyses, we tested the association between these variables and the mean rank order of the information that participants selected to give to the other person. There was a significant association for the importance question, $F(1, 92) = 7.96$, $p < .006$, but not for the likelihood question. There were no significant interactions between the importance question and any of the three independent variables. For this reason, we used the importance question as a covariate in subsequent analyses.

Ratings of expected improvement. We have argued that the rank order of the information that participants chose to share with the other person was an indication of how much they wanted the other person's score to improve on the second test. Hence, there should be a negative correlation between the mean rank order of the information given and the participants' rating of how much they expected the information would improve the other person's score. This was indeed the case, $r = -0.44$, $p < .001$.

Improving Information Given

We present the mean rank orders of the information that the participants selected to give to the other person in Table 4. As discussed earlier, the crucial test of our hypothesis is the difference between the mean rank order of the information given to

Table 4
Mean Rank Order of the Information Given to the Other Participant in Study 3

Familiarity with other	Other outperformed participant		Participant outperformed other		No comparison information	
	High	Low	High	Low	High	Low
Friend	4.07_a	3.01_a	2.37_a	2.55_a	2.90_a	2.81_a
Stranger	2.65_b	3.15_a	2.98_a	3.45_a	3.34_a	3.47_a

Note. Within each column, means that do not share a subscript are significantly different ($p < .05$). *High* and *low* refer to diagnosticity.

friends and strangers in the other-outperformed-participant/high-diagnosticity condition compared with the average of that same difference across the remaining five conditions. To assess whether collapsing across those five conditions is justified, we first tested the interaction between familiarity and the five-level control factor. This effect was not significant. Furthermore, there were no significant differences in the rank order of the information given to friends and strangers within any of the five control conditions. These results suggest that collapsing across those conditions is justified.

We tested the interaction between familiarity and the two-level variable that resulted from combining the five control conditions. This effect was significant, $F(1, 90) = 10.76$, $p < .002$. As predicted, the information that participants gave to friends was ranked higher than the information that they gave to strangers in the other-outperformed-participant/high-diagnosticity condition. As mentioned earlier, there were no differences in rank order of the information given to friends and strangers in any of the remaining five conditions. When these five conditions were combined, however, the difference between friends and strangers was significant, such that the information that participants gave to friends was ranked lower than the information that they gave to strangers.

These data provide further support for the addition to the SEM model. Participants gave less improving information to friends than to strangers, but only when (a) they believed that they had been outperformed by the other and (b) the comparison information was a diagnostic indication of the trait in question.

General Discussion

Summary of Theoretical Orientation and Empirical Findings

The theoretical contribution of this research rests in the addition to the SEM model (Tesser, 1988) that we introduced. The two new key constructs of the addition involve the level of diagnosticity of information that individuals are willing to share with others (i.e., are individuals equally likely to share low- and high-diagnosticity information?) and the temporal reference of this information (i.e., are individuals concerned with the possibility that the information they share with another person may lead them to be outperformed by this person in a future occasion?).

We proposed that individuals are less likely to offer improving information to a close (as opposed to a distant) other, out of concern with being outperformed by the close other in the future. Specifically, we hypothesized that withholding improving information from a close other is evident in domains (such as academic life) in which diagnostic assessment criteria are available (i.e., grades) but not in domains that are characterized by the relative absence of diagnostic success criteria (e.g., social life).

We empirically evaluated the addition to the SEM model in three studies. We tested participants' recall of interactions in Study 1 and participants' recording of current interactions in Study 2. Within the academic life domain, participants gave less improving information to familiar than to unfamiliar others. However, within the social life domain, participants did not differ in the amount of improving information that they offered to familiar and unfamiliar others. In Study 3 we proceeded with manipulating the relevant variables, including information diagnosticity, in a laboratory setting. Participants gave less improving information to friends than to strangers, but only when they believed that they had been outperformed and that the comparison information was highly diagnostic. The manipulation of the key variables in a laboratory setting provides direct evidence for the specific mechanisms that we have hypothesized in the addition to the SEM model.

Reconciling Studies 1 and 2 With Study 3

In Studies 1 and 2 we did not assess participants' beliefs about how they compared with their interactants in the academic or social life domain. Of course, many participants likely held such beliefs. Furthermore, participants probably believed that they compared favorably with some of the interactants and compared unfavorably with others. This possibility presents a contradiction. Why were the results of Studies 1 and 2 replicated in Study 3 only when participants believed that they compared unfavorably with the other person?

