

## The Preoccupational Hazards of Social Life

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### ABSTRACT

Prospective interaction is often considered an antidote to inferential error because perceivers may expend effort making inferences about those with whom they expect to interact. These studies examined the possibility that prospective interaction may *prevent* error by motivating perceivers who have formed biased impressions of a partner to revise those impressions, but that it may also *promote* error by causing perceivers to spend resources preparing their own behavior rather than revising their biased impressions. Three experiments suggested that key features of a prospective interaction (e.g., role activity, goal familiarity, and partner novelty) determine whether people will become preoccupied with the preparation of their own behavior and thereby determine whether the prospect of interaction will prevent or promote inferential error.

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Anyone who has chosen a partner for tennis and another for marriage knows that decisions that have consequences are made differently than decisions that have none. In general, consequential decisions receive greater thought and care and are thus more likely to approach the normative ideal (e.g., [Hogarth, 1981](#); [Kruglanski & Freund, 1983](#); [Sanbonmatsu & Fazio, 1990](#)). In fact, pundits have argued that laboratory demonstrations of inferential error may seriously misrepresent the realities of daily life,

simply because our impressions of videotaped confederates are inconsequential relative to our impressions of the warm-blooded folk with whom we have enduring commerce (e.g., [Swann, 1984](#)). In everyday life, the prospect of social interaction may serve to check and balance the process of social inference: A man cannot thoughtlessly dismiss a woman as impulsive and emotional if he is to debate her, date her, or hire her to remove his gallbladder. Research has shown that a variety of inferential errors are, in fact, attenuated when perceivers expect to justify their conclusions to others ( [Fiske & Neuberg, 1990](#) ; [Tetlock, 1985](#) ). Even in the absence of such accountability pressures, the need for accurate information about an interaction partner can have its own remedial effect on memory and judgment (e.g., [Berscheid, Graziano, Monson, & Dermer, 1979](#) ; [Devine, Sedikides, & Fuhrman, 1989](#) ; [Erber & Fiske, 1983](#) ; [Feldman & Ruble, 1988](#) ; [Harkness, DeBono, & Borgida, 1985](#) ; [Monson, Keel, Stephens, & Genung, 1982](#) ; [Neuberg & Fiske, 1987](#) ; [Omoto & Borgida, 1988](#) ; [Srull & Brand, 1983](#) ). In short, the prospect of social interaction may encourage individuals to construe others in ways that will be justifiable and that will afford accurate prediction of the other's behavior.

There is little doubt that the prospect of interaction can increase a person's willingness to draw effortful conclusions about his or her interaction partner and thereby foster accuracy and reduce error. But we suggest that the consequences of this cut both ways. Just as the prospect of interaction raises the cost to the perceiver of inaccurate and unjustifiable beliefs about another, it may also place a premium on the other's conception of the perceiver and thus encourage the perceiver to spend precious resources crafting his or her own behavior. Such preoccupation with one's own actions can have powerful effects on one's judgments of others. For example, both [Brenner \(1973\)](#) and [Lord and Saenz \(1985\)](#) have shown that people who are absorbed in the preparation of their own behavior have especially poor memory for the behavior of others (see also [Bond & Omar, 1990](#) ; [Saenz & Lord, 1989](#) ; [Swann, Pelham, & Roberts, 1987](#) ). Similarly, [Gilbert, Jones, and Pelham \(1987\)](#) have shown that people can become so immersed in the "power politics" of social interaction that they fail to consider the external influences on their partners' behavior and thus draw biased inferences about those partners. Although people may be motivated to achieve accurate beliefs about consequential partners, they may be equally motivated to shape their partner's beliefs about them.

The prospect of interaction, then, may maximize both normative and self-regulatory concerns and should thus be capable of producing logically opposite effects on judgment. Different forms of interaction place different cognitive demands on the interactants, and these demands may be the critical factors that tilt the balance in favor of inferential accuracy or error. For example, if one's role in an upcoming interaction requires extraordinary activity (e.g., a professor giving a lecture), if one is pursuing an unfamiliar goal (e.g., an inexperienced attorney cross-examining a shifty witness), or if one's partner is particularly unusual (e.g., a therapist treating a paranoid client), one may need to devote a great deal of attention to the artful structuring of one's own behavior and thus have little attention to devote to the task of drawing inferences about one's partner. On the other hand, when an upcoming interaction requires one to play only a passive role (e.g., a student receiving advice on graduate programs), to pursue a familiar goal (e.g., a veteran dental assistant taking a medical history), or to interact with an unremarkable

partner (e.g., a person having coffee with an old friend), then one may behave without premeditation and thereby liberate attentional resources for attributional work.

## **Cognitive Busyness and Cognitive Recovery**

What sorts of errors might the prospect of social interaction inhibit or promote? Past research has shown that perceivers who are made *cognitively busy* tend to draw extreme trait inferences about others, even when the validity of such inferences is clearly belied by the available information. Thus, for instance, busy subjects who saw a woman behave anxiously were particularly likely to take this behavior as evidence of dispositional anxiety—even when they knew full well that the woman was an unwitting hostage to a distressing situation ( [Gilbert, Pelham, & Krull, 1988](#) ). Such instances of unwarranted dispositional inference (or *correspondence bias*; [Gilbert & Jones, 1986](#) ) arise because perceivers often characterize their targets in dispositional terms ("Gosh, what an anxious person she is!") and only later correct those characterizations with information about the target's situation ("But she *is* being interrogated about her sexual fantasies, so perhaps she's not as dispositionally anxious as I thought"). Because this second step is both dependent on and more fragile than the first, it is especially susceptible to impairment from concurrent tasks ( [Gilbert, 1989](#) , [1991](#) ; [Gilbert, Krull, & Pelham, 1988](#) ; [Lupfer, Clark, & Hutcherson, 1988](#) ; [Quattrone, 1982](#) ; [Uleman, 1987](#) ; but also see [Bassili & Smith, 1986](#) ; [Krull, 1990](#) ).

