CHAPTER 2

The Emotional Unconscious

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One of the earliest marks of the cognitive revolution in psychology was a revival of interest in consciousness, rather than behavior. The cognitive psychology that emerged immediately after World War II to replace functional behaviorism focused on the span of apprehension, primary memory, attention, and imagery. Now, as we approach the twenty-first century, cognitive psychology has begun to seriously explore *unconscious* mental life and the *psychological unconscious*: the idea that conscious experience, thought, and action is influenced by percepts, memories, and other mental states that are inaccessible to phenomenal awareness and somehow independent of voluntary control.

THE COGNITIVE UNCONSCIOUS AND BEYOND

In the modern history of cognitive psychology, one can discern four early stages in the conceptualization of the psychological unconscious (Kihlstrom, 1995b, 1999). The first of these might be called the *wastebasket view*: that the unconscious includes unattended and unrehearsed events and memories lost through decay or displacement. There is also the *file cabinet view*: that the unconscious includes passively stored memories that must be actively retrieved into short-term or working memory in order to play any role in cognitive processing. Neither view allows dynamically active unconscious percepts, thoughts, and memories. Thus, they do not really satisfy the definition of the psychological unconscious. The conscious takes a more active role in *preattentive processing*, which holds that stimulus events are subject to unconscious processes of feature

detection, pattern recognition, and the like before conscious attention is directed to them. Finally, an even more active role is suggested by *automaticity*: the notion that some cognitive and motoric skills, once executed deliberately, may become automatized through extensive practice, after which we have no direct introspective access to these procedures or their operations. These ideas include unconscious processes in cognition, but they imply that the psychological unconscious is limited to mental processes and that the contents on which these processes operate are conscious.

Implicit Memory

The idea that mental *contents* might be unconscious, not just the processes that operate on them, is commonly associated with psychoanalytic theory. But as Ellenberger (1970) has shown, ideas about unconscious percepts and memories extend far beyond Breuer and Freud's (1893–1895/1957) studies of hysteria. Contemporary psychology has revived the distinction between explicit and implicit memory (Schacter, 1987). *Explicit memory* refers to the conscious recollection of some past event, as in recall and recognition. By contrast, *implicit memory* refers to any effect of a past event on a person's ongoing experience, thought, and action, regardless of whether that event can be consciously remembered.

Perhaps the most dramatic demonstration of the difference between explicit and implicit memory comes from studies of patients who have suffered bilateral damage to the medial temporal lobes (including the hippocampus) or the diencephalon (including the mammillary bodies). These patients show a gross anterograde amnesia; they generally cannot recall or recognize words that have been presented to them for study. But when asked to perform other tasks, they frequently reveal the preservation of some sort of memory.

The first investigators to document this anomaly under controlled conditions were Warrington and Weiskrantz (1968), who found relatively normal retention when the patients were tested with fragments or stems of list items. Thus, after studying a word like *elastic*, amnesics will be unable to recall or recognize it; but when asked to name a word, any word, that starts with *ela*, they are much more apt to say *elastic* than chance alone would predict. This phenomenon, known as a *priming effect*, has been demonstrated many times. When amnesic patients who cannot remember words from a study list are later given an opportunity to use those words in another sort of task, they use previously studied items more frequently than unstudied neutral items—just as neurologically intact controls do in similar tasks.

Dissociations between explicit and implicit memory can be observed in other forms of amnesia as well, including posthypnotic amnesia (Dorfman & Kihlstrom, 1994; Kihlstrom, 1980, 1985), surgical anesthesia (Cork, Couture, &

Kihlstrom, 1997; Kihlstrom, 1993b; Kihlstrom & Schacter, 1990; Merikle & Daneman, 1996), and the amnesias associated with electroconvulsive therapy (Dorfman, Kihlstrom, Cork, & Misiaszek, 1995; Squire, Shimamura, & Graf, 1985) and multiple personality disorder (Eich, Macaulay, Loewenstein, & Dihle, 1997; Kihlstrom & Schacter, 1995; Kihlstrom, Tataryn, & Hoyt, 1993). So the effect is quite commonly observed. Implicit memory is also spared in normal aging, which has deleterious effects on explicit memory (e.g., Light & Singh, 1987; Light, Singh, & Capps, 1986; for a review, see Schacter, Kihlstrom, Kaszniak, & Valdiserri, 1993).

Dissociations between explicit and implicit memory can be observed even in normal subjects under normal laboratory conditions (for a review, see Roediger & McDermott, 1993). Thus, subjects can show savings in relearning items that they can neither recall nor recognize from a previous study trial (Nelson, 1978). Priming effects are unaffected by variables such as depth of processing during encoding that profoundly affect conscious recollection (e.g., Jacoby & Dallas, 1981). The study of implicit memory in normal subjects, especially as represented by repetition priming, is now a major enterprise within cognitive psychology (for reviews, see Graf & Masson, 1993; Lewandowsky, Dunn, & Kirsner, 1989; Roediger & McDermott, 1993). At the same time, the explicit/implicit distinction has also been extended to other domains of cognition (Kihlstrom, 1987, 1990, 1999).

Implicit Learning

Perhaps the most familiar of these extensions (and one that actually predates the explicit/implicit distinction in memory) is the phenomenon of *implicit learning*, or one's acquisition of new patterns of behavior without being aware of the patterns themselves (for reviews, see Adams, 1957; Berry & Dienes, 1993; Dienes & Berry, 1997; Kihlstrom, 1996a; Neal & Hesketh, 1997; Reber, 1993; Seger, 1994). Implicit learning has been demonstrated by Reber's (1967) work on artificial grammars, in which subjects exposed to letter strings generated according to a complex rule system can classify new instances appropriately yet cannot articulate the rules that define the category. Lewicki (1986) has reported conceptually similar experiments involving social categorization and judgment. Similarly, experiments on the control of complex systems show that subjects can learn to manipulate inputs in order to control outputs, without being able to articulate the relations between them (e.g., Berry & Broadbent, 1984). Finally, subjects apparently can acquire the ability to predict forthcoming events even while they cannot specify the underlying sequential structure they have obviously learned (e.g., Lewicki, Czyzewska, & Hoffman, 1987; Nissen & Bullemer, 1987). Razran (1961) reported that subjects could acquire interoceptive conditioned responses without being aware of either the conditioned or unconditioned

stimuli (also see Papka, Ivry, & Woodruff-Pak, 1996). In all these cases, and others, subjects have apparently learned without being aware of what they have learned.

Implicit memory has been accepted, but claims for implicit learning have been highly controversial (Adams, 1957; Dulany, 1997; Shanks & St. John, 1994). Reber (1967, 1993) has defined implicit learning as abstract, automatic, and unconscious; critics have seriously questioned all three assertions. Though questions concerning automaticity and representation are interesting, in the current context only awareness is relevant. We admit that the available evidence is not completely satisfying. For example, although most subjects in artificial grammar learning cannot articulate the entire Markov process by which the grammatical strings have been generated (Reber, 1967), above-chance classification performance could result from consciously accessible knowledge of legal letters, letter positions, or bigrams. Perhaps the best positive evidence is that the subjects believe they are behaving randomly. In artificial grammar learning, the relationship between accuracy and confidence is very weak (Dienes, Altman, Kwan, & Goode, 1995), whereas in sequence learning subjects rate their accurate predictions as guesses (Willingham, Greeley, & Bardone, 1993).

Implicit Perception

A somewhat more recent extension of the explicit/implicit distinction is to perception. By analogy to implicit memory, we can define *implicit perception* in terms of the effects of a *current* event (or an event in the very recent past) on one's performance, in the absence of conscious perception of that event (Kihlstrom, 1996a; Kihlstrom, Barnhardt, & Tataryn, 1992a). Implicit perception includes *subliminal perception*, also known as *preconscious processing* or *detectionless processing*, as illustrated by priming effects involving weak, brief, or masked stimulus presentations (e.g., Greenwald, Klinger, & Liu, 1989; Marcel, 1983; Merikle & Reingold, 1990; Pierce & Jastrow, 1885). It also covers priming effects observed in experiments involving parafoveal vision (e.g., Bargh & Pietromonaco, 1982; Underwood, 1976) and dichotic listening (Eich, 1984). In each case, the priming is produced by stimuli that can be construed as degraded below the level required for conscious perception.

During the recent history of psychology, claims for subliminal perception have been very controversial (e.g., Bruner & Klein, 1960; Dixon, 1971, 1981; Eriksen, 1960; Holender, 1986; Shanks & St. John, 1994), but the effect has now been demonstrated under conditions that should satisfy all but the most incorrigible critics (Greenwald et al., 1989; Greenwald, Draine, & Abrams, 1996; Greenwald, Klinger, & Schuh, 1995). However, truly subliminal processing seems to be analytically limited to perceptual, simple semantic analyses (Greenwald, 1992; Kihlstrom, 1993a, 1996a).

However, subliminal perception does not exhaust the category of implicit perception effects because priming and similar effects can be observed under conditions not easily classified as subliminal. A neurological case in point is *blindsight* (Weiskrantz, 1986): patient D.B. can make accurate judgments about the visual properties of objects he cannot see. Another example is *neglect* resulting from temporoparietal damage (Bisiach, 1993; Rafal, 1998): in at least some cases, the patient's behavior and judgments can be influenced by stimuli presented in the neglected portion of the visual field (Marshall & Halligan, 1988). Similar effects are observed in the functional anesthesias, such as "hysterical" blindness, associated with conversion disorder (Bryant & McConkey, 1989b) and in some phenomena of hypnosis, such as suggested blindness (Bryant & McConkey, 1989a) and deafness (Spanos, Jones, & Malfara, 1982). In these cases, the stimuli in question are clearly supraliminal, and even though they are not consciously perceived, they clearly influence the subject's experience, thought, and action. For this reason, Kihlstrom et al. (1992a) preferred to use "implicit" rather than "subliminal" perception, to underscore the central role of the subject's phenomenal awareness, as opposed to stimulus properties: the subjects are perceiving yet they are unaware of what they are perceiving. Implicit perception is typically revealed by the same sorts of tasks employed in studies of implicit memory, yet in implicit memory, the events in question were consciously perceived at the time they occurred. On these grounds, then, preserved priming following general anesthesia (Cork et al., 1997) might well be classified as implicit perception rather than implicit memory.

Implicit Thought

After memory, learning, and perception, the catalog of cognition naturally turns to thought. Though William James (1890/1980) characterized unconscious thought as a contradiction in terms, evidence for *implicit thought* is mounting (Dorfman, Shames, & Kihlstrom, 1996; Kihlstrom, Shames, & Dorfman, 1996). Like implicit memory, implicit thought may be said to occur when a thought—for example, the correct solution to a problem—influences experience, thought, or action even though one is unaware of the thought itself. Implicit thoughts may consist of ideas, beliefs, or images—any cognitive content, in fact, that is neither a percept (a representation of a current event) or a memory (a representation of a past event); they appear to be closely associated with the experiences of intuition, incubation, and insight, all hallmarks of creative problem solving.

The notion of implicit thought is exemplified in the research of Bowers and his associates, who showed how the correct solution to a difficult problem can influence choice behavior, even though the subject is not consciously aware of the solution itself (Bowers, 1984; Bowers, Farvolden, & Mermigis, 1995; Bowers, Regehr, Balthazard, & Parker, 1990). Their research involved an adaptation

of the Remote Associates Test (Mednick, 1962), in which subjects are presented with two word triplets, one coherent and one incoherent. In the coherent triplet, the items are all associatively related to a single word (e.g., *playing*, *credit*, *report—card*), whereas in the incoherent triplet, there is no such relation (e.g., *still*, *pages*, *music*). The subjects' task is to inspect both triplets and give the solution to the coherent triplet (the target); if they cannot do so, they indicate only which triplet is coherent. Bowers et al. (1990, experiment 1) found that subjects could discriminate between coherent and incoherent triplets at better than chance levels, even when they could not name the target. They suggested that the subjects' choices reflected information processing outside of phenomenal awareness—something like the accretion of activation spreading from semantic memory nodes representing the elements of the coherent triplet to a node representing the target. In support of this idea, Shames (1994) found that unsolved but coherent triplets primed lexical decisions concerning their respective targets.

Other experiments also show that, even though subjects do not know the correct answer, subjects' intuitions about problems, choices, and judgments are not merely random guesses. For example, Bowers et al. (1990, experiment 2) showed that subjects could discriminate picture fragments that could, when properly assembled, represent familiar objects from those that could not—even though they were unable to tell what the objects were. Durso and his colleagues were able to trace changes in cognitive structure as subjects approached the solution to a problem, even though they had not reached the solution itself (Durso, Rea, & Dayton, 1994). And Bechara and colleagues found that subjects' skin-conductance responses and choice behaviors differed when they were presented with "good" and "bad" decks of cards in a gambling game, even though they could not describe the difference (Bechara, Damasio, Damasio, & Anderson, 1994; Bechara, Damasio, Tranel, & Damasio, 1997; Bechara, Tranel, Damasio, & Damasio, 1996). These results lead to the proposition that thoughts, in the form of ideas, images, biases, and the like, can guide behavior even when we are unaware of them.

THE TRILOGY OF MIND

These four categories of phenomena—memory, learning, perception, and thought—constitute the cognitive unconscious (Kihlstrom, 1987, 1999; Rozin, 1976). But cognition is not all there is to mental life. At least since the nineteenth century, many psychologists and philosophers of mind have divided mental life into three broad faculties, including emotion and motivation as well as cognition (for a review, see Hilgard, 1980). This idea began with Christian Wolfe (1679–1754), who brought the term *psychology* into common use and who classified

the mind into the *facultus cognoscitova* (knowledge) and the *facultus appetiva* (desire). Moses Mendelsohn (1729–1786) added affect (emotion) to the list. The tripartite classification of mental faculties was consolidated by the philosopher Immanuel Kant (1724–1804), who wrote in his *Critique of Judgment* that "there are three absolutely irreducible faculties of the mind, namely, knowledge, feeling, and desire" (Watson, 1888, p. 311). Kant meant that emotion and motivation exist in some sense independently of cognition and of each other. This position contrasts with the idea, still popular in psychology, that emotional and motivational states are cognitive constructions—*beliefs* about what one feels (Schachter & Singer, 1962) or wants (Lepper, Greene, & Nisbett, 1973). Kant's three independent mental faculties reappear in Hilgard's (1980) *trilogy of mind*: cognition, the mental representation of reality through perception, attention, learning, memory, and thought; emotion, the subjective experience of arousal, pleasure, and displeasure, and their expression in behavior; and motivation, the activation of behavior and its direction toward a goal. All three of these mental states affect the determination of behavior.

One usually thinks of the cognitive, emotional, and motivational processes that underlie action in terms of conscious mental states. Aware of what one thinks, feels, and desires, one acts accordingly. But here we have already concluded that cognitive states such as percepts, memories, and thoughts can affect behavior outside of awareness. If cognitive states of perception, memory, and thought can be unconscious, or implicit, can emotional states be unconscious, or implicit, as well?

THE FREUDIAN MODEL

What might implicit emotion look like? Perhaps it resembles Sigmund Freud's vision of unconscious mental life (for a detailed analysis of Freud's theory of psychoanalysis, see Macmillan, 1996). Freud (1916–1917/1963, 1933/1964) asserted that people are affected by emotional or motivational states of which they are not consciously aware. Later, when they reflect on their behavior (or perhaps after they have undergone psychoanalysis), they realize their true feelings and motives. But that insight has the character of an inference, rather than an introspection, and they were never consciously aware of their feelings or desires at the time they acted on them. So the classic Freudian defense mechanisms (A. Freud, 1936/1966; S. Freud, 1926/1959) are all designed to render a person unaware of his or her true emotions. In reaction formation, we profess love but really feel hate; in displacement, we declare hatred of one person, when we really hate another person entirely; in intellectualization and rationalization, our behavior is stripped of all of its emotional connections entirely. Freud claimed

that our behavior manifests our true emotions, even if they were not represented in consciousness.