One resolution for the above issue is the likelihood that participants in Studies 1 and 2 had a disproportionate number of interactions with persons whom they believed to be more successful than themselves. The self-improvement literature (Collins, 1996; Sedikides, 1999; Taylor et al., 1995) is generally consistent with this resolution: Individuals often realize that they have weaknesses in certain domains and strive to improve themselves by seeking interactions with those who have demonstrated competence in these domains. Within the context of Studies 1 and 2, participants may have attempted to improve their academic and social lives in the future by seeking interactions with others who they knew had been successful in the past. This resolution needs to be empirically evaluated in future research by assessing participants' beliefs about the success of their interactants within the relevant domains, as well as the amount of improving information that participants received during the interaction.

Other resolutions of the contradiction originate in methodological differences between the studies. Unlike the social interactions in Studies 1 and 2, the interactions in Study 3 were between participants situated in separate rooms who never saw or spoke directly to each other. Perhaps the direct contact (e.g., nonverbal cues, tone of voice) with the other persons in Studies 1 and 2 led to some degree of threat from relative comparisons across all situations, whereas threat was only present in Study 3 when the participants were informed that they had been outperformed. Still another resolution involves the inclusion in Study 3 of conditions in which participants received no comparison information at all. By eliminating the possibility of relative comparisons in these conditions, we also eliminated the possibility of threat that could stem from these comparisons for one third of the sample. In addition, another third of the participants in Study 3 were explicitly told that they had outperformed the other on the first test. Assuming that this information lowers the threat that results from perceived unfavorable comparisons, two thirds of the sample in Study 3 experienced little or no threat. Of course, it is likely that participants in Studies 1 and 2 were involved in interactions for which they had no comparison information as well as interactions for which they knew that they had outperformed the other in the past. However, we believe that such interactions were not as plentiful as in Study 3.

One way to examine the consistency of findings among the three studies is by testing whether the two-way interaction pattern obtained in Studies 1 and 2 was also present in Study 3. The differences among the studies mentioned above suggest that the Diagnosticity × Familiarity interaction may not be significant in Study 3, but a consistent pattern of means across all three studies would support the essential continuity of this investigation. We conducted a full factorial ANOVA on the data from Study 3. Though the Domain × Familiarity interaction was not significant, $F(2, 82) = 2.12$, $p < .15$, the pattern of means was indeed consistent with the pattern in Studies 1 and 2. In the high-diagnosticity conditions, the mean rank order of the information that participants shared was descriptively higher for friends ($M = 3.11$) than for strangers ($M = 2.99$). However, in the low-diagnosticity conditions, this pattern was reversed ($M = 2.79$ for friends, and $M = 3.36$ for strangers).

Implications

Our research adds to the SEM model in two critical ways. First, we have demonstrated that the theoretical tenets of the model need to be qualified. Processes predicted by the SEM model hold true only when the comparison information is high in diagnosticity. Second, we have shown that the psychological processes and behaviors specified by the SEM model are not limited to beliefs about past performance but can also emerge when individuals have an eye toward future performance. Follow-up research in this tradition will need to address how an individual's current behavior can be motivated by the anticipation of future comparison with others.

One variable that follow-up research will need to consider is the personal relevance of the informational domain. The SEM model specifies that the threat from unfavorable comparisons with close others occurs only when the domain in question is personally relevant to the individual. Our objective in Study 3 was to find out whether the diagnosticity of the comparison information affects this threat, and as a result we kept personal relevance at a high level in all experimental conditions. Future research will need to further validate our addition to the SEM model by manipulating personal relevance of the information.

Our investigation has implications for the self-evaluation literature. Previous empirical work on the self-improvement motive has included other persons only as targets of comparison. Instead, we conceptualized others as agents in the self-evaluation process. Our results, in which individuals gave improving information under some conditions and refrained from giving improving information under others, attest to this active role that others can have in one's improvement.

