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Causal Uncertainty Beliefs and Diagnostic Information Seeking

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This study examined the extent to which chronic causal uncertainty beliefs influence diagnostic information seeking. Situational factors intended to increase the excitation level of causal uncertainty beliefs and the intensity of goal-directed behavior also were investigated. Participants expected to interview either a gender in-group or a gender out-group member, and half of them expected to be held accountable for their understanding of the interviewee. For out-group conditions, those accountable participants who possessed chronically accessible causal uncertainty beliefs revealed the greatest preference for diagnostic information. For in-group conditions, no differential pattern of information seeking as a function of chronic causal uncertainty beliefs or goal importance were found. Results are discussed in terms of a recent model of motivated social cognition proposed by G. Weary and J. A. Edwards (1996).

All the social sciences, in different measure and manner, have been struggling with the place of uncertainty as an aspect of their research and theory. (Fiddie, 1980, p. 6)

Uncertainty has long been viewed as a primary motive guiding behavior in many social situations (cf. Kagan, 1972). Festinger's (1954) original formulation of social comparison theory and subsequent reformulations by different theorists (e.g., Goethals & Darley, 1977), for example, have seen uncertainty as a primary determinant of whether one engages in social comparison for purposes of ability or belief evaluation. Additionally, in the attribution domain, the need to reduce uncertainty about the social world has been viewed as a fundamental motivation underlying individuals' search for and processing of causal information (Heider, 1958; Kelley, 1967, 1973). In a similar vein, Berger and Calabrese (1975) have proposed that interaction partners in communication situations are concerned both with predicting the other's future behavior and explaining the other's present and past behaviors (i.e., proactive and retroactive uncertainty reduction, respectively). With uncertainty playing such a pivotal role in these and many other social phenomena, it is not

surprising that recent work has focused on uncertainty as an individual difference variable as well (e.g., Sorrentino & Short, 1986).

One example of such work is that of Weary and Edwards (1994). This work has focused on chronic individual differences in uncertainty about a particular aspect of the social world, namely uncertainty about the causes of events. More specifically, in the causal uncertainty model, Weary and Edwards (1996) have argued that some people are chronically more motivated than others to resolve causal uncertainty and that such chronic individual differences in uncertainty about one's understanding of social causation have fundamental social-cognitive consequences.

In their model, Weary and Edwards (1996) differentiate two components of the causal uncertainty construct: causal uncertainty beliefs and causal uncertainty feelings. Causal uncertainty beliefs are defined as "generalized self-constructs about one's uncertain or inadequate understanding or detection of causal relations in the social world" (p. 159). The activation potential, or accessibility, of these beliefs can be increased in a variety of ways. Situational perceptions and expectations of uncontrollability, for example, are likely to increase temporarily the accessibility of beliefs that one does not understand the contingencies under which events operate (i.e., causal uncertainty beliefs). Task instructions to form an impression of another person or to focus on the causes of one's own or another's outcomes are two additional, situational ways to increase the momentary accessibility of causal uncertainty beliefs. The environment also may contain other uncertainty-producing cues, such as evidence of multiple sufficient causes underlying a person's behavior.

Whether such situational input actually activates causal uncertainty beliefs, however, depends to some degree on the prior accessibility of those beliefs. If causal uncertainty beliefs are not chronically accessible, the situational input will have to be more detailed, salient, or elaborate for their excitation levels to reach a threshold adequate for activation to occur. Weary and Edwards (1996) suggest that although causal uncertainty beliefs are available for all people, they are chronically accessible for

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some individuals—in particular, for individuals who possess chronic expectations of response–outcome noncontingency.

The second component of causal uncertainty in Weary and Edwards's (1996) model, causal uncertainty feelings, pertains not to emotional feelings (although negative affect is likely to be associated with such uncertainty) but to metacognitive feelings that one does not know or understand (cf. Clore, 1992). These feelings would be manifested as a sense of confusion or puzzlement and would provide an informational cue that one's current state of knowledge is discrepant from one's desired state. In this case, the desired state would be a subjective sense of accurate understanding that may or may not correspond to a more objective assessment of veridicality (Swann, 1984). This accuracy goal, or desired state of certainty, can be activated consciously, or the goal may become active relatively automatically after frequent activation or pairings with situational cues.

The activation of this accuracy goal has several implications; the one that is important here is the proposed effect on the search for and processing of social information. The need to be accurate or to reduce uncertainty should result in information seeking and processing that may be characterized as deliberate, intentional, and often effortful (Weary & Edwards, 1994). Moreover, the likelihood that such goal-directed behavior will be observed is dependent in part on the individual's level of error sensitivity (Hyland, 1987). According to Weary and Edwards (1996), the higher the error sensitivity, the more likely one will be to detect a discrepancy, and the more intensely one will work to reduce this error (e.g., through extensive information search and processing). There are various factors that can increase error sensitivity, such as increasing the salience of the goal or discrepancy, increasing the importance of the goal, heightening the seriousness of the consequences of not attaining the goal, and increasing self-focused attention. Finally, error sensitivity in general is likely to be higher for chronically accessible goals.

This study was designed to investigate one means of reducing the discrepancy between one's desired state of accurate knowledge and one's current state of uncertain knowledge—namely, an extensive search for diagnostic social information without an explicit or experimentally induced goal to confirm or disconfirm a hypothesis about the target person. Although many social psychological investigations of information seeking about another person have incorporated an experimentally manipulated hypothesis-testing goal (e.g., Snyder & Swann, 1978; Snyder & White, 1981; Trope & Bassok, 1982, 1983), at least two studies have investigated diagnostic information seeking without such a goal (see also Neuberg & Fiske, 1987). However, similar to many of the hypothesis-testing experiments, information search in these two experiments was assessed by the questions that the participants selected for an interview with the target person.

