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Thinking Backward
Some Curable and Incurable Consequences of Cognitive Busyness

Daniel T. Gilbert
Department of Psychology University of Texas at Austin
Randall E. Osborne
Department of Psychology University of Texas at Austin

ABSTRACT

Social interaction imposes a variety of attentional demands on those who attempt it. Such cognitively busy persons often fail to use contextual information to correct the impressions they form of others. The 4 experiments reported here examined the corrigibility of this effect. Although formerly busy perceivers were able to correct their mistaken impressions retroactively (Experiment 1), such retroactive correction was not inevitable (Experiment 2). In addition, when perceivers were able to correct their original impressions retroactively, they were still unable to correct subsequent inferences that had been biased by those original impressions (Experiments 3 and 4). As such, perceivers were occasionally able to overcome the primary, but not the sucsidiary, effects of cognitive busyness. The results are discussed in terms of the metastasis of false knowledge.

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Correspondence may be addressed to Daniel T. Gilbert, Department of Psychology, University of Texas, Austin, Texas, 78712.
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Although the most sophisticated robot cannot chew bubblegum while hopping backward on one leg, most schoolchildren can perform this maneuver with an ease that belies the inherent complexity of its component activities. For human children, the simultaneous

performance of complex operations such as chewing and hopping occurs gracefully only because one of them occurs automatically–that is, without a significant degree of conscious attention. The identity of this automatic operation is revealed when the child appropriates attention to an entirely different task: Backward-hopping gum chewers who try to conjugate Latin verbs, for example, are far more likely to fall over than to swallow their gum.

This basic notion–that the attempt to perform several operations at once often results in the failure of the least automatic (most effortful) operation–has enabled investigators to decompose many psychological processes into their relatively automatic and effortful components (e.g., Baddeley & Hitch, 1974 ; Bargh, 1982 , 1984 ;, Bargh & Thein, 1985 ; Ben Zur & Breznitz, 1981 ; Hasher & Zacks, 1979 ; Kruglanski & Freund, 1983 ; Lassiter, 1988 ; Lupfer, dark, & Hutchinson, in press ; Petty, Wells, & Brock, 1976 ; Shiffrin & Schneider, 1977 ; Smith & Lernei, 1986). Recently, it has been suggested that social inference (i.e., the process by which traits are interred from behaviors) is just such a decomposable process. Building on the work of Quattrone (1982) and Trope (1986) , Gilbert, Pelham, and Krull (1988) have argued that social inference consists of an initial *characterization stage* and a subsequent *correction stage* : Perceivers draw character inferences from behavior ("Gus is a gloomy guy") and then correct those inferences with information about the context in which the behavior occurred ("But given that be just came from his mother's funeral, be may be a more upbeat person than be seems today").

Gilbert et al. (1988) contend that, in general, characterization requires less conscious attention (or fewer cognitive resources) than does correction (see also Baron, 1988 ; Lupfer, Clark, & Hutcherson, in press ; Uleman, 1987 ; Winter & Uleman, 1984 ; Winter, Uleman, & Cuniff, 1985 ; cf. Bassili & Smith, 1986). As a result, the addition of another resource-consuming task should impair the latter but not the former operation. In one experiment, perceivers watched an anxious-looking woman who had ostensibly been asked to discuss a variety of anxiety-provoking topics (e.g., her sexual fantasies) with a stranger. Those perceivers who were busy rehearsing word strings during their observation of the woman were particularly likely to consider her dispositionally anxious. These *cognitively busy* perceivers did not use information about situational context (i.e., the anxiety-provoking discussion topics) to correct their characterizations of the woman, despite the fact that the word strings they were rehearsing were the discussion topics themselves. [1]

Although the failure of cognitively busy perceivers to correct their characterizations has been documented with a variety of busyness-inducing tasks (Gilbert, 1989), such tasks are merely experimental mimics of the many resource-consuming tasks of ordinary life. When interacting with others, for example, one must pay considerable attention to the regulation of one's own thoughts, feelings, and actions. These self-regulatory efforts may, in turn, impair one's understanding of those with whom one is interacting. Thus, when people work hard to avoid staring, attempt to ingratiate those whom they inwardly despise, concentrate on assessing their power over another, or merely ruminate about an upcoming event, they may fail to correct their characterizations of their interaction

partners (Gilbert, Jones, & Pelham, 1987 ; Gilbert, Krull, & Pelham, 1988 ; Gilbert, Pelham, & Krull, 1988).

If cognitive busyness is indeed a ubiquitous feature of social life, and if busyness reliably distorts the impressions that persons form of each other, then the corrigibility of this distortion is of some concern. The present experiments are attempts to explore the consequences of busyness-induced misperceptions by determining if and how they can be undone. Are the inferential effects of cognitive busyness curable? The answer to this question is, we will suggest, an unequivocal *sometimes* .

