

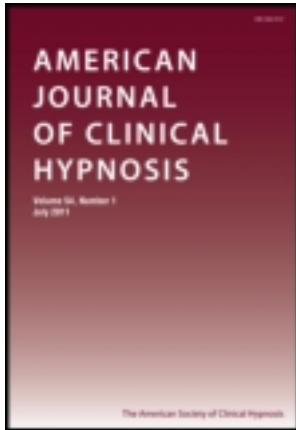
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Attributions, Awareness, and Dissociation: In Memoriam Kenneth S. Bowers, 1937-1996

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Attributions, Awareness, and Dissociation: In Memoriam Kenneth S. Bowers, 1937 - 1996

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Research by Kenneth S. Bowers on posthypnotic suggestion, positive hallucinations, hypnotic analgesia, and posthypnotic amnesia is reviewed, along with his nonhypnotic research on the person-by-situation interaction and on intuition in problem solving. Bowers's intellectual style, *serious curiosity*, is offered as a model for hypnosis research.

It is a great sadness that Ken Bowers is gone from us. I feel the loss especially acutely, because I admired Ken Bowers as I have admired no other colleague, and the year I spent at Stanford with him and Pat Bowers, and their three sons (one of whom, Jeff, I later taught as a graduate student at the University of Arizona), was the best year of my academic life.

Sociologists and historians of science have written about a "Frankenstein syndrome" reflecting scientists' (and the public's) ambivalent attitude toward knowledge and the power that comes with it (e.g., Graubard, 1967). But there's another syndrome which affects some of us, and it hit me hard in the person and work of Ken Bowers. I'm thinking of what might be called the "Faust syndrome," where you read a paper that you'd sell your soul to have done yourself.¹ When I examine my own personal Faustian list, I find more papers on it by Ken Bowers than by anyone else, including Ken's very first published paper, another one that ap-

peared around the time of his death, and several in between. I'd like to review some of these papers, tell you why they're important, and discuss what they reveal about Ken Bowers's life and work.

The first Bowers paper on my Faustian list is, in fact, Bowers' first paper, derived from his doctoral dissertation, which lays the foundation for an attributional analysis of posthypnotic suggestion (Bowers, 1966). This paper came out of nowhere—Ken hadn't done his work at Penn or Stanford or Berkeley or Medfield or Chicago or Sydney, the places where people traditionally did hypnosis research, but at Illinois.² Moreover, attribution theory had only recently been codified by Harold Kelley (1967), in a presentation at the Nebraska Symposium on Motivation that hadn't even been published yet. And finally, Bowers had detected and corrected a subtle flaw in the real-simulator design which had been introduced by Martin Orne (1959), and used by him to great effect in a number of experiments that had become instant classics. The paper just hit me like a ton of bricks when I first read it (in 1970): I remember asking Martin and Emily Orne and Fred Evans, "Who is this guy?" They already knew, of course, because Ken had received the

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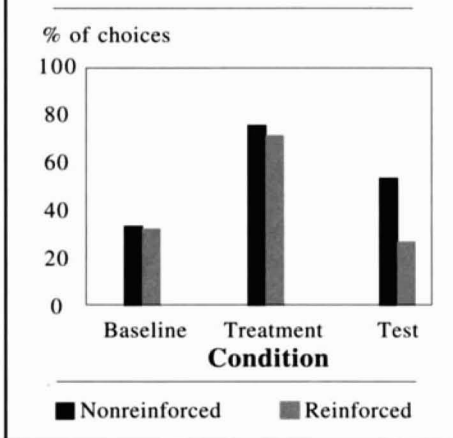
award from the Society for Clinical and Experimental Hypnosis honoring the best research paper in two of the previous three years, and I found out for myself when he received it yet again, a couple of months later, at the 1970 meetings in Philadelphia.