Fortunately, busy perceivers are not fated to act on their biased inferences because such inferences are often amenable to retroactive correction. Busy subjects who erroneously concluded that a fidgety woman was dispositionally anxious, for example, later repudiated those conclusions when they had both the motivation and the cognitive capacity to do so ( [Gilbert & Osborne, 1989](#) ). Such repudiation was possible, but not inevitable: Busy subjects who were later encouraged to imagine the woman in a variety of new situations quickly recognized the speciousness of their biased impressions, whereas busy subjects who were not given the opportunity to think about the woman, or who were encouraged to think about themselves in a variety of new situations, continued to embrace those initial impressions. Retroactive correction, then, requires that perceivers have the opportunity to consider the attributional implications of situational context and use these considerations to undo their initial beliefs about a target. If the perceiver is either unmotivated or unable to take these steps, then the biased impression will persist.

## **Experimental Rationale**

Each of the experiments that follows tested the hypothesis that prospective interaction with a target may *prevent* error by motivating perceivers who have formed biased impressions of a target to correct those impressions before they enter an interaction, but that it may also *promote* error by causing perceivers to spend attentional resources preparing their own behavior rather than correcting their biased impressions. In each experiment we induced subjects (through cognitive busyness) to make correctable errors about a target, and we then manipulated the demands of an upcoming interaction with the target. In Experiment 1, subjects expected to play either an active or a passive role

(interviewer vs. interviewee) in the ensuing interaction; in Experiment 2, subjects expected to pursue either a familiar or an unfamiliar goal (ingratiation vs. "disgratiation" <sup>1</sup> in the interaction; and in Experiment 3, subjects expected to interact with a target whose status did or did not encourage them to regulate carefully their own behavior (a disabled target vs. an enabled target). In each case, we expected that the demands of the interaction would determine whether the prospect of interaction facilitated or inhibited the retroactive correction of subjects' erroneous first impressions.

## Experiment 1

### Method Overview

Subjects watched a silent videotape of an anxious-looking female target engaged in a discussion with a stranger. Half of the subjects learned that the target was discussing her sexual fantasies and public humiliations (*anxious topics* condition), and the remaining subjects learned that the target was discussing her hobbies and ideal vacations (*mundane topic* condition). Half of the subjects rehearsed an eight-digit number while watching the videotape (*busy* condition), and the remaining subjects did not (*not-busy* condition). After watching the videotape, busy subjects stopped rehearsing the number (and thus stopped being busy). All subjects then rated the target on several trait dimensions. Next, subjects were led to expect that they would interact with the target. Half of the subjects were told that they would interview the target during the interaction (*active role* condition), and the remaining subjects were told that they would be interviewed by the target (*passive role* condition). All subjects spent 5 min writing down their thoughts and then rated the target on all measures once again.

### Subjects

Subjects were 59 female students at Luther College in Decorah, Iowa, who participated to receive extra credit in their introductory psychology course.

### Instructions

On arriving at the laboratory, subjects were greeted by a female experimenter who gave them a brief oral introduction to the experiment, provided them with complete written instructions, and then escorted each subject to a room (equipped with a video monitor), where she remained for the duration of the experiment.

The written instructions explained that subjects would be asked to watch two short clips from a videotape of a getting-acquainted conversation that had ostensibly taken place earlier in the year. This conversation was alleged to have been part of a project on the role of discussion topics in friendship formation. Subjects were told that two female students (who had never previously met) had been asked to discuss two topics for about 5 min each, and that subjects would be seeing one 60-s clip from each of these two discussions. It was explained that during the getting-acquainted conversation the camera had been positioned behind a discussant, and thus only one of the discussants (the target)

would be visible in the videotape. These were portions of the same videotapes used by [Gilbert, Pelham, and Krull \(1988\)](#) and by [Gilbert and Osborne \(1989\)](#). Subjects were told that they would be asked to rate the target on a variety of trait dimensions, including dispositional anxiety, and subjects were allowed to familiarize themselves with these measures.

### **Situational Constraint Manipulation**

Subjects were told that to protect the privacy of the discussants, the videotape would be shown without any sound. However, subjects were told that they would be able to tell which of the two topics was being discussed in each of the clips because the topic would be subtitled at the bottom of the screen.

Half of the subjects were randomly assigned to the anxious topics condition. In this condition the two subtitles indicated that the target was discussing anxiety-provoking topics (i.e., her sexual fantasies and her greatest public humiliation), and in both of these video clips the target appeared clearly anxious and ill at ease. The remaining subjects were assigned to the mundane topics condition. In this condition subjects saw the same video clips that were seen by subjects in the anxious topics condition; however, in this condition both of the subtitles indicated that the target was discussing humdrum, everyday topics (i.e., her hobbies and her ideal vacation).

In the anxious topics condition, then, the target's state anxiety could logically be attributed to the nature of the topics she was discussing (i.e., the situational constraints on her behavior) and therefore was not necessarily indicative of dispositional or trait anxiety. In the mundane topics condition, however, the same behavior could not logically have been caused by the nature of the discussion topics, and therefore the target's state anxiety could indeed be a symptom of trait anxiety (cf. [Snyder & Frankel, 1976](#)).

### **Cognitive Busyness Manipulation**

Subjects were assigned to either the busy or not-busy condition. All subjects were told that after watching the videotape they would be asked to estimate the target's trait anxiety (the distinction between trait and state anxiety was clearly explained to all subjects). In addition, subjects in the busy condition were told that the experimenter was interested in learning how well people could perform two dissimilar tasks at the same time and that the subject would therefore be required to rehearse an eight-digit number while watching the videotape. These busy subjects were given 25 s before the start of the videotape to study the eight-digit number and were instructed to hold the number in memory until the experimenter later asked them to report it.