The first of these two experiments was conducted by Swann, Stephenson, and Pittman (1981). They examined the effects of control deprivation and information utility on diagnostic information seeking. They found a significant effect for control deprivation such that control-deprived individuals selected significantly more high-diagnostic questions than those not deprived of control; however, information utility was only a marginal moderator of participants' preferences for high-diagnostic questions. In the second experiment, conducted by Hildebrand-Saints

and Weary (1989), participants were depressed and nondepressed individuals. The researchers found that depression and information utility interacted to determine the number of high-diagnostic questions selected. Depressed participants selected significantly more high-diagnostic questions than the nondepressed participants, but only in the low-information-utility condition. In the high-information-utility condition, there was no difference between the number of high-diagnostic questions selected by the depressed versus the nondepressed participants.

In the present study, we used a procedure similar to Swann et al.'s (1981) and Hildebrand-Saints and Weary's (1989) to determine the effects of individual differences in chronic causal uncertainty beliefs as measured by the Causal Uncertainty Scale (CUS; Weary & Edwards, 1994) on diagnostic information seeking. In this experiment, participants were told that the experimenter was taping a series of interviews to use as stimulus materials for another study. They were informed that they would be assigned to the role of interviewer or interviewee. Participants were told further that they would not have to make up their own questions but would select 10 questions from a prepared list. Unknown to the participants, the list of 30 questions was composed of questions that were piloted to be high, moderate, or low in diagnosticity.

In this study, we also manipulated the error sensitivity, or the importance of the goal of accurate understanding of the interviewee and, consequently, the intensity of the goal-directed behavior. Participants in the high-goal-importance condition were told that following the interview they would have to answer some extensive questions from the experimenter concerning what they had learned about the interviewee. Participants in the low-goal-importance condition, in contrast, did not expect to be questioned by the experimenter about the interview and thus were not likely to perceive themselves as accountable for the accuracy of their understanding of the interviewee.

Additionally, we varied interviewee characteristics so as to increase or decrease situational excitation of participants' available causal uncertainty beliefs. We accomplished this by varying the similarity of the participants' and interviewees' genders. Past research (Krueger & Zeiger, 1993) has shown that social projection of one's attitudes, beliefs, and behavioral responses is more likely to occur for gender in-group than for gender out-group members. Moreover, such social projection engenders an implicit consensus for one's own responses and enables more confident predictions of the others' responses, because a similar other's behavior and opinions should be the same as one's own behavior and opinions (Orive, 1988). When confronted with a member of the out-group, however, one would not be expected to use social projection, because dissimilar others are not expected to share one's own perspective and opinions. This lack of perceived consensus should decrease the certainty or confidence of predictions for the out-group member's behavior and opinions and should therefore increase the excitation levels of causal uncertainty beliefs and feelings.

In summary, this study was an investigation of the effects of chronic sources of causal uncertainty in combination with two situational factors, one that should increase the excitation level of causal uncertainty beliefs and one that should increase the error sensitivity associated with the uncertainty reduction goal.

A three-way interaction of Causal Uncertainty \times Importance of Uncertainty Reduction Goal \times Interviewee Group Membership was predicted. Although either situational or chronic sources of accessibility may result theoretically in activation of causal uncertainty reduction goals, the variation of interviewee in-group or out-group status in the present research was relatively subtle. Consequently, we expected that anticipation of interaction with an out-group member would result in activation of causal uncertainty beliefs only for high-causal-uncertainty participants. Moreover, the need to reduce this resultant uncertainty by means of diagnostic information seeking should have been evident only under conditions in which the goal of accurate understanding of the interviewee was made important. Anticipation of interaction with an in-group member, on the other hand, should not have resulted in activation of causal uncertainty beliefs for either high- or low-causal-uncertainty participants. That is, through social projection, both high- and low-causal-uncertainty participants should have been relatively certain about their impressions of and predictions for the interviewee's future behavior. Moreover, because there presumably would have been relatively little uncertainty to reduce, the goal importance manipulation should have had little effect on diagnostic information seeking. Consequently, no effects of causal uncertainty level or goal importance were expected for in-group conditions.

Method

Participants

In partial fulfillment of their course requirements, 256 introductory psychology students participated in the study. Participants were randomly assigned to either the interviewee in-group or the interviewee out-group condition, and random assignment by sequential blocks was used to assign participants to the goal importance conditions. Data from 17 of the participants were dropped either because of a confederate scheduling problem ($N = 6$) or because suspicion regarding the experimental procedure was expressed during debriefing ($N = 11$). For the latter group, more high-causal-uncertainty participants ($N = 7$) and more low-goal-importance participants ($N = 7$) expressed some degree of suspiciousness.