Suppes and Warren (1975) have proposed a mathematical model of the transformations involved in the Freudian defense mechanisms. They begin with a propositional representation of unconscious affect—of an actor, an action, and an object (x) of the action—as in the prototypic Freudian emotional self-disclosure:

I (actor) *love* (action) *my mother* (object *x*).

They further show that some forty-four different defense mechanisms, including all those included in the standard list, can be produced by just eight transformations applied to the actor, the action, or the object, alone or in combination—for example, the transformation of self to other, of an action into its opposite, or from object *x* to object *y*.

Thus, displacement retains the original actor and action, but changes the object from *x* to *y*: *I love my father.* In reaction formation, the actor and object remain constant, but the action is changed into its opposite: *I hate my mother.* In projection, the action and the object remain constant, but the actor is changed: *Saddam Hussein loves my mother.* Applying all three transformations, we obtain *Saddam Hussein hates my father.* This glib and vulgar Freudianism nicely illustrates the essential process by which the defense mechanisms render the actual emotional and motivational determinants of our behavior inaccessible to phenomenal awareness.

Of course, one need not embrace the whole conceptual panoply of classical (or even neofreudian) psychoanalysis: the division of the mind into id, ego, and superego; the theory of infantile sexuality; the stages of psychosexual development; repression, and so forth. And one certainly need not trace all of one's emotional life to primitive sexual and aggressive instincts. The emotions whose conscious representations pertain can be represented by the everyday concepts of folk psychology, as reflected in the affect circumplex (Russell, 1980; Watson & Tellegen, 1985), Murray's (1938) list of needs, and similar ideas. Significantly, Suppes and Warren (1975) suggest two basic ways in which the emotional unconscious can be expressed: (1) when the original emotion is represented consciously but one is unconscious of the source of that emotion, as in displacement and projection; and (2) when the emotion itself is denied conscious representation, as in reaction formation, intellectualization, and denial.

EMOTION AS AN EXPRESSION OF IMPLICIT COGNITION

In the emotional unconscious, one may be consciously aware of his or her emotional state yet unaware of its source in current or past experience. The

Roman poet and epigrammatist Martial had this concept in mind when he wrote *Epigrammata* (book 1, no. 32, freely translated by the seventeenth-century English satirical poet Thomas Brown; see Hayward, 1927):

Non amo te, Sabidi,	I do not love you Dr. Fell,
Nec possum dicere quare:	but why I cannot tell;
Hoc tantum possum dicere,	But this I know full well,
Non amo te.	I do not love you, Dr. Fell.

Compare Breuer and Freud (1893–1895/1957) in their *Studies on Hysteria*:

> Hysterics suffer mainly from reminiscences. . . . [But in] the great majority of cases it is not possible to establish the point of origin by a simple interrogation of the patient, however thoroughly it may be carried out. . . . principally because he is genuinely unable to recollect it and often has no suspicion of the causal connection between the precipitating event and the pathological phenomenon. (pp. 7, 3)

For Breuer and Freud's unconscious (or repressed) memories, we can substitute implicit ones (for a fuller discussion, see Kihlstrom, 1997b). Thus, a conscious emotional state may serve as an index of implicit perception or memory. Even though one consciously experiences the emotional states themselves, these phenomena of implicit perception and memory deserve to be included in the emotional unconscious.

Emotion as an Expression of Implicit Memory

In a classic demonstration of what we now call spared implicit memory in organic amnesia, Claparede (1911/1951) pricked an unsuspecting Korsakoff's syndrome patient with a pin hidden in his hand and caused her quite a bit of distress. Claparede subsequently left the room and returned after the patient had regained her composure. During questioning, she failed to recognize Claparede and had no recollection of the unfortunate incident. Nevertheless, she refused to shake his hand. When asked why, she replied, "Sometimes people hide pins in their hands." The story illustrates the phenomenon of *source amnesia* familiar in studies of memory (see Kihlstrom, 1995a). But if the prospect of shaking hands made the patient nervous as well, her refusal suggests a dissociation between conscious awareness of an emotional state, which she probably experienced, and conscious recollection of the origins of that state in experience, which she evidently did not.

In a case of hysterical somnambulism reported by Janet (1893, 1904; see Nemiah, 1979), Madame D. suffered a breakdown after some men jokingly deposited her drunken husband on her doorstep and announced that he was dead.

Afterward, the woman had no conscious recollection of the event. But whenever she passed by her front door, she froze with terror. Moreover, she complained of dreams in which her husband was brought home dead. Here again is an example of an emotional state (terror and distress about the dreams) and emotional behavior (freezing, as well as the nightmares) but no awareness of why they occur.

A third example comes from a case of fear of running water reported by Bagby (1928). The patient had no memory of the circumstances under which this intense emotional reaction had been acquired. However, the mystery was solved when the patient was visited by an aunt who said, as an aside, "I have never told." It turned out that when the patient, as a child, had gone on a picnic with the aunt she had disobeyed instructions, strayed into a nearby creek, and become trapped under a waterfall. The child was rescued by the aunt, who promised to keep her transgression a secret. Apparently, the child lost the memory of the incident, perhaps due to a process like repression or dissociation, perhaps merely to childhood amnesia or ordinary forgetting, but the phobia remained solidly entrenched. In this case, the symptom appears to be an implicit memory for an incident lost to explicit memory.

The evidence for emotion as implicit memory is not limited to anecdote (Tobias, Kihlstrom, & Schacter, 1990). Several formal studies also demonstrate that emotional responses can persist even though one does not know how they originated. A study by Johnson, Kim, and Risse (1985) used the *mere exposure effect* on preferences (Zajonc, 1968): repeated exposure to an object tends to increase judgments of likability, even if no substantive information supports such attitudinal change. Johnson and her colleagues exposed Korsakoff patients (who are amnesic as a result of damage to diencephalic structures) and controls to unfamiliar Korean melodies. Some melodies were played only once during the study phase, whereas others were played five or ten times. Later, the subjects heard these melodies, and other Korean melodies that were entirely new, and were asked to indicate which they preferred. As one would expect from the mere exposure effect, both Korsakoff patients and controls preferred old rather than new melodies, although there was no effect of the number of exposures to the old tunes. However, compared to the controls, the patients showed greatly impaired levels of recognition. Thus, exposure affected the amnesic patients' preference judgments, an index of emotional response to the melodies, even though the patients could not remember the exposure trials.

A second study by Johnson et al. (1985) provided subjects with more substantive contact with the stimulus materials. The same amnesic and control patients who served in the melodies study were presented with pictures of two male faces, accompanied by fictional biographical information that depicted one individual positively (the "good guy") and the other negatively (the "bad guy"). When asked whom they preferred, control subjects always chose the face

that had been paired with the positive information, and they were always able to say that they judged on the basis of the accompanying descriptive information. The amnesic patients also showed a strong (though not unanimous) preference for the good guy; however, they could recall only a little of the biographical material presented at the time of study. Again, information presented during the study phase altered some aspect of emotional response—liking of persons instead of melodies—even though patients could not consciously recall the description.

Unfortunately, another group of investigators failed to obtain the mere exposure effect in a mixed group of amnesic patients who were repeatedly exposed to photographs of faces (Redington, Volpe, & Gazzaniga, 1984). Perhaps the exposure effect on preferences is not always dissociable from explicit memory. However, Johnson and Multhaup (1992) essentially confirmed the findings of Johnson et al. (1985) with a new sample of amnesic patients. In the melodies experiment, amnesic patients preferred old melodies to new ones, but controls did not; controls remembered the melodies well, but amnesics did not. In the impression formation experiment, both amnesics and controls preferred the "good guy" to the "bad" one, even though the amnesics recalled very little of the biographical information presented at the time of study.

The dissociation between acquired emotional preferences and explicit memory was confirmed in an experimental case study with a patient called Boswell, who became densely amnesic following a case of herpes encephalitis (Damasio, Tranel, & Damasio, 1989). Despite a profound inability to recognize people, Boswell would go to a particularly generous staff member if he wanted something. In the experiment, Boswell had an extended series of positive, negative, and neutral encounters, respectively, with three different confederates. After questioning, Boswell was unable to recall anything about any of the people and never indicated that they were familiar in any way. Nonetheless, when asked on a forced-choice test whom he liked best, and would approach for rewards and favors, he strongly preferred the "good" confederate to the "bad" one, with the neutral confederate in between.

In addition to the disorders of memory produced by lesions to specific brain structures, there are also functional disorders of memory observed in the dissociative syndromes of psychogenic amnesia, psychogenic fugue, and multiple personality disorder. Interestingly, patients with functional amnesias often display implicit memory for events lost to conscious recollection: sometimes this implicit memory takes the form of an emotional response. For example, in a case Kaszniak and his colleagues reported, a victim of attempted homosexual rape forgot the incident but experienced severe distress when shown a TAT card picturing a person attacking another from behind (Kaszniak, Nussbaum, Berren, & Santiago, 1988). Similarly, Christianson and Nilsson (1989) observed that a woman who had suffered severe amnesia following assault and rape became

extremely upset when she returned to the scene of the crime, even though she had no explicit memory of the event. Finally, in Prince's (1910) case of Miss Beauchamp, a multiple personality, one alter ego, "B-IV," experienced strong emotional reactions to people and places that had emotional meaning to personality "B-I" and vice versa; however, neither personality had explicit memory of the emotionally arousing objects for the other, and each would be puzzled by her inexplicably intense reactions to such stimuli. Similar dissociations of affect from awareness have been reported in "split brain" patients (Gazzaniga, 1985; LeDoux, Wilson, & Gazzaniga, 1977; Sperry, Gazzaniga, & Bogen, 1969).

Amnesia for the source of a consciously experienced emotion is also a familiar phenomenon in hypnosis. Whereas hypnotic suggestions are typically intended to alter some aspect of cognitive functioning, they can also have emotional and motivational effects (see Kihlstrom & Hoyt, 1988, 1990). When these suggestions are accompanied by further suggestions for amnesia, the subject can experience a profound change in mood state, without knowing why. For example, Luria (1932) suggested to hypnotized subjects that they had committed a terrible crime and then covered this paramnesia with a further suggestion for posthypnotic amnesia. On a later word-association test, the subjects showed evidence of anxiety in response to cues related to the suggested crime, even though they had forgotten the suggestion. Luria's findings were essentially replicated by Huston, Shakow, and Erickson (1934).

In a related line of hypnosis research, Levitt (1967; Levitt & Chapman, 1979) administered direct suggestions for anxiety to hypnotic subjects, followed by a suggestion for posthypnotic amnesia. Even though the subjects could not consciously remember the suggestion, they displayed elevated levels of anxiety on various test measures. Blum's (1979) research revealed similar findings; subjects received suggestions to relive a conflictual, ego-threatening experience from early childhood and then a suggestion for posthypnotic amnesia covering the experience. According to Blum, the subject then experienced free-floating anxiety not tied to any hypnotic or childhood experience.

More recently, Bower and his colleagues (Bower, 1981; Bower, Gilligan, & Monteiro, 1981; Bower, Monteiro, & Gilligan, 1978) used a variant on Luria's technique to study mood dependent memory. In these experiments, hypnotized subjects were given suggestions to relive a particularly happy or sad experience from their past and further given posthypnotic suggestions to experience those happy and sad emotional states, stripped of cognitive content about the instigating event, in response to particular cues. The suggestion was further covered by one for posthypnotic amnesia. Thus, during the experiment proper the subjects felt happy or sad without being aware that this response had been suggested to them previously or that it was distantly related to some previous experience in their lives.

The studies just described seem to demonstrate that emotional response can serve as an index of implicit memory. That is, subjects can display emotional responses attributable to some event in their past that they do not remember. However, the evidence in this regard is rather sparse, especially when compared to the vast body of literature on the perceptual and cognitive expressions of implicit memory (e.g., Roediger & McDermott, 1990; Schacter, 1987). Systematic studies need to include both amnesic patients and normal subjects to test the hypothesis that emotional response, as an expression of implicit memory, can be dissociated from conscious recollection.

Emotion as an Expression of Implicit Perception

There is considerably better evidence for emotion as an index of *implicit perception*—that is to say, when one's emotional responses are attributable to some event in the current environment that one does not consciously perceive. Once more we begin with anecdote and proceed to formal studies.

Levinson (1965) reported on a woman who came out of surgery inexplicably weepy, depressed, and disconsolate. The reasons for this state remained obscure until Levinson, on a hunch, hypnotized the patient and regressed her to the time of the surgery she then blurted out, "The surgeon says it might be malignant!" Further investigation revealed that the doctors had discovered a possible malignancy during the surgery and had discussed it while she was anesthetized (subsequent investigation proved it to be benign). Adequate anesthesia, by definition, abolishes conscious awareness and thus explicit memory of surgical events. But, as we said earlier, there is some evidence for the preservation of priming effects. Apparently, implicit perception during anesthesia can appear not just in the form of repetition priming effects but also in terms of full-blown emotional states.

More recently, Traub-Werner (1989) reported on an unusual case of simultaneous panic attacks in two agoraphobic patients. The first patient's symptoms had been well controlled by clonazepam and amitryptyline; but one day, while she was washing her face, she unexpectedly experienced anxiety, fear of falling, palpitations, and depersonalization. She hid under the bed for several minutes, until she felt better. Later that day, she received a phone call from a friend, also agoraphobic, who reported her worst panic attack in years—at precisely the same time of day. Further investigation revealed that an earthquake, registering magnitude 6 on the Richter scale, had occurred not far away at exactly the time of the two patients' panic episodes. In fact, an associated earth tremor, too weak to be consciously felt but perhaps picked up by the vestibular system, may have evoked anxiety in the first patient.

Evidence of emotion as an index of implicit perception has emerged in research on *subliminal mere exposure effects*. Recall Zajonc's (1968) discovery that mere exposure is sufficient to increase judgments of likability, an arguably

affective response, and the evidence from Johnson et al. (1985) and Damasio et al. (1989) that the exposure need not be consciously remembered to affect emotion (see Moreland & Zajonc, 1977, 1979; but also see Birnbaum & Mellers, 1979a, 1979b). Interestingly, apparently the exposure need not be consciously *perceived*, either. For example, Wilson (1979) found that subjects preferred tones presented on the unattended channel during a dichotic listening procedure to tones previously unpresented. Similarly, Kunst-Wilson and Zajonc (1980) presented subjects a set of irregular polygons on a tachistoscope, with exposures so brief that the stimuli were not consciously perceived; nevertheless, rated preference was affected by prior exposure history (for an alternative interpretation, see Mandler, Nakamura, & Van Zandt, 1987).

This subliminal mere exposure effect has been replicated and extended by a number of investigators (e.g., Bonnano & Stillings, 1986; Murphy & Zajonc, 1993; Seamon, Brody, & Kauff, 1983a, 1983b; for reviews, see Bornstein, 1989, 1992). In perhaps the most dramatic extension of the Kunst-Wilson and Zajonc (1980) study, Bornstein and his colleagues found that subliminal exposures can affect not only subjects' preferences for people's faces but also their interpersonal behavior toward those people when they actually meet them later (Bornstein, Leone, & Galley, 1987). Subjects who were subliminally exposed to a picture of a confederate during the study phase were more likely to express agreement with that confederate on a judgment task. However, testing of a separate group of subjects indicated that recognition of the "old" confederate achieved only chance levels, indicating that the faces had not been consciously perceived during the study phase. In fact, a meta-analysis by Bornstein (1989) found that the magnitude of the mere exposure effect was significantly greater with subliminal than with supraliminal stimuli. Apparently, affective judgments are influenced by perceptual fluency, which in turn is enhanced by the priming effects of the subject's initial exposure to the material. When subjects consciously remember the prior exposure, they appear to correct their preference ratings accordingly; but when the initial exposure is subliminal, so that subjects do not consciously perceive (much less consciously remember) it, subjects are unable to engage in discounting, resulting in a stronger effect on preference judgments (Bornstein, 1992; Bornstein & D'Agostino, 1992, 1994; also see Klinger & Greenwald, 1994).