We assumed that this rehearsal task would usurp subjects' processing resources, and our past research provided results consistent with that assumption (e.g., [Gilbert & Hixon, 1991](#); [Gilbert & Osborne, 1989](#)). Nonetheless, it seemed important to provide direct evidence for this claim. If, for example, subjects committed the number to long-term memory (rather than rehearsed it), then their processing resources would not be

diminished during the experimental task. To test this possibility, we invited 15 male and 18 female students at Phillips University (in Enid, Oklahoma) to participate in a probe reaction time (RT) experiment in exchange for extra credit in their introductory psychology course. Half of the subjects were shown an eight-digit number for 25 s and were told to keep the number in memory until they were asked to report it later. The remaining subjects were not shown the number. All subjects were told that they would hear a series of tones and that they should press a particular computer key as quickly as possible when the tone sounded. After seeing or not seeing the number for 25 s, subjects were presented with 29 tones over a 2-min test period. The intertone interval was randomly determined by the computer. At the end of the 2-min test period, subjects who had been shown the number were asked to report it. In the busy condition, 16 of 17 subjects reported the number correctly, and the remaining subject misreported a single digit.

The probe RT data were quite clear. On average, subjects who had been asked to rehearse an eight-digit number responded to the tones more slowly  $M_{RT} = 386$  ms than did subjects who were not asked to rehearse the number  $M_{RT} = 215$ ,  $t_{31} = 5.96$ ,  $p < .001$ . This difference is direct evidence for our contention that subjects do rehearse the number and that this rehearsal usurps processing resources.

### **Dependent Measures: Initial Ratings**

Immediately following the videotape, busy subjects were asked to report the eight-digit number that they had been rehearsing, and thus these subjects were no longer busy when they completed the dependent measures. All subjects estimated the target's trait anxiety on three 13-point scales anchored at the endpoints with the phrases (a) *is probably comfortable (uncomfortable) in social situations*, (b) *is a calm (nervous) sort of person*, and (c) *is generally relaxed (anxious) with people*. Subjects were then asked to recall the topics that the target had ostensibly been discussing. Next, subjects were asked to rate the target on 10 additional trait dimensions ( *honesty, warmth, intelligence, sociability, happiness, dominance, interestingness, cooperativeness, self-confidence, and humor* ). These 10 additional trait scales were included to allow us to measure the subject's liking for the target.

### **Prospective Interaction Manipulation**

After completing these measures, subjects were told that in a few minutes they would have an opportunity to interact with the female target whom they had seen on the videotape. Subjects in the active role condition were told that the interaction would have an interview format and that they would interview the target, whereas subjects in the passive role condition were told that they would be interviewed *by* the target. Subjects who expected to play the role of interviewer were told that they would need to formulate questions to ask the interviewee, whereas subjects who expected to play the role of interviewee were told that they would simply be required to answer the questions posed by the interviewer.

### **Dependent Measures: Secondary Ratings Thought-listing task.**

After being led to expect an interaction with the target, each subject was told to "take a few minutes to organize your thoughts" before the interaction began. Subjects were asked to write their thoughts as they waited for the interaction to begin and were given 5 min to do so. The experimenter emphasized that she did not want the subjects to write about anything in particular; rather, they should feel free to write whatever thoughts came to mind "no matter how irrelevant you think they might be." We hoped to use these protocols as a crude estimate of the amount of thought subjects devoted to the preparation of their own behavior rather than to the target's behavior and personality.

### **Trait ratings.**

After the 5-min writing period had elapsed, subjects were asked again to rate the target on the same 3 trait anxiety scales and the 10 additional scales that were administered earlier. Subjects were told that "some people like to change their ratings after being given time to think, and others become even more sure of their previous ratings. Remember that either changing your ratings or keeping them the same as before is perfectly fine." After completing these ratings, subjects were probed for suspicion, fully debriefed, thanked for their participation, and dismissed.

### **Results and Discussion Digit Recall**

All busy subjects reported the eight-digit number with perfect accuracy. Some theorists have argued that imperfect performance on an overload task is necessary if one is to claim unequivocally that capacity was exceeded (e.g., [Kantowitz, 1974](#)). Thus, our subjects' perfect performances may suggest that the busyness manipulation did not, in fact, usurp subjects' processing resources. Our probe RT data strongly suggest that the typical subject did indeed rehearse the number (rather than merely store it in long-term memory) and that this rehearsal did usurp processing resources. Nonetheless, our interpretation of the results would more likely convince the skeptic if busy subjects had shown minor but ubiquitous errors on the digit-rehearsal task. Future researchers should consider using more demanding manipulations of load to avoid such difficulties in interpretation.

### **Analysis of Trait Ratings**

The dependent measures were completed after busy subjects stopped being busy, and thus we hereinafter refer to them as *formerly busy* subjects.

Subjects' initial ratings of the target on the three trait anxiety scales (anxious, nervous, and uncomfortable) were averaged to create a single trait anxiety index (coefficient  $\alpha = .91$ ). After completing the first set of dependent measures, subjects were led to expect to play either an active role or a passive role in an upcoming interaction with the target, they were allowed 5 min to transcribe their thoughts, and they then completed the second set of dependent measures. Subjects' secondary ratings of the target on the three trait anxiety

scales were also averaged to form a trait anxiety index (coefficient  $\alpha = .88$ ). These indexes were submitted to a 2 (topic: anxiety-provoking or mundane)  $\times$  2 (busyness: formerly busy or not-busy)  $\times$  2 (role: active or passive)  $\times$  2 (ratings: initial or secondary) analysis of variance (ANOVA). Only the last of these was a within-subjects variable. The analysis revealed the predicted four-way interaction,  $F(1, 51) = 2.28, p = .137$ . Although this interaction did not attain conventional levels of reliability, the complexity of the effect and the relatively small cell sizes suggested that it might be profitable to conduct independent analyses of the two trait anxiety indexes. <sup>2</sup>