Experiment 1

Gilbert, Pelham, and Krull (1988) have argued that busy perceivers fail to correct their characterizations because they are too busy to use information about situational context, but *not* because they fail to notice, gather, or recall that information. If this is true, then busy perceivers who fail to correct their characterizations while they are busy should be perfectly able to do so at some later time. Experiment 1 was an attempt to determine whether the busy perceiver's failure to correct is, in fact, reversible.

Method Overview

Subjects watched two silent clips from a videotape of a female target engaged in discussion with a stranger. In each of the clips the target appeared extremely anxious. Half the subjects learned that the target was discussing anxiety-inducing topics (e.g., her sexual fantasies) and the remaining subjects learned that the target was discussing, mundane topics (e.g., her hobbies). Half the subjects in each of these conditions rehearsed an eight-digit number while watching the videotape (*busy condition*), and the remaining subjects did not (*not-busy condition*). All subjects immediately rated the target's trait anxiety and then subjects in the busy condition stopped rehearsing the number. Finally, all subjects spent 5 min writing an essay about the target and then rerated the target's trait anxiety.

Subjects

Forty female students at the University of Texas participated to fulfill a requirement in their introductory psychology course.

Instructions

On their arrival at the laboratory, subjects were greeted by a male experimenter who gave them a brief oral introduction to the experiment, provided them with complete written instructions, and then escorted each subject to a cubicle (equipped with 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 sec clip from each of these discussions. It was explained that during the getting-acquainted conversation, the camera had been positioned behind one of the discussants, 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, Experiment 1) .

Situational Constraint Information

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 appear in subtitles at the bottom of the screen.

Half 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 (namely, 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 (namely, 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. M. L. Snyder & Frankel, 1976).

Cognitive Busyness Manipulation

Subjects were assigned to either the busy condition or the 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). Subjects were allowed to examine the particular measures that they would later be asked to complete. 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 sec prior to the start of the videotape to learn the eight-digit number, and were instructed to hold the number in memory until the experimenter later asked them to recall it.

Dependent Measures Real-time ratings of state anxiety.

Subjects were asked to provide ongoing reports of the target's behavioral or state anxiety as they watched the videotape. Subjects provided these reports by continuously adjusting the position of a pointer on an electronic slide (a linear potentiometer). One pole of the slide was labeled *anxious* and the other pole was labeled *calm* . Subjects were told to adjust the slide continuously so that its position described the target's behavior at any given moment. The position of the pointer was recorded every 500 ms by a computer in another room.

Gilbert et al. (1988) assumed that the effects of cognitive busyness occur at the attributional stage rather than at the preceding behavior-identification stage (cf. Trope, 1986) . That is, busy and not-busy subjects are assumed to perceive the same degree of state anxiety in the target's behavior, but busy subjects are ostensibly more likely to make dispositional attributions for that behavior than are not-busy subjects. Subjects' ongoing reports of the target's behavior in the present experiment provided an opportunity to check on the validity of this assumption.

Initial ratings of trait anxiety.

Immediately following the videotape, 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 ten unrelated trait dimensions (namely, honesty, warmth, intelligence, sociability, happiness, dominance, interestingness, cooperativeness, self-confidence, and humor). [2]

The essay task.

After completing these measures, busy subjects were asked to recall and report the eight-digit number that they had been rehearsing, and thus they stopped being busy. Both formerly busy and never-busy subjects were then asked to spend 5 min writing an essay that answered the question "What kind of person is the target?" It was stressed that subjects should feel free to answer this question in any way they wished.

Post-essay ratings of trait anxiety.

After writing the essay, all subjects were asked to rerate the target's trait anxiety on the same scales that were used before. Subjects were told that "some people like to change their ratings after being given time to think about the woman (the target), and others become even more sure of their previous ratings" After completing these ratings, subjects were probed for suspicion, fully debriefed, and dismissed.

Results Real-time ratings of state anxiety.

Every 500 ms, a computer read and recorded (on a 13-point scale) the position of the electronic slide. These readings were averaged to create a single state anxiety index, which was submitted to a 2 (busyness: busy or not-busy) × 2 (topics: anxious or mundane) analysis of variance (ANOVA). The analysis revealed no significant effects (all F s < 1.2, all p s .25). Although null results allow only tentative conclusions, these findings do suggest that busy and not-busy subjects perceived the same amount of state anxiety in the target's behavior (M s = 10.45 and 10.56. respectively) and that any differences in their ratings of the target's trait anxiety were therefore, attributional (cf. Trope, 1986).

Initial ratings of trait anxiety.