To avoid the potential priming of causal uncertainty beliefs, we administered the CUS at the end of the experimental procedures. The CUS is a 14-item self-report scale designed to assess chronic individual differences in the frequency and globality of causal uncertainty beliefs (i.e., their accessibility and applicability). Respondents indicate on a 6-point scale the extent to which they agree or disagree with 14 statements relevant to their understanding of cause-effect relationships in the social world (e.g., "When things go right, I generally do not understand what to do to keep them that way"; "I do not understand what causes most of the problems that I have with others"). It includes items referring to both positive and negative outcomes and to both achievement and interpersonal domains. Each item is scored from 1 to 6, and a total score is obtained by summing the individual item scores. Thus, higher scores indicate greater causal uncertainty. In two separate samples, the CUS has been shown to have high internal consistency (Cronbach's $\alpha = .83$ and $.86$), good test-retest reliability ($r_s = .62$ and $.79$), and mean interitem correlations of $.25$ and $.31$. Confirmatory factor analyses revealed two highly correlated factors ($r = .65$) that for most purposes are best treated as a unitary construct (Edwards, Weary, & Reich, 1996). Evidence of the scale's construct validity has been reported in two recent reports (Weary & Edwards, 1994, 1996).

Scores for the sample of 239 participants ranged from 14 to 63 with a median of 38. Participants were divided into two groups, high causal uncertainty ($M = 45.9$) and low causal uncertainty ($M = 28.71$), using a median split of the causal uncertainty scores; individuals whose scores fell at the median were excluded. As a result, 232 participants (114 women and 118 men) were available for the final analyses. A 2 (goal importance: high vs. low) \times 2 (interviewee group membership: in-group vs. out-group) analysis of variance (ANOVA) revealed no significant differences in the causal uncertainty scores of the participants in the different conditions ($p_s > .40$). This analysis, then, provides some evidence that the experimental manipulations did not influence causal uncertainty scores.

Information-Seeking Measure

Sixty questions from Taylor and Altman's (1966) compilation of intimacy-related stimuli were selected for preliminary testing. Half of the questions have been used in other studies on information seeking (Hildebrand-Saints & Weary, 1989; Swann et al., 1981). The remaining 30 questions were chosen because they were rated in Taylor and Altman (1966) as moderately intimate. Fifty-six introductory psychology course students received packets containing one of two randomly selected and ordered sets of 30 of the initial 60 questions and the CUS. The order of the CUS and the questions within the packets was counterbalanced. Students were given the packets in class and were asked to return them for extra credit at the next class meeting. For each of the 30 questions, the students were asked to rate on a 7-point scale (1 = *not at all*, 7 = *a great deal*) how much information they thought that they would learn about a college freshman from his or her response. For analyses of these diagnosticity ratings, a median split ($mdn = 38$) of causal uncertainty scores was used to divide students into high-causal-uncertainty ($M = 45.3$, $N = 28$) and low-causal-uncertainty ($M = 28.92$, $N = 26$) groups. Separate one-way ANOVAs revealed causal uncertainty effects for only 5 of the 60 questions. The remaining 55 questions had a mean diagnosticity rating of 4.72 and a standard deviation of 0.44. Questions whose mean ratings were lower than -0.95 SD below, within $\pm .95$ SD, and greater than $.95$ SD above the mean were categorized as low-, moderate-, and high-diagnostic questions, respectively. This categorization yielded equal numbers of low- and high-diagnostic questions (8). Of the 39 questions categorized as moderate in diagnosticity, 14 were randomly selected for the information-seeking measure. The mean diagnosticity ratings of each of the resulting three categories of questions were 3.98, 4.84, and 5.34.

To ensure that the questions differed only with respect to their potential information gain and not to their possible elicitation of intimate or highly personal information, we conducted a second preliminary study. In this study, 47 introductory psychology students completed the CUS and rated on a 7-point scale each of the final 30 questions in terms of how personally intrusive they believed each to be. The order of the CUS and the 30 questions was counterbalanced. A one-way ANOVA performed on students' intrusiveness ratings indicated that there were no differences as a function of question diagnosticity ($M_s = 3.38$, 3.53, and 3.52 for the low-, moderate-, and high-diagnostic questions, respectively). In addition, separate ANOVAs conducted on the intrusiveness ratings for each question similarly revealed no effects associated with students' above median ($M = 47.65$) or below median ($M = 33.14$) CUS scores ($mdn = 41$).

Procedure

Participants arrived at the room in which the experimental session was scheduled to be held and were asked to wait in the hallway until another participant arrived. The other participant was actually one of 12

male or 12 female confederates of the experimenter. The assignment of a confederate on the basis of participant gender (i.e., same or opposite gender) constituted the manipulation of interviewee group membership. After the confederate's arrival, the experimenter explained to both participants that they would be placed in separate rooms for the first part of the experiment. One participant (the confederate) was placed in a room first and was instructed to complete a series of questionnaires until the experimenter returned to give him or her more instructions. Then the experimenter showed the other participant to the second room and gave that participant the experimental instructions.

The experimenter explained to the participants that she was interested in studying the interview process and that she currently was filming a series of interviews to be used as stimulus materials for another study. The experimenter informed the participants that they had been randomly assigned to be the interviewer in one of these interviews and that the other participant (i.e., the confederate) would play the role of the interviewee. Participants who were randomly assigned to the low-goal-importance condition then were told that after the interview they would not have to answer any questions about the interview process itself or what they had learned about the other participant during the interview. That is, they were simply being asked to help create the stimulus materials for another study. Participants who were assigned randomly to the high-goal-importance condition, however, were told that following the interview, the experimenter would ask them extensive questions about the interview, the process itself, and what they had learned about the other participant. More specifically, these participants were told they would be asked questions about their impression of the other participant, the inferences that they could make about him or her, and predictions about the other participant's behavior in similar or different situations.