This paper is framed, as many papers were in the 1960s, in terms of the distinction between trance and demand characteristics, and it used Orne's (1959) real-simulator design in a variant of the verbal-conditioning paradigm known as the Taffel task (Taffel, 1965). In the original Taffel task, subjects are asked to construct sentences beginning with a personal pronoun (I, you, he, she, it, we, they) using particular past-tense verbs. The verbs are constrained by the experimenter, but subjects can choose the pronoun freely. The trick in the experiment, and there's *always* a trick in the experiment, is that the experimenter subtly reinforces the choice of one particular pronoun, and the subject responds by selecting that pronoun at levels above baseline. Bowers (1966) had his subjects make up sentences in response to pictures, but before that he gave reals and simulators a posthypnotic suggestion, covered by amnesia, that they would begin their sentences with particular pronouns (*they* or *he*). When formally tested by a second, blind, experimenter, both reals and simulators responded positively to the suggestion, of course, but that wasn't the point of the experiment. The real action in the experiment was in the postexperimental inquiry, which was conducted under strong demands for honesty by yet a third independent, blind interviewer. When asked about their experimental behavior, all 13 simulators, but only 6 of the 14 reals, commented on their use of pronouns. Moreover, all 13 simulators justified their behavior in terms of

the suggestion they had been given, whereas 12 of the 14 reals did not. Taken together, the two results indicate that the reals were not voluntarily complying with experimental demands, and in fact were not even aware of them.

This emphasis on conscious awareness and control was to run as a thread through the rest of Bowers' work on hypnosis, but a later variant on his dissertation experiment (Bowers, 1975) brought out the attributional aspects in full relief. In this study, the subjects were run in a variant of the Taffel task, in which they were presented with pairs of pictures, each consisting of a portrait and a landscape, and asked to indicate which picture they preferred by reading its identification number to the experimenter (in a small deception, the subjects were misinformed that painting preferences were an expression of their personality traits). Previously, the subjects had received a posthypnotic suggestion to select paintings with the numeral 7 in their identification number. After 20 baseline trials (where the numeral 7 did not appear in the identification number) to establish individual preferences for portraits or landscapes, there were 90 training trials in which the nonpreferred type of painting was paired with the numeral 7. In addition to the posthypnotic suggestion, half the subjects received verbal reinforcement for their choices, as in the traditional Taffel procedure. As Figure 1 indicates, the subjects in both reinforced and nonreinforced groups showed marked changes in their choice behavior compared to baseline, but again that wasn't the point of the experiment. The real action was in a final set of 40 test trials, in which the posthypnotic suggestion was canceled and (if appropriate) the verbal reinforcement discontinued. On these test trials, the rein-

Figure 1
**Choice of Non-Preferred
 Painting Type**



forced subjects reverted to their baseline preferences, while the nonreinforced subjects continued to show a slight preference for paintings of the type that they had *not* preferred on the original baseline testing. The reinforced subjects, apparently, attributed their behavior to the experimenter's verbal reinforcement, and shifted their behavior when the reinforcement was discontinued; but the nonreinforced subjects attributed their behavior to their internal desires and preferences. The behavioral control afforded by posthypnotic suggestion, being outside conscious awareness, is attributed to one's own desires and intentions, not to external constraints, and so it persists when environmental circumstances change.

Another set of experiments from my Faustian list showed how effectively Bowers could make experimental use of honesty demands. Barber's task motivation paradigm (e.g., Barber, 1969), which put very strong pressure on subjects for overt behavioral compliance, had raised ques-

tions in some minds as to whether task-motivated subjects were responding behaviorally to suggestions, in the absence of compelling subjective experience. Bowers (1967) gave a group of task-motivated subjects suggestions for auditory and visual hallucinations; half the subjects also received a demand for honesty in reporting. As Barber and Calverley (1964) had found, the task motivated subjects rated their auditory and visual hallucinations as significantly more "real" than they had at baseline; however, this increase in rated "reality" did not occur for those subjects given the additional honesty instructions — suggesting, indeed, that task-motivated subjects were acting more out of behavioral compliance than subjective conviction.