Other investigators have found similar sorts of effects, when subliminal or unattended stimuli have "emotional" effects on judgments and behavior. For example, Murphy and Zajonc (1993) found, like Bornstein (1989), that subliminal exposure to emotional faces produced increased liking and preference for Chinese ideographs. In their view, the familiarity produced by subliminal exposure created diffuse positive feelings like (though different in valence from) the free-floating anxiety of the clinical concept.

Of course, considerable early evidence for emotion as implicit perception

was provided by investigations of perceptual defense, subception, and other aspects of the "New Look" (Bruner & Klein, 1960; also see Erdelyi, 1974; Greenwald, 1992; Kihlstrom et al., 1992a, 1992b). Bargh and Pietromonaco (1982) found that subjects who had been exposed to hostile words followed by a masking stimulus attributed significantly more negative qualities to a pictured person than subjects who had not received this masked exposure (also see Bargh, Bond, Lombardi, & Tota, 1986). Similarly, Devine (1989) found that unmasked parafoveal presentation of words related to negative stereotypes of African-Americans led to more negative evaluations of a target person whose race was unspecified.

Niedenthal and her colleagues (1990, 1992; Niedenthal, Setterlund, & Jones, 1994) have produced yet another emotional adaptation of the basic subliminal priming paradigm. In the study phase of each experiment, briefly presented primes consisting of faces expressing emotions of joy or disgust were rendered subliminal by a metacontrasting presentation of clearly supraliminal cartoon figures. In the test phase, the subjects were asked to discriminate between old, previously presented cartoons and new distractors. These were also preceded by a face prime, which again was rendered subliminal by metacontrast. On half the test trials, the affect associated with the prime was the same as it had been in the study trials; for the remainder, the prime was drawn from the opposite emotional category. Congruence between the primes generally facilitated recognition of the targets, especially when the prime was negative. A second study, in which emotionally charged faces or scenes primed emotionally neutral women's faces, obtained essentially the same effect. Moreover, the affective valence of the prime influenced the subjects' interpretations of the target's emotional state. Based on research indicating that the perception of emotionally expressive faces induces a similar emotional state in the perceiver, Niedenthal and Showers (1991) have proposed that a subliminal emotional prime elicits a corresponding emotional state in the perceiver; this state then serves as a cue for both perceptual identification and recognition memory. It is also, therefore, an expression of implicit perception.

Additional evidence that subliminal emotional primes actually elicit conscious feeling states comes from Ohman and his colleagues' research on subliminal fear conditioning (for a review, see Ohman, 1999). In one line of research (Ohman, Dimberg, & Esteves, 1989), subjects were conditioned to associate an electric shock with presentation of an angry face (an unreinforced happy face served as a control stimulus). In subsequent unreinforced test trials, the angry face was masked by a neutral face. Even though subjects could not consciously perceive the angry face, they gave conditioned electrodermal responses when it was presented, not during masked presentation of the happy face. A subsequent pilot study showed that acquisition of a conditioned fear response is possible, even when the conditioned stimulus is masked and

therefore not consciously perceptible. In another line of research, Ohman and Soares (1993, 1994, 1998) substituted nonmasked pictures of snakes, spiders, flowers, and mushrooms as conditioned stimuli. In unreinforced test trials, masked pictures of snakes and spiders elicited conditioned electrodermal fear responses, but masked pictures of flowers and mushrooms did not. The researchers interpreted the fear responses to the snake and spider pictures within the framework of Seligman's (1971) preparedness theory of phobias, which claims that, because of our evolutionary history, some stimuli (such as snakes and spiders) automatically initiate rapid and long-lasting conditioned fear responses. In this case, the assumption of automaticity of the association seems valid because the fear response persists even when the fear stimulus is subliminal, and thus unattended.

To date, Greenwald and his colleagues (Greenwald et al., 1989, 1995, 1996) have conducted the most systematic exploration of preconscious emotional processing. These studies examine another emotional response—evaluative judgments of words—and are especially notable because they carefully address methodological criticisms of earlier studies purporting to demonstrate subliminal perception (e.g., Eriksen, 1960; Holender, 1986). In all the experiments of the series, subjects are asked to judge an aspect of the connotative meaning of a word: whether it is affectively positive or negative. In the earliest experiments, the target word was preceded by a prime word that was either affectively positive or negative. The prime was so effectively masked that subjects were unable to determine whether it appeared on the left or the right side of a fixation point. Nevertheless, evaluative judgments of the target were facilitated by primes drawn from the same affective category. More recently, Greenwald et al. (1996) added a further refinement, in which subjects judged the target within a very brief period of time following its presentation, further ensuring that the influence of the prime on the target judgment was not the product of conscious reflection.

On the basis of early studies showing a dissociation between preference judgments and conscious recollection (Moreland & Zajonc, 1977) and conscious perception (Kunst-Wilson & Zajonc, 1980), Zajonc (1980, 1984a, 1984b) maintained that emotional processing is independent of, and temporally prior to, cognitive processing (also see Lazarus, 1982, 1984). However, later studies documenting similar dissociations between explicit and implicit expressions of memory (e.g., Schacter, 1987) and perception (e.g., Kihlstrom et al., 1992a) shed new light on the early results. One might just as well conclude that dissociations between recall and priming show that memory *itself* is independent of, and temporally prior to, cognitive processing. It is now clear that, in the early studies and those that followed, some aspect of emotional response is serving as an implicit expression of perception or memory. But unconscious cognition is still cognition. Furthermore, an emotional system separate from cognition would still need the cognitive capacity to analyze stimuli, link them

to prior knowledge, and generate emotional feelings and expressions (Leventhal, 1980, 1984). Such cognitive processes can go on outside of awareness, so that changes in evaluative judgment and other aspects of emotional response can be dissociated from explicit perception and memory; this process forms one aspect of the emotional unconscious. People can be aware of their emotional states but unaware of the percepts and memories that evoke these states.

"Feeling Memories": A Cautionary Note

Clinical folklore about posttraumatic stress disorder has revived the idea, originated by Breuer and Freud (1893–1895/1957), that unconscious memories of trauma express themselves implicitly as intrusive feelings (Bass & Davis, 1988; Blume, 1990; Frederickson, 1993; Herman, 1992; Terr, 1994; van der Kolk, McFarlane, & Weisaeth, 1996; for a detailed analysis of the parallels between Freud's theories and later clinical practices, see Bowers & Farvolden, 1996; Crews, 1995; Kihlstrom, 1996b, 1997b, 1998b). For example, Frederickson (1993) has distinguished between a conscious *recall memory* and an unconscious *feeling memory*:

> *Feeling memory* is the memory of an emotional response to a particular situation. If the situation we are being triggered to remember is a repressed memory, we will have the feelings pertaining to the event without any conscious recall of the event itself. Feeling memory is often experienced as a flood of inexplicable emotion, particularly around abuse issues. . . . A felt sense that something abusive has happened is a common form of a feeling memory. Some survivors will say, "Yes, I think I was sexually abused, but it's just a gut feeling." These clients are experiencing a feeling memory about being abused, even though at that moment they can recall nothing about their abuse. (p.92)

In some respects, the concept of a "feeling memory" finds support in the literature reviewed in this section, which indicates that emotional responses can indeed serve as expressions of implicit memory. However, there is an important difference: the experimental literature we have reviewed provides independent corroboration of the emotion-eliciting event. Implicit memory may be inferred only when such evidence is available, yet such information is rarely available in clinical practice. Nevertheless, clinical practitioners may infer a history of prior trauma and abuse from a patient's current emotional symptoms and then engage in therapeutic practices intended to recover the traumatic memories and restore them to conscious accessibility. Of course, if one cannot objectively corroborate the patient's history, such inferences are tautological and should be avoided—the techniques used to recover ostensibly lost memories are highly suggestive and may lead patients to reconstruct distorted or false memories of

trauma and abuse (Kihlstrom, 1996b, 1998b; Lindsay & Read, 1994, 1995; Shobe & Kihlstrom, in press).

IMPLICIT EMOTION

But can people be unaware of their emotional states themselves? The proposition seems to contain an internal contradiction, because emotions must be felt, and feeling is by any ordinary definition a conscious experience (Clore, 1994). But environmental stimuli must be felt, too; yet cognitive psychology and cognitive neuroscience are gradually accepting that percepts can be unconscious (Greenwald et al., 1996; Kihlstrom et al., 1992a), just as they earlier agreed that memories can be unconscious (Roediger & McDermott, 1990; Schacter, 1987). If there is a cognitive unconscious, in which percepts, memories, and thoughts influence experience, thought, and action outside of phenomenal awareness, then why can there not be an emotional unconscious as well? The answer depends on how one defines emotion. If emotion is a conscious feeling state, an emotional unconscious is precluded. But if one defines emotion differently, the question could be answered through empirical evidence.

Desynchrony

In classic research on experimental neurosis in dogs, Gantt (1937, 1953) observed that separate components of a conditioned fear response could be acquired and extinguished at different rates and persist for different lengths of time, resulting in an organismic state of *schizokinesis* reflecting the "disharmony or cleavage in behavioral, somatic, and psychophysiological response systems" (Mineka, 1979, p. 987). The clear implication of Gantt's work is a multifaceted emotional response, whose facets can be separated, or dissociated, from each other.

Gantt's observations have been confirmed in more recent research on fear conditioning. For example, Mineka (1979) distinguished four quite different response systems that have been used in the study of fear conditioning in nonhuman animals: conditioned emotional responses, increased rate of conditioned avoidance response, passive avoidance, and conditioned heart rate. Further, she showed that these indices of fear could be dissociated from learned avoidance behavior. Animals can behave as if they are afraid, even if they do not appear to manifest fear according to standard laboratory measures (also see Mineka, 1985a, 1985b, 1992). One interpretation of such findings is that, contrary to Mowrer's (1947) two-process theory, avoidance learning is not motivated by fear. Another is that the subjective experience of fear is only one component of a broader emotional response to fear stimuli. Similar observations have been

made in the case of human fears and phobias. For example, in a study of systematic desensitization of snake phobia, Lang and Lazovik (1963) found that some subjects would show substantial changes in avoidance behavior, while still expressing fear of the snake; other subjects would deny fear of the snake but show elevated cardiovascular activity and persisting avoidance behavior.

Based on observations such as these, Lang (1968, 1971, 1978, 1988; Lang, Rice, & Sternbach, 1972) proposed a *multiple-system theory of emotion.* According to this theory, every emotional response consists of several components: verbal-cognitive, corresponding to subjective feeling state (e.g., fear); overt motor, or behavioral, response (e.g., escape or avoidance); and a covert physiological response mediated by the autonomic and skeletal nervous systems (e.g., skin conductance or heart rate). Lang further proposed that these three systems are partially independent, although they also interact with each other in important ways. When all three systems act together, a person experiences intense emotional arousal. Under circumstances of attenuated emotion, however, the correlations among these systems tend to break apart, as their individual levels of activity diminish.

Moreover, Lang proposed that the different components of emotion can have different developmental histories. For example, autonomic responses to emotional stimuli may appear early in development, with the behavioral and cognitive responses emerging only later. Or, alternatively, the cognitive component of an emotional state can be acquired first, as, for example, through the social learning of fear, with the behavioral and physiological components coming later, if at all. Lang further suggested that effective psychotherapy for anxiety states and other emotional disorders should be directed at all three components: one cannot assume, for example, that flooding directed at reducing compulsive behavior will necessarily reduce subjective anxiety and physiological arousal as well.

Rachman and Hodgson (1974; Hodgson & Rachman, 1974; Rachman, 1978, 1981, 1990) adopted Lang's theme and explored the implications of *desynchrony* among emotional systems, especially between overt behavior and covert physiology, for the treatment of anxiety disorders. They proposed that different forms of treatment would have different effects on the components of fear and anxiety: for example, flooding might reduce avoidance behavior but leave autonomic arousal largely intact; on the other hand, spontaneous remission would affect autonomic arousal first, but behavioral avoidance would persist for a longer period of time. Like Lang (1968), Rachman and Hodgson believed that fear and anxiety should be assessed in terms of all three components and that treatment should be directed toward the most "abnormal" component (also see Norton, DiNardo, & Barlow, 1983). Somehow, however, the remaining components would eventually catch up. If one component persisted unchanged, in this view, the likelihood of relapse remained high.

The general idea of desynchrony is that an emotional response can be manifest at one level but not at another (Hugdahl, 1981; Turpin, 1991). Given that Lang (1968) and Rachman (1978) were writing from a tradition of behavior therapy that emphasizes objective measurement, it is perhaps natural, and certainly understandable, that both focused on desynchrony between the behavioral and physiological components of emotion. However, here we are most interested in cases that represent the emotional analog of the explicit/implicit distinction in memory: when the subjective component of an emotion (conscious feeling state) is absent, while the behavioral and physiological components persist outside of phenomenal awareness. The snake phobic denies fear but somehow never quite manages to go near the reptile house at the zoo; the agoraphobic claims to be cured and even ventures outside the house, but blood pressure and heart rate still increase dramatically. If such observations reflected merely denial, or a flight into health, they would not be too interesting. But suppose that the patients' reports accurately reflect the subjective state of affairs—that they really do not experience the emotions that used to bother them. If the behavioral and autonomic signs of emotion persist unabated, why can we not say that they are displaying unconscious emotion—or at least an unconscious emotional response?

Apparently, dissociations between subjective feelings and covert psychophysiological response are found quite commonly in the anxiety disorders (e.g., Barlow, Mavissakalian, & Schofield, 1980; Craske, Sanderson, & Barlow, 1987; Vermilyea, Boice, & Barlow, 1984; for reviews, see Barlow, 1988; Rachman, 1990). Indeed, such findings were a primary motive for Lang's proposal of the multiple-systems theory of emotion (Lang & Lazovik, 1963). Cardiology clinics frequently encounter patients who complain of behavioral and physiological symptoms associated with panic disorder yet report no subjective fear or distress, aside from concern about the presenting complaints themselves (Beitman, Mukerji, Russell, & Grafing, 1993; Kushner & Beitman, 1990). Similar patients have been seen in neurology clinics (Russell, Kushner, Beitman, & Bartels, 1991). A survey of students with a history of panic attacks found that those who reported "fearless" panic attacks were less likely to engage in avoidance behavior or to use alcohol or drugs as coping strategies.

Some evidence for desynchrony between subjective experience and covert psychophysiology is also provided in the literature on child-parent attachment. Dozier and Kobak (1992) administered an attachment interview in which subjects were asked to express their feelings about imagined scenarios involving separation from their parents. Subjects scoring high on a dimension of *deactivation/hyperactivation*, who strategically divert attention from thoughts, memories, and feelings related to attachment, showed greater skin conductance responses to scenarios involving parental separation, rejection, threats of separation, and changes in relationship with the parents.

Unfortunately, dissociations between subjective feelings and overt behavioral response appear to be much less common (e.g., Lang, Lazovik, & Reynolds, 1965). Moreover, evaluations of treatment outcome typically indicate that cognitive subjective fear persists even as behavioral and psychophysiological indices of fear diminish—precisely the opposite of the pattern of desynchrony implied by the concept of implicit emotion (Gerew, Romney, & LeBoef, 1989; Lang & Lazovik, 1963; Thomas & Rapp, 1977).

