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Altering States of Consciousness

Consciousness can be characterized as a state of mental alertness and awareness. Conscious people experience concurrent, retrospective, or prospective awareness of events in their environment—an awareness that exists even in the absence of their ability to report it to others. Consciousness can also be characterized as the experience of voluntariness. People experience themselves as deliberately focusing attention on one object or idea rather than another and choosing among them to respond to environmental demands or to achieve personal goals—goals of which they are aware.

A person is in an altered state of consciousness to the extent that these monitoring and controlling functions have been modified or distorted (Farthing, 1992; Kihlstrom, 1984). For example, a person may be unaware of current or past events that nonetheless are affecting his or her experience, thought, and action; or a person may represent objects and events in a manner that is radically discordant with objective reality; or a person may be unable to exert ordinary levels of voluntary control over attention and behavior. Yet, in contrast, an individual in an altered state of consciousness may be *more* aware of events than usual or otherwise able to transcend the limits of normal voluntary control. In this respect, altered states of consciousness are relevant to enhancing human performance.

An altered state of consciousness can be defined by four features (Kihlstrom, 1984): (1) operationally, as the product of a particular induction technique; (2) phenomenologically, as an individual's subjective report of altered awareness or voluntary control; (3) observationally, as changes in overt behavior corresponding to a person's self-report; and (4) physiologically, as a particular pattern of changes in somatic functioning. In principle, every altered state of consciousness would be associated with a unique combination of these four attributes. For example, dreaming sleep is induced by going to bed, closing one's eyes, and counting sheep; by subjective reports of a lapse in consciousness or dreaming; by observable behaviors such as closed eyes, prone position, and slow breathing; and by high-frequency, low-amplitude, desynchronized brain waves accompanied by rapid, synchronous eye movements. However, such clear specification of the four features does not characterize most altered states of consciousness.

In some instances, this lack of clear specification reflects the state of current technology and incomplete knowledge from available experimental work. But it is also not clear that the relationship between mind and brain is such that it will ever be possible to specify unique psychophysiological correlates of different states of consciousness. Accordingly, in this chapter, we consider the effects on human performance of a number of conditions that are conventionally defined as altered states of consciousness. Chief among these is hypnosis, a technique that has been widely used in attempts to enhance human performance. We also discuss restricted environmental stimulation and update the committee's previous reviews of sleep learning and meditation (see Druckman and Swets, 1988:Ch.4; Druckman and Bjork, 1991:Ch.7).

HYPNOSIS

Hypnosis is a social interaction in which one person, a hypnotist, offers suggestions to another person, a subject, for experiences involving alterations in perception, memory, and the voluntary control of action. Hypnosis is typically induced by suggestions for relaxation, focused attention, and closing one's eyes. After a subject's eyes have closed, the procedure continues with suggestions for various sorts of imaginative experiences. The range of such experiences is very broad: a hypnotist may ask a subject to extend his arm and suggest that he is holding a very heavy object, whose weight is pressing the hand and arm down; a hypnotist may ask a subject to interlock her fingers and suggest that her hands are glued together so that they cannot be pulled apart; a hypnotist may suggest that there is a voice asking questions over a loudspeaker, to which the subject should reply; a hypnotist may suggest that a subject cannot smell an odorous substance held near his nose; or a hypnotist may suggest that a subject is growing younger and reliving an experience from early childhood. A subject may also be given a posthypnotic suggestion: for example, that after hypnosis, when the hypnotist picks up a pencil, the subject will stand up, stretch, and change chairs—but forget that he had been told to do so. A subject may also receive a suggestion that upon awakening she will be unable to remember the events and experiences that transpired during the hypnotic session;

after the session, she may indeed display a posthypnotic amnesia, but when the hypnotist gives the prearranged cue, she may well execute the suggested behavior. Typically in these situations, the subjects are unaware that they are carrying out a posthypnotic suggestion until the suggestion for amnesia has been canceled by a prearranged cue. These experiences and their accompanying behaviors are often associated with a degree of subjective conviction bordering on delusion and an experience of involuntariness bordering on compulsion.

Hypnosis has a long history, by some accounts extending back to a version of suggestive therapeutics practiced by the ancient Greeks and Romans in the temples of Aesculapius, and the technique of "animal magnetism" promoted by Franz Anton Mesmer in eighteenth-century Vienna and Paris. The term "hypnosis" itself was coined by Braid in 1842-1843; interest in the technique was revived by French neuropsychiatrists, particularly Hippolyte Bernheim, Jean Baptiste Charcot, Pierre Janet, and A. A. Liebeault in the late nineteenth century and brought into academic psychology in the early twentieth century by William James, Morton Prince, and Boris Sidis. Hypnosis then became a salient topic for psychological research, with pioneering programmatic investigations by P. C. Young, Clark L. Hull, and Milton H. Erickson, among others. The modern era of hypnosis research was inaugurated in the 1950s by the systematic work of Martin T. Orne, Ernest R. Hilgard, and Theodore X. Barber.

The most important finding from 100 years of formal research is that there are wide individual differences in response to hypnotic suggestions (Hilgard, 1965). These differences are measured by standardized scales of hypnotizability such as the Stanford Hypnotic Susceptibility Scale, which are constructed as work-samples of hypnotic performance. The distribution of scores on such scales is quasinormal: relatively few subjects score at the very highest levels of the scale, relatively few are entirely refractory to hypnosis and respond to hypnotic suggestions to at least some degree. The available evidence indicates that hypnotizability is a cognitive skill that reaches a peak in the years immediately before adolescence, remains highly stable during most of adulthood, and may decline during middle and old age. There is some controversy over whether hypnotizability can be modified by cognitive-behavioral interventions.

A great deal of research has attempted to isolate correlates of hypnotizability within the larger domain of personality and cognitive assessment, which would permit the confident prediction of hypnotic responsiveness. While hypnotizability is essentially unrelated to conventional personality characteristics, as measured by such well-known instruments as the Minnesota Multiphasic Personality Inventory and the California Psychological Inventory, hypnotizability has been found to be related to individual differences in "hypnosis-like" experiences in the ordinary course of everyday living, in which attention is tightly focused, imagined events become real, or a person loses touch with objective reality. For example, a classic paper by Tellegen and Atkinson (1974) showed that hypnotizability was modestly but significantly related to "absorption," defined as a person's tendency to completely commit his or her cognitive resources to a "unified representation" of attentional objects; or, alternatively, as a person's disposition to enter stages of cognitive restructuring marked by narrowed or expanded attention (for a review, see Roche and McConkey, 1990). Interestingly, scores on Tellegen's absorption scale do not correlate with scores on extraversion or neuroticism, the two major dimensions represented in the usual personality inventories. Thus, it is not surprising that those instruments have not proved useful to measure hypnotizability. But absorption is related to a broader construct of openness to experience, which has been promoted by McCrae and Costa (1985) as one of the five major dimensions of personality—but a dimension that had been largely ignored in earlier psychometric work on individual differences (for a review, see Glisky et al., 1991).

Although we have characterized hypnosis as a social interaction, involving a hypnotist and a subject, it should be understood that in a very real sense all hypnosis is self-hypnosis (L. S. Johnson, 1981; Orne and McConkey, 1981). A hypnotist does not hypnotize a subject: rather, a hypnotist functions somewhat like a coach or tutor, who helps a subject hypnotize him- or herself. A number of investigations have shown that, with only minimal instructions and practice, subjects can successfully learn to induce hypnosis in themselves (Fromm et al., 1981; L. S. Johnson, 1979; L. S. Johnson et al., 1983; L. S. Johnson and Weight, 1976; Ruch, 1975; Shor and Easton, 1973). However, we note that much of what is described as "self-hypnosis" in the clinical and self-help literature is really only suggestions for relaxation, imagery, and reverie. Yet, hypnosis need not involve relaxation—subjects have been successfully hypnotized while engaged in vigorous exercise (Banyai and Hilgard, 1976)—and it goes beyond imagery and reverie to produce subjectively compelling alterations in conscious awareness and control. The rest of this section reviews the use of hypnosis for a wide variety of purposes, including enhancing performance.

Analgesia and Pain Control

To begin, many limitations on performance are caused by physical pain and hypnosis has long been acknowledged as an effective technique for pain control. Hypnosis, in its earlier form of animal magnetism, was used in major surgery as early as 1821; in fact, the reports by Elliotson (1843) and Esdaile (1850, written in 1846) were substantially responsible for the revival, at midcentury, of medical and scientific interest in the phenomenon. The introduction in the late 1840s of chemical anesthetics such as ether and chloroform supplanted these psychological techniques, but they were revived in the twentieth century along with general scientific interest in hypnosis. The voluminous clinical and experimental literature on hypnosis for pain relief has been summarized by Hilgard and Hilgard (1975, 1983) and others (Chaves, 1989; D'Eon, 1989; Turner and Chapman, 1982; Wadden and Anderton, 1982).

A large number of clinical studies indicate that hypnosis can effectively relieve pain in a substantial proportion of patients that are suffering pain from a wide variety of causes, including burns, cancer, child birth, and dental work. Hypnosis has also been used as the sole analgesic agent in a number of major surgeries, including those for abdominal, breast, cardiac and genitourinary reasons, and for the repair of fractures and dislocations. Although it is likely that fewer than 10 percent of ordinary patients can tolerate major surgery under hypnosis alone, it appears that approximately 50 percent of patients can gain significant pain relief in other procedures and the likelihood of effective analgesia increases in patients who are hypnotizable. This conclusion is underscored by recent clinical studies of pain secondary to the treatment of cancer. For example, Reeves et al. (1983) obtained significant reduction in pain among hypnotizable, but not insusceptible, patients undergoing tumor destruction by hyperthermia (this pain is not usually affected by chemical analgesics). Similarly, Hilgard and LeBaron (1984) found that more than half of a group of hypnotizable children experienced palpable relief of pain during bone-marrow aspirations for treatment of leukemia; none of the insusceptible children did so.[1]

The comparative effectiveness of hypnotic analgesia was evaluated in a provocative study conducted by Stern and his associates (Stern et al., 1977), who exposed volunteer subjects to both the ischemic pain induced when blood-flow to the forearm is cut off by a tourniquet and the cold-pressor pain induced by immersion of the forearm in cold water (both are excellent laboratory analogues of clinical pain). Pain was treated by a number of agents, including hypnosis, morphine, diazepam, aspirin, and acupuncture, as well as placebos for acupuncture and each of the chemical agents. Hypnosis proved to be more effective with both types of pain than any other technique; morphine was the next most effective treatment; acupuncture was the third; valium and aspirin were not more effective than placebos. Hypnotizability was related to the effectiveness of hypnosis, but not that of any of the other treatments.[2] Other studies also indicate that hypnosis is superior to acupuncture (Knox and Shum, 1977; Knox et al., 1978, 1979, 1981). Hypnosis is at least as effective as biofeedback in the treatment of chronic pain, and it requires less equipment (Elton et al., 1980).

Although research clearly documents the effectiveness of hypnotic analgesia, its mechanisms are still subject to debate. Pain is often analyzed in two components: sensory pain, which informs a person of the location and extent of insult, injury, or disease; and suffering, which has to do with the meaning of pain to the person experiencing it. In hypnotizable subjects, at least, hypnosis has equivalent effects on both sensory pain and suffering (Knox et al., 1974); and the analgesic effects of pain as not mediated by the sedative effects of hypnosis-induced relaxation (Greene and Reyher, 1972; Hilgard et al., 1974). Moreover, a clever study showed that hypnotic analgesia is not merely a placebo effect (McGlashan et al., 1969). In this study, insusceptible subjects were convinced through a surreptitious manipulation during a pretest involving painful electric shock that they could respond positively to suggestions for hypnotic analgesia. These subjects did show some reduction in ischemic pain when given hypnotic suggestions for analgesia but the extent of the reduction was no greater than achieved by a placebo for people who thought they were getting Darvon (a chemical painkiller). The hypnotizable subjects showed much greater reductions in pain, both compared with the insusceptible subjects and with their own performance with a placebo. All active agents have a placebo component, and hypnosis is no exception: apparently, insusceptible subjects can derive some measure of pain relief from the placebo component in hypnotic analgesia; but for hypnotizable subjects, the benefits of hypnosis far outweigh those of placebo.