The questions that served as the measure of diagnostic information seeking then were presented. The 30 questions covered a wide range of areas, including politics, religion, and interpersonal relationships. The experimenter asked the participants to choose 10 questions from the list that they would most like to ask the interviewee during the videotaped interview. The experimenter, furthermore, emphasized that there was no right or wrong strategy for selecting the questions and that the interview did not necessarily have to be modeled after a job interview.¹

After the participants selected the questions for the interview, they were asked to answer four questions that served as checks on procedural details and on the manipulation of goal importance. The questions asked the participant to indicate what role they would play in the interview, where the questions came from (i.e., made up by the participants or selected from a list provided by the experimenter), to what degree they thought the experimenter would ask them questions about the interview, and to what degree they expected to use the information from the interview to convey their impression of the other participant to the experimenter. In addition, they were asked to complete several rating scales designed to assess their concerns about the experimenter's and the interviewee's evaluation of them and about their future interaction with the interviewee. Finally, the participants were informed that the psychology department had requested that experimenters who had extra time in their studies ask participants to complete a packet of questionnaires that would be used to get validation information for several scales. Because the interviewee was reportedly still working on his or her tasks, the experimenter requested that the participants complete the packet provided by the department while they waited. This packet was given to the participants in a separate folder and contained several questionnaires including the CUS, the Perceptions of Control Scale (Edwards, Jacobson, & Weary, 1995), and the Beck Depression Inventory (BDI; Beck, 1967). The experimenter asked the participants to let her know when they had completed the questions for this study and the departmental questionnaire packet so that she could talk to them briefly before the interview.

When the participants informed the experimenter that they had com-

pleted the questionnaire measures, the experimenter returned to the room and told the participants that there would be no interview for reasons that she would explain later. The experimenter then asked a series of questions to determine whether participants were suspicious about the interview (e.g., about whether or not it would occur), whether they were suspicious about the true status of the other participant (i.e., whether the participant suspected that the other participant was not another psychology student but rather was a confederate of the experimenter), and any other concerns that the participants may have had about the experiment that could have influenced their responses. The participants then were debriefed about the true goal of the experiment and about why the interview was unnecessary for this goal. When the participants had no further questions, they were dismissed.

Results

Procedural and Manipulation Check Measures

All participants correctly answered the first two questions, which pertained to the general experimental procedure. They all were able to identify their role in the interview as that of the interviewer, and they all correctly indicated that they had chosen the questions from a list of 30 questions. The other two questions were intended to check on the importance of uncertainty reduction goal manipulation. A 2 (causal uncertainty: high vs. low) \times 2 (goal importance: high vs. low) \times 2 (interviewee group membership: in-group vs. out-group) ANOVA was conducted separately for each of these manipulation check questions. The first importance manipulation check pertained to the degree to which the participants expected to be questioned by the experimenter about the interview (1 = *not at all*; 7 = *a great deal*). As expected, there was a significant goal importance main effect for this question such that participants in the high-goal-importance condition ($M = 5.80$) thought that they would be questioned to a greater extent than the participants in the low-goal-importance condition ($M = 2.45$), $F(1, 223) = 266.33$, $p < .001$. There also was an unexpected significant Causal Uncertainty \times Goal Importance interaction, $F(1, 223) = 5.84$, $p < .05$. Examination of the means involved in this interaction suggested that the manipulation of goal importance was slightly stronger for low- than for high-causal-uncertainty groups. Pairwise comparisons (Tukey's honestly significant difference [HSD]) revealed that there was a significant difference between the high- and low-goal-importance conditions for both high- and low-causal-uncertainty people ($ps < .05$). There was not, however, any difference between high- and low-causal-uncertainty participants within either goal importance condition.

The other goal importance manipulation check pertained to the degree to which the participants expected to use the information learned during the interview to convey their impression of the other participant to the experimenter (1 = *not at all*; 7 = *a great deal*). As expected, there was a significant goal impor-

¹ Most of the initial participants indicated that they perceived the experimenter to be interested in studying job interviews, but many of the questions on the list would be inappropriate for or unusual to ask in a job interview situation. Thus, the latter instruction was added to encourage the participants to consider all of the questions on the list and not just the ones that would typically be asked in a job interview.

tance main effect for this question. The participants in the high-goal-importance condition ($M = 5.68$) thought that they would use the information gained in the interview to convey their impressions of the interviewee to the experimenter to a greater extent than did the participants in the low-goal-importance condition ($M = 2.43$), $F(1, 223) = 260.20$, $p < .001$. As with the first goal importance manipulation check, there also was an unexpected significant Causal Uncertainty \times Goal Importance interaction for this manipulation check question, $F(1, 223) = 5.48$, $p < .05$. Pairwise comparisons of the means involved in this interaction (Tukey's HSD) revealed a pattern similar to that of the first importance manipulation check. That is, there was a significant difference between the goal importance conditions for both high- and low-causal-uncertainty participants ($ps < .05$), but there was no causal uncertainty difference within the high- or the low-goal-importance conditions. The manipulation of goal importance, then, appears to have been successful for both causal uncertainty groups.

Information-Seeking Measure

To obtain an index of participants' diagnostic-information-seeking activity, we computed an index of the number of high-diagnostic ($M = 3.91$) relative to moderate-diagnostic ($M = 4.83$) and low-diagnostic ($M = 1.31$) questions selected. This computation involved subtracting for each participant the sum of moderate- and low-diagnostic questions from 2 times the number of high-diagnostic questions selected. (This weighting of the high diagnostic questions takes into account the fact that there are 2 [low and moderate] vs. 1 [high] question diagnosticity levels on each side of the minus sign in the index equation²). The weighted difference scores then were subjected to a 2 (causal uncertainty: high vs. low) \times 2 (goal importance: high vs. low) \times 2 (interviewee group membership: in-group vs. out-group) ANOVA.