The emotional deficits (e.g., "blunted" or "inappropriate" affect) commonly associated with schizophrenia also suggest desynchrony (Dworkin, 1992). Thus, *anhedonia* refers to a deficit in the conscious experience of emotion, which may leave behavioral or physiological expressions of emotion unimpaired. Similarly, *flat affect* refers to a deficit in the behavioral expression or display of emotion, which may not extend to subjective experience or covert physiology. In fact, Kring and colleagues (Kring, Kerr, Smith, & Neale, 1993; Kring & Neale, 1996) found that schizophrenic patients expressed significantly less emotion than normal controls in response to emotional film clips; however, self-reports of emotional experience were similar for both groups, and the schizophrenics actually showed greater skin conductance reactivity. Obviously, this is not the desynchrony implied by the concept of implicit emotion.

Individual Differences in Emotional Experience and Expression

A lack of awareness of emotion may be implicated in several individual differences, the most obvious of which is repression. Though conceived by Freud as a general psychological process available to everyone, repression used as a defense may be reconstrued as an individual difference variable, as Rosenzweig did in his work on the Zeigarnik effect in memory and in his exploration of the personality correlates of hypnotizability (Rosenzweig, 1938; Rosenzweig & Mason, 1934; Rosenzweig & Sarason, 1942; Sarason & Rosenzweig, 1942). Unfortunately, early attempts to measure individual differences in repressive tendency through questionnaires, as exemplified by Byrne's (1961, 1964) Repression-Sensitization Scale (RSS; Bell & Byrne, 1978), failed because of discriminant validity: somewhat paradoxically, perhaps, repression as measured by the RSS proved to be highly correlated with anxiety and distress. However, other measurement approaches may prove more useful in explaining the differences between explicit and implicit emotion.

Repressive coping style. Weinberger and his associates have attempted to construct a measurement of repressive coping style that is free of such confounds (Weinberger, 1990; Weinberger, Schwartz, & Davidson, 1979). In Weinberger's original procedure in 1979, subjects who showed extremely low levels of trait

anxiety, as shown on Taylor's Manifest Anxiety Scale (MAS; Taylor, 1953), but extremely high levels of defensiveness, as shown by the Marlowe-Crowne Social Desirability Scale (SDS; Crowne & Marlowe, 1960), are "repressors." Though repressors do report low levels of anxiety, Weinberger et al. found that they showed elevated levels of physiological response—EMG, heart rate, and galvanic skin resistance—to sexual and aggressive phrase stems. In fact, their levels of physiological reactivity were comparable to those for high-anxious, nondefensive subjects. Asendorpf and Scherer (1983) later confirmed the majority of these results. Consequently, one might want to speculate that repressors have a talent for desynchrony: they may not display high levels of stress to others, or even experience it themselves; but, at the same time, their physiology is humming anxiously. This situation may be construed as a dissociation between explicit (subjective) and implicit (behavioral or physiological) components of emotion.

Unfortunately, however, no one has followed up this early evidence of implicit emotion. Instead, research has focused on the development of new methods for assessing repression and on repression as a risk factor for medical complaints. Thus, Weinberger (1990, 1997; Weinberger & Schwartz, 1990) reformulated the concept of repressive tendency and introduced a new instrument, the Weinberger Adjustment Inventory (WAI), for measuring individual differences in repressive coping style. The WAI follows the same logic as the earlier procedure, but yields six categories instead of the original fourfold typology (produced by splitting the two dimensions of anxiety and social desirability). Subjects who are low in distress (anxiety, depression, low self-esteem, and low well-being) and at least moderately high in restraint (impulse control, suppression of aggression, consideration of others, and responsibility) are candidates for identification as repressors. However, a measure of defensiveness (denial of distress, repressive defensiveness) is added to the mix to distinguish between genuine repressors and the merely self-assured. As a means of identifying repressors, the WAI is more conservative than the older procedure employing the MAS and SDS (Mulvaney, Kihlstrom, Figueredo, & Schwartz, 1992). Still, to date no research has attempted to replicate the observations of Weinberger et al. (1979) with the new measure. And, except for the replication of Asendorpf and Scherer (1983), no one has examined repressive style in relation to implicit emotion.

Alexithymia and anhedonia. Another potentially relevant personality construct is *alexithymia* (Nemiah, Freyberger, & Sifneos, 1981; Nemiah & Sifneos, 1970; also see Apfel & Sifneos, 1979; Taylor, 1984; Taylor & Bagby, 1988; Taylor, Bagby, & Parker, 1997; Taylor & Taylor, 1997), in which people cannot easily describe their emotional states or even discriminate one state from another. Alexithymia, or restricted emotionality, seems at least superficially similar

to the repressive coping style (Weinberger, 1990). Again, perhaps alexithymic individuals have "no words for feelings"—a fairly direct translation from the Greek roots—because they are not aware of their feelings in the first place (Lane, Ahern, Schwartz, & Kaszniak, 1997).

Alexithymia is a prominent feature among neurological patients with hemispheric commissurotomy (e.g., Hoppe & Bogen, 1977; TenHouten, Hoppe, Bogen, & Walter, 1985, 1986; TenHouten, Walter, Hoppe, & Bogen, 1988). Thus, the division in awareness affecting such patients includes an inability to communicate emotion arising from centers in the right hemisphere, via the language centers of the left hemisphere. In other words, the left hemisphere might not be aware of, and thus unable to communicate, emotions the right hemisphere is perfectly aware of—awareness that might be revealed if the right hemisphere possessed the same language skills as the left. In any event, the alexithymic patient's inability to discriminate between such feelings as anger and sadness suggests a rather marked deficit in explicit emotion. The question, then, is whether one can find evidence for *implicit* emotion in these patients, in terms of behavioral or physiological indices. Clinical lore, as well as an increasing body of empirical data, indicates that alexithymics are at risk for psychophysiological and somatoform disorders. Perhaps alexithymics, like repressors, have a talent for desynchrony, expressing emotion physiologically even if not subjectively.

Alexithymia should be distinguished from *anhedonia*, an inability to experience positive emotions (Chapman, Chapman, & Raulin, 1976). However, the alexithymic inability to communicate emotions to others is correlated with social anhedonia, or a preference for solitary rather than social activities (Prince & Berenbaum, 1993). However, we offer the hypothesis that physical anhedonia affects explicit (subjective) components of positive emotion, leaving implicit (behavioral and physiological) components of positive emotion intact.

Levels of emotional awareness. Lane and his colleagues (Lane, Quinlan, Schwartz, Walker, & Zeitlin, 1990; Lane & Schwartz, 1987, 1992) have drawn on the developmental theories of Piaget and Werner (for a review, see Flavell, 1963) to propose five levels of emotional awareness determined by the organization of an individual's emotional states. According to their theory, emotional experience is progressively differentiated and integrated as the individual develops cognitively. At the lowest level, roughly corresponding to the earliest sensorimotor stage of Piagetian theory, a person is aware only of bodily sensations; at the next level, corresponding to later sensorimotor stages, a person is also aware of action tendencies. In neither case, however, is one aware of emotion as such. Awareness of emotion occurs at a level corresponding to the preoperational stage, where it is confined to awareness of single, pervasive emotions. At a level corresponding to the stage of concrete operations, a person is aware

of emotion blends and simultaneous opposites. And at a level corresponding to formal operations, he or she can be aware of subtle nuances of emotion, as well as a difference between his or her own emotional reactions and those of others.

However, shifts in emotional awareness from one level to the next are not coterminous with the progress of cognitive development. Thus, it makes sense to distinguish among levels of emotional awareness in adults. Some adults, while firmly ensconced in formal operations for their cognitive abilities, may have only primitive, sensory-motor, emotional reactions—in other words, no *emotional awareness* at all. Interestingly, some of Lane's levels of emotional awareness correspond well to the three components of emotional response postulated by multiple-systems theory: physiological (bodily sensations), behavioral (action tendencies), and subjective (single emotions, blends, and nuances). Thus, one way of conceptualizing a desynchrony between explicit and implicit emotion poses an impairment at higher levels of emotional awareness that leaves lower levels intact.

Hypnotic Analgesia

A further example of desynchrony between the subjective experience of emotion and the behavioral and physiological expressions of it is provided by hypnotic analgesia (Hilgard & Hilgard, 1975). Following appropriate suggestions, many highly hypnotizable subjects report feeling no pain when exposed to normally painful stimulation. Though analgesia may be construed as a special case of sensory anesthesia, in fact pain has two components: *sensory pain*, providing information about the location and severity of irritation or injury, and *suffering*, a psychological reaction of unpleasantness that depends on the meaning of the sensory pain (Eich, Brodkin, Reeves, & Chawla, 1999; Hilgard, 1969; Melzack & Torgerson, 1971). Suffering is an explicitly emotional component not always present in the other skin senses, such as touch and temperature. Sensory pain and suffering are dissociable in terms of subjects' pain ratings (Gracely & Nabiloff, 1996; Melzack, 1975; Melzack & Torgerson, 1971) and also appear to be mediated by different brain systems: sensory pain by the somatosensory cortex, suffering by the anterior cingulate cortex (Rainville, Duncan, Price, Carrier, & Bushnell, 1997). Ordinarily, hypnotic suggestions for analgesia diminish awareness of both sensory pain and suffering (Hilgard, 1969; Knox, Morgan, & Hilgard, 1974); however, it is also possible to alter suffering while leaving sensory pain unaffected (Rainville et al., 1997).

Clinical studies conducted since the mid-nineteenth century indicate that hypnotic analgesia can be highly effective in relieving the pain of major surgery. However, the more common clinical use of analgesia is in the treatment of postoperative pain, episodic pain associated with specific medical and surgical procedures, burns, obstetrics, dentistry, and chronic pain associated with illness

(Hilgard & Hilgard, 1975; Hilgard & LeBaron, 1984). These clinical results are confirmed by more tightly controlled laboratory studies. For example, Stern and his colleagues found that hypnosis was more effective than acupuncture, placebo acupuncture, morphine, aspirin, diazepam, and placebo in counteracting both cold-pressor and ischemic pain (Stern, Brown, Ulett, & Sletten, 1977). Other laboratory studies indicated that hypnotizable subjects respond differently to analgesia suggestions than do insusceptible subjects instructed to simulate hypnosis (Hilgard, Macdonald, Morgan, & Johnson, 1978). Hypnotic analgesia is not mediated by placebo effects (McGlashan, Evans, & Orne, 1969) or by the tranquilizing effects of relaxation (Greene & Reyher, 1972). Hypnotic analgesia is not reversed by naloxone, an opiate antagonist, so it is not mediated by the release of endogenous opiates (Goldstein & Hilgard, 1975; Spiegel & Albert, 1983).

Most important, hypnotic analgesia does not appear to be mediated by a subject's engagement in *stress inoculation procedures*, such as self-distraction and reinterpretation, which alter the subject's response to, but not his or her awareness of, the pain stimulus. Miller and Bowers (1986) found that subjects administered hypnotic suggestions for analgesia did not report engaging in such strategies. Moreover, response to hypnotic analgesia suggestions was mediated by hypnotizability, whereas response to stress-inoculation instructions was not. A second essay by Miller and Bowers (1993) showed that stress inoculation strategies interfered with performance on a difficult vocabulary test, whereas hypnotic analgesia did not. Finally, Hargadon, Bowers, and Woody (1995) showed that the use of counterpain imagery, a common stress inoculation strategy, had no effect on hypnotic analgesia. These studies show that stress inoculation can reduce pain (Chaves, 1989; Meichenbaum, 1977; Spanos, 1986), but it does not mediate pain reduction in hypnotic analgesia.

Hilgard (1973, 1977) has proposed that hypnotic analgesia is mediated by an amnesia-like dissociative barrier that partially or fully blocks a subject's conscious perception and awareness of pain. Some evidence for this dissociative process comes from studies using the *hidden observer technique*, in which the analgesic subject receives a suggestion that a "hidden part" of the person may have registered, and can report, the true level of pain stimulation. In response, some subjects will report levels of pain comparable to those experienced in the absence of analgesia suggestions (Hilgard, Hilgard, Macdonald, Morgan, & Johnson, 1978; Hilgard, Morgan, & Macdonald, 1975). The hidden observer is a metaphor for the continuing cognitive representation of pain outside of conscious awareness and the means by which it may be accessed. Though hidden observer instructions may be interpreted as altering contextual demands to report pain (Spanos, 1986) or expectations about pain (Kirsch & Lynn, 1998), hypnotic subjects are much less responsive to manipulations of the testing context than are subjects instructed to simulate hypnosis (Kihlstrom, 1998a; Laurence, Perry,

& Kihlstrom, 1983). Therefore, the demand characteristics (Orne, 1962, 1979) of the experimental situation apparently are not sufficient to produce the hidden observer in analgesic subjects.

Although hypnotic analgesia alters a person's subjective awareness of pain and distress, it has little impact on physiological responses to pain stimulation (Barber & Hahn, 1962; Hilgard et al., 1974; Sears, 1932; Shor, 1962; Stern et al., 1977; Sutcliffe, 1961). This finding is ambiguous, however, because psychophysiological parameters do not show the lawful covariation with intensity of stimulation shown by self-reports of pain (Hilgard, 1969). The preservation of physiological responses to the pain stimulus does not impeach subjects' self-reports of analgesia, however, because the same dissociation is found with other analgesic agents, including aspirin, diazepam, and morphine (Stern et al., 1977). However, the basic finding of a dissociation between self-reports of analgesia and persisting physiological responses to the pain stimulus confirms desynchrony between the subjective and physiological components of pain as an emotional state. Obviously, however, further research needs to evaluate the hypothesis of desynchrony as applied to hypnotic analgesia (or, for that matter, any other analgesic). Such research should compare self-reports with psychophysiological indices but also with overt behavioral indices of pain, such as facial expression.

Implicit Attitudes

Other evidence bearing on the concept of implicit emotion comes from recent social psychological work on attitudes, stereotypes, and prejudice. In social psychology, attitudes have a central affective component: they are dispositions to favor or oppose certain objects, such as individuals, groups of people, or social policies. The dimensions of favorable-unfavorable, support-oppose, pro-anti naturally map onto affective dimensions of pleasure-pain or approach-avoidance. As Thurstone (1931) put it, "[A]ttitude is the affect for or against a psychological object" (p. 261). Like emotions, attitudes are generally thought of as conscious mental dispositions: supposedly, people are aware that they are opposed to nuclear power plants or favor a woman's right to choose. Similarly, people are generally believed to be aware of their stereotyped beliefs about social outgroups and of their prejudiced behavior toward members of such groups. And for that reason, researchers usually measure such attitudes and stereotypes by asking subjects to reflect and report on their beliefs or behavior. However, Greenwald and Banaji (1995) proposed an extension of the explicit/implicit distinction into the domain of attitudes. Briefly, they suggest that people possess positive and negative *implicit attitudes* about themselves and other people, which affect ongoing social behavior outside of conscious awareness.

Following the general form of the explicit/implicit distinction applied to

memory, perception, learning, and thought in the cognitive domain, one may define an explicit attitude as the conscious awareness of one's favorable or unfavorable opinion concerning some object or issue. By contrast, an implicit attitude refers to any effect of such an opinion on a person's ongoing experience, thought, and action, whether that opinion can be consciously reported or not. From a methodological point of view, a researcher would assess explicit attitudes through tasks requiring a subject's conscious reflection on his opinions; implicit attitudes through tasks that do not require such reflection.

Greenwald and Schuh (1994) provide a particularly provocative demonstration of implicit attitudes affecting behavior in an analysis of reference citation practices among social scientists (study 1) and prejudice researchers (study 2). In these studies, the authors' names, and the names of the authors cited in their essays, were classified into three ethnic categories: Jewish, non-Jewish, or other. Authors were approximately 40% more likely to cite colleagues from their own ethnic category, a significant difference that could not be attributed to either differential assortment by ethnicity to research topic or the tendency for authors to cite their personal acquaintances. Though few of the authors in question likely would consciously admit to ethnic prejudice (social scientists in general, and prejudice researchers in particular, tending to be rather liberal), their *behavior* suggests the operation of negative attitudes toward members of a religious outgroup.