Subjects' initial ratings of the target on the three trait scales (comfortable-uncomfortable, calm-nervous, and relaxed-anxious) were averaged to create a single trait anxiety index (coefficient α = .94), and this index was submitted to a 2 (busyness: busy or not-busy) × 2 (topics: anxious or mundane) ANOVA . The analysis revealed a main effect of topic, F (1, 36) = 35.43. p < .001. which was qualified by the predicted Busyness × Topic interaction, F (1, 36) = 51.31, p < .001. As Table 1 shows, the undercorrection effect demonstrated by Gilbert, Pelham, and Krull (1988) was strongly replicated: Whereas not-busy subjects considered the target more trait anxious when she discussed mundane than anxiety-inducing topics, F (1, 18) = 85.99, p < .001. busy subjects did not, F < 1. Despite the fact that busy subjects failed to consider the contribution of the discussion topics to the target's apparent anxiety, all busy subjects (as well as their not-busy counterparts) recalled these topics with complete accuracy.

Post-essay ratings of trait anxiety.

Busy subjects were asked to report (and therefore, to stop rehearsing) the eight-digit number, and all were able to do so. All subjects then spent 5 min writing about the target and then rerated the target's trait anxiety. A post-essay trait anxiety index was computed as above (coefficient α = .95) and submitted to a 2 (busyness: formerly-busy and never-busy) × 2 (topics: anxious or mundane) ANOVA , which revealed only a main effect of topic, F (1, 36) = 66.85. p < .001. As Table 1 illustrates, when busy subjects stopped being busy, they were able to use the discussion topics that they remembered so well to correct their characterizations of the target. In addition, these formerly busy subjects' post-essay ratings were statistically indistinguishable from the never-busy subjects' initial ratings, F (1, 36) < 1. After just a few minutes of writing about the target, formerly busy perceivers spontaneously corrected their characterizations and achieved precisely the same impressions as had their never-busy counterparts.

It is, of course, virtually impossible to know with precision the specific sequence of cognitive operations that enabled the formerly busy perceivers' recovery. Nonetheless, attribution theories do suggest that correction (or discounting) is a resource-consuming operation in which the attributional implications of situational context (a) are realized ("Gosh. I guess that talking about sex with a stranger would make anyone nervous") and (b) are then used to undo one's initial belief about a particular target ("So maybe this

woman isn't such a nervous person after all;" see Gilbert, 1989 ; Jones, 1979 ; Quattrone, 1982). Recovery, then, is merely the post hoc performance of this familiar discounting operation. Is there any evidence that recovery from busyness effects requires formerly busy perceivers to do the very sort of attributional work that busyness prevented in the first place?

Four independent judges (who were blind to condition) read the subjects' essays with the goal of determining (on a 5-point scale) how much each subject's essay about the target explicitly emphasized transient situational causes for her behavior as opposed to enduring dispositional factors (interrater reliability = .82). Although never-busy and formerly busy perceivers composed essays of the same length ($F < 1$), formerly busy perceivers apparently did more *corrective thinking* ; that is, they more about the causal efficacy of the target's situation than did never-busy perceivers, M s = 3.9 and 3.0 respectively, t (32) = 2.93. $p < .01$. This suggests that formerly busy perceivers wen indeed doing in retrospect the very sort of attributional work that never-busy perceivers were able to do on-line.

Experiment 2

If formerly busy perceivers corrected their characterizations of the target because the essay-writing task encouraged corrective thinking (i.e., the realization of situational efficacy and the application of that realization to an attributional analysis of the target's behavior), then recovery should not be an inevitable consequence of the cessation of busyness. When formerly busy perceivers subsequently turn their attention to other cognitive tasks, they should not correct their characterization of a target unless those subsequent tasks specifically encourage corrective thinking about the target. Experiment 2 was an attempt to demonstrate that corrective thinking is, in fact, a necessary precursor of recovery and that without it, formerly busy perceivers do not correct their characterizations of the target.

Method

Subjects saw the anxious topic or mundane topic videotapes while they rehearsed an eight-digit number. Alter watching the videotape, subjects recalled the number (i.e., stopped being busy). Some subjects then thought about the target in a variety of mundane and anxiety-provoking situations (*other-thought condition*) while other subjects thought about themselves in the same situations (*self-thought condition*). All subjects then rated the target's trait anxiety. Next, subjects who had thought about the target then thought about themselves, and vice versa. Finally, subjects rerated the target's trait anxiety.

Subjects

Forty University of Texas undergraduates participated to fulfill a requirement in their introductory psychology course.