### **Initial ratings: Does busyness induce a correspondence bias?**

The initial trait anxiety index was submitted to a 2 (topic)  $\times$  2 (busyness)  $\times$  2 (role) ANOVA, which revealed a main effect of topic,  $F(1, 51) = 35.43, p < .001$ . This main effect was qualified only by the predicted Topic  $\times$  Busyness interaction,  $F(1, 51) = 62.34, p < .001$ . As the first row in [Table 1](#) shows, the busyness-induced correspondence bias demonstrated by [Gilbert, Pelham, and Krull \(1988\)](#), [Gilbert, Krull, and Pelham \(1988\)](#), and [Gilbert and Osborne \(1989\)](#) was replicated: Not-busy subjects used standard attributional logic and considered the target more trait-anxious when she discussed mundane topics than when she discussed anxiety-provoking topics (i.e., they used their knowledge of the situation to discount and augment their initial characterizations of the target). However, formerly busy subjects failed to use information about the situation to discount or augment their characterizations and thus considered the target equally trait-anxious regardless of the topics she was ostensibly discussing. Note that although the formerly busy subjects apparently did not consider the topics when making their attributions, they recalled those topics with complete accuracy.

### **Secondary ratings: Was the correspondence bias corrected?**

The secondary trait anxiety index was submitted to a 2  $\times$  2  $\times$  2 ANOVA (as above), which revealed a main effect of topic,  $F(1, 51) = 84.15, p < .001$ ; a Topic  $\times$  Role interaction,  $F(1, 51) = 10.54, p < .01$ ; and a Topic  $\times$  Busyness interaction,  $F(1, 51) = 17.50, p < .001$ . All of these effects, however, were qualified by the predicted Topic  $\times$  Busyness  $\times$  Role interaction,  $F(1, 51) = 5.04, p < .05$ .

As the second row in [Table 1](#) shows, all subjects rated the target as more trait-anxious when she discussed mundane rather than anxiety-provoking topics—*except* formerly busy subjects who expected to play an active role. Formerly busy subjects who expected to take a passive role corrected their original inferences about the target, but formerly busy subjects who expected to take an active role continued to ignore the contribution of the discussion topics to the target's behavior and continued to regard her as dispositionally anxious. In short, the prospect of interaction does seem to motivate subjects to engage in the kind of thinking that promotes recovery from undercorrected impressions; although the passive role of interviewee allows such thinking, the active role of interviewer seems to preclude it.

### **Thought Listings: What Mediated Correction of the Correspondence Bias?**



We predicted that subjects who expected to engage in interaction with a target would be motivated to correct their biased inferences about the target by thinking about her, but that subjects who expected to play an active role in that interaction would instead spend time preparing their own behavior and thus fail to do the cognitive work necessary for such corrections. The pattern of initial and secondary trait ascriptions strongly supports this reasoning. But is there any evidence to indicate that such self-regulatory efforts mediated the failure to correct?

Subjects' thought listings were coded by three judges who remained blind to condition. The judges rated each thought listing on a series of 5-point scales. One scale measured the extent to which each thought listing contained thoughts about the target's behavior and personality (e.g., subjects wrote "She seems to be a nervous person," "She obviously doesn't have very good social interacting [sic] skills," and "I swear she makes me feel as tense as she looks. If she doesn't smoke she should start"). This category explicitly excluded thoughts that happened to be about the target but that were not about her behavior or personality (e.g., subjects wrote "(Her) teeth sure are yellow" and "She is rather a plain person"). Another scale measured the extent to which each thought listing contained explicit behavioral preparations for the upcoming interaction (e.g., subjects wrote, "I'll ask her about her hair and where she bought her clothes at [sic]" and "I'll have to be careful not to ask too personal questions"). The scales were anchored with the phrases *very little* (1) and *most of the time* (5). Ratings on these scales (hereinafter called the *target-thought score* and the *preparation-thought score*, respectively) were reliable across the three judges (Kendall's coefficients = .85 and .87, respectively), and thus the ratings were collapsed across judges. <sup>3</sup>

Preparation-thought scores were submitted to an ANOVA (as above), which revealed only the predicted main effect of role,  $F(1, 58) = 40.51, p < .001$ . As the fourth row in [Table 1](#) shows, subjects who expected to play an active role devoted more of their thoughts to the preparation of their own behavior than did subjects who expected to play a passive role. A similar ANOVA performed on target-thought scores also revealed only the predicted main effect of role,  $F(1, 58) = 38.09, p < .001$ : As the fifth row of [Table 1](#) shows, active-role subjects thought less about the target's personality than did passive-role subjects.

If subjects failed to correct their initial impressions because they spent time thinking about their own behavior rather than about the target, then we might expect the magnitude of such self-regulatory efforts to be inversely related to the magnitude of attributional correction. A *correction score* was computed for each subject by subtracting the subject's secondary ratings on the trait anxiety index from her primary ratings on that index and then multiplying that score by  $-1$  for subjects in the mundane topic conditions. This recoding was necessary to collapse the data across topic conditions, because correction in different topic conditions was reflected by difference scores of opposite sign. For subjects in the anxious topic condition, correction took the form of discounting (a negative change on the anxiety index), whereas for subjects in the mundane topic condition, correction took the form of augmenting (a positive change on the anxiety index; see [Kelley, 1972](#)). A *self-regulation score* was computed for each subject by

simply subtracting the subject's target-thought score from her preparation-thought score, hence providing a measure of the relative amount of thought subjects devoted to their own versus the target's behavior. Analyses revealed an inverse relation between self-regulation and correction scores,  $r(57) = -.32, p < .01$ . Subjects who made the greatest self-regulatory effort were least likely to correct their impressions of the target—and clearly, subjects who expected to play an active role made the greatest self-regulatory effort. Finally, each of the 10 additional trait scales described a valenced dimension (e.g., warmth, intelligence, and so on), and subjects' secondary ratings on these scales were averaged to create a general liking index (coefficient  $\alpha = .87$ ). An ANOVA performed on this index revealed no effects (all  $F$ 's  $< 1.32$ ); in particular, active-role and passive-role subjects liked the target equally well ( $F < 1$ ).