This analysis revealed only the predicted three-way interaction, $F(1, 224) = 4.48$, $p < .04$. To provide tests of the central hypotheses, we conducted comparisons of the means involved in the Causal Uncertainty \times Goal Importance interaction at each level of the interviewee group membership. We expected that interacting with a dissimilar other would activate only high-causal uncertainty participants' causal uncertainty beliefs; the need to reduce this uncertainty should have been evident only in the high-goal-importance condition. The means involved in this predicted two-way interaction are presented in Table 1, and an examination of them reveals a pattern generally consistent with predictions. An *a priori* focused contrast of these means (Kirk, 1968) revealed that high-causal-uncertainty participants in the high-goal-importance condition selected more diagnostic questions than did participants in all other conditions combined, $t(224) = 1.77$, $p < .05$.

No effects associated with participants' causal uncertainty levels or the manipulation of goal importance were expected on the index of diagnostic information seeking for participants in the in-group conditions (i.e., same-gender pairs). Through social projection, they should have been fairly certain about how similar or same-gender others would respond to the questions. Consequently, activation of causal uncertainty beliefs should

have been unlikely, even for the high-causal-uncertainty participants, and diagnostic information seeking in the service of uncertainty reduction goals should not have occurred. The means for the in-group conditions also are depicted in Table 1. To examine the simple main and interaction effects of causal uncertainty level and goal importance, we used two-tailed tests, as no differences among means were expected. Although neither the simple main effect of causal uncertainty nor the simple interaction of causal uncertainty level and goal importance were significant ($ps > .15$), there was a tendency for participants in high- compared with low-goal-importance conditions to seek more diagnostic information, $t(224) = 1.68$, $p < .10$.^{3,4}

² We thank one of the anonymous reviewers for suggesting this index.

³ In this study, we had no direct data to support our arguments with respect to the notion that similarity results in social projection and a consequent reduction in uncertainty about the likely future behavior of the interviewee. We purposefully did not include measures designed to assess the theoretical effects of the in-group-out-group manipulation, because we did not want to artificially draw attention to this variable and thereby create other potential alternative explanations for our results. Still, we would have greater confidence in our interpretation of our results had some assessment of the effectiveness of the in-group/out-group variation been possible. A second study was conducted for this purpose.

The 96 participants in this second study were randomly given one of four scenarios, written in the second person, that described the actual interview study reported in this paper, including the exact instructions that participants had been given about the interview that was to occur and the nature of the question selection. In addition, half of the participants read the instructions for the high-goal-importance manipulation as they had been delivered by the experimenter in the actual study, and half of the participants read the instructions for the low-goal-importance manipulation. The gender of the other participant also was varied in the scenarios. For two of the four scenarios, the other participant was explicitly referred to by the name Teresa and by female pronouns, and for the other two scenarios, the other participant was referred to by the name Dan and by male pronouns. Thus, the out-group manipulation entailed having male participants read the scenario about Teresa or having female participants read the scenario about Dan; the in-group manipulation entailed having male participants read the scenario about Dan or female participants read the scenario about Teresa.

Before the participants read the scenario, they were told that they should try to put themselves in the place of the person in the situation that would be described. They were asked to try to visualize themselves in the situation and to imagine the thoughts and feelings that they might have had if they actually had experienced the events. When they had finished reading about the experiment, they completed measures of similarity and social projection (Orive, 1988). Finally, the participants completed the CUS and answered the same experimental procedure and goal importance manipulation check items from actual interview study, plus two new items intended to assess the manipulation of the other participant's gender.

Results indicated that over 94% of participants answered the manipulation check questions appropriately. Additionally, participants perceived the in-group interviewee as more similar to themselves than the out-group interviewee, $t(87) = 1.94$, $p < .03$, one-tailed. They also evidenced greater projection of the in-group compared with the out-group interviewee's likely responses to the interview questions, $t(88) = 4.79$, $p < .001$, one-tailed, and expressed more interest in learning about the out-group than in-group interviewee's responses, $t(88) = 1.89$, $p < .03$, one-tailed.

This support for our argument regarding the effects of the gender in-group-out-group variation was obtained despite the simulated nature of

Table 1
Means and Standard Deviations for the Index of
Diagnosticity of Information Seeking as a Function of Causal
Uncertainty, Goal Importance, and Interviewee
Group Membership

Goal importance	Causal uncertainty	
	Low	High
Out-group		
Low		
<i>M</i>	1.71	1.13
<i>SD</i>	3.41	2.86
<i>N</i>	31	24
High		
<i>M</i>	0.57	2.35
<i>SD</i>	3.37	3.37
<i>N</i>	23	24
In-group		
Low		
<i>M</i>	1.38	1.23
<i>SD</i>	3.62	3.10
<i>N</i>	29	31
High		
<i>M</i>	3.15	1.58
<i>SD</i>	3.14	4.05
<i>N</i>	34	26

Note. The higher the mean is, the greater is the number of high- relative to moderate- and low-diagnosticity questions selected.

Ancillary Measures⁵

Although the results for the measure of diagnostic information seeking were in accord with predictions, we wished to rule out several potential alternative interpretations of them.