Of course, Bowers' 1967 experiment didn't have a hypnosis condition, raising the question of whether hypnotized subjects would respond any differently to honesty demands than did task-motivated ones. But when Spanos and Barber (1968) did the required comparison, they found — at least for the more difficult visual hallucination — that not only did hypnotic subjects rate their hallucinations as more real than did the task motivated subjects, these ratings were not corrected by honesty demands. To put the icing on the cake, Bowers and Gilmore (1969) showed that simulators' hallucination reports were corrected by honesty demands in a postexperimental interview, while those of real hypnotic subjects were not. The convergence of results, which would never have occurred had Bowers not published his initial critique of the task-motivation paradigm, shows fairly clearly that hypnosis and task-motivation differ in terms of the subjective reality of suggested effects. Not long afterward, the task-motivation paradigm es-

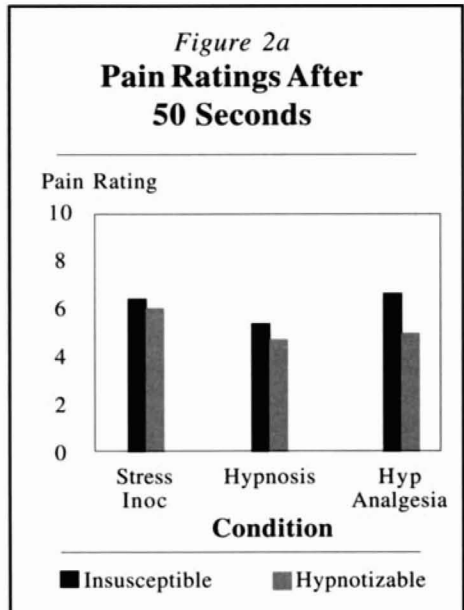
entially dropped out of sight, and the debate over hypnosis shifted to different ground.

That shifting ground was represented by the new “sociocognitive” view of hypnosis promoted by Spanos and Chaves (e.g., Barber, Spanos, & Chaves, 1974), which acknowledged the importance of subjective experience over overt behavior, even as it continued to assert that the genuine changes in subjective experience produced by hypnotic suggestion were not the product of some “altered” or “special” state of consciousness, like dissociation, but rather of more mundane cognitive strategies deliberately invoked in response to situational demands. For example, Spanos (1986) had suggested that pain relief in hypnotic analgesia was achieved not by some esoteric division of conscious awareness, as suggested by Hilgard’s (1977) neodissociation theory of divided consciousness, but rather by the deployment of certain coping strategies identified by Meichenbaum (1977), one of Bowers’ colleagues at Waterloo, as stress inoculation. Now, there’s no doubt that distraction, relaxation, imagining situations inconsistent with pain, and resistance to “catastrophizing” can result in substantial pain relief — the question is whether these strategies account for hypnotic analgesia.

Together with Mary Miller, Bowers did the test. In their first study, Miller and Bowers (1986) ran groups of hypnotizable and insusceptible subjects through an experiment involving cold-pressor pain induced by immersing the subject’s hand and arm in circulating ice water. One third of the subjects in each group was hypnotized and given suggestions for analgesia. Another third was hypnotized, but not given any analgesia suggestions. The remaining subjects were not hypnotized

at all, but were instructed in the use of stress inoculation strategies of the sort that Spanos had proposed as mediators of hypnotic analgesia. Subjects rated their pain and distress on numerical scales, and also were interviewed concerning their use of cognitive strategies. The result (Figure 2a) was a highly significant, and revealing interaction between treatment condition and hypnotizability: stress inoculation worked as expected, producing substantial pain relief, but hypnotizable and insusceptible subjects achieved the same effect. By contrast, hypnotic suggestions for analgesia were much more effective for hypnotizable than for insusceptible subjects.

Note, too, that for hypnotizable subjects, hypnotic analgesia produces more pain relief than stress inoculation. But the important observation is that hypnotic analgesia is mediated by hypnotizability, while stress inoculation is not. The postexperimental interviews were also revealing, because they showed im-