Banaji and Greenwald (1995) provide a more tightly controlled demonstration of implicit attitudes in a study of the *false fame effect.* In the typical false fame procedure (Jacoby, Kelley, Brown, & Jasechko, 1989), subjects are asked to study a list consisting of the names of famous and nonfamous people. Later, they look at another list of names, including the names studied earlier and an equal number of new names and try to identify the names of famous people. Subjects, they found, are more likely to identify new rather than old nonfamous names as famous. In their adaptation, Banaji and Greenwald included both male and female names in their lists and found that subjects were more likely to identify male names as famous. This result suggests that the average subject is more likely to associate achievement with men than with women, a common gender stereotype.

Similarly, Blair and Banaji (1996) conducted a series of experiments in which subjects were asked to classify first names as male or female. Prior to the presentation of each target, the subjects were primed with a word representing a gender-stereotypical or gender-neutral activity, object, or profession. In general, Blair and Banaji found a gender-specific priming effect: judgments were faster when the gender connotations of the prime were congruent with the gender category of the name. This means that gender stereotypes influenced subjects' classification behavior.

In a study of racial stereotypes, Gaertner and McLaughlin (1983) employed

a conventional lexical-decision task with positive and negative words related to stereotypes of blacks and whites and the words *black* or *white* serving as the primes. There was a priming effect when positive targets were primed by *white* rather than *black*, but no priming was found for the negative targets, regardless of the subjects' scores on a self-report measure of racial prejudice. Thus, the effect of attitudes on lexical decision making was independent of conscious prejudice.

Similarly, Dovidio, Evans, and Tyler (1986) employed a task in which subjects were presented with positive and negative trait labels and asked whether the characteristic could ever be true of black or white individuals. Although the judgments themselves did not differ according to race (even the most rabid racist will admit that there are some lazy whites and smart blacks), subjects were faster to endorse positive traits for whites and to endorse negative traits for blacks. Thus, even though conscious attitudes did not discriminate between racial groups, response latencies did.

These studies, and others like them (e.g., Devine, 1989), seem to reveal the implicit influence of sexist or racist attitudes on behavior. However, at present, interpretation of these results is incomplete. In the first place, the logic of the research maintains that stereotype-specific priming indicates that subjects actually hold the stereotype in question, that, for example, the subjects in Blair and Banaji's (1996) experiment really (if unconsciously) believe that men are athletic and arrogant whereas women are caring and dependent. However, perhaps these priming effects reflect the subjects' abstract knowledge of stereotypical beliefs held by members of society at large, though they themselves personally reject them, both consciously and unconsciously. Thus, a subject may know that people in general believe that ballet is for women and the gym is for men, without sharing that belief. Even so, this knowledge may affect a subject's performance on various experimental tasks, leading to the incorrect attribution of the stereotypical beliefs to the subject.

Moreover, most studies of implicit attitudes lack a comparative assessment of explicit attitudes. Though one might like to think that the average Gentile social psychologist is not anti-Semitic (Greenwald & Schuh, 1994), this may not be so. Implicit measures of attitudes may be useful additions to the methodological armamentarium of the social psychologist, but here their interest value rests on demonstrations of dissociations between explicit and implicit expressions of emotion. Accordingly, research should show that implicit measures reveal attitudes different from those revealed explicitly. Just as the amnesic patient shows priming while failing to remember, and the repressive subject shows autonomic arousal while denying distress, we want to see subjects displaying attitudes or prejudices they deny having and acting on stereotypes they deny holding.

Recently, Wittenbrink, Judd, and Park (1997) performed a formal comparison

of explicit and implicit racial attitudes. Their subjects, all of whom were white, completed a variety of traditional questionnaire measures of self-reported racial attitudes. They also performed a lexical-decision task in which trait terms drawn from racial stereotypes of whites and blacks were primed with the words *black*, *white*, or *table*. Analysis of response latencies found, as one might anticipate from the studies already described, a race-specific priming effect: *white* speeded lexical judgments of positive traits, whereas *black* speeded judgments of negative traits. However, the magnitude of race-specific priming was correlated with scores on the questionnaire measures of racial prejudice. In this study, then, implicit attitudes about race were not dissociated from explicit ones. Such a finding does not undermine the use of implicit measures in research on attitudes and prejudice (Dovidio & Fazio, 1992), but a clear demonstration of a dissociation is critical if we are to accept implicit attitudes as evidence of an emotional unconscious whose components differ from those accessible to phenomenal awareness.

Neuroscientific Perspectives

In desynchrony, repression, alexithymia, hypnotic analgesia, and implicit attitudes, people are seemingly subjectively unaware of an emotional state that nevertheless influences behavioral and physiological outcomes. We propose a formal distinction between two expressions of emotion, explicit and implicit. Explicit emotion refers to a person's conscious awareness of an emotion, feeling, or mood state; implicit emotion, by contrast, refers to changes in experience, thought, or action that are attributable to one's emotional state, independent of his or her conscious awareness of that state. For measurement, explicit emotion tasks require the subject to reflect on, and report, his or her conscious feeling states; implicit emotion tasks do not.

Convincing evidence for emotion as implicit memory results from studies of neurological patients who acquire new emotional responses but who, because of their brain damage, cannot consciously recollect the experiences by which this learning took place. Similarly, brain-damaged patients may not subjectively experience emotional feeling states but nevertheless display overt behavioral and covert physiological responses that would be regarded as emotional. Dissociations among emotion systems would seem a natural topic for study by neuropsychologists, who are quite familiar with the concept of multiple systems in memory (Schacter & Tulving, 1994) and vision (Ungerleider & Haxby, 1994).

Unfortunately, neuropsychological evidence of multiple emotional systems, some supporting conscious feeling states and others supporting unconscious behavioral displays and physiological responses, is not readily available (for reviews, see Heilman, Bowers, & Valenstein, 1993; Kolb & Wilshaw, 1996). Perhaps the attention of most clinical and experimental neuropsychology has

been focused on cognitive, rather than emotional and motivational, functions. The readers of this chapter will immediately recognize the terms *cognitive neuropsychology* and *cognitive neuroscience;* the terms *affective neuropsychology* and *emotion neuroscience* may not be as familiar to them.

The idea of dissociable emotion systems is related to the concept of the *emotional brain*, as it has evolved from Cannon (1929) and Bard (1929) to Papez (1937), MacLean (1949), and LeDoux (1996). Cannon and Bard found that decorticate animals show fear responses as long as the thalamus and hypothalamus remain intact. These observations led them to propose that the diencephalon, which contains these structures, was the seat of the emotions, which mediated skeletal and autonomic emotional responses; in contrast, the conscious experience of emotion is actually mediated by the cortex, activated by fibers ascending from the hypothalamus. In such a system, a disconnection (Geschwind, 1965) between the diencephalon and the cortex would impair the subjective experience of emotion yet leave the behavioral and physiological components intact.

Later research broadened the theory of the emotional brain to include the entire limbic system (MacLean, 1949, 1952; Papez, 1937)—a move that, according to one commentary, "had the appeal of combining behavioral phenomena having no known neurological substrates with anatomical structures having no known function" (Kolb & Wilshaw, 1996, p. 418). Papez believed that the afferent messages arriving at the thalamus were transmitted in two separate streams to the sensory cortex (the stream of thought) and to the hypothalamus (the stream of feeling). The hypothalamus, in turn, generated skeletal and autonomic responses to the stimulus and also transmitted sensory information to the cingulate cortex, which also received inputs from the sensory cortex. When inputs from the hypothalamus integrated with inputs from the sensory cortex, an emotional feeling state originated. In such a system, three different disconnections could create a desynchrony between the explicit subjective and implicit emotion: (1) between the thalamus and the sensory cortex, (2) between the hypothalamus and the cingulate cortex, and (3) between the sensory cortex and the cingulate cortex. In any of these cases, the behavioral and physiological responses to an emotional stimulus would run off unimpaired, in the absence of any corresponding subjective feeling state.

MacLean (1949, 1952, 1970, 1990) expanded the definition of the limbic system even further, including the amygdala and other structures connecting directly to the hypothalamus, and proposed that a *paleomammalian* brain mediates the visceral and emotional life of the (mammalian) organism, while the *neomammalian* brain mediates consciousness, language, and other complex cognitive functions. Thus, as Papez (1937) believed, a disconnection between the paleomammalian and neomammalian brains could impair explicit emotion while sparing implicit emotion.

Most recently, LeDoux (1995, 1996) has proposed a more specific variant on the Papez/MacLean theory in terms of both anatomy and psychology. Briefly, LeDoux claims that a particular structure in the limbic system, the amygdala, mediates a particular emotion, fear (for a similar analysis, see Damasio, 1994). Based on his studies of fear conditioning in rats and other nonhuman animals, but supported by studies of human patients who have suffered damage to the amygdala and surrounding brain tissue (e.g., Adolphs, Damasio, Tranel, & Damasio, 1996; LaBar, LeDoux, Spencer, & Phelps, 1995), LeDoux has proposed that fear stimuli are processed by the amygdala, which then generates appropriate behavioral, autonomic, and endocrine responses. Cortical arousal, feedback of somatic and visceral data, and information about the fear stimulus are then integrated in working memory to generate the subjective experience of being afraid. As in the simpler systems Papez and MacLean described, a disconnection between the amygdala and the cortex can produce a dissociation between explicit and implicit emotion: a person will respond in a fearful manner without feeling fear or anxiety. LeDoux's system is especially appealing because it also offers a mechanism by which fear can serve as an implicit expression of memory: if the eliciting stimulus is not represented in working memory, the person will experience fear without being aware of the fear stimulus.

LeDoux's (1995, 1996) analysis of the amygdala applies only to the emotion of fear. Whereas Papez and MacLean implied that all emotions were mediated by a single system (Papez's circuit or the limbic system), LeDoux postulates a number of different systems, each mediating conscious experience, motor behavior, and somatic changes, corresponding to different emotional domains. The number of such systems is not clear, but if Ekman and Friesen (1975) are right that some patterns of emotional expression have deep evolutionary roots, one can reasonably hypothesize at least seven separate systems, corresponding to the "basic emotions" of surprise, happiness, sadness, fear, anger, and disgust. Thus, the range of possible dissociations is not just between explicit and implicit expressions of emotion *in general.* At least in principle, it may also be possible to observe, in a single patient, desynchrony between explicit and implicit fear as well as synchrony between explicit and implicit anger. Of course, this prospect would daunt any prospective researcher of the emotional unconscious.

The State and the Stimulus

The logic of inferring unconscious emotions is just as daunting. Consider the analogy to implicit memory. We know that priming is evidence of implicit memory because we can trace its facilitation in lexical decision, perceptual identification, and the like to a specific, objectively observable event: the nature of the prime presented to the subject. Furthermore, we can specify objectively the relationship between the prime and the target: same versus different word, same

word/same appearance versus same word/different appearance, and so on. In other words, we can identify an implicit expression of memory because we know what happened to the subject in the past—what the subject *should* be remembering.

But, by the same logic, to identify an implicit expression of emotion, we must know which emotional state the subject *should* be experiencing, or which emotional state is being represented, and expressed, outside of conscious awareness. Applying the logic of explicit and implicit memory to the problem of emotion, then, to find evidence of a dissociation between explicit and implicit emotion, demands methodological strategies.

First, one needs an adequate stimulus for emotion—that is, a set of stimuli that, under ordinary circumstances, reliably elicits particular emotions in subjects. Unfortunately, the search for such reliable elicitors has not been particularly fruitful. Apparently, just as the experience of pain depends on the subjective meaning of the pain stimulus, a person's emotional response to a situation depends greatly on his or her cognitive appraisal of that situation (e.g., Ellsworth, 1991; Lazarus, 1991; Lazarus & Smith, 1988; Ortony, Clore, & Collins, 1988, Smith & Ellsworth, 1985).

Still, some nearly universal elicitors of emotion hold promise for desynchrony research. Ekman and Friesen (1975) found, among other relations, that actual or threatened harm elicits fear, whereas loss of an object to which one was attached induces sadness. Similarly, Scherer, Wallbott, and their colleagues have found that basic pleasures elicited joy, whereas separation elicited sadness (Scherer & Walbott, 1994; Scherer, Wallbott, & Summerfield, 1986). Certainly, the relations in question are moderated to some degree by context-specific appraisals and cultural considerations, but enough cross-situational consistency pertains to offer some hope of measuring subjects' subjective, behavioral, and physiological responses to stimuli that should elicit certain emotions. If one observes diminished subjective awareness coupled with persisting behavioral and physiological responses, one would have evidence of a dissociation between explicit and implicit emotion.

Of course, documenting such a dissociation also requires reliable measures of the subjective, behavioral, and physiological responses to the emotion stimulus; and these are serious problems. Subjective feeling states can be assessed by the usual self-report measures, but one must carefully distinguish between the subjects' failure to consciously feel a particular emotion and their willingness to report what they feel to an experimenter. Implicit emotion is about awareness, not denial. The behavioral component of emotion might be indexed by gross patterns of approach/withdrawal, flight/fight, or activation/inhibition (Gray, 1987), and the physiological component by generalized levels of autonomic arousal (Schachter & Singer, 1962). Ideally, however, one would prefer implicit measures more specifically isomorphic to the lexicon of conscious emotions. For overt behavior, one

possibility is facial expressions like those documented by Ekman and Friesen (1975), as well as cognate postural and gestural expression. For covert physiology, Levenson (1988, 1992) and his colleagues have been able to document specific patterns of autonomic response accompanying particular indices of emotion (Ekman, Levenson, & Friesen, 1983; Levenson, Carstensen, Friesen, & Ekman, 1991; Levenson, Ekman, & Friesen, 1990; Levenson, Ekman, Heider, & Friesen, 1988). Similarly, Davidson (1993) has suggested that particular patterns of cortical activation may also differentiate certain basic emotions.

The ideal structure of an experimental demonstration of implicit emotion is now clear. To begin, we assume that a particular emotional state is a hypothetical construct defined by the logic of converging operations (Garner, Hake, & Eriksen, 1956; also see Campbell & Fiske, 1959; Kihlstrom, 1984; Stoyva & Kamiya, 1968). These operations include specification of an eliciting stimulus and the measurement of subjective experience, overt behavior, and physiological response, as indicated in figure 2.1.

When all operations agree, we can be fairly confident that a person is in an emotional state such as fear or happiness. Agreement among the operations is sufficient to establish the presence of an emotional state. However, none of these operations is necessary for this purpose; under some circumstances two or three operations combined would suffice. For example, a subject might report feeling no fear in response to a real or imagined threat. At the same time, he or she should continue to manifest facial, postural, and gestural expressions of fear, as well as autonomic and cortical signs of fear. Under these circumstances, the diagnosis of implicit fear might well be irresistible. As researchers grow more confident about classification of emotional stimuli, and multivariate measurement of emotional responses, they can better evaluate the multiple-systems theory of emotion and search for evidence of desynchronies between emotional systems—especially the particular pattern(s) of desynchrony characteristic of a dissociation between explicit and implicit emotion.

COGNITION, EMOTION, CONSCIOUSNESS, AND THE SELF

We have presented an overview of what the emotional unconscious might look like. The research is not definitive, but we see a number of well-documented instances in which current or past events influence our emotional states outside of phenomenal awareness and another set of plausible instances when emotional responses themselves affect experience, thought, and action outside phenomenal awareness. One further question remains; what is the difference between conscious and unconscious cognitions and emotions?