Evaluative concerns. The first of these entailed a potential alternative explanation for the effects of interviewee group membership. In particular, we wanted to rule out the possibility

the study (i.e., the rather subtle manipulation of the variables that resulted from reading about the experiment rather than actually experiencing the events). In all likelihood, the effects of this manipulation were even more pronounced in the actual interview study.

⁴ The proportions of high to low intrusive questions were equivalent across each of the three diagnosticity levels, with an equal number of high and low intrusive questions within a given level of diagnosticity. Question intrusiveness, then, does not present a problem for interpretation of the question diagnosticity results. However, we wanted to see whether participants tended to avoid asking high- compared with low-intrusive, high-diagnostic questions. To the degree that there was a tendency, then, the number of potential high-diagnostic questions from which participants could have chosen would have been halved, and the magnitude of effects for this level of diagnosticity would have been artificially lessened. A Causal Uncertainty \times Goal Importance \times Interviewee Group Membership \times Question Intrusiveness mixed ANOVA conducted on the number of high-diagnostic questions chosen revealed a significant main effect only for question intrusiveness. Nearly twice as many low ($M = 2.59$) as high ($M = 1.31$) intrusive questions were chosen by participants, $F(1, 222) = 152.47, p < .001$.

that the effects associated with interviewee out-group gender status were due not to the activation of causal uncertainty beliefs but rather to the arousal of concerns about either the experimenter's or the interviewee's evaluation of participants or about the interview itself. Accordingly, participants were asked to indicate on separate 7-point scales (1 = *not at all*, 7 = *a great deal*) the degree to which (a) they thought the experimenter would evaluate them and their interview skills on the basis of the questions selected, (b) they cared about the other participant's evaluation of them and the questions selected, (c) they selected questions that would be easy for the participant to respond to, (d) their selection of questions was based on their desire to make the interaction with the interviewee more comfortable, and (e) they avoided selecting questions that were too personal or intrusive. These ratings were subjected to separate 2 (causal uncertainty: high vs. low) \times 2 (goal importance: high vs. low) \times 2 (interviewee group membership: in-group vs. out-group) ANOVAs. The only significant effect to emerge across these analyses was for the question asking participants the degree to which they cared about the other participant's evaluation of them and the questions selected. The ANOVA conducted on participants' ratings of this item yielded a significant interviewee group membership effect, $F(1, 139) = 4.60, p < .05$; participants in the gender in-group, when compared with those in the gender out-group, expressed greater caring. Even on this item, however, interviewee group membership did not interact with any of the other variables. Consequently, it seems unlikely that participants' evaluative or interpersonal concerns represent a viable alternative interpretation of the information-seeking effects obtained in this study.

Depression. Second, we wished to provide evidence that the effects associated with causal uncertainty levels were not attributable to variables with which causal uncertainty scores are known to covary. Two critical variables in this regard are participants' levels of depressive symptomatology and generalized perceptions of control loss. In the present study, the correlations of causal uncertainty scores and of BDI (Time 2, see below) and Perceptions of Control Scale scores were .48 and .66 ($ps < .0001$), respectively. An ANOVA revealed that the experimental manipulations of goal importance and group membership produced no significant main or interactive effects on either participants' BDI ($ps > .23$) or control scale scores ($ps > .10$).

To rule out depression as an alternative interpretation, we used a two-pronged analytic approach. First, a 2 (causal uncertainty: high vs. low) \times 2 (goal importance: high vs. low) \times 2 (interviewee group membership: in-group vs. out-group) ANCOVA was conducted on the index of diagnostic information seeking, with BDI score at the time of the experiment as a covariate. This analysis revealed that BDI was not a significant covariate ($p > .81$). This null result is perhaps not surprising, because participants were not selected on the basis of their levels of depressive symptomatology, and consequently, the vast majority of them received scores in the nondepressed range (M

⁵ These measures were added after a number of participants already had been run.

= 6.59, $SD = 6.83$). Although the Causal Uncertainty \times Goal Importance \times Interviewee Group Membership interaction was no longer significant in this analysis ($p < .23$), a significant Causal Uncertainty \times Group Membership interaction was obtained, $F(1, 141) = 4.90, p < .03$; high-causal-uncertainty, out-group participants and low-causal-uncertainty, in-group participants chose more high- relative to moderate- and low-diagnostic questions. The failure in this analysis of the three-way interaction to achieve conventional levels of significance was not entirely surprising in that causal uncertainty theoretically is thought to be associated with state feelings of mild depression and anxiety, and the BDI has been shown to be sensitive to state affect. Moreover, the power of this analysis is reduced by the multicollinearity of the CUS and BDI and by the smaller sample size.

A second analysis involving BDI scores also was conducted. In this analysis, we substituted participants' depression status for their causal uncertainty category.⁶ Additionally, because we had prescreening BDI scores for a number of the participants, only those participants whose scores remained in the depressed or nondepressed categories on both assessment sessions were included in the analyses. This analysis revealed no main effect for depression or any interactions associated with BDI, including the three-way interaction of Depression Status \times Goal Importance \times Group Membership, $F(1, 167) = .00, p > .99$. Together, these two analyses suggest that depression is not a viable alternative explanation for the effects associated with causal uncertainty levels.