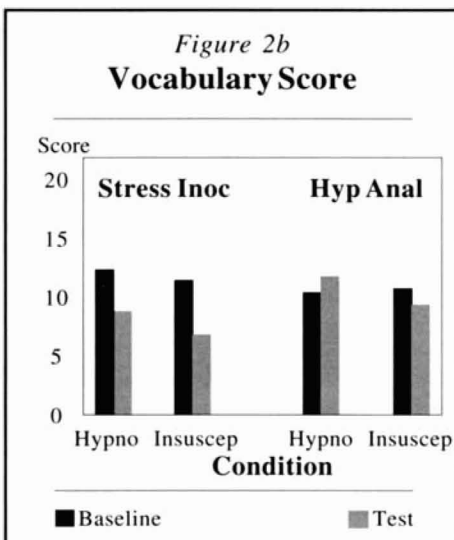


portant group differences in strategy use. The stress inoculation subjects typically reported using stress inoculation strategies at high rates — as, indeed, they were instructed to do. So did the subjects who were merely hypnotized, without receiving an analgesia suggestion. But by and large, the hypnotic analgesia group did not report using such strategies. Such experimental dissociations indicate that whatever its underlying mechanisms, hypnotic analgesia is not produced by stress inoculation. As a byproduct, this study also yielded some information of use to clinical practitioners: for hypnotizable subjects, hypnotic analgesia is probably preferable to stress inoculation as a psychological technique for pain control; but for insusceptible subjects, incapable of responding to hypnotic suggestions, stress inoculation strategies can produce considerable benefit.

Not completely satisfied with these results, Miller and Bowers (1993) performed an even more stringent study of the same general type. Again, subjects of low and high hypnotizability were as-

signed to stress inoculation and hypnotic analgesia conditions, but with a somewhat fiendish twist: during the cold-pressor tests, which as anyone who's tried them will tell you hurt like hell under normal circumstances, the subjects were also administered a difficult vocabulary test. The idea is that consciously deployed cognitive strategies, such as those taught in stress inoculation, consume attentional capacity. To the extent that the subject's cognitive resources are tied up in self-distraction, relaxation, and counterpain imagery, performance on the vocabulary test ought to be impaired. This was, in fact, the outcome for the subjects in the stress inoculation condition (Figure 2b): regardless of hypnotizability, performance on the vocabulary test decreased when the subjects were simultaneously deploying cognitive strategies to control their pain. In the hypnotic condition, however, vocabulary scores were essentially unaffected, and for hypnotizable subjects actually *went up* a little. Stress inoculation strategies achieve their results at the expense of consuming cognitive resources; apparently, hypnotic analgesia does not incur this expense — at least for hypnotizable subjects. Again, the dissociation indicates that hypnotic analgesia is not mediated by stress inoculation and other consciously deployed cognitive strategies.

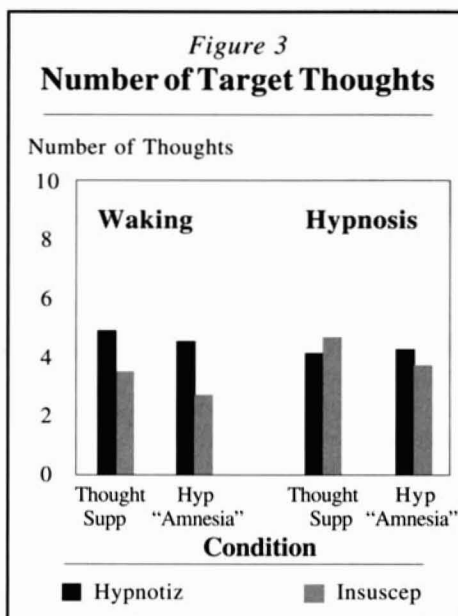
In the third and final paper in the series, Hargadon, Bowers, and Woody (1995) sought to clarify some ambiguities in the previous studies by focusing specifically on the role of counterpain imagery. Hypnotizable subjects were given analgesia suggestions of two types — one suggested a lot of counterpain imagery, the other did not. Of course, some subjects in the imagery condition didn't use imagery, and some in the no-imagery



condition used imagery anyway, and some just focused on the pain and catastrophized. The important result was that the use of imagery, whether instructed or spontaneous, had no impact on the success of hypnotic analgesia in these hypnotizable subjects — suggesting, once more, that counterpain imagery is not central to hypnotic analgesia.