Schacter (1990) proposes a cognitive module, which he calls the Conscious Awareness System (CAS), corresponding to a brain module or a system of

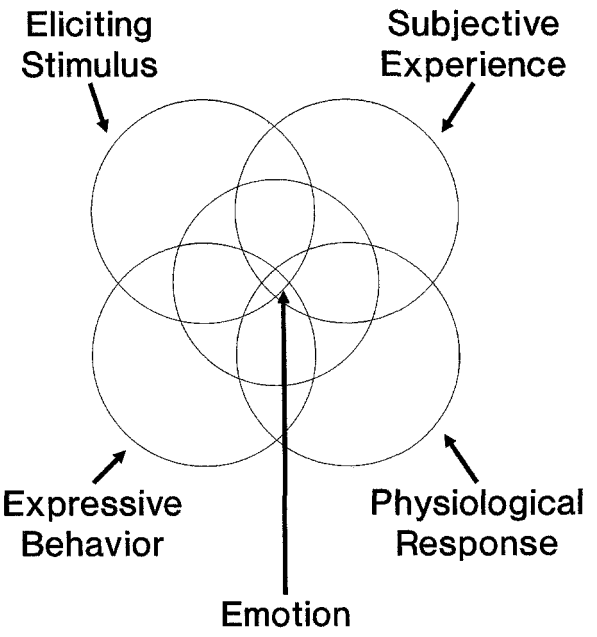

FIGURE 2.1. Emotional state defined by the convergence of eliciting stimulus, subjective experience, expressive behavior, and physiological response.

modules, that connects with other modular systems involved in perception, memory, language, emotion, and other mental functions. Damage to CAS, or more likely to the connections between it and other systems, will produce a loss of conscious awareness but not a complete loss of function. Thus, for example, if the connection between CAS and the visual system is broken, a person will not be aware of seeing but may still respond to visual events, as in the neuropsychological syndrome of blindsight. If the connection between CAS and the episodic memory system is broken, a person will be unaware of past events yet may still be influenced by the past, in the form of implicit memory, as observed in the organic amnesic syndromes. By extension, a disconnection between CAS and an emotion system would prevent one from being aware of his or her emotional states, at the same time as behavioral and physiological components of emotional response continue outside of conscious awareness. The system proposed by LeDoux (1996), though more complicated, is similar.

In psychological analysis, very close to phenomenal experience, the dissociations discussed here may be mediated by associations between various mental representations of experience and the mental representation of the self as the agent or patient of some action or as the stimulus or experiencer of some state (Kihlstrom, 1993c, 1997a). One remembers James's (1890/1980) comment:

> The universal conscious fact is not "feelings and thoughts exist," but "*I* think" and "*I* feel." (p. 221; italics original)

According to James, consciousness comes when we take possession of our behaviors, thoughts, feelings, and desires—in other words, when we acknowledge them as our own, or inject ourselves into them.

Janet (1907) articulated similar ideas about the role of the self in consciousness, employing an early metaphor for spreading activation:

> There are then in the [statement] "I feel," two things in presence of each other: a small, new, psychological fact, a little flame lighting up—"feel"—and an enormous mass of thoughts already constituted into a system—"I." (pp. 304-305)

The great French neurologist Claparede (1911/1951; see Kihlstrom, 1995a) discusses a classic early case of what we now call implicit memory:

> If one examines the behavior of such a patient, one finds that everything happens as though the various events of life, however well associated with each other in the mind, were incapable of integration with *the me* itself. (p. 71)

This connection to the self appears to be absent in the phenomena of implicit cognition and emotion. When we perceive an event, assume that we activate fragments of preexisting knowledge; when we attend to the event, its mental representation becomes part of our working memory, along with information about our emotional reaction to the event. These activated knowledge structures then have the opportunity to contact other activated knowledge structures such as one's current processing goals, the current spatiotemporal context, and the self.[1] This self-structure, resident in working memory, routinely comes into contact with other activated pieces of knowledge about the environment in which the person exists, current and past events, and other information activated by perceptual processing, memory retrieval, and other acts of thought. This connection, which defines the self as the agent or experiencer of some ongoing event, or the stimulus or experiencer of some state, is the key to consciousness. The cognitive situation is schematically depicted in figure 2.2.

Consider what happens when a perceived or remembered event generates an emotional response. Under ordinary circumstances, the event, its surrounding context, and the emotional response all contact the self in working memory, and all are represented in consciousness (fig. 2.2A). The person knows what he or she feels, and knows why.

Now consider what happens if the subjective component of the emotional response connects to the self, but the representation of the instigating event does not (fig. 2.2B). Under these circumstances, a person will be aware of the situ-

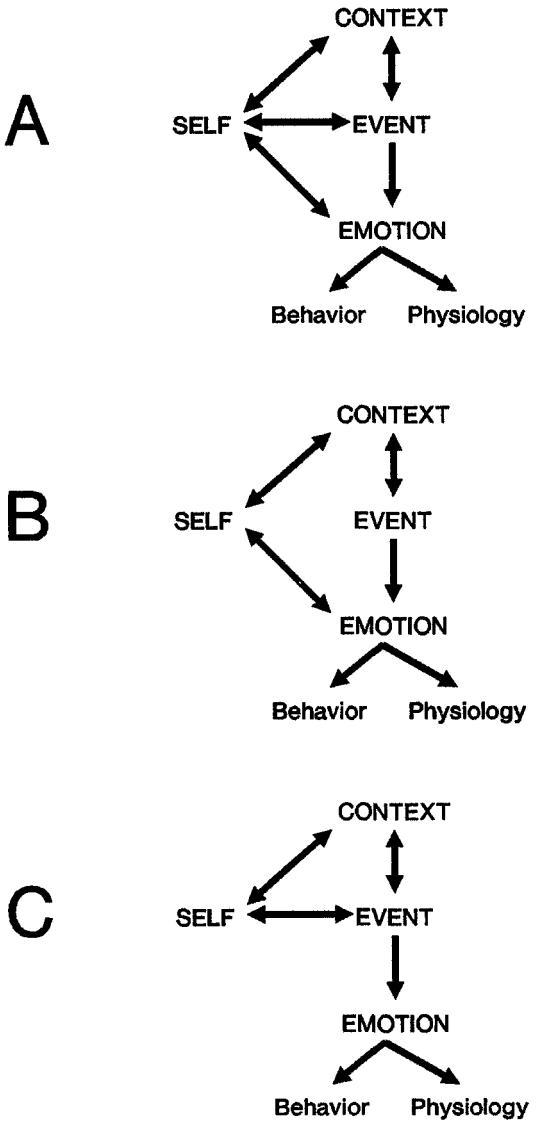

FIGURE 2.2. Mental representation of self linked to co-activated mental representations of an event, its episodic context, and emotional state (A). When the link between self and event is disrupted (B), emotional state is an expression of implicit perception or memory. When the link between self and emotion is disrupted (C), expressive behavior and physiological activity serve as implicit expressions of emotion.

ational background, his or her emotional state will be experienced consciously, and the behavioral and physiological consequences of that state will continue unimpaired. But the person will not know *why* he feels what he feels, because he is not aware of the instigating event perceived in the present or retrieved from the past. This is the usual form of a state of dissociation, reflecting an impairment of explicit perception or memory, with the emotional response reflecting spared implicit memory.

Under other circumstances, the representation of the instigating event will connect to the self, but the representation of the emotional state may not. Although the behavioral and physiological components of the emotion will manifest activity, the emotion itself will not be experienced consciously (fig. 2.2C). The result will be a state of desynchrony, reflecting an impairment of explicit emotion, whereas implicit emotion, in the form of expressive behavior and physiological changes, will be spared.

CAN EMOTIONS BE NONCONSCIOUS?

The emotional unconscious, then, has two different aspects. On the one hand, we may be unaware of the percepts, memories, and thoughts that give rise to our emotional feelings. In this case, emotion serves as an implicit expression of perception, memory, learning, or thought. On the other hand, we may be aware of what we are perceiving, remembering, and thinking but unaware of the emotions instigated by these cognitions. In this case, behavioral and physiological changes serve as implicit expressions of emotion.

But can emotions really be unconscious? A recent symposium answered this question negatively (Clore, 1994; Davidson & Ekman, 1994; LeDoux, 1994; Zajonc, 1994). There was general agreement that the cognitive and brain processes underlying emotions could operate outside of conscious awareness and conscious control. Thus, we might not be conscious of the source of our emotions; without source awareness, we might not know precisely which emotion we are experiencing. But as Clore (1994) put it, the essence of emotion is feeling, and "emotions that are felt cannot be unconscious by definition" (p. 285). And if, as Clore and Schwarz (Clore, 1992; Clore, Schwarz, & Conway, 1994; Schwarz & Clore, 1983, 1988) propose, the function of emotion is to provide information appraising the (past, current, or anticipated future) situation, the process would certainly be dysfunctional if emotions were unconscious.

On the other hand, if, as James (1884, 1890/1980) supposed, emotion is the perception of bodily (muscular and visceral) activity and perception can be unconscious, as seems the case from the literature documenting dissociations between explicit and implicit perception (Kihlstrom et al., 1992a), then emotions

can be unconscious, at least in principle, and can express themselves outside awareness through overt behavioral and covert physiological responses.

Although the experimental and clinical evidence for a dissociation between explicit and implicit emotion is not yet convincing, and the methodological requirements for such a demonstration have not yet been met, the hypothesis of unconscious emotional states cannot be rejected totally. If we are willing to speak of implicit percepts, memories, and thoughts dissociated from their explicit counterparts, then we must be willing to speak of implicit emotions in the same terms. It does not matter whether unconscious emotions would be dysfunctional. What matters is whether they can happen, how they can happen, and how we would know.

NOTES

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1. The self, as Janet (1907) noted, is a very rich mental structure. In the social intelligence theory of personality (Cantor & Kihlstrom, 1987, 1989; Kihlstrom & Cantor, 1989, in press), the self is an organized knowledge structure that stores what one knows about oneself. This would include semantic knowledge about one's physical and personality attributes, social status, and the like. It is also tightly linked to episodic knowledge forming one's autobiographical history; autobiographical memories must represent the self, by definition (Kihlstrom & Cantor, 1984; Kihlstrom et al., 1988; Kihlstrom & Klein, 1994, 1997; Kihlstrom, Marchese, & Klein, 1997).

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CHAPTER 5

Q and A

Eric Eich

As the Introduction noted, one of the main aims of *Counterpoints* is to provide contributors with a venue in which they can compare their personal points of view and voice their opinions on issues of wider interest. To that end, this chapter shares the answers that Professors Bower, Forgas, Kihlstrom, and Niedenthal gave to a series of questions I raised after they had read each other's work. In addition to clarifying several specific issues that arose in the core chapters, their replies candidly assess the general state of cognition/emotion research—its successes, its shortcomings, and its prospects for the future.

Before turning to the first question, a couple of comments are in order. First, apart from adding bibliographic references, I made no major editorial changes to the contributors' answers—for the simple reason that none seemed necessary or useful, either to me or my co-authors (among whom all replies were circulated). Further, all questions were raised, and answers received, via electronic mail—the only viable option, given that when this book was being written, its five authors were living in four countries on three continents. As it happened, e-mail proved more than merely practical; it seemed to prompt answers that were not only scientifically informed but refreshingly informal as well. Though e-mail may not be as "hot" a medium (in the McLuhan sense of the term) as, say, radio, it is certainly "warm," an altogether fitting attribute for a book about the interplay between emotion and cognition.

GENERAL QUESTIONS

1. Looking back over the past 20 years or so of research on cognition/emotion interactions, what do you see as the major accomplishments (theoretical or empirical) of this period? What concepts or findings published by others have most influenced your own work?

Gordon Bower

There have been several empirical and theoretical accomplishments in the past two decades. Some major contributions that I have found impressive are the following:

1. Eric Eich's (1995) resolution of when mood dependent memory is observed in laboratory studies and when it isn't, and why. That line of research required courage to undertake (given its prior odds of success) and great care in critically analyzing procedural variables that made a difference to the outcome of such experiments. Mood dependence should now be viewed as a robust finding that can take its proper place in the archives of our field, thanks largely to Eric's labors.
2. Research by Jerry Clore, Norbert Schwarz, Joe Forgas, Klaus Fiedler, and their associates delineating limits and boundary conditions for demonstrations of mood congruence in personal judgments and memory (e.g., Clore & Parrott, 1994; Fiedler, 1990; Forgas, 1995; Schwarz & Clore, 1988). As discussed in our chapter, classifying and systematizing these side conditions has been a central focus of Joe's affect infusion model.
3. Henry Ellis's demonstrations of various cognitive deficits caused by induced elation and depression have been very important. Ellis and his associates (e.g., Ellis, Ottaway, Varner, Becker, & Moore, 1997; Ellis, Seibert, & Herbert, 1990; Seibert & Ellis, 1991) have found that moods—even positive ones—interfere with central cognitive tasks (memorizing complex materials, detecting contradictions in texts, etc.), and they do so by evoking task-irrelevant thoughts. While that idea has been around for ages in the research on performance anxiety (e.g., Mandler & Sarason, 1952), Ellis was the first to demonstrate robust effects for positive mood inductions and to measure irrelevant thoughts with an on-line thought listing procedure. Ellis has also helped elucidate the boundary conditions for producing mood congruence effects in memory.
4. Paula Niedenthal and colleagues have succeeded in showing mood congruent effects in low-level perceptual tasks such as lexical decision and word naming (see Niedenthal & Halberstadt, this volume; Niedenthal, Halberstadt, & Set-

terlund, 1997; Niedenthal & Setterlund, 1994). Jerry Clore and I had searched for these perceptual effects long ago without much success (see Bower, 1987; Gerrig & Bower, 1982). Paula appears to have produced the effect by using more restricted, emotion-specific sets of words and stronger mood inductions.

5. On the theoretical side, useful advances have occurred in the way psychologists now characterize emotions and the stimulus conditions that elicit them. Beginning with the emotional attribution work of Bernie Weiner (1982) and Ira Roseman (1984), the field has been materially advanced by the careful semantic analyses of emotional terms by Andrew Ortony, Jerry Clore, and Allen Collins (1988), the hierarchical classification scheme proposed by Phillip Shaver and his associates (Shaver, Schwartz, Kirson, & O'Connor, 1987), and the dimensional analyses of emotional feelings proposed by Phoebe Ellsworth and Craig Smith (1988). Moreover, the eliciting conditions for various emotions have been well described by Nico Frijda (1988), by Nancy Stein and Judy Levine (1989), and by Keith Oatley and Phil Johnson-Laird (1987). These researchers have greatly advanced our description of the emotional domain.

Additionally, Richard Lazarus (1991), Bob Levenson (1992), and Albert Bandura (1997) have continued providing demonstrations of the importance of individuals' coping skills in modulating their emotional reactions to external situations. Of relevance to clinicians, Bandura (1991, 1994) has continued his analyses of conditions that help people learn how to cope with otherwise threatening and debilitating emotional situations.

Joseph Forgas

I think the past twenty years have seen a steady development in our understanding of how affective states and cognition interact. I would say that the major research achievements during this time were threefold. First, the discovery, documentation, and initial theoretical explanation of basic affect priming phenomena. Second, the empirical demonstration of the boundary conditions for mood congruence in memory and judgments, and early research demonstrating the information-processing consequences of affective states. And third, the emergence of more comprehensive integrative theories of cognition and emotion, and the rapid extension of cognition/emotion research to a variety of applied areas (clinical, social, developmental, organizational, etc.).

The first several years of this period (up to 1985) were marked by an intense initial interest in mood congruence phenomena. The associative network model proposed by Gordon Bower (1981) provided perhaps the first truly integrative theoretical treatment of these findings. The next period could be described as one of growing complexity, and even confusion. There was growing recognition

that mood congruence in cognition is not as robust or reliable a phenomenon as first assumed, and it also turned out that mood dependence in memory is itself subject to boundary conditions. Competing theoretical explanations for affect congruence and affect incongruence in cognition were proposed. Finally, the information-processing consequences of affective states started to be systematically explored for the first time. This period probably lasted from 1985 until about 1992. The third period, still continuing today, was marked by the emergence of integrative theoretical models seeking to account for mood congruence and incongruence within a comprehensive framework, and the extension of cognition/emotion research to a large number of applied areas. I think Gordon's network theory was the single most influential development in this regard.