Although it has been speculated that endogenous opiates (endorphins) play a role in hypnotic analgesia, research does not support this view. For example, narcotic antagonists such as naloxone do not block hypnotic analgesia (J. Barber and Mayer, 1977; Goldstein and Hilgard, 1975; Spiegel and Albert, 1983). Nor are there any changes in blood concentrations of endorphins during hypnotic analgesia (De Benedittis et al., 1989; Domangue et al., 1985; Guerra et al., 1985; Olness et al., 1980).[3]

The endorphin results suggest that the most appropriate explanation for hypnotic analgesia is to be found at the psychological, rather than the biological, level of analysis. There are two competing explanations for a psychological effect. According to Hilgard (1977, 1986), highly hypnotizable subjects reduce their awareness of pain through dissociation—by erecting an amnesia-like barrier that diminishes their awareness of the pain. The pain is still registered, as indicated by physiological responses to the stimulus, but the pain is not consciously felt or is, at least, substantially reduced. In contrast, Spanos (1989) and Chaves (1989) argue that hypnotic analgesia is mediated by self-distraction, stress-inoculation, reinterpretation, and other tension-management techniques. Their research indicates that successful response to hypnotic suggestions is often accompanied by the deliberate use of cognitive strategies, such as distraction or pleasant imagery. One dimension of coping appears to be related to individual differences in pain perception in both hypnosis and the normal waking state.

There is little doubt that cognitive strategies can reduce pain (Turk et al., 1983), and they may be of considerable value for subjects who are insusceptible to hypnosis. But recent research support the view that analgesia in highly hypnotizable individuals may be achieved by means of a dissociative mechanism. In an important study by Miller and Bowers (1986), individuals classified as low or high in hypnotizability were subjected to cold-pressor pain under one of three conditions: stress inoculation, in which subjects were instructed to use certain cognitive strategies to cope with pain (self-distraction, imaginative transformation of the stimulus, fantasies incongruent with the pain experience, avoidance of "catastrophizing"); hypnotic suggestions for analgesia; and stress-inoculation instructions masquerading as hypnotic suggestions. The results, based on reports of both pain intensity and subjective distress, were clear: all three conditions produced significant amounts of pain relief in comparison with no-treatment baselines. However, the success of stress inoculation, regardless of whether it was defined as hypnosis, was not correlated with hypnotizability, while the success of hypnotic analgesia was strongly associated with hypnotizability. Moreover, while 92 percent of the subjects in the stress inoculation conditions reported using cognitive strategies to control pain (as they were instructed), such reports were made by only 17 percent of subjects in the hypnotic analgesia condition. The fact that coping can be taught leaves open the possibility for successful cognitive control of pain even in subjects who are insusceptible to hypnosis.

The more important finding is that hypnotic analgesia is mediated by processes other than the usual cognitive control strategies and seem to involve some dissociation of the experience of pain from conscious awareness. Hypnotic analgesia was superior to stress inoculation for hypnotizable subjects, but not for their insusceptible counterparts. A later study by these same investigators (Miller and Bowers, 1993; see also Bowers and Davidson, 1992; Spanos and Katsanis, 1989) found that stress inoculation techniques, as practiced by both hypnotizable and insusceptible subjects, distracted subjects from performance on a simultaneous cognitive task; however, such interference did not occur in the hypnosis condition, especially among the high hypnotizables. Thus, while stress inoculation does appear to be effective in reducing pain, it does not seem to be involved in the hypnotic analgesia experienced by hypnotizable subjects.

In order to reduce the debilitating effects of pain and fatigue on performance, it seems reasonable to teach cognitive strategies such as self-distraction and stress inoculation, which can effectively reduce pain in a wide segment of the population. However, these strategies can also have detrimental effects on ongoing task performance by using cognitive resources. Hypnotic analgesia might be more effective than stress inoculation, but even if it is not, it appears to produce its effects in a manner that does not interfere with other ongoing tasks. Accordingly, individuals who have at least a moderate capacity for hypnosis (and especially those who are highly hypnotizable) might also be taught to reduce pain through self-hypnosis. Even if hypnotic suggestion does not enhance human performance per se, hypnotic analgesia appears to offer some promise for regulating pain and, thus, indirectly enhancing performance in those individuals who have a capacity for hypnosis.

Strength and Endurance

Almost from the beginning of the modern period of research, there have been claims that hypnotized individuals are able to transcend their normal nonhypnotized capacities, showing dramatic improvements in muscle strength, sensory acuity, intelligence, and even clairvoyance. Hypnosis is also frequently used by professional and amateur athletes in both team and individual sports (for reviews, see W. R. Johnson, 1961; Morgan, 1980). However, these claims for hypnotic enhancement of human performance have rarely been subjected to empirical verification.

The history of empirical studies goes back to the nineteenth century revival of hypnosis in France. Rieger (1884) reported an "immensely increased capacity to resist gravity and fatigue" in an experiment in which the task was to hold one's arm in a horizontal position for an indefinite period of time. Similarly, Charcot (1889) reported that a hypnotized subject placed in such a position showed "excellent performance," with no tremor and normal rates of respiration. This was consistent with Charcot's contention that catalepsy was, along with lethargy and somnambulism, one of three stages of hypnotic depth. But Hull (1932:229) remarked that "it would be difficult to imagine a more thoroughly bad experiment" than Charcot's. In a careful experiment performed in Hull's own laboratory, Williams (1930) asked seated subjects to hold their arms in a horizontal position and measured both deviations from horizontal and amount of tremor with a recording device attached to the wrist by a thread. He found no significant differences between hypnosis and a control condition. With respect to resistance to fatigue, Nicholson (1920) carried out an experiment using a Mosso ergograph, in which subjects were required to flex their index fingers in order to pull a three-kilogram weight. Measuring the amount of work performed over a 10-minute period, he obtained a large difference between hypnosis and control conditions and concluded that in hypnosis "the capacity for work seemed endless." However, a careful repetition of the experiment by Williams (1929), in which hypnotized subjects received suggestions both for resistance to fatigue and anesthesia to pain, showed only a very small difference between the hypnosis and control conditions.

Concerning grip strength, Hadfield (1924) reported dramatic improvements in performance with a hand dynamometer, but Young (1925, 1926) reported no differences between conditions at any level of hypnotizability.

Over subsequent decades, continuing research on strength and endurance produced a similar mix of positive (Manzer, 1934; Wells, 1947) and negative (e.g., Eysenck, 1941; W. R. Johnson and Kramer, 1960, 1961; W. R. Johnson et al., 1960) results; to further complicate the picture, some investigators, such as Roush, reported positive results with some tests and negative results with others (Mead and Roush, 1949; Roush, 1951).

These earliest studies of the hypnotic enhancement of muscular performance present a number of difficult conceptual and methodological problems. For example, hypnosis, as defined at the outset, is an essentially subjective experience: a subject experiences the world as different from what it is objectively. Put another way, an important aspect of hypnosis is the fact that the imagined state of affairs is not the same as the objective state of affairs. Thus, it is not at all clear that subjects who have received suggestions that they are stronger (for example), should actually grow stronger—any more than that subjects who receive suggestions for age regression should grow smaller in the chair. Yet there is enough research on the "self-fulfilling prophecy" to lend credence to the idea that a person's belief that he or she is stronger might actually lead to enhanced performance on tasks of strength and endurance—although there is no basis for believing (nor any research) that any observed increase will reach superhuman levels.

Modern consideration of this early literature and modern research have suggested some standards for acceptable research. From a psychometric point of view, for example, any specific effect of hypnosis should be correlated with hypnotizability. If ostensibly hypnotic effects are independent of hypnotizability, they may reflect nothing more than expectancies associated with the hypnotic setting or procedures or the efforts of highly motivated individuals to try harder under hypnotic conditions. Expectational and motivational effects are not uninteresting, but the general view is that hypnosis adds something special to them: this extra factor should be correlated with hypnotizability. However, expectations and motivations concerning hypnosis might well be higher in hypnotizable subjects than in those who are insusceptible to hypnosis.

Consider, for example, a study by Orne (1959), in which subjects were asked to hold a 1-kilogram weight at arm's length for as long as possible. The subjects were tested first in hypnosis and then in the normal waking state. Before the nonhypnotic test, however, they were informed (incorrectly) that other subjects had been able to perform even better than they and were paid a bonus for achieving that level. All of the subjects were able to surpass their hypnotic performance when appropriately motivated in the normal waking state. Orne (1965, 1966) concluded that, at least in the context of performance enhancement, hypnosis was only "one of many motivational techniques which will induce an individual to exert himself more than usual" (1965:291-292).

This possibility has led to the development of a number of experimental designs for studying the effects of hypnosis on performance while taking account of the effects of positive expectations and motivations. In the 1960s, for example, T. X. Barber (1969) introduced the task-motivation design, in which hypnotized subjects are compared to controls who have been exhorted to give maximal performance in the normal waking state. The "task-motivation" instructions employed by T. X. Barber (1969:46) were phrased as follows:

> You did not do as well as you really could. Some people think it is difficult to do this task, and therefore do not really try hard. However, everyone is able to do this if they really try. I myself can do it quite easily, and all the previous subjects that participated in this experiment were able to do it when they realized it was an easy thing to do. I want you to score as high as you can because we're trying to measure the maximum ability of people. If you don't try to the best of your ability, this experiment will be worthless and I'll tend to feel silly. On the other hand, if you try to imagine to the best of your ability, you can easily imagine and do all the interesting things I tell you and you will be helping the experiment and not wasting any time.

The general finding of Barber's research was that the performance of task-motivated subjects equalled that of hypnotic subjects. For example, T. X. Barber and Calverley (1964b) took baseline measures of grip strength and weight-holding endurance in a group of subjects and then repeated the tests in hypnotic and task-motivation conditions. There were no effects of either hypnosis or task-motivation on strength of grip. Hypnosis increased endurance, but only when accompanied by task motivation instructions and nonhypnotic task motivation produced even greater increases. Similarly, Levitt and Brady (1964), in a study of highly hypnotizable subjects, found no difference between hypnosis and task motivation. Barber's research has often been criticized for failing to take account of individual differences in hypnotizability, leaving open the possibility that his design obscured performance improvements that occurred among especially hypnotizable subjects. That is, genuine performance enhancements observed in the relatively small number of highly hypnotizable subjects may have been swamped by the lack of enhancement observed in the relatively large number of subjects of low and moderate hypnotizability. Nevertheless, the very fact that unselected task-motivated subjects showed levels of performance better than those of unselected hypnotic subjects underscores the problem of controlling for motivation, in comparison with hypnosis per se.

In his analysis of the performance effects of hypnosis, Orne has underscored the "demand characteristics" inherent in most hypnosis research, by which an experimenter communicates an expectation that performance will be improved in hypnosis, leading to apparent gains that in fact are artifactual (Orne, 1959, 1962). For example, when the same subjects are tested in both hypnotic and nonhypnotic conditions, it may be clear to them that hypnosis is of interest, leading them to hold back on their nonhypnotic performance (Evans and Orne, 1965). Counterbalancing the order of testing, which controls for the effects of fatigue, does not eliminate this possibility. The only solution is to get a baseline that is truly independent of hypnosis, a feature that is rare in this body of research.

In another approach, Slotnick and London (1965) attempted to cope with the problem that the mythology of hypnosis leads most people to expect that hypnosis will improve their performance—even more so to the extent that they are hypnotizable (see also London and Fuhrer, 1961). That is, differences in responses to hypnotic suggestions may be an artifact of differences in expectations related to self-perceptions of hypnotizability. They invented a way of convincing insusceptible subjects that they were, in fact, hypnotizable (Slotnick and London, 1965:40).

> Most people wonder just how well they have been responding to the suggestions that are given . . . . The fact is that your performance earlier indicated clearly, in some of its significant details, that you would be an excellent subject for the purposes of the experiment we are doing now. Because you *are* a good subject, we want you to participate in the hypnotic phase of this experiment.