Procedure

Subjects watched either the anxious topic or mundane topic videotapes used in experiment 1. The instructions and procedures were identical to those used in the first phase of Experiment 1 except that (a) all subjects rehearsed an eight-digit number while watching the videotape (i.e., all subjects were busy), and (b) the electronic slide was not included.

Immediately after watching the videotapes, subjects were asked to recall and report the eight-digit number. Half the subjects were then assigned to the other-thought condition. These subjects were asked to spend 1 min imagining the target in each of six hypothetical situations (several of which were anxiety-provoking) and to think aloud as they did so. The situations were (a) the target arrives at a party at which she knows no one, (b) the target learns that she has won a contest, (c) the target is solicited by a menacing door-to-door salesman, (d) the target is asked to help an unmarried, pregnant girlfriend break the news to her parents, (e) the target is hiking in a national forest, and (*f*) the target sees a handsome male classmate in the cafeteria. These situations were described to the subject in greater detail than they are here, and the subject was then asked to "describe what the target would do, what she would think, and how she would feel."

We assumed that this think-aloud task would have two general effects. First, thinking about a person's behavior in a variety of situations should facilitate realization of the attributional implications of situational context (Shoda, Mischel, & Wright, 1989 ; Wright & Mischel, 1987). Indeed, when people explicitly consider the possibility that another person might behave differently in different circumstances, they are less prone to erroneous trait inferences (Swann, 1984 ; see also Lord, Lepper, & Preston, 1984). Second, because the think-aloud task required subjects to focus specifically on the target, we assumed it would encourage them to apply this "attributional realization" to a subsequent analysis of the target's behavioral anxiety.

Subjects in the other-thought condition spent a total of 6 min (1 min for each of the six situations) on the think-aloud task. The remaining subjects were assigned to the self-thought condition. These subjects were asked to imagine *themselves* (rather than the target) in each of the six hypothetical situations and to think aloud as they did so. Thus, self-thought subjects served as yoked controls in that they imagined precisely the same situations as did other-thought subjects and therefore were similarly encouraged to realize the attributional implications of situational context'; however, because the self-thought task did not necessitate thinking about the target per se, we assumed that it would not encourage subjects to apply this attributional realization to their subsequent analysis of the target's actions. [3]

After completing the think-aloud task, all subjects were asked to recall and report the topics that the target had ostensibly been discussing in the videotape and to estimate the target's trait anxiety on the three scales used in Experiment 1. Next, all subjects performed a second thinkaloud task. Subjects in the other-thought condition (who had previously imagined the target in each of six situations) were now asked to imagine themselves in each of those situations, and vice versa for subjects in the self-thought condition. On completion of this second think-aloud task, all subjects rerated the target's

trait anxiety on the same three scales. Finally, subjects were fully debriefed, probed for suspicion, and dismissed.

Results

As in Experiment 1, all subjects were able to recall both the discussion topics and the eight-digit number accurately. Subjects' initial estimates of the target's trait anxiety on the three scales were averaged to create a trait anxiety index (coefficient $\alpha = .86$). Scores on this index were submitted to a 2 (initial task: other-thought or self-thought) × 2 (topics: anxious or mundane) ANOVA , which revealed a main effect of topic, F (1, 36) = 5.16, $p < .05$. This effect was qualified, however, by the predicted Initial Task × Topic interaction. F (1. 36) = 25.28, $p < .001$. As the upper panel of Table 2 shows, formerly busy subjects who imagined the target in a variety of situations corrected their characterizations of the target, F (1, 18) = 27.18, $p < .001$, whereas formerly busy subjects who imagined themselves in precisely the same situations did not, $F < 1$.

A second trait anxiety index (coefficient $\alpha = .93$) was constructed from ratings made after the second think-aloud task. A 2 × 2 ANOVA (as aforementioned) performed on this index revealed a main effect of topic, F (1, 36) = 35.09, $p < .001$. but no hint of a Topic × Initial Task interaction, F (1. 36) = 1.23, $p > .25$. As the lower panel of Table 2 shows, self-thought subjects, who failed to correct their characterizations of the target after imagining themselves in a variety of situations, spontaneously did so after imagining the target in those same situations. In short, the corrective effects of thought occurred at both the between- and within-subject levels.

Experiment 3

The results of Experiments 1 and 2 suggest that the failure to correct can be reversed if the formerly busy perceiver does the very sort of corrective thinking that busyness prevented in the first place. Of course, these results may appear to put us in the somewhat unenviable position of the optometrist who cured myopia and wound up proudly unemployed: If the inferential consequences of cognitive busyness are easily undone by a few minutes of thought, then perhaps the initial misperception is of no practical concern. We don't believe this is so, and an analogy may serve to explain why.