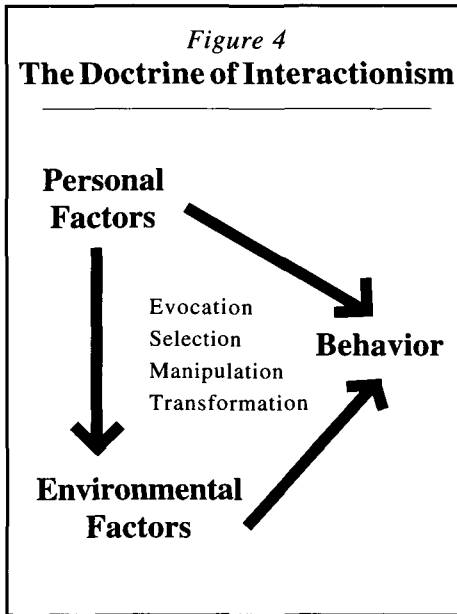
Turning away from analgesia, Bowers and Woody (1996) employed essentially the same experimental strategy to study the processes underlying hypnotic amnesia, in a paper that appeared immediately after Bowers' death. As with analgesia, Spanos and other sociocognitive theorists had proposed that hypnotic amnesia is mediated by such consciously deployed cognitive strategies as self-distraction, relaxation, and imagining a situation in which one could not remember. Furthermore, the fact that hypnotic amnesia occurs as a result of suggestion raised the question of its relation with other self-regulatory phenomena of memory, such as *thought suppression* (Wegner, 1989). A typical thought suppression experiment asks subjects to deliberately suppress thoughts about a particular topic, such as white bears. In a number of experiments, Wegner (1989) has found that conscious attempts at thought suppression have an ironic effect: as soon as the subjects consciously decide not to think of white bears, such thoughts begin to cross the subjects' minds more and more frequently.

Bowers and Woody (1996) reasoned that if hypnotic amnesia involved the same sort of conscious thought suppression, as suggested by some sociocognitive theorists, then it ought to produce the same kind of ironic rebound effect. In their experiment, hypnotizable and insusceptible subjects were given either a



thought suppression instruction or an amnesia-like hypnotic suggestion, both to the effect that they should not think about their favorite automobile (Pilot research had shown that, for unclear reasons, these Canadian subjects had little difficulty suppressing thoughts of white bears!). The results were very clear (Figure 3). The standard thought-suppression instructions produced quite a bit of ironic rebound, as we would expect from Wegner's (1989) research. However, the amnesia-like suggestion produced very little rebound, especially when delivered during hypnosis to hypnotizable subjects. The experiment revealed some other interesting effects as well, but the important take-home message was that conscious thought suppression and hypnotic amnesia appear to be mediated by very different processes, with very different consequences with respect to ironic rebound.

Although Bowers' research career focused on hypnosis, he was always inter-



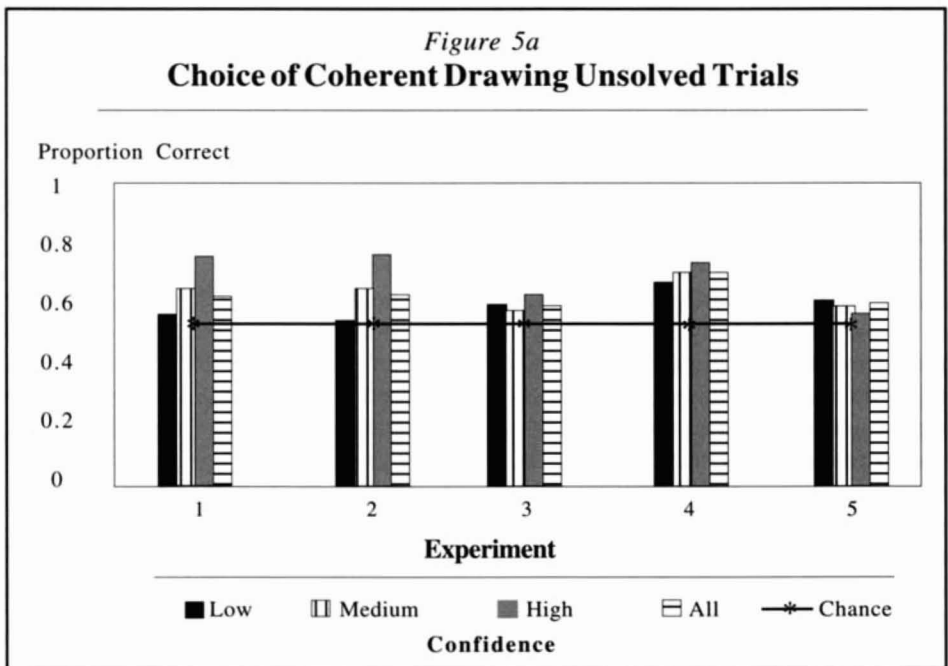
ested in broader issues. Early in his career, for example, he published a paper on personality theory that quickly, and deservedly, became an officially designated Citation Classic (Bowers, 1973). In the late 1960s, the trait approach which had dominated personality theory and research for almost 50 years came under strong attack by theorists, such as Mischel (1968), who argued that traits are at best only very weak determinants of experience, thought, and action, and at worst may be entirely illusory. This critique, many of whose points were very trenchant, sometimes seemed to degenerate into revival of Skinnerian situationism (e.g., Skinner, 1953), in which the control of behavior was ascribed to external, environmental factors, instead of internal, personal ones. Bowers' take on this controversy was strongly influenced by his understanding of hypnosis, especially the Hilgards' "developmental interactive" theory of hypnotic susceptibility (Hilgard, 1965), in which hypnotic response is de-