Somewhat surprisingly, under these conditions insusceptible subjects showed greater improvements following hypnotic suggestion than hypnotizable subjects. London and Fuhrer (1961) found the highest scores for both dynamometer strength and weight endurance among insusceptible subjects given exhortations for enhanced performance, regardless of whether these exhortations were delivered in hypnosis. Other investigations confirmed the basic finding that hypnosis did not improve performance over appropriately motivated nonhypnotic conditions (Evans and Orne, 1965; Rosenhan and London, 1963; Slotnick and London, 1965), indicating that "hypnosis, as such, adds nothing magical to performance" (London and Fuhrer, 1961:332). Publication of these experiments essentially ended this line of research.

Nevertheless, a follow-up experiment by Slotnick et al., (1965), which has been largely ignored, offers some reasons for reopening this research. In this experiment, highly hypnotizable subjects were asked to lift a 2.5-kilogram weight and hold it at shoulder height. In one condition, the subjects were given exhortations for maximal performance, similar to those used by London and Fuhrer. In the other condition, they were given the same exhortation followed by "involving" instructions, in which the subjects were asked to imagine themselves becoming "stronger and more capable." Among these hypnotizable subjects, performance was significantly greater with the involving instructions than exhortation of the sort used by previous investigators.

Although the Slotnick et al. (1965) experiment needs to be repeated in a full version of the London-Fuhrer design, it suggests that significant improvements can be obtained by treatments that capitalize on the ability of hypnosis to alter perception, especially self-perception, in hypnotizable people. That is, the key to hypnotic enhancement of human performance may lie not in the ability of hypnotic suggestions to passively augment performance, but rather in the interaction of involving suggestions to interact with the capacity for absorption and imagining characteristics of hypnotizable individuals. In the absence of a definitive study, however, the conclusion about the hypnotic enhancement of muscular performance must be negative: hypnotized subjects, even those who are highly hypnotizable, do not appear to be capable of exceeding the performance of highly motivated unhypnotized subjects. And even if the Slotnick et al. findings were to be confirmed, the conclusion would be that hypnosis improves performance to the extent that it increases the subject's involvement in the task at hand.

Learning

A history similar to that for strength and endurance can be given for studies of the effects of hypnosis on learning capacity. This line of research received some impetus from the reports of many nineteenth-century authorities that mesmerized or hypnotized subjects gave evidence of transcending of normal capacity: changes ranging from increases in verbal fluency and physical strength to clairvoyance. Nevertheless, an early study by Gray (1934) answered the question only weakly in the affirmative: a small group of poor spellers improved their spelling ability somewhat when the learning occurred in hypnosis. Similarly, Sears (1955) reported that subjects who learned Morse code in hypnosis made fewer errors than those whose learning took place under nonhypnotic conditions.

More dramatic results were reported in a series of studies by Cooper and his associates, using hypnotic time distortion and hallucinated practice. Subjects were asked to hallucinate engaging in some activity, and at its conclusion were given suggestions that a long interval had passed (e.g., 30 minutes) when the actual elapsed time had been considerably shorter (e.g., 10 seconds). The idea was that this expansion of subjective time would effectively increase the amount of study, or practice, that could be performed per unit of objective time. Cooper and Erickson (1950, 1954) reported, for example, that hallucinated practice led to marked improvement in a subject's ability to play the violin. A more systematic study by Cooper and Rodgin (1952), concerned with the learning of nonsense syllables, also had positive results. Unfortunately, there were no statistical tests of the differences between treatment conditions. Even so, the effects of hypnotic time distortion and hallucinated practice were seen only on the immediate test: the superiority of hypnosis virtually disappeared at retest, 24 hours later. Another study, by Cooper and Tuthill (1952), found no objective improvements in handwriting with hallucinated practice in time distortion, even though the subjects generally perceived themselves as having improved. More recent experiments also yielded negative results (T. X. Barber and Calverley, 1964a; Edmonston and Erbeck, 1967).

In contrast, Krauss et al. (1974) reported positive findings in a study of verbal learning in which hypnotized subjects were allotted 3 minutes to study a list of words, but were told they had studied it for 10 minutes. Unfortunately, R. F. Q. Johnson (1976) and Wagstaff and Ovenden (1979) failed to replicate those results: in fact, their subjects did worse under time distortion than in control conditions. In the most comprehensive study to date, St. Jean (1980) repeated the essential features of the Krauss et al. design, paying careful attention to details of subject selection and the wording of the suggestion. Although highly hypnotizable subjects reported that they experienced distortions of the passage of time, as suggested, there were no effects on learning.

The combination of time distortion and hallucinated practice is ingenious, but of course it makes some assumptions that are not necessarily valid. First, can mental practice substitute for actual physical practice? There is in fact considerable evidence for this proposition (Feltz and Landers, 1983), and since hypnotic hallucinations are closely related to mental images, there is no reason to think that hallucinated practice might not also be effective. But time distortion is another matter: the assumption is that the hallucination of something is the same as the thing itself, and there is no reason or evidence for this assumption. In fact, such an assumption flies in the face of a wealth of research on hypnotic hallucinations, which shows that they are inadequate substitutes for the actual stimulus state of affairs (Sutcliffe, 1960, 1961; Kihlstrom and Hoyt, 1988). Thus, while hypnosis, and hypnotic suggestion, can produce distortions in time perception just as they can produce other distortions in subjective experience, these distortions do not necessarily have consequences for learning and memory (St. Jean, 1989).

A rather different approach to the question has been taken by investigators who have offered subjects direct suggestions for improved learning, without reference to time distortion or hallucinated practice (e.g., Fowler, 1961; Parker and Barber, 1964). Unfortunately, interpretation of such studies is difficult because of the same methodological problems encountered in the studies of muscular strength and endurance. For example, the induction of hypnosis might merely increase the motivation of subjects to engage in the experimental task, independent of any effects of hypnosis per se. Moreover, subjects may respond to the demand characteristics of such an experiment by holding back on their performance during baseline tests and other nonhypnotic conditions, which would result in an illusory improvement under hypnosis. As in the studies of strength and endurance, some of the problems have been addressed by studies using the paradigm invented by London and Fuhrer (1961), in which hypnotizable subjects are compared with objectively insusceptible subjects who have been persuaded that they are responsive to hypnosis.

As noted above, studies of muscular performance using the unadorned London-Fuhrer design have generally found that when subjects are given hypnotic exhortations for enhancement, hypnotizable subjects and insusceptible subjects who believe that they are hypnotizable perform the same. Measures of rote learning show similar results (Evans and Orne, 1965; London et al., 1966; Rosenhan and London, 1963; Schulman and London, 1963). Thus, the available evidence suggests that hypnotic suggestions do not enhance the learning process. However, most of these studies have used a hypnotic induction based on suggestions for relaxation and sleep, which might interfere with both motor performance and learning. Relaxation is not necessary for hypnosis, however (Banyai and Hilgard, 1976), and it remains possible that different results would be obtained if the suggestions were for an active, alert form of hypnosis. Moreover, suggestions that capitalize on a hypnotized subject's capacity for imaginative involvement may prove to be better than mere exhortations (Slotnick et al., 1965). In sum, the question of the possibility of hypnotic enhancement of learning and performance is not closed.

Sensory Acuity and Perceptual Accuracy

Exactly the same considerations apply to studies of the effects of hypnosis on sensory-perceptual acuity (for example, word-recognition thresholds) that apply to learning. In response to suggestions, subjects may very well experience themselves as seeing and hearing objects more clearly and easily—in the absence of any objective change in sensory acuity. The question is whether these changes in subjective experience reflect actual changes in objective performance in tasks involving sensory detection and discrimination, that is, whether a true hyperesthesia can be induced by means of hypnosis. Early authorities, such as Braid (1843) and Bramwell (1903), concluded that this was possible: Bergson (1886) even reported that a hypnotized boy could read, out of the cornea of a man standing in front of him, the letters on a page that the man was holding behind the boy's head.

Beside such dramatic claims, the results of formal research have been much less positive. Again, the earliest investigator in this area was Young (1925), who investigated changes in tactile pressure sensitivity: despite suggestions for heightened sensitivity and the belief of the subjects that they were responding positively to the suggestions, tactile thresholds were actually higher in hypnosis. In a follow-up study, Young (1926) found no effect of hypnosis on the ability to detect subtle differences among postage stamps. Sterling and Miller (1940) likewise failed to find differences in figure-recognition and visual and auditory detection attributable to hypnosis.

Zamansky and his associates revived interest in the question and developed special procedures to evaluate order effects driven by expectancies generated by the comparison of hypnotic and nonhypnotic conditions. In their first study (Scharf and Zamansky, 1963), these investigators obtained a significant reduction in sensory thresholds for subjects who received hypnotic suggestions and a smaller reduction for subjects who received waking suggestions. Detailed analysis (and postexperimental interviews) led these investigators to speculate that the subjects, expecting to be hypnotized on subsequent trials, may have artificially elevated their thresholds on baseline tests. To test this possibility, they replicated tests but did not inform the subjects about the hypnotic test until after the baseline test had been completed. Under these circumstances, baseline thresholds were high in both hypnotic and control conditions: postexperimental interviews indicated that subjects expected to be hypnotized in both conditions (because they could not distinguish between hypnotic and control baselines) and elevated their pretest thresholds accordingly. A later study, with even more careful manipulation of subjects' expectations, showed that baseline thresholds were higher when subjects expected to be subsequently hypnotized (Zamansky et al., 1964).

The Zamansky-Scharf studies underscore the difficulty in performing valid comparisons between hypnotic and nonhypnotic conditions: subjects are capable of modulating their nonhypnotic performance in such a way as to leave considerable room for improvement in hypnosis. This possibility cannot be controlled by conventional counterbalancing; it can only be eliminated by the very difficult strategy of keeping subjects ignorant of the hypnotic tests until baseline testing has been completed. Obviously, holding back on nonhypnotic baselines is a possibility in the studies of strength, endurance, and learning reviewed above. The fact that the nonhypnotic tests may have been unrepresentative of subjects' true nonhypnotic capacity only accentuates the need for caution in interpreting the few positive results obtained in those studies.

This is an area that cries out for application of modern signal-detection theory (Green and Swets, 1966; Pastore and Scheirer, 1974) because of the distinction that approach makes between actual sensitivity and decision criterion. Studies using classical psychophysical techniques for threshold-determination are not able to discriminate between changes in sensitivity and changes in response criterion. Consider, for example, subjects in Young's (1925) study, who are asked to indicate whether they have been touched by one of two stimuli, strong or weak. Increased sensitivity is indicated by an increase in detection of the weak stimulus. But subjects who wished to appear highly sensitive could simply indicate that they felt the stimulus on each trial. Such subjects would show a high rate of "hits," appearing to detect the stimulus each time it was presented; however, they would also show a high rate of "false alarms," indicating that they felt the stimulus even on "catch trials" when it had not in fact been applied. Signal-detection theory takes account of both hits and false alarms and produces a estimate of a subject's sensitivity that is uninfluenced by the subject's response criterion.

In one portion of a larger experiment focusing on hypnotic deafness (which, as an ostensible impairment of human performance, is beyond the scope of this volume), Jones and Spanos (1982) gave subjects suggestions for increased auditory acuity. Hypnotizable subjects did show a slight, nonsignificant increase in sensitivity under these conditions, but they also showed significantly lower levels of sensitivity in a baseline condition—thus suggesting, as in the work of Zamansky and Scharf, that they were holding back in the control test. There was also a change in response bias, with hypnotizable (but not insusceptible) subjects showing an increased bias toward reporting that the signal was present, even when it was not. Although these results are not clear-cut—for example, response criterion did not show a comparable shift in the other direction when hypnotizable subjects were given suggestions for *decreased sensitivity*—they do underscore the problem of response bias in tests of the hypnotic enhancement of performance.