Imagine a serum that, on injection, went directly to the liver and magically reversed the progress of malignant hepatoma. If the disease was identified early on, a drug of this sort would be remarkably effective and the malady would indeed be of no practical concern. On the other hand, an organ-specific agent of this sort would be quite worthless to a patient whose tumor had metastasized or spread to other systems. Although the liver itself might be repaired by the organ-specific drug, organs such as the pancreas and spleen might already have received the disease, and the final result would (from the patient's point of view) be sadly the same.

We believe that an undercorrected impression of another person can have effects analogous to those of a metastatic physical process. The undercorrected impression itself

may be undone by a few minutes of thought, but this does not mean that the subsidiary effects of this impression are equally easy to eradicate. For example, if a busy perceiver erroneously concludes that another person is dispositionally anxious, then this erroneous belief may color the perceiver's subsequent reading of neutral or ambiguous information ("Those are definitely the kind of earrings an apprehensive woman would wear"). Once this has happened, a few minutes of corrective thought may be no more beneficial than an organ-specific drug. Corrective thinking may well repair the original misperception but. because that misperception has already contaminated subsequent information processing, a complete cure may be unattainable.

Analogies should not be strained, but this one does suggest that the immediacy of corrective thinking (like the early treatment of metastatic disease) may be critical to a full reversal of busyness-induced misperceptions. If an undercorrected impression is not repaired before further information is processed, then even the magic bullet of corrective thinking may be too little and too late. Experiment 3 was an attempt to determine whether undercorrected impressions do indeed metastasize. and if so, whether a retrospective dose of corrective thought would (like an organ-specific drug) prove overly specific in its curative capacity.

Method Overview

Subjects watched the anxious topic videotape while rehearsing an eight-digit number. After seeing the videotape, subjects stopped rehearsing the number and listened to an uninformative interview with the target. Subjects in the *early-cure condition* were allowed to engage in corrective thought prior to hearing this interview, whereas subjects in the *late-cure condition* were allowed to engage in corrective thought only after hearing the interview. Subjects in the *no-cure condition* were given no opportunity for corrective thought. Ultimately, all subjects estimated the target's trait anxiety.

Subjects

Thirty female students at the University of Texas participated to fulfill a requirement in their introductory psychology course.

Procedure

While rehearsing an eight-digit number, subjects watched the an anxious topic videotape used in experiments 1 and 2 and by Gilbert, Pelham, and Krull (1988) . Thus, all subjects were busy and all subjects saw the same videotape, otherwise, the instructions and procedures were identical to those used in the previous experiments.

After watching the anxious topic videotape, subjects were asked to recall the eight-digit number. Subjects in the early-cure condition were then allowed 5 min to write an essay that answered the question "What kind of person is the target?" (This was the same procedure used to evoke corrective thinking in Experiment 1). After completing the essay, these subjects were told that they would receive some additional information about

the target before rating her trait anxiety. Subjects listened to a 20-s, audiotaped interview with the target, in which she answered several rather dull and uninformative questions (e.g., "What is your major?" "How long have you lived in Texas?") in a rather dull and uninformative way (e.g., "I'm an English major and I've lived in Texas for seven years"). Subjects were then asked to recall the discussion topics and to rate the target's trait anxiety on the three scales used in Experiments 1 and 2.

Subjects in the late-cure condition performed each of these tasks, but in a somewhat different order. These subjects watched the anxious topics videotape, recalled the eight-digit number (i.e., stopped being busy), listened to the uninformative audiotape. and *then* wrote an essay. Next, these subjects recalled the discussion topics and rated the target. Finally, subjects in the no-cure condition were not given an opportunity to write an essay at all. These subjects merely watched the anxious topics videotape, recalled the eight-digit number, listened to the uninformative audiotape, recalled the discussion topics, and then rated the target.

Results

As in Experiments 1 and 2, all subjects recalled the eight-digit number and the discussion topics accurately. A trait anxiety index (coefficient $\alpha = .89$) was constructed by averaging subjects' ratings on the three dependent measures. A one-way ANOVA performed on this index revealed a significant difference between conditions, F (2, 27) = 24.14, $p < .001$. An inspection of the ratings in each condition showed the predicted ordering of means: 6.8 in the early-cure condition, 9.3 in the late-cure condition, and 11.3 in the no-cure condition ($n = 10$ in each condition). Post hoc tests revealed that each of these conditions differed from the others: The late-cure condition differed significantly from both the early-cure condition, F (1, 18) = 12.23, $p < .01$, and from the no-cure condition, F (1, 18) = 14.02, $p < .01$.