Control loss perceptions. To examine the possible role of participants' generalized perceptions of control loss, we used their scores on the Perceptions of Control Scale as covariates in a 2 (causal uncertainty: high vs. low) \times 2 (goal importance: high vs. low) \times 2 (interviewee group membership: in-group vs. out-group) ANCOVA.⁷ This analysis indicated that control loss was not a significant covariate ($p < .25$). It is important to note that in this analysis, the three-way interaction remained significant, $F(1, 222) = 4.14, p = .04$. Generalized perceived and expected control loss, then, does not appear to be a viable interpretation for the effects associated with causal uncertainty levels.

Discussion

There is a substantial literature showing that people prefer or seek information that satisfies various motives, and the predominant motives studied have been hypothesis confirmation (e.g., Snyder & Swann, 1978; Snyder & White, 1981; Trope & Bassok, 1982, 1983) and dissonance reduction (for an extensive review, see Frey, 1986). Although information seeking in the present study was not relevant to these two motives, the results shed light on another motive that may determine information-seeking behaviors. This motive, unlike those focused on in previous research in the information-seeking literature, is a dispositional motive. Participants in the present research selectively sought information that would best satisfy a motive to reduce uncertainty about the target's personality, attitudes, and interests; in other words, they sought information that would reduce their uncertainty about the causes of the interviewee's likely

behaviors and would permit at least temporary satisfaction of a superordinate control motive.

Stated more specifically, the purpose of the present study was to assess the effects of one chronic and two situational factors, similarity of one's interaction partner and the importance of the uncertainty reduction goal, that should increase the excitation level of causal uncertainty beliefs and the intensity of the resultant goal-directed behavior. Interacting with a similar other was proposed to be unlikely to increase the accessibility of causal uncertainty beliefs because through social projection, individuals should have been fairly certain about how the other will behave, think, and respond to the questions (i.e., a similar other should act and think like oneself).⁸ Interacting with and trying to understand a dissimilar other, on the other hand, should have activated causal uncertainty beliefs only for individuals who scored high on causal uncertainty (i.e., individuals for whom causal uncertainty beliefs were chronically accessible). Unlike low-causal-uncertainty individuals, for whom this situational input may have been too subtle, high-causal-uncertainty individuals should have needed less excitation from an external input in order to activate their chronically accessible causal uncertainty beliefs. Furthermore, the resultant motivation to reduce causal uncertainty by means of diagnostic information seeking should have been evident only when accurate understanding of the other person was made consequential. Thus, when paired with a member of the out-group (i.e., with a partner of the opposite gender), high-causal-uncertainty, high-goal-importance participants were expected to demonstrate the greatest preference for diagnostic information. The results of this study were generally in line with predictions. Participants' causal uncertainty level and the importance of the uncertainty reduction goal interacted in the manner predicted in the out-group conditions. Moreover, no effects of these variables were evident in the in-group conditions.

Although the effects of causal uncertainty level and goal importance in the in-group condition were, as predicted, nonsignificant, it is important to note that the pattern of means for these cells was unexpected. In particular, one of the cells had

⁶ The cutpoints for categorizations of participants as depressed or nondepressed were > 9 and < 6 , respectively. It is important to note that scores on the BDI reflect the severity of depressive symptomatology but do not necessarily indicate the full clinical syndrome of depression.

⁷ The Perceptions of Control Scale is composed of items that have been used by others (Lewinsohn, Hoberman, & Rosenbaum, 1988; Mirovsky & Ross, 1991; Weisz, Weiss, Wasserman, & Rintoul, 1987) in investigations of the relationship between depression and perceived control loss. Not surprising, given previous use of several of the items by others, the scale has been shown to have adequate reliability and discriminant and convergent validity (Edwards et al., 1995).

⁸ Citing the out-group homogeneity effect, one anonymous reviewer questioned our prediction of perceived in-group similarity. However, in-group homogeneity and out-group heterogeneity effects also exist (e.g., Brewer, 1993; Brown & Wootton-Millward, 1993; Simon, 1992a) and are supportive of our predictions for social projection to in-group members. Indeed, as Simon (1992b) has pointed out, "There is clear empirical evidence that the outgroup homogeneity effect is not a universal law. Rather, the ingroup homogeneity effect is also an empirically proven and theoretically plausible reality" (p. 23).

the greatest mean relative number of high-diagnostic questions chosen over all of the other conditions. Specifically, low-causal-uncertainty participants in the high-goal-importance, in-group condition, on average, sought the most diagnostic information from their interaction partners. This pattern is worthy of further consideration, because activation of causal uncertainty beliefs by the anticipated interaction with a similar other was not expected. Moreover, uncertainty beliefs should have been particularly difficult (i.e., a greater situational input should have been needed) to activate in the low-causal-uncertainty individuals, for whom such beliefs were not chronically accessible. Could this result instead have been due to the in-group condition eliciting a goal other than causal uncertainty reduction?

Although future research will need to replicate this pattern of means and examine the specific motivation underlying the results, a possible answer may be derived from work on social comparison. Fazio (1979) proposed that there are two goals that motivate social comparison: construction and validation. Construction occurs when individuals seek information from others about an object or issue because they feel that they lack sufficient information about it. Validation, in contrast, involves seeking information because individuals desire to determine whether their judgments are valid.