Although hypnosis does not appear to enhance the performance of people whose sensory and perceptual abilities are intact, it may have positive effects on those whose capacities are impaired. In a provocative paper, Graham and Liebowitz (1972) examined the effects of hypnotic suggestion on visual acuity in myopes—that is, people who suffer an impairment in distance vision. The task was to determine the orientation of the break in a series of 19 rows of "Landolt Cs": from the top row to the bottom, the size of the break in the letter "C" progressively diminished. After threshold determination, subjects were hypnotized and given suggestions to relax their eye muscles in order to permit clearer vision; they were also given the same suggestions posthypnotically. Over a period of 3 weeks, the subjects' visual acuity increased, both in and out of hypnosis, as confirmed by independent optometric examination; the extent of improvement was greater than that observed in nonhypnotized controls. A follow-up study showed that the improvements were only observed in highly hypnotizable subjects: measurements with a laser scintillation technique indicated that the improvements were not a function of accommodation or other structural changes in the eyes.

A conceptual replication of this experiment was reported by E.P. Sheehan et al. (1982), who improved on the methodology by carefully matching hypnosis and control subjects for hypnotizability and degree of myopia; they also supplemented the conventional threshold assessments with signal-detection procedures in which the subjects had to distinguish between open and closed lines, the gaps in which were equivalent to those of the "Landolt Cs" used by Graham and Liebowitz (1972). The hypnotic subjects showed a significant improvement on the signal-detection measure of accuracy, but no change in response bias, in comparison with the waking controls. However, there was no correlation between improvement and hypnotizability. Although Wagstaff (1983) questioned their statistical analysis, Smith et al. (1983) were able to defend their procedures. In addition, a secondary analysis by Tataryn (1992) showed that the improvement in acuity corresponded to a fairly substantial experimental effect.

Time Perception

A rather large research literature exists on hypnosis and the perception of time.[4] In the nineteenth century, many investigators claimed that hypnotized subjects were extraordinarily accurate in time perception. For example, Bramwell (1903) gave suggestions to a subject that she should perform some act after a specified interval (in one instance, 4,453 minutes—or 3 days, 2 hours, and 13 minutes) and found that she was accurate (within 5 minutes) on the vast majority of trials—a feat that Bramwell considered to be beyond the capacity of normal subjects. Unfortunately, Bramwell neglected to make the same request of unhypnotized controls, and it is not at all clear that individuals of normal intelligence are incapable of carrying out the elementary arithmetic operations involved. In any event, these early claims have not been upheld by more formal experiments.

Some experiments in this area involve intentional, prospective procedures in which subjects are instructed to perform some response after the passage of a specified interval of time. In one such experiment, Stalnaker and Richardson (1930) found no differences between hypnotic and control intervals in the estimation of intervals of one to three minutes. Sterling and Miller (1940) also found no effect, but Eysenck (1941) reported greater accuracy in hypnosis (for a review of the early literature, see Loomis, 1951). More recently, Tebecis and Provins (1974) found that the prospective estimations of hypnotizable subjects were no more accurate in hypnosis than in the normal waking state. Overall, the experimental literature, which is somewhat sparse, has yielded a mix of results that tend to support negative conclusions (for a recent review, see St. Jean, 1989)—although none of these modern investigations concerned extremely long time intervals of the sort studied by the early researchers.

Another procedure, more recently introduced, involves incidental, retrospective time estimation: without any warning, subjects are asked to estimate how much time has passed since an event specified by the experimenter. With this approach, Schwartz (1978, 1980) and Bowers (1979; Bowers and Brenneman, 1979) found a general tendency of subjects to retrospectively underestimate the length of time that they had been hypnotized, but this tendency was independent of hypnotizability. A series of studies by St. Jean (St. Jean and MacLeod, 1983; St. Jean and Robertson, 1986; St. Jean et al., 1982) confirmed this general finding, which appears to be unrelated to hypnosis per se, but rather to the fact that hypnotic subjects are engaged in demanding, interesting tasks.

Of course, time is not a sensory modality like vision or audition. Although the passage of time can be measured objectively, there is no proximal stimulus for time, and no receptor organs that can extract information about the duration of the interval between two events. Ultimately, time perception is a matter of judgment and inference, and its underlying mechanisms remain largely unknown (Doob, 1971; Fraisse, 1984; Gibbon and Allan, 1984; Ornstein, 1969).

Memory

In recent years there has emerged a considerable literature on the use of hypnosis to enhance memory for knowledge acquired outside hypnosis—what is known as hypnotic hypermnesia (vivid recall). The most prominent question investigated is whether hypnotic suggestion can enhance the memories of witnesses and victims of crime, but essentially the same question can be asked about the prospects for improving memory "bandwidth" in other applied situations. (For a complete review, see Kihlstrom and Barnhardt, 1993, on which this section is based.)

Laboratory studies of hypnotic hypermnesia have a history that extends back to the beginnings of the modern period of hypnosis research (for recent reviews, see Erdelyi, 1988; Kihlstrom and Barnhardt, 1993; Smith, 1983). For example, Young (1925, 1926) taught his subjects lists of nonsense syllables in the normal waking state and then tested recall in and out of hypnosis, each time motivating subjects for maximal recall. There was no advantage of hypnosis over the waking test. Later experiments with nonsense syllables also failed to find any effect of hypnosis, although studies that used meaningful linguistic or pictorial material have sometimes shown hypermnesia effects. For example, Stalnaker and Riddle (1932) tested college students on their recollections for prose passages and verse that had been committed to memory at least 1 year previously and found that hypnotic suggestions for hypermnesia produced a significant enhancement over waking recall. Although this kind of laboratory evidence suggests support for the use of hypnosis to enhance memory, the effects achieved in the laboratory (though sometimes statistically significant) are rarely dramatic. Moreover, it is fairly clear that any gains obtained during hypnosis are not attributable to hypnosis per se, but rather to hypermnesia effects of the sort that occur in the normal waking state. At least four investigations (Nogrady et al., 1985; Register and Kihlstrom, 1987, 1988; Whitehouse et al., 1991) found significant increments in memory for pictures or words in trials conducted during hypnosis but these increments were matched or exceeded by gains made by control subjects tested without hypnosis.

Most important, it seems clear that increases in valid memory (or "hit rate") may be accompanied by an equivalent or greater increase in confabulations and false recollections ("false alarms"). In an experiment by Stalnaker and Riddle (1932), for example, hypnosis produced a substantial increase in confabulation over the normal waking state, so that overall memory accuracy was very poor. The hypnotized subjects were apparently more willing to attempt recall and to accept their "memories"—however erroneous they proved to be. These conclusions are supported by more recent experiments by Dywan (1988; Dywan and Bowers, 1983) and Nogrady et al. (1985), who found that hypnotic suggestions for hypermnesia produced more false recollections by hypnotizable than insusceptible subjects. Whitehouse et al. (1991) found that hypnosis increased subjects' confidence of memory reports that had been characterized as mere guesses on a prehypnotic test. Dywan and Bowers (1983) have suggested that hypnosis impairs the process of reality monitoring, so that hypnotized subjects are more likely to confuse imagination with perception (M. K. Johnson and Raye, 1981).

Proponents of forensic hypnosis often discount these sorts of findings on the ground that they are obtained in sterile, laboratory investigations that bear little resemblance to the real-world circumstances in which hypnosis is actually used; but the evidence supporting this assertion is quite weak. Reiser (1976), a police department psychologist who has trained many investigators in hypnosis, claimed that the vast majority of investigators who tried hypnosis found it to be helpful, but such testimonials cannot substitute for actual evidence. In fact, a study by Timm (1981), in which police officers themselves were witnesses to a mock crime (after having been relieved of their firearms through a ruse!), found no advantage for hypnosis. A later study by Geiselman et al. (1985), using very lifelike police training films as stimuli and actual police officers as investigators, found that the benefits of hypnosis were matched by unhypnotized subjects led through a "cognitive interview" capitalizing on various cognitive strategies. In sum, the available evidence does not indicate that hypnosis has any privileged status as a technique for enhancing memory.

In evaluating the effects of hypnosis on the recovery of forgotten memories, it is important to remember that hypnosis entails enhanced responsiveness to suggestion. Therefore, if memory is tainted by leading questions and other suggestive influences, as Loftus's (1975) work suggests, these elements may be even more likely to be incorporated into memories that have been refreshed by hypnosis. Putnam (1979) exposed subjects to a variant of Loftus's paradigm, in which subjects viewed a videotape of a traffic accident followed by an interrogation that included leading questions. Those subjects who were interviewed while they were hypnotized were more likely to incorporate the misleading postevent information into their memory reports (see also Sanders and Simmons, 1983; Zelig and Beidelman, 1981). Register and Kihlstrom (1987), using a variant of Loftus's procedure introduced by Gudjonsson (1984), failed to find that hypnosis increased interrogative suggestibility, but did find that errors introduced during the hypnotic test did carry over to subsequent nonhypnotic tests. An extensive and complex series of studies by Sheehan and his colleagues (reviewed by P. W. Sheehan, 1988a, 1988b) found that subjects tested during hypnosis were more confident in their memory reports than were those tested in the normal waking state—regardless of the accuracy of the reports.

The situation is worsened when the suggestions are more explicit, as in the case of hypnotically suggested paramnesias—a confusion of fact and fantasy (for a recent review, see Kihlstrom and Hoyt, 1990). Laurence and Perry (1983) suggested to a group of hypnotized subjects that on a particular night they had awakened to a noise. After hypnosis, a majority of subjects remembered the suggested event as if it had actually occurred; almost half of the subjects maintained this belief even when told that the event had been suggested to them by the hypnotist. Similar results have been obtained by a number of investigators, although the precise conditions under which the pseudomemory effect can be obtained remain obscure. Equally important, it remains unclear whether the pseudomemories reflect actual changes in stored memory traces or biases in memory reporting—an issue that also has been raised in the postevent misinformation effect observed outside hypnosis (e.g., McCloskey and Zaragoza, 1985; Metcalfe, 1990; Loftus et al., 1985; Tversky and Tuchin, 1989).

Direct suggestions for hypermnesia are often accompanied by suggestions for age regression: that the subject reverts to an earlier period in his or her own life, relive an event, and act in a manner characteristic of that age (for recent reviews, see Kihlstrom and Barnhardt, 1993; Nash, 1987).[5] With respect to the reinstatement of childlike modes of mental functioning, As (1962) found a college student who had spoken a Finnish-Swedish dialect until age 8, but who no longer remembered the language; his knowledge of the language improved somewhat under hypnotic age regression. More dramatic findings were obtained by Fromm (1970) in a *nisei* student who denied any knowledge of Japanese; when age regressed, she broke into fluent, childish Japanese.

In contrast, experimental studies have found no convincing evidence favoring the reinstatement of childlike modes of mental functioning, whether these are defined in terms of physiological responses (e.g., the Babinski reflex, in which the toes fan upward in response to plantar stimulation), scores on achievement tests, reversion to preconceptual (Werner) or preformal (Piaget) modes of thought (e.g., failing to predict the order in which three spheres will emerge from a hollow tube after it has been rotated through half or whole turns; defining right or wrong in terms of what is rewarded or punished), or perceptual processes (e.g., changes in magnitude of the Ponzo and Poggendorf illusions; the return of eidetic imagery ostensibly prominent in children). Nash (1987) also found that age regression does not necessarily revive specific childhood memories. It may, however, reinstate childlike modes of emotional functioning.

A third component of age regression, revivification, is conceptually similar to the recovery of memory in hypermnesia. In fact, Young (1926) was able to elicit a substantial number of early recollections, whose accuracy was independently verified, in two hypnotizable subjects. And more recently, Hofling et al. (1971) compared subjects' recall of personal experiences to actual diary entries made at the time and found superior memory during hypnosis in comparison with a nonhypnotic session. Unfortunately, neither of these experiments examined false recollections that may have been produced by the subjects. The obvious difficulty in obtaining independent verification effectively prevents many more studies of this sort from being done in order to understand better the conditions under which these improvements in memory might be obtained.

In the absence of independent confirmation, it should be understood that the apparent enhancement of memory occurring as a result of hypnosis may be illusory. But even independent confirmation does not guarantee that hypnosis itself is responsible for the appearance of revivification: the enhancement of memory may come from general world knowledge or cues provided by the experimenter, rather than improved access to trace information. The salient cautionary tale is provided by True (1949), who reported that age-regressed subjects were able to identify at better than chance levels the day of the week on which their birthdays, and Christmas, fell in their 4th, 7th, and 10th years. But the experimenter in question knew the answers to the questions as they were asked: when the experimenter is kept blind to the correct answer, response levels fall to chance (O'Connell et al., 1970). In general, when the testing environment is controlled in such a manner as to eliminate potentially informative cues, there is no evidence that age regression can enhance memory for past experiences. There is often an experience of increased memory but like so much else about hypnosis, the experience is illusory.