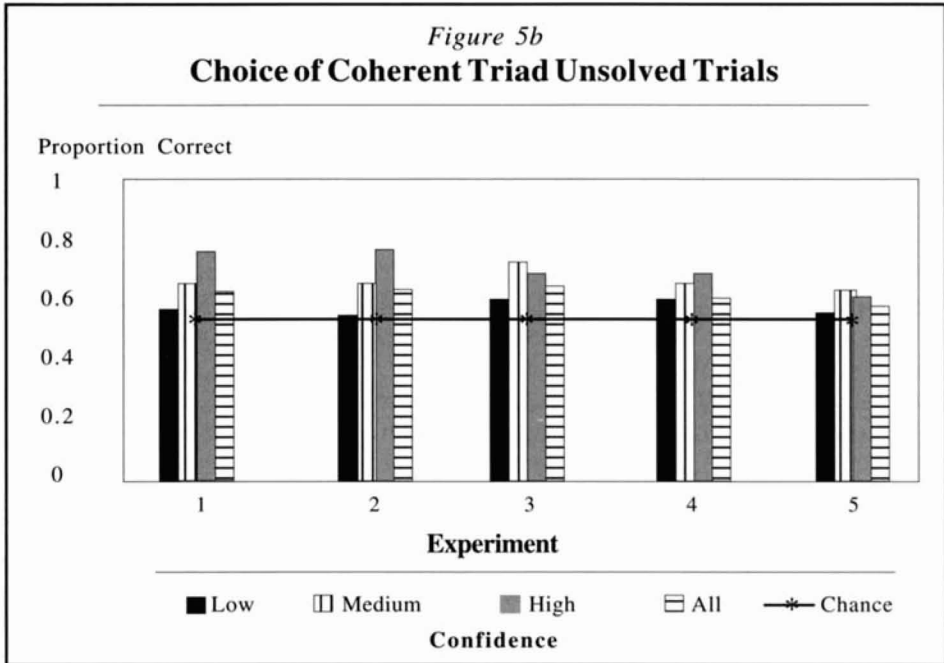
termined both by the details of the suggestion (an external factor) and the subject's level of hypnotizability (an internal factor), acting in concert. This view led him to propose what has come to be called the *Doctrine of Interactionism*, in which personal and environmental factors were not construed as independent, much less mutually exclusive, determinants of behavior, but rather as operating in concert (Figure 4). As he put it in his now classic formulation: "The situation is as much a function of the person as the person's behavior is a function of the situation." With this summation, Bowers broke a logjam in personality theory, and laid the foundation for extremely interesting programs of contemporary research in which other investigators have analyzed how persons construct the situations to which they in turn respond, through both passive processes of evocation and active processes of selection, behavioral manipulation, and cognitive transformation (e.g., Snyder & Ickes, 1985).