Individual differences will also need to be addressed in follow-up investigations. Research by Dweck and her colleagues is particularly relevant (Dweck, 1999). It is likely that individuals who have learning goals (i.e., are motivated to increase their competence on a domain) are less influenced by the diagnosticity of assessment information than are individuals who have performance goals (i.e., an interest in favorable external judgments of their competence). Finally, the possible interdependence between the giver and the receiver of the improving information is also relevant. Although we have focused exclusively on the giver of the information, in everyday interactions it is often the case that both interacting parties are giving and receiving information. Individuals may be more likely to help each other improve in situations in which the two interactants are dependent on one another for success (Campbell, Sedikides, Reeder, & Elliot, 2000; Sedikides, Campbell, Reeder, & Elliot, 1998, in press).

Our research is a demonstration of the complex interplay among motives, behavior, and social context. Individuals are influenced by motives (e.g., improving the self or others), and motives do affect behavior (e.g., helping versus nonhelping others), depending on social context (e.g., whether others are relationally close or distant to the self). Our research suggests that the interrelation among motives, behavior, and social context is a function of a critical variable: the diagnosticity of the information exchanged.

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Appendix A

Interaction Record Sheets Used in Study 2

Date ____ Starting time ____ AM ____ PM Length ____ hr ____ min

(circle one)

Was the interaction related to your or the other person's academic lives? No Yes (if yes, answer questions below)

Academic improving information given:

None 1 . . . 2 . . . 3 . . . 4 . . . 5 . . . 6 . . . 7 A lot

Your knowledge of the other's academic life

Very little 1 . . . 2 . . . 3 . . . 4 . . . 5 . . . 6 . . . 7 A great deal

---

(circle one)

Was the interaction related to your or the other person's social lives? No Yes (if yes, answer questions below)

Social improving information given:

None 1 . . . 2 . . . 3 . . . 4 . . . 5 . . . 6 . . . 7 A lot

Your knowledge of the other's social life

Very little 1 . . . 2 . . . 3 . . . 4 . . . 5 . . . 6 . . . 7 A great deal

---

Gender of the other person: male female

Your relationship to other person: stranger acquaintance casual friend close friend romantic partner spouse parent other relative

Education of other person? Current college student Former college student Never attended college Don't know

Familiarity of other to you: Not at all familiar 1 . . . 2 . . . 3 . . . 4 . . . 5 . . . 6 . . . 7 Very familiar

Your liking of other: Not at all 1 . . . 2 . . . 3 . . . 4 . . . 5 . . . 6 . . . 7 Very much

Importance of this interaction to you?: Not at all important 1 . . . 2 . . . 3 . . . 4 . . . 5 . . . 6 . . . 7 Very important

Description of the interaction:

(*Appendixes continue*)

Appendix B

Training Packet Given to Participants in Study 3

Below are the 10 pieces of information that could help you to improve your score on the second Briggs-Alston Verbal Creativity Test.

Pay special attention to the rank orders. Remember that the piece of information ranked #1 has been shown to affect scores the most, whereas the piece of information ranked #10 has been shown to affect scores the least.

Example words are given to help clarify each piece of information. These example words are all words that can be made from the following target word:

ECHINODERMATOUS

1. If all other things are equal, words that are commonly used are worth fewer points than words that are not commonly used (i.e., ROE is worth more than TIN).
2. If all other things are equal, words that have their letters in the proper order in the target word are worth fewer points than words where letters are not in the proper order in the target word (i.e., NEAT is worth fewer points than DARE).
3. Words that differ by only one letter (i.e., HAT, RAT, CAT, MAT) are worth fewer points than those same words would be otherwise.
4. Words that are derivations of other words that you have written (i.e., HINT and HINTED) are worth fewer points than those same words would be otherwise.
5. If all other things are equal, shorter words are worth fewer points than longer words (i.e., SAT is worth fewer points than NOTICE).
6. If all other things are equal, words where the letters are directly next to each other in the target word are worth fewer points than words where the letters are not directly next to each other (i.e., ARM is worth fewer points than the word MAD).
7. You lose points by writing words that are not actually words in the English language (according to the *Webster's Unabridged Dictionary*).
8. If the entire word appears in the target word with all of its letters next to each other *and* in the proper order in the target word (i.e., NOD or MAT) the word is worth fewer points than it would be otherwise.
9. You lose some points for each word you write that cannot really be made from the letters of the target word.
10. If all other things are equal, 'slang' terms (i.e., NERD) are worth fewer points than other words.

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