Forensic Hypnosis

Despite the poverty of evidence supporting the idea that memory can be enhanced by hypnotic suggestions, hypnosis has come to be used by police officers, attorneys, and even judges in an effort to refresh or bolster the memories of witnesses, victims, and suspects in criminal investigations. Although hypnosis does appear to have been helpful in some cases, a number of instances have been recorded in which the memories produced by hypnotized witnesses and victims have proved highly implausible or even false (Orne, 1979). The inherent unreliability of hypnotically elicited memories—the difficulty of distinguishing between illusion and reality, the susceptibility of hypnotically refreshed memory to distortion by inadvertent suggestion, and the tendency of subjects to enhance the credibility of memories produced through hypnosis—creates problems in the courtroom or in any environment in which the factual accuracy of hypnotically refreshed memory is critical (Orne et al., 1984, 1988).[6]

For these reasons, and in response to a number of cases that were prosecuted on the basis of evidence that later proved to be incorrect, both the medical establishment (American Medical Association, 1985) and the courts have begun to establish guidelines for the introduction and evaluation of hypnotically elicited memories. In some sense, of course, these guidelines are superfluous: because there is no evidence that hypnosis enhances memory, there is no reason to use it at all for this purpose. In fact, the current dominant position in the state courts appears to be a per se exclusion of all hypnotically elicited evidence, and some courts have gone so far as to exclude from testimony even the prehypnotic memories of a witness who has been subsequently hypnotized, on the grounds that hypnosis may distort prehypnotic as well as hypnotic memories—for example, by inflating the subject's confidence in what he or she had already remembered (Scheflin and Shapiro, 1989).

However, it seems clear that investigators, persuaded by anecdotal evidence of the efficacy of hypnosis in individual cases, will continue to use hypnosis for this purpose, despite the scientific evidence. Accordingly, some guidelines for the use of hypnosis seem necessary at this time. Those who use hypnosis to enhance memory should be aware of the dangers posed by its use and should conform their procedures to the sorts of procedural safeguards, adopted in many legal jurisdictions, to minimize the possibility that the witness' independent memory will be contaminated by hypnosis, and to maximize the likelihood that such contamination will be detected if it has occurred.

One set of guidelines, based on those proposed by Orne (1979) and adopted in the United States by the Federal Bureau of Investigation (Ault, 1979), has been proposed by Kihlstrom and Barnhardt (1993:106-114):

(1) There should be a prima facie case that hypnosis is appropriate. Memories that have not been properly encoded are not likely to be retrieved, even by heroic means. Thus, hypnosis will be of no use in cases where the witness did not have a good view of the critical events, was intoxicated, or sustained head injury at the time of the crime.

(2) For the same reason, there should be an objective assessment of the subject's hypnotizability, using one or another of the standardized scales developed for this purpose. Hypnosis will be of no use with subjects who are not at least somewhat hypnotizable.

(3) The hypnotist should be an experienced professional, knowledgeable of basic principles of psychological functioning and scientific methods. Forensic hypnosis raises cognitive issues, such as the nature of memory, and clinical issues, such as the subject's emotional reactions to any new information yielded by the procedure, and the hypnotist must be capable of evaluating and dealing with the situation on both counts.

(4) The hypnotist should be a consultant acting independently of any investigative agency, either prosecution/plaintiff or defense/respondent, so as to emphasize the goal of the procedure: collecting information rather than supporting a particular viewpoint.

(5) The hypnotist should be informed of only the barest details of the case at hand in order to minimize the possibility that his or her preconceptions will influence the course of the hypnotic interview. A written record of all information transmitted to the hypnotist should be preserved.

(6) A thorough interview should be conducted by the hypnotist, in advance of the hypnotic session, in order to establish a baseline against which any subsequent changes in memory can be evaluated.

(7) Throughout the prehypnotic and hypnotic interview, the hypnotist and the subject should be isolated from other people, especially those who have independent knowledge of the facts of the case, suspects, etc., so as to preclude the possibility of inadvertent cuing and contamination of the subject's memory.

(8) A complete recording of all interactions between hypnotist and subject should be kept to permit evaluation of the degree to which any influence may have occurred.

Because these standards are difficult to meet, and because of the continuing legal controversy attached to forensic hypnosis, investigators are advised to confine their use of hypnosis to the gathering of investigative leads. Under these circumstances, hypnotically refreshed memories are not introduced into evidence, and cases are based solely on independently verifiable evidence.

Hypnosis has the potential to permanently distort a person's memory, and it can increase the likelihood of both unintended confabulations and the influence of leading questions and other misinformation. The confusion between illusion and reality that is part and parcel of the hypnotic experience may be fascinating in the laboratory and perhaps useful in the clinic, but it is problematic in the real world, outside the laboratory or doctor's office. The myths surrounding the wonders of hypnosis may lead consumers of hypnotically elicited memories to inappropriately inflate their confidence in what they remember or to inappropriately accept such memories as accurate.

TRANSCENDENTAL MEDITATION

Meditation can be defined as a broad class of ritual techniques designed to alter the deployment of attention, and thus consciousness itself, in the hope that the practitioner will achieve a higher level of personal and spiritual growth. In some practices, the alteration of attention is toward focused concentration on a single stimulus, image, or idea; in others, the alteration of attention is toward an expanded awareness encompassing everything that is available to consciousness. Meditation has a place in a wide variety of religious traditions, both East and West: many Hindus, Buddhists, Jews, Christians, and Muslims, among adherents of other religions, practice meditation as an important part of their devotional lives—for example, as a way of achieving union with God or transcending the boundaries of worldly, corporeal existence. In this century, moreover, a number of avowedly secular systems of meditation have been developed to enable individuals to reduce stress and increase feelings of well-being, independent of their religious beliefs and practices. In either form, it seems clear that the disciplined practice of meditation might have positive effects on human performance.

Accordingly, the committee's second report, *In the Mind's Eye* (Druckman and Bjork, 1991), included a broad overview of the relevant literature on the effects of meditation. In light of extraordinary claims for performance enhancement made by some enthusiasts, the committee's initial work focused on methodological problems in evaluating research on meditation. For example, much research on religious and mystical forms of meditation necessarily uses adherents of the particular religious tradition as subjects; as a consequence, it is difficult to separate the effects of meditation per se from those of religious belief or from changes in life-style that are conditioned on such belief. For example, it would be no surprise to find that meditators are less prone to coronary heart disease if the meditators are also vegetarians. In addition, in many studies that show differences between meditators and nonmeditators, a key question is whether individuals who choose a life-style of disciplined meditation may have possessed the characteristic in question even before they began meditating. Moreover, the confounding of practice with belief raises questions of generalizability: If the rationale for a particular meditative practice is to be found in a particular religious tradition, are the salutary effects of this practice available to individuals who do not share those beliefs or practice that life-style?

In addition, most meditative practices consist of several components, including seated or prone posture, relaxation, attention to one's breathing or to a word or phrase serving as a "mantra," and the development of a passive frame of mind. The heterogeneity within meditative practices makes it difficult to select appropriate control procedures. Many studies of the effects of meditation have no control group at all, and many of the remainder simply compare meditators to individuals who do not meditate. But it is clearly not sufficient to compare one group of people who meditate with another group of people who do nothing. Even if such a comparison showed differences that favor meditation, the specific factors that are responsible for the effect would still not be known. For example, the positive effects might be due solely to the subject's state of relaxation, and not to posture, breathing, the mantra, or mental passivity. A properly designed analysis of the effects of meditation would analyze its components and then systematically vary them: for example, there would be a group that sits prone and focuses on a mantra but does not relax and does not maintain a passive state of mind. As an extreme example, study of religious meditation would include a control group who practiced all of the components of the meditative discipline in question without adhering to the religious belief that ostensibly underlies that discipline.

In its previous report, the committee lamented that few if any studies met this standard, making it difficult to draw firm conclusions about the effects, or even the potential, of meditation. Many of the studies of meditation we reviewed lacked even elementary controls for relaxation. Even the results of the best controlled studies often showed that the effects of meditation are often duplicated in individuals who simply rest quietly in chairs for an equivalent amount of time or who engage in progressive muscle relaxation. It is not clear that the effects obtained from the controlled, peaceful confines of the meditation room or research laboratory would generalize to the real world—a battlefield, a corporation, or an assembly line. This methodological situation has not changed. At the same time, however, the committee is aware of the existence of a large body of literature pertaining to the effects of one particular meditative discipline: the transcendental meditation (TM) technique, and the TM-Sidhi program, introduced by the Maharishi Mahesh Yogi and based on the Vedic tradition of Hinduism. In light of the high degree of popular interest in this technique, the committee decided to review the research on TM as part of its continuing work. Even with this narrow focus, however, the task is daunting: one collection of papers pertaining to TM, entitled *Scientific Research on Maharishi's Transcendental Meditation and TM-Sidhi Program,* contains 430 articles in 5 volumes. Accordingly, this section focuses only on the studies summarized in three published meta-analyses of the effects of TM: on physiological arousal (Dillbeck and Orme-Johnson, 1987); on relaxation and anxiety (Eppley et al., 1989); and self-actualization and psychological health (Alexander et al., 1991).

Theory and Practice

In TM, practitioners begin by assuming a comfortable posture, relaxed but alert, in an environment free of interruptions. After a few minutes of quiet relaxation, they close their eyes and mentally repeat a Sanskrit sound, or mantra, which provides a focus of attention. After about 20 minutes, they stop attending to the mantra and open their eyes but remain comfortably relaxed for a few more minutes before beginning (or resuming) their ordinary business. This cycle is performed twice daily, typically in early morning and late afternoon. It may be practiced individually or in groups. The purpose of the regimen is to shift the person from an active mode of consciousness, oriented toward the external world, to a more passive state oriented toward one's internal, subjective experience. Disciplined practice of TM, which is taught according to a standardized syllabus for a fee by certified trainers, eventually results in a state characterized as "pure consciousness," or samadhi, in which thought ceases but awareness is maintained. The regular achievement of this state of contentless awareness, through TM, is held to have a wide variety of beneficial effects for the person.[7]

The practice of TM is based on a classification of consciousness into seven qualitatively distinct states, including the waking, sleeping, and dreaming familiar in ordinary life, but also extending to four other states that may be achieved as a result of the disciplined practice of meditation: transcendental consciousness reflects the temporary breakdown of the usual cognitive boundaries between the knower, the object of knowledge, and the process of knowing; cosmic consciousness is achieved when transcendental consciousness becomes a permanent feature of mental life, maintained not only in waking but also in sleeping and dreaming; refined cosmic consciousness entails the further breakdown of the distinction between self and object, although there is still some residual awareness of self; finally, unity consciousness involves the complete disintegration of the sense of self and of any boundary between the individual and the universe.

In developing TM as a "science of creative intelligence," Maharishi and his disciples have sought to connect the rather obscure (to a Westerner) concepts of Vedic psychology with the rather more familiar concepts of cognitive and personality psychology (for a summary, see Alexander et al., 1991).[8] Thus, the achievement of transcendental consciousness is likened to Maslow's concept of self-actualization, and its effects are likened to Maslow's peak experiences, involving "cognition of being," or B-cognition. Similarly, the distinction between transcendental and cosmic consciousness is analogous to Maslow's distinction between "peakers," who have frequent peak experiences, and "transcenders," for whom the peak experience is more stable. In another line of argument, the successive achievement of higher states of consciousness is sometimes attached to Piaget's stages of cognitive development (Alexander and Langer, 1990). Whereas Piaget emphasized the child's developing ability to form and manipulate mental representations of the external world, the implication of Vedic psychology is that there are "postrepresentational" stages in which the distinction between an object and its mental representation progressively disintegrates. Thus, just as a child who has learned formal operations has abilities that one in the sensorimotor stage does not, an adult who is capable of cosmic consciousness is held to be capable of feats that are simply beyond the reach of one whose consciousness is limited to waking, sleeping, and dreaming.