## Experiment 2

Experiment 1 showed that the demands of one's role in an upcoming interaction can determine whether the prospect of interaction will induce or inhibit the retroactive correction of biased inferences. It is somewhat ironic to note that when subjects were given the task of learning about their partner (i.e., the role of interviewer) they were less likely to correct their erroneous impressions of that partner than when they were given the task of informing their partner about themselves (i.e., the role of interviewee). Nonetheless, role activity affected behavioral preparation in a rather direct way (i.e., by requiring such preparation as part of the task), and in Experiment 2 we sought to show that similar effects could be caused by a more subtle aspect of the prospective interaction, namely, the familiarity of the perceiver's interaction goal. People enter interactions with a variety of different motives, some of which are common to almost all interactions (e.g., being liked), and others of which may be quite specialized (e.g., political persuasion). Familiar goals are usually achieved by executing an overlearned set of "standard operating procedures" ( [Jones & Thibaut, 1958](#) ), whereas unfamiliar goals require consciously directed behavior for their attainment (see [Bargh, 1990](#) ). As such, the pursuit of unfamiliar (but not familiar) goals should interfere with one's ability to correct a biased impression of one's partner.

In Experiment 2, we asked perceivers to pursue either a familiar goal (ingratiation) or an unfamiliar goal (disgratiation) in an upcoming interaction. There is evidence to suggest that when people wish to elicit the positive regard of others, they execute a highly routinized set of ingratiating behaviors that amount to little more than "turning on the charm" (e.g., [Godfrey, Jones, & Lord, 1986](#) ; [Jones, 1964](#) ; [Jones & Pittman, 1982](#) ). When people want to be liked, they generally do not have to think about the appropriate behaviors; rather, they merely "try to convey an impression of liking, with the leaning and smiling and gazing following from that, perhaps even out of their awareness" ( [DePaulo, in press](#) ). Conversely, eliciting disdain is a relatively unusual goal whose execution should require some strategic planning ( [Baumeister, Hutton, & Tice, 1989](#) ; [Gilbert, Krull, & Pelham, 1988](#) ) and should thus usurp resources that might otherwise be used for attributional correction.

### Method Overview

Subjects watched the same videotape as in Experiment 1. Half of the subjects rehearsed an eight-digit number as they viewed the tape (busy subjects) and the remaining subjects did not (not-busy subjects). After watching the videotape, busy subjects were asked to recall the number (and, thus, were no longer busy). All subjects then rated the target on several trait dimensions and were led to expect an upcoming interaction with the target. Half of the subjects were told that they should ingratiate the target (familiar goal condition), and the other half were told that they should disgratiate the target (unfamiliar goal condition). Before the interaction, subjects spent 5 min listing their thoughts. Subjects then rated the target on all measures once again, were thoroughly debriefed, thanked for their participation, and dismissed.

## Subjects

Subjects were 80 female students at Luther College in Decorah, Iowa, who participated to receive extra credit in their introductory psychology course.

## Instructions

The instructions, procedures, and measures were identical to those used in Experiment 1, except that subjects were not assigned an active or a passive role in the upcoming interaction; rather, subjects were asked to pursue different goals in the interaction. Subjects who were assigned to the *familiar goal* condition were told that their goal in the interaction would be to ingratiate the target; subjects in the *unfamiliar goal* condition were given the more unusual goal of disgratiating the target.

## Results and Discussion Correspondence Bias

As in Experiment 1, all subjects were able to recall the eightdigit number and the discussion topics with perfect accuracy. Subjects' initial ratings of the target's trait anxiety on the three scales were averaged to create an initial trait anxiety index (coefficient  $\alpha = .92$ ). Subjects' secondary ratings of the target's trait anxiety were averaged to create a secondary trait anxiety index (coefficient  $\alpha = .91$ ). Scores on these indexes were submitted to a 2 (topic: anxious or mundane)  $\times$  2 (busyness: not-busy or formerly busy)  $\times$  2 (goal: familiar or unfamiliar)  $\times$  2 (ratings: initial or secondary) ANOVA, with the last of these being a within-subjects variable. The analysis revealed only a four-way interaction,  $F(1, 72) = 29.87, p < .001$ .

This interaction was decomposed by submitting each index to a 2 (topic)  $\times$  2 (busyness)  $\times$  2 (goal) ANOVA. Analysis of the initial trait anxiety index revealed a main effect of topic,  $F(1, 72) = 15.65, p < .001$ , which was qualified only by the predicted Topic  $\times$  Busyness interaction,  $F(1, 72) = 23.46, p < .001$ . As the first row in [Table 2](#) shows, the findings of Experiment 1 were strongly replicated: Formerly busy subjects failed to use the situational constraint information (i.e., the topics) to correct (i.e., discount or augment) their characterizations of the target and thus considered her equally trait-anxious regardless of the topics she was ostensibly discussing.

Did subjects' goals in the upcoming interaction affect their ability to recover from this correspondence bias? The secondary trait anxiety index was submitted to an ANOVA (as above), which revealed a main effect of topic,  $F(1, 72) = 41.19, p < .001$ , and a Topic  $\times$  Goal interaction,  $F(1, 72) = 8.95, p < .01$ . Both of these effects were qualified by the predicted Topic  $\times$  Goal  $\times$  Busyness interaction,  $F(1, 72) = 18.85, p < .001$ . As the second row in [Table 2](#) shows, all subjects rated the target as more traitanxious when she discussed mundane rather than anxiety-provoking topics—*except* formerly busy subjects who were assigned an unfamiliar goal. Formerly busy subjects who expected to pursue a familiar goal corrected their original inferences about the target, but formerly busy subjects who expected to pursue an unfamiliar goal did not. (In fact, these subjects showed an unexpected and marginally reliable reversal of this tendency.) In short, the prospect of interaction did indeed cause subjects to correct their biased inferences, but only when their goal in that interaction was so familiar that it did not itself diminish their capacity for conscious reasoning.