John Kihlstrom

I think that the major accomplishment of this period is that we have begun to talk about emotion at all. And not only that, but emotion researchers have now begun to band together into a kind of interdisciplinary affective science, or affective neuroscience, expressly modeled on cognitive science and cognitive neuroscience.

The history of the cognition/emotion interface is quite interesting (see LeDoux, 1996). I think it's fair to say that research on emotion and memory revived the whole field of emotion. Before that, there really wasn't that much outside the clinical literature on anxiety, depression, and other affective disorders. Cannon's critique had pretty much dispensed with the James-Lange theory. There was some interest in the limbic system, but most of it was hand waving. Schachter and Singer (1962) changed all of that by showing that the physiological basis of emotion was undifferentiated autonomic arousal, and that the factors that differentiated the emotions were the attributions that people made about their emotions. That led to the view that emotions were products of cognitive construction—that they were, in essence, "beliefs" about one's emotions. This cognitive-constructivist view was further developed by George Mandler (1975, 1980), Richard Lazarus (1991; Lazarus & Smith, 1988), Phoebe Ellsworth and Craig Smith (1988; Smith & Ellsworth, 1985), Gerald Clore and Andrew Ortony (Ortony, Clore, & Collins, 1988), Keith Oatley and Philip Johnson-Laird (1987), and others. At the same time, Aaron Beck (1967, 1976), Martin Seligman (1971), and others, including Lyn Abramson, Lauren Alloy, and Susan Mineka (Alloy & Abramson, 1988; Mineka & Tomarken, 1989), introduced cognitive theories of clinical depression, which were followed quickly by cognitive theories of clinical anxiety.

So by the mid-1970s there was a kind of cognitive hegemony over the study of emotion. Almost two centuries earlier, in the *Critique of Pure Reason*, Immanuel Kant had argued that emotion was an irreducible mental faculty, along

with cognition and conation. But now, psychologists had reversed all that: far from being seen as a separate mental faculty, with its own structure and rules of operation, emotion was viewed as a cognitive construction (something similar happened to motivation, too, though that's another story). This turn of events had two consequences, in my view.

The first was to legitimize the study of the emotional effects on cognition. This had been a favorite topic of the psychoanalysts, of course, and Bruner and Postman (1949) had revived the issue as part of the New Look. But now it became possible to talk about such matters as mood congruent and mood dependent memory. Gordon Bower's (1981) *American Psychologist* article was extremely important in stimulating work on this topic. Later, Joe Forgas and others (Forgas, 1995; Forgas & Bower, 1987; Forgas & Moylan, 1991) expanded research on emotional effects on cognition to judgment and decision making, and Paula Niedenthal and Shinobu Kitayama (1994) revived the New Look program in the domain of perception. All of this work, in turn, increased the contact between cognitive and social personality psychology, and between the laboratory and the clinic.

The second consequence was to induce a reaction to the hegemony of the cognitive. Social psychology has always been ambivalent about the proposition, which I take to be axiomatic, that social behavior is cognitively mediated, and emotional constructs, at least in the form of attitudes, have always been central to the understanding of social behavior. So what happened, and it happened pretty quickly, was that some social psychologists began to argue that there were at least some aspects of emotion that were independent of cognitive involvement. Robert Zajonc's (1980) article was seminal in this regard, and his ensuing debate with Richard Lazarus (1982, 1984), mostly played out in the pages of the *American Psychologist*, is emblematic of the whole controversy. Richard Solomon's (1977) opponent-process theory of acquired motivation strongly suggested that some aspects of emotional experience were nonassociative. The work of Ekman and Friesen (1971, 1975) on facial expressions of emotion also contributed to the reaction, because they claimed that certain aspects of emotional experience and expression (the basic emotions) were part of our evolutionary heritage, prewired, automatic, and thus in some sense noncognitive. The anticognitive reaction was also fueled by research by Gary Marshall and Philip Zimbardo (1979), and by Christina Maslach (1979), which cast strong doubt on Schachter and Singer's original findings and theory. In the early 1980s, John Cotton (1980; Cotton, Baron, & Borkovec, 1980) and Rainer Reisenzein (1983) had published devastating reviews of the whole Schachterian program.

So now we have to be prepared to live with the proposition that at least some aspects of emotional experience and expression may not involve cognition at all—at least not in any important way. We no longer take it for granted that emotion is a cognitive construction. Fortunately, work on the cognition/emotion

interface doesn't require that emotion be a product of cognition. All we need is a way for the cognitive and emotional systems to connect, so that each can influence the other. And it's pretty clear that they do connect, so that emotional states can influence perception, memory, and thought, and cognitive processes can influence emotional states.

Finding out where, and how, and to what extent cognition and emotion connect is an enduring problem for psychology. Some theorists have had interesting things to say about this issue. Howard Leventhal (1984), for example, has been very influential on my own thinking: his perception-motor theory of emotion carefully works its way between the Scylla of cognitive constructivism and the Charybdis of affective independence and deserves a lot more attention than it has gotten. And Peter Lang's (1984) multiple systems view of emotion, coupled with Jack Rachman's (1990) notion of desynchrony, on which my own speculations about the emotional unconscious are based, also provide a means for understanding the cognitive and noncognitive contributions to emotion.

Paula Niedenthal

Here in France I am teaching a course in cognition and emotion, a course that I have not taught for about 10 years. So, it has been interesting to examine the contributions to this area in the last 10 years. There seem to me to be three conceptual advances, but I have to admit that I think that they are rather modest advances.

First, it seems to have taken a long time to get past the debate between Zajonc (1980) and Lazarus (1982) on the primacy of affect versus cognition, but it has been achieved. The realization that there are reciprocal influences, and that we need to be much more careful in our definitions of the two processes, or the parts of the two processes of interest, has allowed researchers to go beyond this question and to study both the emotional consequences of various cognitive processes and structures (as in self-discrepancy theory, self-complexity theory, work on counterfactual thinking and emotion, models of adult attachment, etc.) and to examine the influences of emotion in perception, categorization, impression formation and judgment, without getting hung up on which comes first, affect or cognition.

Getting away from which comes first has gone hand in hand (in my mind) with the refinement of, if not the concepts, perhaps at least the differential manipulation of and measurement of affect, evaluations, moods, emotions, and emotional traits. I have no doubt that this issue has not been of equal concern to even the authors of this volume. However, for years I worried about the fact that models of cognition/emotion interactions, as well as models of emotion per se, were based primarily on studies of pain, which should not stand alone as a prototypical emotion; or phobias, which should not be considered to be the same

as the state of fear; or depression, which probably should not be considered the same as sadness. Furthermore, very often researchers who intended to manipulate emotions in the laboratory manipulated processes in addition to emotion (e.g., self-evaluation). Often they have not been precise about which emotion they were inducing, or else they induced a mood and called it an emotion or vice versa. I believe that researchers in this area are finally worried about the differences among the concepts I mentioned and seem to be a little more concerned about what generalizations that can be drawn from a given set of studies, given the population studied or the manipulation techniques employed.

Relatedly, there is finally some concern about whether emotional state is required for conclusions about observed changes in cognitive processes at all. Not too long ago the Velten technique was used to induce emotions. Or else subjects were told to imagine an emotion-inducing experience from their past. Or else they were instructed to read a sad or exhilarating story. Then, as the dependent measure, changes in the accessibility of memories or the learning of words was assessed. Because there was no reason to reject the alternative hypothesis that the priming of verbal material by verbal material accounted for the observed effect, in fact there was often no need to invoke the notion of emotional feelings to explain the observed results.

Third, I would say that a sufficient number of researchers have broken away from a concern with the influences of emotion on the content of mental activity and focused on changes in the processes themselves. The possibility, which originates perhaps most clearly in the work of Alice Isen, that emotions influence not only the content of information processing but also the structure of information processing itself, has certainly become important in this area. Thus, a fair number of researchers, particularly those who study persuasion, stereotyping, and decision making, have suggested that certain emotions or certain moods are associated with more or less systematic processing, or more or less bottom-up encoding of information.

2. To what extent, and in what specific ways, has research on cognition/emotion interactions (a) contributed to a better understanding of basic cognitive processes, and (b) changed the way we think about the nature and function of emotions (or "affects" or "moods")?

Paula Niedenthal

In response to (a), I must say that this was the intention of the work that Jamin Halberstadt and I presented in our chapter for this volume. We specifically tried to link work on emotion to the literature on basic processes of categorization and to argue that a full account of categorization requires attention to the idea that emotions serve to organize categories of objects and event in memory.

In their reviews of the manuscript that reports the theory and data in their entirety (Niedenthal, Halberstadt, & Innes-Ker, 1999) categorization researchers were initially very skeptical that emotion belonged anywhere in their models and that the representation and processing of emotion was neither specific nor unique. Existing models of category learning and use could account for the whole story. They seemed to be satisfied with the final story, however. And people who study abstract concepts and decision making are starting to tell me that they are thinking seriously about emotion.

It is hard to imagine that the cognitive community as a whole would be much influenced by the work on emotion and cognition, however. Many cognitive psychologists think that emotion is too vague a concept to work with, and they mostly see emotional states as annoying potential sources of error. I must say that the researchers of cognition/emotion interactions also contribute to this problem. Those of us who have been trained in social and clinical psychology have not always followed carefully enough the research on the basic cognitive processes that we invoke in our models, or presume to be measuring, and thus often do not report work that the cognitive psychologists would ever consider pertinent to them. It seems to me, for example, that many researchers confuse emotion congruence effects themselves with the network model of emotion. There is the effect and there is the model. The effect is predicted by the model for certain responses. The presence of the effect does not actually always provide support for the model because the prediction is made by many accounts. But, more critically, the *absence* of emotion congruence often cannot be interpreted as a failure to support the model. Any time a task is used in which automatic spread of activation is not measured, all bets are off. Take an impression formation task, which measures many cognitive processes, not only priming. If emotion congruent priming does occur, this may or may not be revealed in the impression that is formed. But if emotion congruence is not observed, this fact says absolutely nothing about the possibility that emotions prime emotion-related material in memory. And yet the reasoning in the literature is all over the place. Why should cognitive psychologists care if we can't discuss the emotion network model separately from an emotion congruence effect?

Furthermore, I think we often leave emotion seeming to be an example, and potentially not an interesting one, of another category of cognitive process. Cognitive psychologists do sometimes call emotions just another feature (e.g., of a category), just another cognitive context, just another type of cognitive load, or just another retrieval cue. I am not sure that enough of the work in emotion and cognition has forced cognitive psychologists to take seriously the possibility that emotion processes are fundamental and special.

With regard to (b), I do think that research on emotion/cognition interactions has contributed to how theorists think about the function of emotion. In order to account for changes in processing style during positive and negative moods,

several writers (e.g., Schwarz & Bless, 1991) have proposed that positive moods signal that the environment is safe, which triggers the use of heuristic, or top-down, processes. That is, since the environment is safe, there is no reason to scrutinize the environment very carefully. If, on the other hand, negative moods signal the presence of danger or cause for concern, this would perhaps account for the findings that negative moods of some kinds enhance the use of systematic or bottom-up processing. Thus, as emotions theorists have often argued, emotions and moods can in part be thought of as signals about the state of the current environment and the necessity for action.

Barbara Fredrickson (1998) has recently posed the question: what good are positive emotions? In developing her answer, she invokes the work of Alice Isen and others (e.g., Isen, Niedenthal, & Cantor, 1992) on the relation between positive emotions and information processing. The possibility that positive emotions in particular enhance creativity through the priming of distantly associated ideas and through the sometimes novel reorganization of material in memory seems one of many adaptive functions of positive emotions. In general, such emotions are less urgent and longer lasting, and so perhaps exert interesting effects later in the time course of experience than do many negative emotions.

Gordon Bower

Concerning the first part of your question, from my perspective as a cognitive psychologist, there's been precious little influence of emotion research upon theorizing in cognitive psychology, certainly not in publications in the standard cognitive journals. The main cognitive theorists simply have other interests that engage their attention. Emotion receives mention in most cognitive textbooks only briefly as an example of a "context" effect rather like that produced by situational (room) cues. Emotion is also sometimes mentioned in reference to memory enhancement for emotional material, as in "flashbulb" memories. Possibly cognitive researchers have been turned off by replication failures of some of the earlier mood dependent effects on memory, as well as by unresolved controversies about whether emotion helps or hinders memory.

Concerning the second part of your question, my impression is that the change in thinking for people working in the emotion field has been substantial (see my reply to question 1). Regrettably, this sophistication regarding conceptions of emotions has not yet spread beyond our narrow confines into the outer reaches of academic psychology.

Joseph Forgas

I think cognition/emotion research has had a major impact on our understanding of basic cognitive processes. For the first time, affective reactions could be seen

as an important and independent source of functional information in realistic information-processing tasks, rather than merely as a source of noise and disturbance in idealized "affect-less" cognition. There have been several accompanying developments that hastened the reincorporation of affect into cognition, such as (a) the growing popularity of functionalist, evolutionary theories of affect, and (b) neuroanatomical evidence indicating the close involvement of affective reactions as a source of adaptive information in complex cognitive processing. Research during the past 20 years also helped to fundamentally change how we think about the nature and functions of emotions. By employing cognitive, information-processing theories, the careful and precise analysis of the role of affective reactions in thinking and behavior became possible for the first time. Rather than seeing affect as a source of irrational, dangerous, and disruptive impulses, as was often the case in much psychological theorizing previously, affect has now been reintegrated into mainstream psychological research.

John Kihlstrom

For cognitive psychology, I think that the most important legacy of cognition/emotion research has been to bring the study of cognitive processes into closer contact with "the real world." While earlier cognitive psychology (and its predecessor, the study of human learning) tended to be somewhat dry and abstract, with stimulus materials selected either arbitrarily or with an eye toward internal methodological considerations (e.g., holding imagery value or number of syllables constant in a word list), once you start studying emotion you almost inevitably move into the area of personal, social, and cultural meaning. The cognition/emotion interface took us farther from Ebbinghaus and closer to Bartlett, who, you'll remember, proposed that the first thing people remember about an event is their attitude toward it. And the consequence of that was to give cognitive psychology a fuller, richer understanding of how the mind operates.

For the psychology of emotion, I think that the most important legacy of cognition/emotion research has been to raise questions about the independence of affect from cognition. In a very real sense, the "affective revolution" began in 1980, with the appearance of both Gordon Bower's analysis of the mood effects on memory and Bob Zajonc's declaration that "preferences need no inferences." I think that Zajonc overstated his case: in my view, he erred both in identifying "cognition" with the conscious and the deliberate (when in fact some cognitive processes are unconscious and automatic) and in identifying emotion with preferences (when in fact emotions go beyond mere preferences, and preferences can be formed on purely cognitive grounds). But he did issue an important challenge to cognitive psychology, and it may well be that there

are certain aspects of our emotional lives in which cognition plays a relatively trivial role.

And if that's the case, cognitive psychology can't provide a complete account of mental life. Nor, by extension, can cognitive science or cognitive neuroscience do so, for the simple reason that, evidently, there's more to mental life than cognition. And it's the dawning awareness that there might be more to mental life than cognition that has led to the development of affective neuroscience. If cognitive science could do it all, then we wouldn't need affective neuroscience. Before we're finished, we'll probably see the development of a conative neuroscience as well, studying motivation independent of both cognition and emotion. And then the question will be whether these interdisciplinary fields really should be proceeding independently of each other. I think that the answer will be negative: they shouldn't proceed independently, and they *can't* proceed independently, because ultimately cognition, emotion, and motivation are inextricably linked.