Later, and again operating under the influence of his research on hypnosis, Bowers took up an entirely different issue, the role of intuition in problem solving. Cognitive psychology hasn't had much to say about intuition, perhaps because it is so hard to study objectively, perhaps because many cognitive psychologists are still uncomfortable with talk about consciousness. In Bowers' formulation, intuition is an unconscious influence on problem solving, because the people are unaware of *why* they think or feel the way they do — they just happen to think or feel that way. In any event, the chief problem has been methodological — to develop an experimental paradigm which would convincingly show intuitions in operation. Bowers and his associates (Bowers, Regehr, Balthazard,

& Parker, 1990) did so, in an adaptation of an ancient psychological test, Mednick's (1962) Remote Associates Test (RAT). In the RAT, the subject is presented with a set of three words, and the task is to generate a fourth word which is associatively related to all three. An example, admittedly somewhat dated, is DEMOCRAT GIRL FAVOR, three words which have the associate PARTY in common. Bowers et al. presented their subjects with two such triads: one of these is coherent (e.g., PLAYING CREDIT REPORT), because there is a solution to the problem (i.e., CARD); the other is incoherent (e.g., STILL PAGES MUSIC), because — barring psychotically loose associations, of course — there is no associate that the three cues have in common. The subjects' task is to give the solution to the coherent triad; and if they cannot, they are to try to guess which triad is coherent. Bowers et al. found that sub-

jects can distinguish between coherent and incoherent triads at levels significantly above chance, *even though they cannot give the solution to the coherent triad* (Figure 5a). A conceptually similar effect was obtained in the perception of fragmented pictures: subjects could distinguish between organized and disorganized collections of picture fragments, even though they could not tell what the pictures were pictures of (Figure 5b). Bowers suggested that these guessing effects are mediated by the activation spreading from mental nodes representing the cues and accumulating at a node representing the target, albeit to a level below the threshold for conscious awareness. As a result of these experiments, cognitive psychologists interested in problem solving have revived their interest in unconscious processes in problem solving, and the role they might play in such phenomena as intuition, incubation,





and insight (e.g., Dorfman, Shames, & Kihlstrom, 1996; Kihlstrom, Shames, & Dorfman, 1996).

The final Bowers entry on my Faustian list³ is a book, *Hypnosis for the Seriously Curious* (Bowers, 1976), and frankly I don't know whether it's the book that's on my list, or just the title. I think I wish I had written the book, but I know I wish I had thought of the title. Anyway, the title perfectly captures Ken's intellectual style, *serious curiosity*: not bound to received wisdom, always looking for something new and interesting, always trying to understand things on their own terms, not trying to impose an a priori understanding on them. And the amazing thing is that Ken didn't have to sell his soul to do what he did. He wasn't driven the way so many of us are; it all seemed to come so naturally to him. He took so much pleasure in his work, and he was willing to take the time needed to get a project right

before writing it up for publication. I have to think that if we were all more like Ken Bowers, more often, the study of hypnosis would be a much more rewarding enterprise than it often is, and much closer to achieving its promise of contributing to our understanding of mind and behavior.

Author Notes

Paper presented at a special symposium honoring Kenneth S. Bowers at the 14th International Congress of Hypnosis, San Diego, June 1997. The point of view represented in this paper is based on research supported by Grant MH-35856 from the National Institute of Mental Health. I thank Heidi Wenk for comments during the preparation of this paper, which honors Ken Bowers the way he honored Jack and Josephine Hilgard almost 20 years ago (Bowers, 1978).

Footnotes

- ¹ For the record, this is not the Faust syndrome discussed by Jodorkovsky (1994), in which a patient achieves a kind of immortality through interminable psychoanalysis.
- ² Actually, hypnosis *was* in the air at Illinois. Bowers (1966) wrote an author note acknowledging the help of J.A. Williamsen, who had already published an investigation of posthypnotic amnesia (Williamsen, Johnson, & Eriksen, 1965) which laid the foundation for my own later work on explicit and implicit memory (Kihlstrom, 1980). But this study, too, seemingly “came out of nowhere,” as none of the faculty at Illinois at the time had any reputation as hypnosis researchers.
- ³ There are other Bowers papers that I like, very much, but they don’t rise to the level of my Faustian list: two reviews focusing on the psychosomatic effects of hypnotic suggestions (Bowers, 1977; Bowers & Kelly, 1979), a set of experiments on active and passive modes of attention (Bowers & Brenneman, 1981), and a critique of recovered memory therapy (Bowers & Farvolden, 1996).

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