Physiological Effects

In a reply to Holmes's (1984) critical review of the effects of meditation on autonomic arousal, Dillbeck and Orme-Johnson (1987) briefly reviewed the results of 32 (not 31 as indicated in the paper) studies of the physiological effects of TM; 23 of these papers had appeared in refereed journals. All of the studies employed a pretest-posttest design, in which measures of physiological arousal were taken before and during a period of TM or eyes-closed rest. Some of the studies involved only TM; other studies compared both TM and rest in the same experiment. From each study, the investigators were able to extract information on the effects of TM or rest on one or more of five measures of physiological arousal. For three of these measures—basal galvanic skin response (GSR), respiration rate, and plasma lactate level—TM produced greater reductions from baseline than did eyes-closed rest. For the remaining two measures (spontaneous GSRs and heart rate), the differences were in the same direction, but not significant. For four of the variables (all but basal GSR), there were also significant baseline differences between meditators and nonmeditators, indicating that subjects who were experienced in meditation showed lower levels of arousal even when they were not meditating.

Unfortunately, this conclusion is somewhat weakened by a crucial design feature of the research: in most of the studies reviewed, there is a confounding of subjects and experimental conditions in that experienced meditators are placed in the meditation condition, while subjects in the rest condition have no experience with the technique. For example, in a study by Orme-Johnson (1973), meditators had lower baseline levels of spontaneous GSRs and showed greater decreases in spontaneous GSRs while meditating in comparison with control subjects. However, one group of subjects assigned to the meditation condition had practiced TM twice a day for an average of 15 months; another group had practiced TM for an average of 24 months. In contrast, none of the subjects assigned to the rest condition had any experience with TM (some were on a waiting list for TM instruction). Similarly, in a study by Jevning et al. (1978) plasma lactate levels declined during TM, but not during a control period of rest: but, again, the subjects in the TM group had been practicing TM twice a day for an average of a year, while those in the control group had not practiced TM. According to Jevning et al. (1992:416), "most recent investigations [of TM] have studied the long-term meditator," defined as an individual who has been regularly meditating for at least 5 years, usually has had additional meditation experience at special courses, and is often him- or herself an instructor of TM. Generally, subjects in control groups do not have any similar kind of experience.

The consequences of this confounding problem are twofold: it is possible that experienced meditators would show decreases in physiological arousal during an eyes-closed rest period, without meditating, that are similar to those shown when they are actually meditating; alternatively, it is possible that subjects who had practiced eyes-closed rest (i.e., without meditating) for 20 minutes a day, twice a day, for 15-24 months would show decreases in physiological arousal during an eyes-closed rest period that are comparable to those shown by the meditators during meditation. Comparisons of this sort may be impossible—experienced meditators may find it difficult to rest with their eyes closed without slipping into meditation, and individuals who routinely practice eyes-closed rest may be hard to find. But without such rigorous comparisons, it is not possible to draw definitive conclusions about the effects of TM on physiological arousal.

Relaxation, Anxiety, and Self-Actualization

Similar problems are encountered in the second review, concerning relaxation and anxiety (Eppley et al., 1989). In this case, comparisons were made between TM and other meditation techniques, progressive muscle relaxation (including Benson's "relaxation response," which is a secularized version of TM), and other relaxation techniques such as electromyographic biofeedback, in which subjects are taught techniques for reducing activity in the skeletal musculature. There were 105 separate studies, of which 47 appeared in peer-reviewed journals. Trait anxiety, as a characteristic of personality, was measured by the Spielberger State-Trait Anxiety Inventory or similar measure. The salient finding was that practitioners of TM showed substantially lower levels of trait anxiety than did those who practiced the other comparison techniques. Similarly, a meta-analysis of 34 articles (17+ by Alexander et al., 1991) found a greater effect of TM than comparison meditation or relaxation treatments on self-actualization, as measured by scores on the Personal Orientation Inventory or similar instruments.

The trait-anxiety meta-analysis was much more extensive than the one on somatic arousal, and so the investigators were able to develop statistical controls for a wide variety of potentially confounding variables. Again, however, a combination of subject-selection procedures and the special characteristics of TM undermine the strength of the positive conclusions about TM. Eppley et al. (1989) show that, across experiments, the subjects in the TM conditions practiced the technique for an average of 2.5 months, while the other techniques had been practiced for an average of 1.6 months. Similarly, TM practitioners had received an average of 5.5 hours of instruction in the first week, compared with 1.5 hours for the others. And TM participants had an average of 14 practice sessions per week following instruction, compared with 10.1 sessions for the other techniques. Thus, at the time the comparisons were made, TM practitioners had experienced an average of 140 sessions (2.5 months at 14 sessions per week), while the subjects in the other groups had an average of only 64.6 (1.6 months at 10.1 sessions per week). In an attempt to control for potentially confounding differences between the groups, Eppley et al. (1989) statistically adjusted for the duration of training, attrition rate, and hours of follow-up instruction. However, it is clear that this adjustment cannot take account of the fact that subjects in the TM groups practiced their technique more than twice as much as did those in the comparison groups.

Equally important, it should be remembered that TM is not just a method of relaxed concentration: the complete TM package includes a religious (or quasireligious) rationale. After all, the program is explicitly intended to promote a greater sense of happiness and well-being. Practitioners are informed that TM is the entryway into a new "age of enlightenment" characterized by personal fulfillment, community harmony, and peace. And they are specifically given the expectation that the disciplined practice of TM will lead to self-actualization and the attainment of higher stages of consciousness. It would be surprising if these kinds of expectations, especially in the context of the commitment of practitioners to the TM program and the social support provided by other TM practitioners, did not lead to reductions in trait anxiety and increases in self-actualization, especially when measured by self-reports. And it is not yet known whether the other techniques, practiced as long as TM and embedded in the same sort of culture, expectations, and beliefs that surround TM, might not show the same effects. Again, although Eppley et al. (1989) appear to demonstrate that TM reduces anxiety and Alexander et al. (1991) appear to demonstrate that TM increases self-actualization and feelings of well-being, the literature does not provide the kinds of controlled comparisons that are necessary to scientifically evaluate the specific effects of the TM program.

Concluding Comment

In undertaking even its limited review of the literature on TM (limited to a focus on those effects on human performance that have received the most extensive study), the committee was mindful of the fact that it was evaluating a psychological technique that is also an inherent part of a system of religious beliefs. It should be clear that TM is, for all intents and purposes, a form of religion. It has its origins in Hinduism, a religious tradition which arose on the Indian subcontinent, worships a divine trinity of Brahma, Vishnu, and Shiva as well as a number of other deities, emphasizes religious duty or dharma, and adheres to scriptures such as the Rig-Veda, the Upanishads, and the Bhagavad-Gita. TM, as practiced by the followers of the Maharishi Mahesh Yogi, is somewhat secularized, based in a Vedic "science of creative intelligence" and requiring no belief in divine beings as such. However, much of its vocabulary, including references to cosmic and unity consciousness and to higher states of being, is essentially religious in nature. Maharishi himself is referred to as "His Holiness" in official TM literature. In some respects, TM is to Hinduism what Unitarian Universalism is to Christianity.

The committee attempted to approach TM as a strictly secular practice, analogous to conventional therapeutic techniques such as biofeedback or progressive relaxation, but in the final analysis any attempt to isolate TM from the context of its surrounding structure of religious beliefs may be a serious distortion.

RESTRICTED ENVIRONMENTAL STIMULATION (REST)

REST refers to a set of techniques aimed at reducing the level of environmental stimulation to a practicable minimum. The acronym was coined by Suedfeld in 1980, who recommended it as a replacement for sensory deprivation—a term that had gained currency in the 1950s due in large measure to the work of Donald Hebb and his colleagues at McGill University (Shurley, 1992; Zubek, 1969). Suedfeld's suggested change in nomenclature, which was rapidly and widely adopted by other investigators in the area, made sense for several reasons. Chief among these is that modern REST techniques seek to drastically reduce or restrict environmental stimulation, and the McGill group's studies had relied on procedures that typically, and ironically, produced high levels of monotonous stimulation. As noted by Adams (1990), the earlier studies entailed neither silence nor darkness—defining characteristics of contemporary REST research—but rather brightly lit rooms, into which was piped low to moderate levels of unvarying white noise. Subjects were confined to these quarters for a protracted period (several days being the norm), during which time they lay on a bed while wearing a translucent mask over their eyes (to prevent the perception of clear, meaningful visual patterns) and cardboard cuffs or gloves on their hands (to reduce tactile stimulation).

Unsurprisingly, the psychological impact of these methods was inimical: hallucinations, stress reactions, and signs of thought disorder were common among participants in the McGill project (see Solomon et al., 1961). Subsequent studies, however, suggested that these outcomes may have been produced, at least in part, by expectations derived from the demand characteristics of the sensory deprivation situation (M. Barabasz and Barabasz, 1987; Suedfeld 1969). In any event, more recent research involving restricted—rather than monotonous—stimulation has frequently failed to replicate the negative effects reported earlier. To the contrary, this research has revealed that most people find restricted environmental stimulation to be an interesting, pleasant, and relaxing experience and that REST techniques may aid in the treatment of a broad spectrum of clinical disorders, particularly those related to stress or addictive behaviors (see Suedfeld and Borrie, 1993; Suedfeld and Kristeller, 1982). This suggests that REST, far from being a laboratory model of psychosis, may be used to enhance human performance in a number of domains.

In current use, REST typically involves two different procedures. In chamber REST, participants sit or lie on a bed in a completely dark and soundproofed room; a chemical toilet is provided for the subject's comfort and convenience, and both food and water are available on an as-needed basis. In flotation REST, participants are immersed in a quiet, dark tank of water warmed to body temperature and saturated with Epsom salts. (For hygienic reasons, people who participate in a "wet float" procedure are required to shower before as well as after they float, and the tank water is continuously circulated and purified. To simplify matters, some investigators prefer the "dry float" method, whereby subjects lie on a plastic liner that separates them from the water.) The typical chamber session lasts 24 hours, the typical tank session lasts 60 minutes. In either situation, hallucinations, stress reactions, or other adverse effects are rare, unless subjects are given to expect them.

Whether in a chamber or in a tank, the principal application of REST has been as an adjunctive treatment in medically or psychologically oriented therapy, especially for habit control and stress management. Evidence from several studies (reviewed by Suedfeld, 1990) suggests that compared to controls (selected from waiting lists for REST), 24 hours of chamber REST (regardless of whether or not it was accompanied by persuasive messages) both decreased the amount of smoking and increased the rate of abstinence as assessed 6 to 24 months after the single-session treatment. Similarly, 5 hours of chamber REST, during which snake-phobic patients were presented with photographic slides depicting snakes, was demonstrated to decrease behavioral avoidance of an actual, live snake more than did presentation of the same slides without REST (Suedfeld and Hare, 1977). Other clinical or health-related issues to which REST has been brought to bear include alcohol intake (Cooper et al., 1988), essential hypertension (Turner et al., 1987), and rheumatoid pain (Mereday et al., 1990).

In comparison with the body of research concerning the clinical and health applications of restricted stimulation, the literature on REST and the enhancement of human performance is much smaller and less well developed. For the purposes of this section, two aspects of this literature are of chief concern: effects of REST on memory and cognitive tasks and on psychomotor or athletic skills.

In an early experiment related to the effects of restricted stimulation on memory, problem solving, and other cognitive processes, Grissom (1966) read subjects a one-page passage from Tolstoy's *War and Peace* shortly after they had entered a chamber REST environment. Retention of the passage (scored in terms of verbatim recall) was assessed both immediately (as the subjects knew it would be) and either 8, 16, 20, or 24 hours later (which came as a surprise). During the intervening period, subjects either went about their normal daily affairs or they remained in the chamber (i.e., control versus REST conditions). The subjects who had stayed in the chamber forgot significantly less of what they had recalled initially than those who had left the chamber (as measured by the signed difference between immediate and delayed recall performance). The advantage of REST over control conditions was graded, in that it was strongest after 24 hours and weakest after 8.