### **Mediators of the Correspondence Bias**

Subjects' thought listings were coded on the same scales used in Experiment 1 by three judges who remained blind to condition. <sup>4</sup>Preparation-thought and target-thought scores were reliable across the three judges (Kendall's coefficients = .85 and .93, respectively) and were thus collapsed across judges. Preparation-thought scores were submitted to an ANOVA (as above), which revealed the predicted main effect of goal,  $F(1, 72) = 19.87, p < .01$ . As the fourth row of [Table 2](#) shows, subjects who expected to pursue an unfamiliar goal devoted more of their thoughts to the preparation of their own behavior than did subjects who expected to pursue a familiar goal. A similar ANOVA performed on the target-thought scores also revealed the predicted main effect of goal,  $F(1, 72) = 19.36, p < .001$ . As the fifth row of [Table 2](#) shows, unfamiliar-goal subjects devoted less thought to the target than did familiar-goal subjects. In short, subjects who expected to pursue an unfamiliar goal thought more about their own behavior and less about the target's personality than did subjects who expected to pursue a familiar goal. Finally, self-regulation scores and correction scores were computed (as in Experiment 1), and analysis revealed an inverse relation between these scores,  $r(78) = -.53, p < .001$ . Once again, those subjects who performed the greatest self-regulation also performed the least attributional correction.

### **An Alternative Explanation?**

Our primary finding was that formerly busy subjects who expected to pursue an unfamiliar goal continued to embrace their conclusion that the target was a dispositionally anxious person. One alternative explanation for this finding arises from the way in which we manipulated goal familiarity. Subjects who were assigned to pursue an unfamiliar goal were asked to elicit the target's disdain, and most subjects probably planned to accomplish this task by acting disagreeably toward the target. Is it possible that subjects sought to justify their intended antagonism by derogating the target (e.g., by insisting that she was a dispositionally anxious person who deserved to be antagonized)?

This alternative explanation is implausible for two reasons. First, not-busy subjects who expected to disgratiate the target did not consider her particularly anxious, though they should have experienced the same need to justify their behavior as did the formerly busy subjects. Second, subjects' secondary ratings on the 10 trait scales were averaged to create a general liking index (coefficient  $\alpha = .97$ ). This index was submitted to an ANOVA (as above), which revealed no effects (all  $p$ 's  $> .15$ ). As the sixth row in [Table 2](#) shows, the main effect of role was strikingly absent ( $F < 1$ ). Subjects who expected to disgratiate the target were no more likely to derogate her on the 10 valenced dimensions than were subjects who expected to ingratiate the target. This fact is convincing evidence against the behavioral justification alternative.

### Experiment 3

Experiments 1 and 2 provide evidence for the debilitating effects of prospective interaction when the person's role or goal requires sustained, thoughtful activity. In Experiment 3, we attempted to show that interactions can create demands that are not specifically linked to a person's role or goal, but rather, to the nature of the interactants themselves. Some investigations suggest that people typically find it difficult to interact with stigmatized or "marked" individuals because they are unsure whether they should explicitly inquire about, tacitly acknowledge, or simply pretend not to notice the other's "mark" (see [Goffman, 1963](#); [Jones et al., 1984](#); [Langer, Taylor, Fiske, & Chanowitz, 1976](#)). Such uncertainty may cause people to regulate carefully their words and deeds, lest their unregulated behavior inadvertently offend the marked partner (e.g., staring at an amputee's missing limb or asking blind persons if they "see" the point; see [Omoto & Borgida, 1988](#)). With these considerations in mind, we led subjects in Experiment 3 to expect to interact with either a disabled or an enabled target, and we predicted that the prospect of interaction with a disabled target would impair recovery from correspondence bias just as active roles and unfamiliar goals had done in the earlier experiments.

#### Method Subjects

Subjects were 26 female students at Luther College in Decorah, Iowa, who participated to receive extra credit in their introductory psychology course.

#### Procedures

Subjects were given the same instructions that were given to busy subjects in Experiments 1 and 2. These two experiments (as well as the four experiments described in [Gilbert & Osborne, 1989](#)) clearly show that when recovery occurs, it is complete. That is, when formerly busy subjects recover, their ratings of the target are identical to the ratings of not-busy subjects. As such, we did not feel it was necessary to include a not-busy control condition in Experiment 3, and all subjects were made busy by rehearsing an eight-digit number while they watched the videotape. In addition, Experiments 1 and 2 showed that the pattern of data in the mundane topics condition (in which correction takes the form of augmenting) is the mirror image of the pattern of data in the anxious topics condition (in which correction takes the form of discounting). Thus, we did not

include a mundane topics condition in this experiment; instead, all subjects were shown the anxious topic tape used in Experiments 1 and 2.

Immediately after watching the videotape, subjects were asked to recall the eight-digit number and the discussion topics. Subjects then rated the target on the same scales used in Experiments 1 and 2 (the  $T_1$  ratings). Next, subjects were told that they would be interacting with the woman from the videotape. Half the subjects were told that the interaction would take place in the room in which they were currently seated (*enabled target* condition), and the remaining subjects were told "I'll have to take you to another room, however, because I can't fit her wheelchair in here" (*disabled target* condition). Subjects were given no other information about the form of, or their role in, the interaction. Subjects were then given 5 min to list their thoughts, and after doing so, they again rated the target on all scales the  $T_2$  ratings. Finally, subjects were thoroughly debriefed, thanked for their participation, and dismissed.