These data support the hypothesis that undercorrected impressions are metastatic, and this can best be illustrated by assigning hypothetical values to the two sources of information that subjects received and then using these values to "predict" our findings (see Figure 1). First, imagine that seeing the videotape while busy *increased* subjects' ratings of the target's trait anxiety by 2 scale units, and that corrective thought *decreased* these ratings by 2 scale units. (These particular values are suggested by the results of Experiment 1, in which uncorrected impressions were about 2 units above the scale midpoint or baseline). Second, imagine that the information in the uninformative audiotape had no effect whatsoever on subjects' ratings when it was encountered *after* the subject had engaged in corrective thought (i.e., when the initial misperception was "cured" before it could "infect" the processing of neutral information), but that it increased these ratings by 2 scale units when encountered *before* the subject engaged in corrective thought (i.e., when the initial misperception was allowed to infect the processing of neutral information).

If we grant these assumptions, then the data conform to "prediction" with remarkable accuracy. As Figure 1 shows, all subjects should have begun by assuming that the target had an average amount of trait anxiety (in this case the midpoint, or 7 scale units).

Subjects in the no-cure condition should have experienced an increase of 2 units from the videotape and a further increase of 2 units from the "contaminated" audiotaped interview. In fact, the observed mean of 11.3 is very close to this expected mean of 11.0. Subjects in the late-cure condition should have experienced a similar increase of 2 units from the videotape and 2 units from the contaminated audiotape, but should also have experienced a decrease of 2 units as a result of corrective thought. The observed mean of 9.3 is once again quite close to this predicted mean of 9.0. Finally, subjects in the early-cure condition should have experienced an increase of 2 units from the videotape, an immediate decrease of 2 units as a result of corrective thought, and finally, no increase at all from the uncontaminated audiotape. The observed mean of 6.8 is strikingly close to this predicted mean of 7.0.

Experiment 4

The results of Experiment 3 support the hypothesis that undercorrected impressions can affect subsequent information processing in ways that are not easily reversed by corrective thought. Of course, this interpretation assumes that subjects in the late-cure and no-cure conditions actually perceived greater state-anxiety in the target's neutral behavior during the audiotaped interview than did subjects in the early-cure condition. Although this assumption is reasonable, it seemed important to demonstrate directly that such perceptual assimilation of the target's neutral behavior had indeed occurred.

Method Overview

Subjects watched the anxious topics videotape while rehearsing an eight-digit number and then listened to an uninformative interview with the target. Subjects in the cure condition were allowed to engage in corrective thought prior to hearing this interview, whereas subjects in the no-cure condition were not. Subjects in a control condition simply heard the interview. Ultimately, all subjects rated the anxiousness of the target's behavior during the interview.

Subjects

Thirty female students at the University of Texas participated to fulfill a requirement in their introductory psychology course.

Procedure

While rehearsing an eight-digit number, subjects in the cure and nocure conditions watched the anxious topic videotape used in the previous experiments. A third group of subjects in the control condition did not see this videotape. Otherwise, the instructions and procedures were identical to those used in Experiment 3.

After watching the videotape, subjects in the cure condition were asked to recall the eight-digit number. These subjects were then allowed 5 min to write an essay that answered the question "What kind of person is the target?" After completing the essay,

these subjects listened to the audiotaped interview used in Experiment 3. Subjects were then asked to rate the target's behavior during the audiotaped interview on three 13-point scales anchored at the extremes with the phrases *she seemed not at all uncomfortable (nervous, anxious)* and *she seemed somewhat uncomfortable (nervous, anxious)* . Subjects were then asked momentarily to disregard the content of the target's answers and to rate her tone of voice during the interview on a 4-point scale that was labeled with the phrases not at all (*Slightly, moderately, extremely*) anxious or nervous. Finally, subjects were asked to recall the five specific pieces of information offered by the target during the interview.

Subjects in the no-cure condition watched the anxious topics videotape while rehearsing an eight-digit number, recalled the eight-digit number (i.e., stopped being busy), listened to the audiotaped interview, rated the target's behavior during that interview, and recalled the information offered during the interview. Finally, subjects in the control condition simply heard the audiotaped interview and then completed these same measures.

Results

As in the previous experiments, all subjects who rehearsed an eight-digit number were able to recall it perfectly. With the exception of 1 subject who failed to recall one of the five pieces of information, all subjects showed perfect recall of the target's answers to the interview questions. A state-anxiety index (coefficient $\alpha = .98$) was constructed by averaging subjects' ratings on the three 13-point scales, and a one-way ANOVA performed on this index revealed a significant difference between conditions, F (2, 27) = 55.13, $p < .001$. A similar difference was found on the tone-of-voice measure. F (2, 27) = 45.46, $p < .001$.