Technically speaking, Grissom's results could be—and, indeed, have been (Suedfeld, 1980)—interpreted as evidence that REST enhances memory performance. It should be recognized, however, that these results are neither surprising nor do they imply anything special about REST. According to classic interference theory, the introduction of an "altered stimulating condition" (McGeogh and Irion, 1952) between the occasions of information acquisition and delayed retention testing decreases retroactive interference and thereby increases memory performance. This is just what Grissom found, as did Jenkins and Dallenbach (1924) long ago, in a study in which natural sleep served as the altered stimulating condition and as did Parker et al. (1981), in a more recent study involving acute alcoholic intoxication.

A potentially more interesting and informative approach to understanding the cognitive effects of REST concerns the idea that under restricted environmental circumstances patterns of thought become more flexible and inwardly focused. This idea derives from basic research indicating that REST reduces a person's resistance to counter-attitudinal information (Suedfeld, 1980) and provides the rationale for clinical interventions aimed at "unfreezing" the rigid structure of thoughts, emotions, motivations, and behaviors that support addictive habits, such as smoking (Suedfeld and Borrie, 1993). If this is indeed so, one might expect REST to promote problem solving, creativity, or other forms of "higher" cognitive functions. However, empirical investigations of this issue have yielded results that appear to be inconsistent, even contradictory.

On one side are studies showing that REST typically impairs the performance of cognitive tasks (e.g., verbal fluency or divergent thinking) that are "complex" in that they "require the solver to combine dimensions flexibly and to use unfamiliar procedures (frequently in combination); allow for a number of possible methods of reaching a solution; and have an open-ended or vague definition of the goal point" (Suedfeld et al., 1983:729). On the other side is a study by Suedfeld et al. 1987 (see also Metcalfe and Suedfeld, 1990) in which seven psychology professors each spent six, 90-minute sessions sitting alone in their office and six, 1-hour sessions in a wet-REST environment. During the office sessions and for 30 minutes after each float, subjects dictated ideas related to their research into a tape recorder. The recordings generated from each session were transcribed and returned to the subjects approximately 3 months after their final session. Subjects were asked to identify distinct "idea units" in their transcripts; indicate whether a given idea was novel or whether it was one they had pondered prior to the study; and rate the quality and creativity of each distinct thought. The ideas generated shortly after REST sessions were rated as being more novel and more creative than those developed during the office sessions. Along with this finding was a somewhat contrary subjective result: five of the seven subjects believed that REST either had no effect or a deleterious influence on the creativity of their ideas.

Although the authors interpreted these results as evidence for an enhancement of scientific creativity through REST, they were cognizant of an obvious methodological shortcoming, namely, that the novelty or creativity of a given idea was assessed by the subjects themselves. Whether the subjects' own ratings would square with those made by a disinterested third-party expert is an important—but regrettably unanswerable—question. For argument's sake, assume that those results are real and not simply a reflection of subjective bias or expectation effects (an assumption that is strengthened by the subjective reactions of the professors). How, then, does one reconcile the negative influence of REST on the performance of complex problem solving or reasoning tasks with its evidently positive effects on creativity?

One possible explanation of the contradictory findings relates to whether or not subjects are constrained to channel their cognitive resources on problems or ideas that are not of their own choosing. Although the deep sense of relaxation most people experience during REST (especially of the flotation variety) is partly attributable to pleasant somatic sensations (warmth, weightlessness, etc.), it is also due to the pleasant psychological realization that solitude has its own rewards. For as long as the REST session lasts, there are no telephones to answer, no errands to run, no outside demands to deal with. A person is thus free to focus inward and to devote attention to issues and problems of personal significance. Under such circumstances, the requirement to perform a standardized, research-oriented task of reasoning or problem solving (e.g., "unscramble the anagram ACHENEN" or "list as many words as possible that begin with K") is apt to be regarded by a subject as an intrusion rather than a challenge, with poor performance being the predictable outcome. Just how real or remote this possibility is remains to be seen through future studies of the relationship of REST and cognition.

Keyed by case studies and anecdotal reports (e.g., Hutchinson, 1984; Stanley et al., 1987), a number of REST researchers have recently begun to explore the second aspect of performance enhancement we are considering, psychomotor or athletic skills. In the first controlled study of this subject, Lee and Hewitt (1987) randomly assigned 36 female gymnasts (ranging in skill level from novice to intermediate) to one of three conditions: visual imagery practiced in a floatation tank, visual imagery practiced while on a mat, or a no-treatment control. Subjects in either the REST or mat conditions participated in six, 40-minute sessions held once a week. During each session, the subjects listened to a tape containing both relaxation suggestions and guided imagery instructions for visualizing various gymnastic routines. Every subject later participated in three state gymnastic meets, where her performance was scored by meet judges who did not know the condition assignments. Performance scores averaged across these three meets, and the subject's responses to a checklist of physical symptoms or complaints administered at the conclusion of the study, served as the dependent measures. The results showed that subjects given the imagery-plus-REST treatment attained a significantly higher performance score (regardless of their skill level) than either their control- or mat-condition counterparts, and they also reported marginally fewer physical complaints (provided they were intermediate rather than novice gymnasts).

In a conceptually related study, McAleney et al. (1991) examined the effects of REST-plus-imagery on the competitive performance of expert intercollegiate tennis players. Twenty varsity tennis players (at a Pac-10 university) mentally practiced a variety of shot-making skills (suggested to them through audio tape) either while seated in a well-lit room or while floating in a REST tank. Six, 50-minute treatments (REST-plus-imagery or imagery alone) were administered over the course of 3 weeks. At the end of the period, every participant played against a competitor who had been matched for ability by the team's coaches at a tournament prior to the experiment. Videotapes of the (posttreatment) matches were analyzed by blind raters and scored with respect to first-service performance (aces, services in play, or faults); key shots (winners, forced errors, or unforced errors); and the number of points won during the first 50 points of the match. Of the various dependent measures so derived, only one—the number of first-service winners—revealed a statistically significant advantage of REST-plus-imagery over imagery-alone conditions.

Two studies have to date been reported that deal with another form of athletic performance, basketball. In one of these studies (Suedfeld and Bruno, 1990), 30 university students (either occasional or novice basketball players) attempted 20 free throws one day before and one day after a single, 60-minute session. During the session they listened to a tape recording guiding them through multisensory (e.g., visual, tactile, proprioceptive) imagery of basketball foul shooting while floating in a REST tank, while reclining in a comfortable lounge chair, or while seated in a large, egg-shaped "alpha chair" designed to induce relaxation and improve concentration. Subjects practiced shooting free throws in their mind's eye. As one might expect, there were no appreciable differences among conditions in pretreatment shot-making success. On posttreatment, however, REST subjects made significantly more baskets (mean of 57%) than did subjects who had sat in either the alpha chair or in the more conventional recliner (means of 36% and 32%, respectively).

In the second study, Wagaman et al. (1991) asked 22 varsity basketball players to imagine themselves shooting, passing, and dribbling with precision in six separate sessions, spread over a 5-week interval. Every subject completed these sessions in either a lighted office or a flotation REST tank and subsequently participated in five regularly scheduled intercollegiate contests. Performance during these games was assessed by means of both an objective composite score (indexing the difference between, say, points scored and passes completed versus travelling violations and personal fouls) and coaches' subjective ratings.[9] In comparison with the control subjects, REST subjects achieved a significantly higher composite score, and were rated by their coaches as being better in terms of passing and shooting, but not in terms of dribbling, defense, or all-around ability.

In each of the four studies just noted, REST was always applied in tandem with imagery training. Consequently, as Suedfeld et al. (1993:153) have commented, these studies cannot answer the key question of whether REST "merely potentiates the effects of imagery, interacts synergistically with it, or is itself responsible for all or most of the effect." To address this question, Suedfeld et al. (1993) conducted an experiment in which they independently varied flotation REST and an imagery training and relaxation script as techniques for improving accuracy among 40 novice, intermediate, and expert dart players. Results indicated that a single, 1-hour session of REST by itself, and REST combined with the imagery/relaxation script, were equally effective in enhancing performance: shots on target increased by about 12 percent from pre- to posttreatment, irrespective of skill level. In contrast, the script alone and a no-treatment control condition produced no significant change in test-retest measures. Similar results have recently been reported by A. Barabasz et al. (1993) in their study of 24 students enrolled in a rifle marksmanship training course. The students were randomly assigned to one of two treatments: either 50 minutes of dry-float REST or an equal period of hypnotically suggested relaxation. (Neither the REST nor the relaxation subjects had been given any form of guided imagery training.) The REST subjects outperformed the relaxation subjects in a subsequent test of rifle-shot accuracy, as scored by ROTC instructors (who were blind to the experimental condition).

Considered collectively, the research to date provides suggestive evidence that REST—either alone or in combination with guided imagery/relaxation training—may enhance the performance of a variety of athletic or sporting skills. This evidence, however, cannot be construed as compelling, for several reasons. At present, the research consists of only six published papers (three of which appeared in nonrefereed journals), each reporting a single study involving a modest sample size. Moreover, in studies entailing multiple measures of performance (McAleney et al., 1991; Wagaman et al., 1991), it is not uncommon to find positive effects with certain measures and no effects with others. Whether such a mixture of outcomes reflects theoretically important and empirically principled dissociations among various performance indices, differences in measurement sensitivity, or merely the occurrence of type-I errors[10] is a difficult but important issue that remains to be resolved (see below).

Given these considerations, we believe that what is needed now is not just more research on the performance enhancing effects of REST but also better research—integrated, tightly reasoned investigations, organized in multi-experiment reports. Such investigations will need to address a long list of issues, none of which is news to researchers already active in the area, but which might appeal to the editors and reviewers of high-profile journals and attract new researchers to the field. For example, how long do the performance-enhancing effects of REST last? Are these effects unique to dry and wet flotation, or can they also be elicited through chamber REST?[11] What aspects of athletic performance are most amenable to improvement through REST?

One hypothesis, advanced by Suedfeld et al. (1993:153), is that skills that "require relatively low arousal and a full measure of control over a complex coordinated movement" are more apt to benefit from REST that are "activities that overwhelmingly emphasize brief bursts of speed or strength, or quick changes in motion or attention in response to the acts of other competitors." Although the implications of this idea need to be worked out and explored, the hypothesis does provide a plausible explanation for at least some of the mixed results reported, such as the observation of McAleney et al. (1991) that REST-plus-imagery improved first-service accuracy but did not effect either key shot success or points won—measures of performance that are more reflective of reaction. Finally, and most critically from a theoretical standpoint, how and why does REST enhance skilled performance? Do REST-related enhancements signify nothing more than expectancy or placebo effects? (The fact that Suedfeld and Bruno [1990] observed no intergroup differences in subjects' expectations of improvement that were solicited before treatment suggests otherwise.) Alternatively, is it the case that REST "potentiates internally generated imaginal activity (subjects' spontaneous imagining) and that such activity can be reactivated, at will, sometime after the REST experience despite the intrusion of normal levels of stimulation," as A. Barabasz et al. (1993:871) contend? Or might the key lie not in the reactivation of imaginal activity, but rather in the realization of profound levels of relaxation that were previously attainable only in the restricted sensory environment (see Suedfeld et al., 1993). Answers to these kinds of questions are essential if the promise of REST as a technique for enhancing human performance is to be turned into established fact.

SLEEP LEARNING

Sleep is universally recognized as an altered state of consciousness. Especially in some stages of sleep, an individual appears unresponsive to exogenous stimuli (unless they are very intense); at the same time, he or she may experience dreams, nightmares, and other endogenous mental events (although they are usually quickly forgotten upon awakening). The committee's first report, *Enhancing Human Performance* (Druckman and Swets, 1988), raised the possibility that sleepers, while appearing oblivious to environmental events, may nonetheless be able to process environmental events to some degree and retain them in memory after awakening. For example, a study of World War I military recruits by Thurstone seemed to indicate that learning of Morse code was facilitated by presenting lessons at night, when the soldiers were presumably asleep, as well as during regular daytime classes (Simon and Emmons, 1955). Reports of successful sleep learning also filtered out of the former Soviet Union and countries of Eastern Europe during the Cold War. Yet, most formal studies of sleep learning have yielded negative results, and most instances of positive findings were either anecdotal in nature or marred by the absence of proper controls or inadequate psychophysiological monitoring of sleep (for reviews, see Aarons, 1977; Eich, 1990).