In the out-group condition, the motive to reduce causal uncertainty should have functioned similarly to Fazio's (1979) notion of construction. Individuals in the in-group condition, however, should not have been motivated to reduce causal uncertainty, because social projection should have led them to believe that similar others would act and think as they do. Hence, little uncertainty should have been elicited in this condition, particularly for the low-causal-uncertainty participants, for whom causal uncertainty beliefs were less accessible. Instead, when anticipating an interaction with a similar other, people may have been motivated to validate or confirm their beliefs. This motive should have been more pronounced when the consequences of being inaccurate became more serious (i.e., when error sensitivity was high). Individuals under these circumstances presumably would have wanted to ensure that their perspective was appropriate and in agreement with everyone else's beliefs, particularly when they were unconcerned about uncertainty reduction. Consequently, low-causal-uncertainty participants in the high-goal-importance, in-group condition may have been motivated to seek more information from the interviewee because of the validation concerns engendered by the experimental situation.

Perhaps the most important contribution of the research reported herein is the evidence that it provides for Weary and Edwards's (1996) model of causal uncertainty. The results of this research suggest that causal uncertainty beliefs, even for those people for whom these beliefs are more accessible, are not always activated. In the present study, future interaction with a less predictable member of an out-group was necessary for the excitation level of the beliefs to reach threshold and for the information seeking consequences to be exhibited. Moreover, the importance of the uncertainty reduction goal combined additively with the situational source of uncertainty to influence information seeking. Neither factor alone was sufficient to induce increased information-seeking behavior in individuals for

whom causal uncertainty beliefs were more chronically accessible.

In addition to its theoretical import, the present research contributes to a growing literature (Weary & Edwards, 1994, 1996) concerned with the discriminant validity of the CUS. In this study, we provided such evidence with respect to one cognate and one affective variable with which causal uncertainty has been shown to covary, namely generalized control loss beliefs and level of depressive symptomatology. Despite moderate to substantial correlations with causal uncertainty scores, neither was able to account for the obtained information-seeking results.

Several caveats with respect to the results of this research should be mentioned. First, we are not claiming that goal-directed behaviors will always or necessarily follow from arousal of causal uncertainty reduction motives. When no effective strategies are available or when competing goals are activated, attempts to reduce causal uncertainty may well not be evident. Indeed, we suggested earlier that other social motives suppressed to some extent participants' diagnostic social information seeking (see Footnote 4). Future research will need to address the situational input and task factors that facilitate or inhibit strategies of causal uncertainty reduction.

Second, causal uncertainty beliefs are not the only dispositional motives that might result in a greater search for diagnostic social information. Neuberg and Newsom (1993) and Webster and Kruglanski (1994), for example, have developed scales that assess a personal need for structure and a need for closure, respectively. It is quite likely that when these related dispositional motives or needs are aroused, individuals low in the need for structure and the need for closure will seek more diagnostic social information. It is unlikely, however, that either of these motives represents alternative interpretations for the results of the present research. Although these motives may influence how people respond to causal uncertainty, there is no reason to believe that they pertain to the existence of causal uncertainty beliefs or to their activation. Indeed, the need for structure scale bears only a modest relationship and the need for closure scale, a negligible relationship to the CUS (Weary & Edwards, 1994, 1996).

Third, we do not wish to suggest that members of gender in-groups will never be the source of causal uncertainty beliefs and feelings for perceivers. Whereas gender was the single, salient, distinguishing characteristic of confederates that could have been gleaned by participants during their brief 3–8-s exposure to confederates in the study, in most everyday situations considerably more data would be available about members of perceivers' gender in-groups (e.g., personality characteristics, socioeconomic status, social roles, interaction task requirements). Alternative categorizations based on such additional information may well arouse causal uncertainty reduction motives with respect to gender in-group members. For similar reasons, members of perceivers' gender out-groups may not, under certain conditions, necessarily result in the arousal of such motives.

Finally, the notion that chronic causal uncertainty reduction motives play an important role in depressed people's search for and processing of social information also has been proposed by Weary and her colleagues (Weary, Marsh, Gleicher, & Edwards, 1993). Their model of the social-cognitive consequences of

depression is concerned with the effects of control loss perceptions on depression and social information processing. In this model, causal uncertainty is an important consequence of generalized control loss perceptions and is postulated to activate a subjective accuracy goal. This goal, in turn, is thought to motivate an extensive search for and processing of information that might reasonably render the social environment more understandable, predictable, and controllable. There is by now considerable support for the viewpoint that depression facilitates effortful processing and for the mediational role of control concerns (e.g., Edwards & Weary, 1993; Gleicher & Weary, 1991; Yost & Weary, 1996). Evidence regarding the hypothesized role of causal uncertainty, however, has awaited theoretical elaboration of the causal uncertainty construct and the subsequent development of a reliable and valid index of such beliefs. The research reported herein provides important support for the nomological network within which the causal uncertainty construct has been embedded and for the instrument designed to measure it. We hope that both will contribute to the growing literatures on motivated social-cognitive processes characteristic of depressed and normal populations.

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Sternberg Appointed Editor of *Contemporary Psychology* (*APA Review of Books*), 1999–2004

The Publications and Communications Board of the American Psychological Association announces the appointment of Robert J. Sternberg, Yale University, as editor of *Contemporary Psychology* (*APA Review of Books*) for a 6-year term beginning in 1999.

Sternberg, at the request of the Publications and Communications Board, as well as many readers, will be embarking on a program to make the journal more timely, more interesting, and more relevant to psychologists during his editor-elect year in 1998. Some of the changes envisioned include fewer but longer and more thoughtful reviews of books, reviews only of "new" books (with a few noteworthy exceptions), comparative textbook reviews at strategic times of the year, and changes in publication frequency and pricing. Sternberg welcomes suggestions for improving the journal and serving reader needs.

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