## Results and Discussion

As before, all subjects reported both the eight-digit number and the discussion topics with complete accuracy, except for 1 subject who transposed two digits.

Subjects' initial and secondary ratings of the target's trait anxiety were averaged across the individual scales to create two trait anxiety indexes (coefficient  $\alpha$ s = .89 and .90, respectively). These indexes were submitted to a 2 (target: disabled or enabled)  $\times$  2 (ratings: initial or secondary) ANOVA that revealed a main effect of ratings,  $F(1, 24) = 43.18, p < .001$ , as well as the predicted Ratings  $\times$  Target interaction,  $F(1, 24) = 54.10, p < .001$ . As the third row of [Table 3](#) shows, subjects who expected to interact with an enabled target corrected their impressions of the target's dispositional anxiety, whereas subjects who expected to interact with a disabled target did not.

Subjects' thought listings were coded on the same scales used in Experiments 1 and 2 by three naive judges who remained blind to condition. Both preparation-thought scores and target-thought scores were reliable across the three judges (Kendall's coefficients = .85 and .82, respectively) and were thus collapsed across judges. Both preparation-thought scores and target-thought scores were submitted to separate ANOVAs (as above), both of which revealed only the predicted main effect of target,  $F(1, 24) = 31.15, p < .001$ , and  $F(1, 24) = 46.18, p < .001$ , respectively. As the fourth and fifth rows of [Table 3](#) show, subjects who expected to interact with a disabled target devoted more thought to the preparation of their own behavior and less thought to the target's personality than did subjects who expected to interact with an enabled target. This is particularly striking in that one might naively predict that subjects would think *more* about an unusual or atypical target, but in fact the opposite was the case. Self-regulation scores and correction scores were computed as in Experiments 1 and 2, and analysis revealed an inverse relation between the two scores,  $r(24) = -.56, p < .001$ . Once again, those subjects who made the greatest self-regulatory efforts did the least attributional correction. Finally, a general liking index was computed as in Experiments 1 and 2 (coefficient  $\alpha = .88$ ), and

an ANOVA (as above) performed on this index indicated that subjects did not differ in their liking for the disabled and enabled targets ( $F < 1$ ).

## General Discussion

The prospect of consequential interaction with a person is a double-edged sword: Although it may motivate perceivers to reanalyze their first fleeting impressions, it may also cause them to become so preoccupied with the construction and maintenance of their own behavior that such reanalyses are impossible. In the present experiments, perceivers who anticipated playing a demanding role in an upcoming interaction were especially unlikely to correct their biased impressions of their interaction partner (Experiment 1), as were perceivers who expected to engage in some unusual self-presentational maneuvers (Experiment 2) or who expected to meet an unusual partner (Experiment 3).

At first blush these may seem like innocuous lapses. One of the lessons to be learned from these experiments (as well as from the experiments of [Gilbert & Osborne, 1989](#)) is that people are quite capable of retrospectively eliminating busyness-induced correspondence biases from their judgments. As such, the manipulations in the foregoing experiments (role activity, goal familiarity, and partner novelty) may not present enduring obstacles to correction so much as temporary delays in such corrections. After all, if perceivers fail to correct their impressions of a target in the moments before interaction, might they not simply correct them in the moments that follow?

Such an account ignores the fact that erroneous beliefs may contaminate subsequent information processing in ways that are not amenable to remediation. In an experiment by [Gilbert and Osborne \(1989\)](#), busy perceivers erroneously concluded that a target who discussed anxiety-provoking topics was dispositionally anxious. These perceivers were then allowed to hear an audiotape in which the target calmly provided a few unremarkable facts about herself. Not surprisingly, the perceivers rated the target's voice as betraying considerable anxiety. When ultimately given the opportunity to retrospect, these perceivers repudiated their original misconceptions ("I guess she acted anxious during the initial interview because of the topics she was discussing") but persisted in their mistaken beliefs and cited the information in the audiotape as evidence ("But I still think she's an anxious person because of the nervous way she talked in the tape I just heard").

This experiment suggests that (a) when mistaken impressions are allowed to persevere they can color new information and (b) this fact is not taken into account when the perceiver performs retroactive correction. These facts, combined with the results of the present experiments, suggest that perceivers who engage in behavioral preparation in the moments prior to an interaction with a target may enter that interaction with their biased impressions intact and thus may go on to interpret the particulars of the interaction in a biased way. When subsequently allowed the opportunity to rethink their impressions, these perceivers may repudiate their *initial* misperceptions, but they may be unlikely to realize that their initial misperceptions affected their interpretation of the subsequent

interaction. In short, biased impressions that are not corrected quickly may eventuate in errors that cannot be corrected at all.

Our experiments show that the degree to which one engages in preparatory self-regulation can determine whether the prospect of interaction extends or extinguishes inferential error. The demands of one's role, the familiarity of one's goal, and the novelty of one's partner are only some of the important factors that should encourage behavioral preparation. For example, behavioral preparation will probably be promoted more by the prospect of interaction with powerful than with powerless individuals: A young man who is about to meet the parents of his intended will probably be more preoccupied with the regulation of his behavior than the parents will be with their own, despite the fact that the roles are equally novel and demanding for all participants. Similarly, the timing of a prospective interaction may partially determine its effects: A professor may ruminate about how best to greet a distinguished colleague at the airport, but she is more likely to be preoccupied by such matters when the plane is due at noon than when it is due next February.