As Table 3 shows, control subjects (who merely listened to the audiotaped interview) perceived little anxiety in either the target's behavior or voice. Post hoc tests revealed that subjects in the cure condition perceived equally little anxiety in the target's behavior and voice (F s < 1). As predicted, however, subjects in the no-cure condition perceived more anxiety in the target's behavior and voice than did subjects in either the control condition, F (1, 18) = 110.34 and 72.90, respectively, $p < .001$, or the cure condition, F (1, 18) = 78.37 and 72.90, respectlively, $p < .001$. In short, subjects who were not allowed to engage in corrective thinking perceived the target's neutral behavior as particularly anxious, whereas subjects who were allowed, to engage in corrective thinking did not. These data provide strong evidence for the assumed mediators of the effects seen in Experiment 3.

General Discussion

When people do too many things at once, they often do some of them badly, and understanding others is no exception to this rule. Cognitively busy perceivers often fail to use information about the situational context in which actions occur, and therefore risk misperceiving those with whom they interact. The present experiments suggest that such misperceptions can be retroactively cured by a dose of thought, but that this antidote is

something short of a panacea. Misperceptions are metastatic in that they influence other psychological processes, and when this happens, a shot of corrective thinking may purge the original misperception but fail to undo the subsidiary changes that this misperception has wrought.

But what accounts for the specificity of recovery? That is, why don't formerly busy perceivers retrospectively correct both the original misperception and its sequela? Two possibilities seem plausible. First, people may be unaware that misperceptions have metastatic effects, and second, people may be incapable of isolating those effects for remedy. Both possibilities warrant discussion.

Folk Theories of Misperception

In the spirit of Abbott and Costello, Ichheiser (1949) noted that

> If people who do not understand each other at least understand that they do not understand each other, then at least they understand each other better than when, not understanding each other, they do not even understand that they do not understand each other, (p. 37)

In other words, social perceivers' theories about their own capacity for inferential error may enable them to abrogate, at least in part, the error's ill effects (cf. Nisbett & Wilson, 1977) For example, if perceivers realize that being busy distorts their perception of others, then they may be particularly willing to discard the beliefs they achieve under such suboptimal conditions. Many of us can recall being preoccupied during a colloquium, only to wonder afterward if the speaker's incoherence could have been our fault rather than his or hers. Such musings suggest that people do indeed have naive theories that encourage them to discount impressions formed under busy conditions. Just how well these theories work and just what circumstances invoke them remain matters of speculation; but, to the extent that persons do realize that they are too busy to draw accurate conclusions. they may be particularly willing to rethink these conclusions at some later time.

On the other hand, it seems quite unlikely that people have equally good theories of metastasis. Decades of research attest to the metastatic nature of beliefs, which may alter related beliefs (Abelson el al., 1968), influence the availability of certain memories (Anderson, Lepper, & Ross, 1980 ; Ross, Lepper, & Hubbard, 1975 ; M. Snyder & Uranowitz, 1978 ; Zadny & Gerard, 1974), channel information-seeking activities (Snyder & Swann, 1978 ; Swann, 1987), and promote the assimilation of newly encountered evidence (Fiske & Neuberg, in press , Lord, Ross, & Lepper, 1979 ; Taylor & Crocker, 1981). The fabric of belief is indeed so tightly knit that the dropping of a single stitch can induce a run throughout the entire bolt–and yet, this basic psychological truism is not a conspicuous piece of our cultural wisdom. Judges routinely instruct jurors to disregard what they have heard, and jurors routinely act as if they could do so. Neither acknowledges that even if a stray remark were itself expunged, the remark might already have affected a variety of other mental products and processes and could thus continue to

affect the juror's judgment. In short, people may not rectify the metastatic effects of misperception simply because they are unaware that misperceptions can and do have such properties (cf. Anderson, 1982).

Folk Remedies for Misperception

Which may be just as well. Even if people were to recognize metastasis as a problem, they would probably be unable to do much about it. The trouble is that beliefs, once established, generally don't say where they came from. Mental representations rarely contain explicit information about their own origins, and thus people are often reduced to guessing the source of a belief by examining its topological features (Johnson, 1985 , 1988 ; Johnson & Raye, 1981 ; see also Jacoby, Kelley, & Dywan, 1989). Furthermore, when source information is explicitly encoded. it is often dissociated from the belief and forgotten (Cruder et al., 1978 ; Hovland & Weiss, 1951 ; Jacoby, Kelley, Brown, & Jasechko, 1989). All of this means that when a particular piece of information is found to be false, it may be impossible to determine which other beliefs have been affected by it in the interim. One may discover that an elderly gentleman with a charming accent hails not from Paris hut from Oklahoma City, and one may correct one's mistaken belief. Nonetheless, when planning a dinner party some months later, one may assume that the gentleman prefers brie to Cheese-Whiz without being able to remember just where this odd fact was encountered.