Sleep learning has remained an open question for two reasons. First, a series of dramatic experiments by Evans and his associates (reviewed by Evans, 1990), appeared to show that some subjects could respond discriminatively to hypnosis-like suggestions for motor activity while remaining asleep, show an amnesia for these responses on awakening, and continue to respond on subsequent nights without further administration of the suggestions. These suggestion-induced changes in behavior qualify as learning. Second, recent studies of the amnesic syndrome and other disorders of memory support a distinction between explicit memory, or conscious recollection, and implicit memory, in which task performance is affected by past events even though subjects do not remember them (for reviews, see Schacter, 1987, 1992). If brain-injured and anesthetized patients can show evidence of implicit memory, it seems plausible that sleeping subjects might also do so.

The studies of sleep suggestion were performed by Evans and his associates (Evans et al., 1969, 1970; Perry et al., 1978). In the earlier studies (Evans et al., 1969, 1970), sleeping subjects received suggestions during the stage of sleep known as REM (rapid eye movement) that they would scratch their noses when they heard the word "*itch*" and adjust their pillows when they heard the word "*pillow*". Testing in the same or subsequent sleep stages revealed that subjects gave appropriate responses to between 14 and 20 percent of the cue words. Positive responses persisted, to at least some degree, over five subsequent nights without any repetition of the suggestion—and, for a subgroup of subjects who could be retested, some 5 months later as well. When interviewed after waking, however, the subjects had no awareness that they had received such cues or responded to them. Because such responses require perception of the cue, and their carryover to subsequent sleep stages and nights requires memory, this study makes a prima facie case for the acquisition and retention of memories during sleep, memories that are expressed implicitly, in response to the cue, rather than explicitly as conscious recollections of experience. Unfortunately, this study was beset by a number of flaws (for a detailed critique, see Wood, 1989). There was no control group to provide baseline information on nose-scratching and pillow-adjusting, and these behaviors were evaluated by a judge who was aware of the suggestions that the subjects had received. Perhaps most important, however, the investigators failed to follow conventional procedures for sleep staging. According to standardized criteria, stage REM is indicated by three criteria: high-frequency, low-amplitude beta activity in the electroencephalogram (EEG), with no low-frequency, high amplitude alpha activity (the latter indicative of cognitive arousal); rapid eye movements (REM) in the electrooculogram (EOG); and absence of submentalis muscle tone in the electromyogram (EMG). Evans et al. recorded only EEG and EOG, which makes it extremely difficult to discriminate stage REM sleep from waking. This last point is crucial, because motor activity is incompatible with stage REM: in other words, a subject who is in stage REM cannot physically respond to instructions for motor activity such as scratching one's nose and adjusting one's pillow. In recognition of this fact, Evans et al. referred to "Stage 1" sleep with and without REM. But the fact remains that on the psychophysiological evidence, the subjects who responded to the sleep suggestion might well have been at least partially awake. If so, then the studies of sleep suggestion do not count as evidence for sleep learning after all.

In an attempt to correct these problems, Perry et al. (1978) attempted a replication of the earlier study by Evans et al. (1969, 1970), but with tighter controls. A within-subjects control presented subjects with cues, like leg and blanket, for which no suggestions had been given, but to which appropriate responses were recorded; moreover, all responses were recorded on video tape (by means of an infrared camera), and evaluated by judges who were blind to the suggestions that had been given. Unfortunately, Perry et al. (1978) did not consider EMG criteria in the staging of sleep. In the final analysis, however, there proved to be no difference in response rate to critical and control cues. Although some degree of nose-scratching and pillow-adjusting was observed in response to suggestions, the levels of these activities did not differ from leg-moving and blanket-pulling. Thus, regardless of questions about the appropriateness of the stage of sleep and whether the subjects might have been at least partially awake during the tests, there was no evidence of differential responsiveness to the cues, and thus no evidence of implicit perception or memory during sleep.

In light of these findings, Wood and his colleagues (Wood et al., 1992) conducted the first formal comparison of explicit and implicit memory for information presented during sleep. In one test, subjects received presentations, during either stage REM or stage 2 (early non-REM sleep) of paired associates consisting of a homophone (e.g., hare, hair) and a context word (e.g., tortoise). In the other test, the paired associates consisted of a word and its category label (e.g., metal-gold). In each case, subjects received approximately five presentations of the list. The stage of sleep was measured by standard criteria, including EMG as well as EEG and EOG, and presentation of the list was interrupted as soon as the subject showed any signs of arousal (e.g., the appearance of alpha activity in the record). A control group heard the same lists while lying awake in a darkened room. Ten minutes after presentation of each list, the subjects were awakened (if they were sleeping). Those in the normal waking state showed clear priming effects, in that they were more likely to spell previously presented homophones in accordance with the context word (see Eich, 1984) and to generate category instances that had been presented earlier (see Kihlstrom, 1980). However, those who had slept during the stimulus presentations showed no evidence of priming.

Given the problems in the studies by Evans et al. (1969, 1970), and the negative results of Wood et al. (1992), it is difficult to be sanguine about the possibility of learning during sleep and retaining even implicit memories in a subsequent waking state. However, it is possible that the sleep-staging criteria employed by Wood et al. (1992) were too strict. That is, some evidence of sleep learning might be obtained in experiments that allow some level of cortical arousal in the subjects—conditions that approximate Thurstone's early study of Morse code. Subjects who are partially aroused during stimulus presentations might well show implicit memory for such events, in the absence of explicit memory. Wood (1989) has characterized this as "quasi sleep learning," because the person is aroused to some degree while encoding the information. This proposal is consistent with the suggestion made in Druckman and Swets (1988) that individuals might profit from "dynamic sleep-learning procedures" in which presentations are timed to coincide with periods when sleepers are relatively aroused (see Eich, 1990). Finally, even without cortical arousal, it is possible that some forms of implicit memory, such as repetition priming, might be preserved even when input takes place while a person is unambiguously asleep. The study by Wood et al. (1992) appears to rule out semantic priming, based on associative or conceptual relationships between the items presented during sleep. But it does not rule out repetition priming, or other forms of implicit memory that are mediated by presemantic, perceptual representations of stimulus input.

Although some knowledge might be acquired by means of quasi-sleep learning and expressed implicitly if not explicitly in subsequent waking life, any such effects might be offset by two costs. First, there is no reason to believe that sleep learning, even if it is possible, is anywhere near as efficient as learning in the normal waking state. Sleeping subjects, and those who are on the margins of wakefulness, may be unable to perform the elaborative and organizational activity necessary for good learning. However, to the extent that material is encoded at all during those "twilight" states, the principle of encoding specificity in memory might operate so as to make such material especially accessible when the person is sleep-deprived or exhausted. Second, quasi sleep learning effectively deprives a subject of sleep, so acquiring information during sleep may produce detrimental effects on performance the next day. However, dynamic sleep-learning procedures may make it possible to present information without arousing a person from the deepest, most restorative stages of sleep. For the present, the possibility of sleep learning, expressed in implicit memory during states of wakefulness, or in a state-dependent fashion in states of sleepiness, deserves further investigation. But any discussion of the benefits of sleep learning should include a comparative evaluation of the costs to effective and efficient performance in the normal waking state.

CONCLUSIONS

Consciousness may be altered by increasing a person's awareness of the surrounding world, decreasing that awareness, or changing the contents of that awareness. Thus, hypnotized subjects may believe that the world is as it is suggested by the hypnotist; a sleeper appears oblivious to events in the surrounding environment; restricted environmental stimulation interrupts the normal flow of sensory-motor activity; and transcendental meditation may induce a state of alert but content-free "pure consciousness." Claims have been made for the performance-enhancing qualities of each of these states, but a critical review of the available literature indicates that most of these claims are unsupported by scientific data. That is, either the results have been negative, or positive results have been contaminated by the lack of certain critical controls.

To the extent that performance is impaired by subjective feelings of pain and fatigue, hypnosis can enhance performance by reducing a subject's awareness of these potentially demoralizing conditions. This possibility is limited by the role of hypnotizability in moderating the effects of hypnotic suggestion: not everyone is hypnotizable enough to experience this effect. However, even individuals who are not hypnotizable may receive some benefit from the placebo component in hypnotic analgesia or from training in nonhypnotic stress inoculation.

By and large, direct hypnotic suggestions for enhanced performance have no effect on muscular strength and endurance, sensory thresholds, learning, and memory retrieval. Hypnotized subjects may *believe* that they are doing better, and this belief may have positive motivational properties, but the subjective experience of performance enhancement appears to be illusory.

Transcendental meditation (TM) has been offered as a means of enhancing performance, chiefly by reducing the deleterious effects of stress. Although TM has generated a voluminous body of research, the available studies suffer from a variety of methodological flaws that preclude firm conclusions. For example, it is not clear whether the positive effects observed in TM are due to the specific effects of the unique features of TM or to the frequency and discipline with which TM is practiced.

Restricted environmental stimulation (REST) has been offered as a technique for enhancing human performance, but most of the evidence supporting this proposal is based on the proven therapeutic effects of REST in controlling habit behaviors. There is some anecdotal evidence of the performance-enhancing effects of REST, and a few formal studies, but not enough for firm conclusions about the effects, if any, and their underlying mechanisms.

Although sleep learning is ineffective when measured in terms of an individual's ability to consciously remember material presented during sleep, the committee's last report raised the possibility that sleep learning could be expressed as implicit memory, in the absence of explicit recollection: more recent evidence indicates that this is not the case. Some degree of quasi sleep learning may be possible, but if so it is both likely to be inefficient and to have detrimental effects on a person's subsequent waking performance.

NOTES

[1]Although a variety of slightly different terms can be used to characterize an individual's "hypnotizability," we use the terms "hypnotizable" and "insusceptible" to characterize those who can and cannot be hypnotized.

[2]Hypnosis might share features in common with some of the other effects. It has been suggested that acupuncture is just a peculiarly Chinese form of hypnosis, or that hypnotic analgesia is just a placebo. If this were so, we would expect that response to acupuncture, or placebo, would be a function of hypnotizability. But they are not. The dissociation between hypnosis and acupuncture, or between hypnosis and placebo, the former mediated by hypnotizability but the latter not, shows that hypnosis is different from acupuncture or placebo.

[3]This study used a variant on the London-Fuhrer paradigm (discussed below); for a detailed secondary analysis, see Hilgard and Hilgard (1975, 1983); for a substantial replication, see Spanos et al. (1989).

[4]This research is independent of the literature, reviewed above, on the effects of hypnotically suggested time-distortion on memory.

[5]There is no evidence that a subject in a state of age regression to childhood loses access to his or her adult knowledge and abilities (O'Connell et al. 1970; Orne, 1951; Perry and Walsh, 1978).

[6]Hypnosis is clearly established as a potentially efficacious treatment modality in medicine and psychotherapy (American Medical Association, 1958), particularly for the relief of pain, but the general consensus within the field is that hypnotically refreshed memory is inherently unreliable (American Medical Association, 1985).

[7]The TM-Sidhi program, an extension of conventional TM technique that is available to highly experienced practitioners, is held to permit individuals to enhance sensory thresholds, perceive hidden objects, achieve direct awareness of past and future, become invisible, and levitate; for the adept, it also enhances feelings of inner peace, friendliness, and compassion. This brief review focuses entirely on standard TM and does not address the claims of the TM-Sidhi program.

[8]Similarly, the rationale for the TM-Sidhi program makes use of concepts in quantum mechanics.

[9]The coaches did not know the experimental conditions of the players.

[10]This occurs when an hypothesis of no difference between a treatment and a control condition is *rejected* when in fact it is true. (A type-II error, on the other hand, occurs when an hypothesis of no difference is *accepted* when in fact it is false.)

[11]In this regard, it merits mentioning that the positive finding noted above for REST as an effective method of smoking cessation was true for chamber—but not flotation (see Suedfeld, 1990).

Learning, Remembering, Believing

ENHANCING HUMAN PERFORMANCE

Daniel Druckman and Robert A. Bjork, *Editors*

Committee on Techniques for the Enhancement of Human Performance

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