Our experiments, then, are not intended to provide an exhaustive or even an extensive list of the factors that can promote behavioral regulation prior to social interaction. Rather, they are meant to illustrate the wide variety of such factors and to show how changes in seemingly minor aspects of an upcoming interaction can cause that interaction to become an impediment to, rather than a catalyst for, accurate social judgment. Psychologists have long known that the prospect of interaction with a person can have dramatic effects on what and how one thinks about that person. Our research simply suggests that these effects will not always be of one kind. Just as prospective interaction can motivate careful analysis of one's partner, it can also prove a distraction from such analysis. It may well be that the consequential decisions of the "real world" are made with special care, and that laboratory experiments sometimes underestimate the abilities of the social perceiver. But it is equally clear that the prospect of interaction can have conceptually opposite effects and is certainly not a panacea for inferential ills.

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## 1

*Disgratiation* is a term coined to indicate eliciting disdain.

## 2

All lower order analyses in this and the next study used the error terms and degrees of freedom from the respective four-way ANOVAs.

## 3

Past research ( [Gilbert & Osborne, 1989](#) ) has shown that when subjects are asked to think about a target in general, their recovery from the correspondence bias is facilitated. As such, we coded only the two general aspects of the thought listings mentioned earlier. There are several reasons why the thought listings do not readily lend themselves to more specific sorts of coding (e.g., coding for explicitly corrective thoughts). First, there is enormous variability in the way subjects complete the task: Some subjects write coherent paragraphs and others simply list words and phrases in a stream-of-consciousness manner. Second, it is not always easy to know whether a particular thought about the target is corrective (e.g., subjects wrote, "I wonder if she's always this nervous?" and "First impressions are deceiving, so I'll take it easy on this one"). Finally, thought listings are extremely crude indexes of what subjects are actually thinking, and it is quite possible for subjects to report a precorrective thought (a subject wrote, "A lot of people get camera-shy") that leads to an unreported corrective thought.

Judges also estimated other parameters of the thought listings (e.g., how certain the subject was of her thoughts about the target, the extent to which thoughts about the target focused on situational vs. dispositional factors, etc.). A full discussion of these measures may be found in [Osborne \(1990\)](#).

Table 1.

Table 1  
*Subjects' Ratings of Target's Trait Anxiety and Content of Subjects' Thought Listings: Experiment 1*

Rating	Not-busy				Formerly busy			
	Active		Passive		Active		Passive	
	Mundane	Anxious	Mundane	Anxious	Mundane	Anxious	Mundane	Anxious
Anxiety index at T <sub>1</sub>	11.62	> 7.24	11.29	> 7.19	9.00	= 9.71	9.43	= 9.63
Anxiety index at T <sub>2</sub>	10.95	> 7.19	11.19	> 6.86	9.13	= 9.13	10.62	> 7.46
Change (T <sub>1</sub> - T <sub>2</sub> )	0.67	0.05	0.10	0.33	-0.13	0.58	-1.19*	2.17**
Preparation-thought score	4.57	3.71	2.29	2.00	4.13	4.00	1.57	2.13
Target-thought score	1.86	2.14	3.57	4.00	2.13	2.13	4.43	3.63
Liking index	6.77	6.57	7.03	7.60	8.09	7.22	7.70	7.38
Cell <i>n</i>	7	7	7	8	7	7	8	8

Note. T<sub>1</sub> = time of initial ratings; T<sub>2</sub> = time of secondary ratings. Simple effects tests were performed on all change scores and all contrasts marked as equalities (=) or inequalities (>). Contrasts marked as inequalities are  $p < .01$ ; contrasts marked as equalities are  $p > .22$ ; unmarked change scores are  $p > .25$ .

\*  $p < .07$ . \*\*  $p < .001$ .

Table 2  
*Subjects' Ratings of Target's Trait Anxiety and Content of Subjects' Thought Listings: Experiment 2*

Rating	Not busy				Formerly busy			
	Familiar		Unfamiliar		Familiar		Unfamiliar	
	Mundane	Anxious	Mundane	Anxious	Mundane	Anxious	Mundane	Anxious
Anxiety index at T <sub>1</sub>	11.37	> 7.77	11.50	> 7.50	9.97	= 10.10	9.93	= 10.00
Anxiety index at T <sub>2</sub>	10.53	> 7.77	11.40	> 7.47	11.77	> 6.50	9.27	≤ 10.97
Change (T <sub>1</sub> - T <sub>2</sub> )	0.84	0.00	0.10	0.03	-1.80*	3.60*	0.66	-0.97
Preparation-thought score	2.40	2.80	4.80	4.50	2.10	3.10	4.50	4.30
Target-thought score	4.70	5.20	3.50	3.90	5.70	5.90	3.20	3.10
Liking index	6.15	6.93	6.80	6.82	7.00	7.85	6.22	11.52

Note. Cell *ns* = 10. T<sub>1</sub> = time of initial ratings; T<sub>2</sub> = time of secondary ratings. Simple effects tests were performed on all change scores and all contrasts marked as equalities (=) or inequalities (> or ≤). Contrasts marked as inequalities are  $p < .01$ ; contrasts marked as equalities are  $p > .25$ ; the contrast marked as ≤ is  $p = .06$ ; unmarked change scores are  $p > .25$ .

\*  $p < .04$ .

**Table 3**  
*Subjects' Ratings of Target's Trait Anxiety and Content of Subjects' Thought Listings: Experiment 3*

Rating	Target	
	Disabled	Enabled
Anxiety at T <sub>1</sub>	9.59	= 9.46
Anxiety at T <sub>2</sub>	9.72	> 7.18
Change (T <sub>1</sub> - T <sub>2</sub> )	-0.13	2.28*
Preparation-thought score	4.00	> 1.85
Target-thought score	1.77	< 3.92
Liking index	7.61	= 7.49

*Note.* Cell *ns* = 13. T<sub>1</sub> = time of initial ratings; T<sub>2</sub> = time of secondary ratings. Simple effects tests were performed on all change scores and all contrasts marked as equalities (=) or inequalities (> or <). Contrasts marked as inequalities are  $p < .01$ ; contrasts marked as equalities are  $p > .19$ ; unmarked change scores are  $p > .25$ .

\*  $p < .001$ .