If beliefs are unlabeled with respect to their origins, then how can people know which of their beliefs require reassessment when a key piece of information is discredited or changed? At present, there is neither a clear logical nor psychological answer to this question. Indeed, the problem is so vexing that it represents a significant stumbling block in the development of artificially intelligent systems, which cannot evaluate the veracity of new information unless they know beforehand which old information is logically related and should therefore be checked for contradiction (see Dreyfus, 1979 ; Fodor, 1987 ; Minsky, 1975 ; Pylyshyn, 1987). This proactive *frame problem* has a retroactive counterpart: When computers use erroneous information in their calculations, they are, for the most part, unable later to determine which computational products were affected by the misinformation and which were not. As such, they must generally erase all questionable products and begin again. Fortunately or unfortunately, the human mind seems designed to prevent instantaneous and widespread erasures at all costs (Bjork, 1972 ; Wegner, Schneider, Carter, & White, 1987). This suggests that people either live with the unidentifiable byproducts of false knowledge, or they use some unknown means to isolate and annul them. [4]

Coda

Psychologists who study social misperception are sometimes criticized by those who suggest that in the real world such misperceptions are easily rectified by a moment of thought, a more sincere desire for truth, or the admonitions of one's fellows; as such, misperceptions may be considered too fragile or fleeting to merit real concern. Such criticisms miss a fundamental point: The insidiousness of misperceptions lies not in the

misperceptions themselves, but in their untraceable effects on other mental events. The timeworn contention that errors in the laboratory have no consequences in the real world may, in fact, prove to be more than a harmless optimism. Our human history of conflict and carnage attests to both the ubiquity and the virulence of social misunderstanding; if we fail to acknowledge that the roots of these events lie in ourselves, and not in our stars, then we may be doomed to witness their endless repetition.

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1

We use the term *cognitive busyness* rather than the more familiar *cognitive load* because (a) busyness describes a mental stale rather than the activity that gives rise to that state, and (b) load lends itself rather comically to use as an adjective (e.g., "The loaded subjects were unable to locate their fingers during the Power failure").

2

These ratings were collected as part of a pilot project on cognitive busyness and inferential complexity. Busy subjects ultimately formed less complex impressions of the target than did not-busy subjects but, because these findings are only tangentially related to the concerns of this article, these data will not be discussed further.

3

It may seem that a more appropriate control condition would have been one in which subjects were given no think-aloud tusk. Unfortunately, it is likely that some subjects in such a condition would spontaneously engage in corrective thinking and others would not, thus rendering impossible any prediction of the subjects' subsequent impressions. The self-thought control group provides a very conservative test in that it requires subjects to perform a task that is identical to the task performed by other-thought subjects–minus one critical ingredient, namely, focus on the target.

4

It may be worth noting that some theorists have argued for the viability of *reversible computers* which never loose information about how they attained their present states (see Bennett & Landauer, 1985 ; Fredkin & Tomasso, 1982).

Table 1
Estimates of Target's Trait Anxiety in Experiment 1

	Discussion topic		
Time of ratings	Mundane	Anxious	Difference
	Initial ratings		
Busy	9.0	9.4	−0.4
Not busy	11.3	6.8	4.5
	Post-essay ratings		
Formerly busy	10.4	6.9	3.5
Never busy	11.1	6.5	4.6

Note. Higher values indicate greater estimates of dispositional anxiety, nervousness, and uncomfortableness on a scale of 1–13. $n = 10$ in each cell.

Table 2
Estimates of Target's Trait Anxiety in Experiment 2

	Discussion topic		
Time of ratings	Mundane	Anxious	Difference
	Ratings after first task		
Other thought	10.6	7.3	3.3
Self-thought	9.3	10.5	−1.2
	Ratings after second task		
Other thought	10.1	7.2	2.9
Self-thought	11.6	7.3	3.3

Note. Higher values indicate greater estimates of dispositional anxiety, nervousness, and uncomfortableness on a scale of 1–13. *Other thought* denotes subjects who thought about the target first and themselves second; *self-thought* denotes subjects who thought about themselves first and the target second. $n = 10$ in each cell.

Table 3
Ratings of Target's Neutral Behavior in Experiment 4

	Condition		
Dependent measure	Control	Cure	No cure
Target's behavior	3.17_a	3.77_a	10.23_b
Target's tone of voice	1.40_a	1.40_a	3.20_b

Note. Higher values indicate greater estimates of behavioral or paralinguistic anxiety on a scale of 1–13. Values in the same row that do not share a common subscript differ at the .01 level. $n = 10$ in each cell.

Figure 1. Order of (and hypothetical values for) information encountered by subjects in each condition of Experiment 3.