And when it's all said and done, we'll need a framework for putting the cognitive, affective, and conative neurosciences back together. That framework, of course, is the old discipline of psychology. I've been a member of two different cognitive science programs, but I've always believed that cognitive science didn't do anything that a properly defined psychology couldn't have done on its own, and there are things that psychology can do that cognitive science can't. Cognitive science arose chiefly because psychology, which should have been studying how the mind worked, was mired in functional behaviorism instead. And it's told us a lot about cognition, and there are some things that cognitive science can do which psychology can't—like build machines with their own intelligence. But it's not clear that cognitive science can tell us what we need to know about emotion and motivation, and how cognitive, emotion, and motivation interact to produce behavior in an interpersonal context. Maybe only psychology can do that. And if so, that discovery is one of the byproducts of cognition/emotion research.

3. Generally speaking, are you satisfied with or disappointed by the rate at which the field has progressed?

Joseph Forgas

I would have to say that I am fundamentally satisfied with the achievements of cognition/emotion research during the past two decades. There has been a steady progression of interesting empirical findings, an exemplary development of evermore sensitive and inclusive theories, and the discovery of important and reliable effects that have widespread practical implications. Many of the most fruitful ideas started out from information-processing cognitive psychology and were

concerned with memory effects (e.g., Gordon Bower's work). Inevitably, the kind of robust and universal predictions initially made needed to be qualified and modified as the theories were tested in ever-more complex and realistic circumstances. I know that Gordon was kind of disappointed that his affect priming predictions also proved to be context dependent. Perhaps because of my different background, coming to this from social psychology, I always expected context dependence and was in a way more interested in the puzzle of when and why the effect occurs, and when and why it does not.

Paula Niedenthal

One might ask if this is a field—I am not sure it is. Am I too cynical for this book? I find that often people who are studying cognition/emotion interactions are not the same people as those who are working on emotion or affect, and that communication between the two groups is rather weak. Thus, sometimes developments in emotion research are not incorporated in the interactions work. This seems to me perhaps one reason why the interaction work of some researchers is quite atheoretical. In interactions work, there is often little concern with how emotions are conceptualized, how to manipulate emotions thus conceptualized, or the possible differences among emotions. Furthermore, emotion or mood is often used as an independent variable. That is fun, but the underlying models and, therefore, the predictions, are weak and very vague.

Partly as a consequence of a failure to conceptualize emotion and related constructs, some empirical findings have had a larger impact on theory building than perhaps they should have. As an example, in investigations of the influence of emotion on memory, the typical finding is that happiness enhances access to happy memories. Although the parallel prediction is made for sadness, this effect is not always detected. Rather than asking whether this asymmetry occurs for statistical or experimental reasons, some theorists have gone ahead and interpreted it, arguing that positive and negative emotional states have different effects on memory and coming up with possible reasons why. However, when you look at the results of experiments focusing on the effects of positive and negative emotions on memory, sometimes there actually is a similar effect of the negative mood, but it just does not quite reach an accepted level of significance.

More often, however, the conceptualization of negative mood and the related stimuli is just not well developed, or at least not developed as well as it is for positive mood. That is, there is usually only one positive state induced in the laboratory—happiness—and the effects of this state are examined only on one type of information—happy information. The manipulation of happy mood and the selection or coding of happy material is usually realized experimentally in a reasonable way. With negative states, however, things are more complicated.

Sometimes sadness is induced, sometimes anger, and sometimes a negative state that cannot be categorized as sad or angry. Then the stimuli are used as the dependent variable, or the coding of subject-generated material, are negative in general, and are not clearly associated with the induced state (at least as far as the reader can tell). Sometimes, both the negative state and the related material are just vaguely negative. This means that the effects of positive states are given a much stronger test than are the effects of negative states. Before developing models in which positive and negative emotions have different influences (i.e., one yields mood congruence and the other one doesn't), the states need to be defined and manipulated according the definition. Then stimuli, or systems for coding subject-generated output, have to be equally well matched to the induced states. In my opinion, the tendency to generate theories based on a small number of empirical findings that could be the consequence of the failure to adopt a clear theory or definition is all too common in this area.

There are also effects that have been demonstrated once or twice, or for which there are competing accounts that have never been eliminated, never mind addressed empirically. They sound good and so take on a reality all their own. I think the problem is that people want to have their own models or effects, and not necessarily the best ones. And so there is not enough competitive model testing in this area. The result is a bunch of models that are not even designed to account for the same data. Since they cannot speak directly to each other, there is no way to evaluate different models.

Gordon Bower

I'm pleased about progress in some areas, but disappointed in others. The pleasing aspect is the extension of mood congruence findings into far-reaching domains of social and personality psychology.

The truly disappointing feature for me is the near total lack of more precise computational (or computer simulation) models of the emotional system—of how emotions are triggered, interact with past habits and one another, and produce varieties of performances. Ken Colby's (1976) early simulation work on PARRY still stands as the primary major effort in this respect. Paul Cohen and I made a further stab at the goal (Bower & Cohen, 1982), but Paul earned his PhD and went off into computer science and I hadn't the skills to pursue the matter. Few people seem to have noticed or followed up on the Bower-Cohen chapter. Nico Frijda (1987) and his associates developed circumscribed computer models that recognized several types of emotional situations, but they did little with them. Colby's UCLA student Bill Faught (1978) also wrote interesting computer simulation programs for emotional interactions, but most of this work is still unknown to emotion researchers and the theories have never been criti-

cally examined and tested by detailed experimental work (but see Colby, Gould, & Aronson, 1989).

The absence of actual computational models exacts a considerable cost on this field, which is renown for its "fuzziness," partly because the central constructs (emotions) can't be observed and counted directly like responses in a Skinner box. The popular verbal theories of emotion often stumble into apparent conflicts with one another, which provoke debates that quickly degenerate into quagmires of semantic vagueness replete with the slipperiness of everyday language terms—debating whether a given result is due to affect or cognition, whether one comes before the other, whether mood congruence is only semantic or cognitive priming, whether it reflects an automatic or interpretive influence, whether it reflects conscious or unconscious factors, and so on. Not enough researchers recognize the costs exacted on scientific progress by such vague verbal theories. The problem was long ago recognized in mathematical models of learning—Bill Estes (1987) and Doug Hintzman (1991) wrote dire warnings—but that lesson seems not have been taken to heart by emotion theorists (admittedly, including the present author).

Consequently, a number of fundamental issues have still not been resolved and in our present circumstances appear to be unresolvable. Examples are questions such as these:

What exactly is an emotion as distinct from a thought or cognition?

Can one feel an emotion without a corresponding thought?

Can one "be having" an emotion without feeling it, without awareness of it?

How many emotions are there? How are we to decide?

Are some emotions primary and others derivative? How are we to decide that?

How do people's emotional experiences and expressions vary with their cultural or familial upbringing that's taught them how (or how not) to talk and think about their emotions?

Do people really remember emotional feelings directly or only covert memories that restimulate those emotions in the present?

Why exactly do emotional thoughts reverberate longer than unemotional ones, and why are they harder to suppress?

How are we to understand phenomena surrounding "dissociation" from emotional feelings, about "numbing" of emotions? How are we to understand alexithymia?

Can people feel several emotions at one time? Or do they just rapidly shift their attention among different causal antecedents (e.g., when witnessing a beating, feeling distressed when attending to the victim and angry when attending to the bully)?

Can we have blended emotions?

Which emotions amplify or inhibit which other emotions?

And that's just a small list. Does anyone see any real prospects for genuine answers to such questions in the coming decade? I am fairly skeptical. Consider just one example: we had formerly believed that the research of Paul Ekman (1989, 1992) on universal facial expressions of emotions could be used to resolve questions regarding innate primary emotions and their universality. But Jim Russell (1994) has provided a trenchant methodological critique of that earlier cross-cultural work, raising doubts about Ekman's interpretation and conclusions. Although Ekman (1994) has tried to counterargue Russell's critique, the issue is far from resolved and still posted on the research agenda.

Another disappointment to me is that laboratory emotion researchers have yet to fully exorcise the demon called "experimenter demand." It is an albatross, a bugaboo that, no matter how often it has been discounted in previous work, nevertheless continually arises to haunt our every step along the research path. Practically every mood induction used in the laboratory has the potential for introducing either experimenter demand (e.g., for the subject to "play the role" of a depressed person) or some variety of unwanted cognitions (e.g., an experimenter-contrived failure causes the subject to believe he is more similar to, and thus more attracted to, others who fail). Such unintended factors can confound the interpretation of the mood effects.

A small methodological advance on this problem capitalizes on "natural occurring" emotions by questioning unsuspecting people just after they've experienced an emotional event—sports fans just after their team wins or loses a match, cinema-goers after a comedy or tragedy, and so on. But these methods carry their own problems, including the fact that contact with the respondent is necessarily brief and one-shot, and that the emotions are evoked emphatically and are not quite "real" (e.g., viewers know that the movie stalker is not about to kill them).

As a curmudgeon in this field, I may perhaps be permitted a perogative to issue the standard lament, that some of my critics seem not to have familiarized themselves sufficiently with my former writings on cognition and emotion. I would estimate that about 90% of the criticism of my cognition/emotion theorizing has taken aim solely at my early *American Psychologist* article (Bower, 1981). But they fail to note that I was delivering an award speech at an American Psychological Association (APA) convention, during which time pressures re-

quire speakers to simplify rather than exhaustively cover all aspects of the topic and elaborate its complexities.

A more extensive treatment was provided in the Bower and Cohen (1982) chapter, which I mentioned earlier. That chapter anticipated and tried to respond to several deficiencies and potential criticisms of the simple theory advanced in my APA speech—indeed, several that were later discovered by our critics. To illustrate, the network theory in the APA speech has often been criticized for not making a distinction between "hot" versus "cold" uses of emotion, between talking about versus feeling emotions. Yet exactly that distinction (between words, concepts, and bodily referents) was discussed and modeled in the Bower and Cohen chapter. The distinction is completely parallel to the manner in which associative networks have dealt with distinctions between action terms (walk, grasp, press space-bar, turn 90-degrees left), their corresponding concepts, and a command to execute that action by the control system (e.g., the model of typing developed by Norman and Rumelhart, 1975; the internal knowledge base of any of the robot systems that execute external actions). Obviously, one needs a goal-based control system that can activate different productions depending on its temporary "goal"—whether that be talking cooly about the concept of fear or noticing conditions and then activating the emotion itself (feeling fear now). The APA speech has been similarly criticized for not dealing with antecedents of emotions, although time limitations confined my speech to dealing only with some consequences of emotional arousal. Antecedents were discussed in the Bower and Cohen chapter, and in a chapter published in the later *Handbook of Emotion and Memory* (Bower, 1992).

John Kihlstrom

I'll confine my comments to the memory literature, which Eric Eich and I recently reviewed for a conference sponsored by the NIH Office of Behavioral and Social Science Research (Kihlstrom, Eich, Sandbrand, & Tobias, 2000). Here we have the oldest, most-studied aspect of the emotion cognition interface. We know a great deal about it, but there is still a great deal that we don't know.

One area where we have made genuine progress has to do with mood dependent memory, which has experienced a genuine reversal of fortune. It can be predicted on theoretical grounds, having to do with Tulving and Thomson's (1973) principle of encoding specificity, that mood should have state-dependent effects on memory, just as many centrally acting drugs do. Gordon Bower and his colleagues (Bower, 1981; Bower, Monteiro, & Gilligan, 1978) initially demonstrated the effect, in studies that really got research on the cognition/emotion interface going, and which, in my judgment, sparked the "affective revolution" in psychology. But later, Bower (1987; Bower & Mayer, 1989) expressed doubts about the phenomenon, and it fell into a kind of disrepute. But now, Eric Eich

and his colleagues (Eich, 1995; Eich, Macaulay, & Ryan, 1994) have demonstrated the phenomenon convincingly. It's a relatively weak effect, but all such effects on human memory are relatively weak, regardless of whether they involve emotional states, drug states, or environmental context. The progress is that we can now believe in the effect again. But there are other effects that haven't shown that kind of progress.

The oldest reported effect of emotion on memory is the affective valence effect: material associated with a positive emotional valence is remembered better than material associated with a negative valence. More than 50 years ago, David Rapaport (1942) reviewed this literature in the context of Freud's theory of repression, and Matlin and Stang (1979), following Boucher and Osgood (1969), called it the "Pollyanna Principle." The claim makes a great deal of sense, intuitively, and especially if you are something of a Freudian, but it's still controversial. In particular, Banaji and Hardin (1994) showed that when you control for the intensity of affect represented by the stimulus, the affective valence effect disappears. Put another way, on average, positive items seem to be more arousing than negative ones, and it's this intensity of arousal, not its direction, that determines memory. We need more research to determine whether there is an affective valence effect on memory, independent of affective intensity.

A second area that deserves more research is the effect of emotion on the allocation of cognitive resources. Henry Ellis and his colleagues (Ellis & Ashbrook, 1989; Ellis, Thomas, & Rodriguez, 1984) have argued that depression increases a person's information-processing load, and thus impairs performance on effortful memory tasks, but not automatic ones. I think the empirical point is now pretty well established, but as I noted in a commentary on Ellis'work (Kihlstrom, 1989), the mechanisms underlying the effect remain open to question. For example, does depression, as an emotional mood state, drain cognitive resources directly, or is it the person's thoughts about being depressed that do the draining? Or is the impairment in effortful cognitive processing just a by-product of low levels of motivation, and not really a resource allocation effect at all? Is the effect specific to depression, or do other emotional states also drain resources?

A third area has to do with mood congruent effects on memory. These are pretty well established, but again I think we need to know more details—for example, comparing the effects of mood congruence on encoding with its effects on retrieval. But it has also been proposed that there are mood incongruent effects on memory—for example, that being happy makes sad events stand out in memory (see Keuler & Safer, 1998; Parrott & Sabini, 1990). Mood incongruence might seem to contradict mood congruence, but in fact there is a precedent. Long ago, Reid Hastie (1981) resolved the controversy over Bartlettian schematic effects on memory by showing that memory favors both schema con-

gruent and schema incongruent material—the first mediated by cues generated at the time of retrieval, the second by elaboration at the time of encoding. By analogy, mood congruence might occur because mood itself functions as a retrieval cue, while mood incongruence might occur because of a kind of contrast between the emotional valence of the event and the valence of the person's background mood. But all this is hypothesis, and I think we need a more determined effort to see whether mood incongruent memory actually occurs.

4. How do you see the area of cognition and emotion developing over, say, the next 10 or 20 years? What problems are most important to pursue?

John Kihlstrom

In addition to resolving the leftover problems of emotion and memory just discussed, and problems of emotion and thought (as represented by Gordon Bower and Joe Forgas, and by Paula Niedenthal and Jamin Halberstadt, in their contributions to this volume), I'd like to see more work done on the perceptual side. Niedenthal and Kitayama (1994) have done a great service by reviving, in their book, the whole question of whether, how, and to what extent emotion can influence "lower" cognitive processes such as perception, as well as "higher" cognitive processes such as memory and thought. But I think that they'd be the first to say that they've only broken the surface here, and there's lots more to be done.

Joseph Forgas

I think cognition/emotion research is likely to be influenced by two major factors during the next 10 years. The first is the very rapid advances being made in neuropsychology and neuroanatomy, which should help us better understand the interaction between affective and cognitive functions. The second area that I think will be interesting is the more precise exploration of how affective states and different information-processing strategies interact. I think much of the existing research up to now was concerned with unidirectional relationships—the exploration of how affect influences cognition, and how cognition influences affect (appraisal theories). There is now strong evidence that positive and negative affective states produce very different information-processing strategies, although there is not yet clear agreement as to the mechanisms responsible for this. I think the stage is just about set for theories that will start looking at emotion and cognition in a genuinely interactive, dynamic way. For example, it is possible that continuous and gradual shifts in information-processing style (leading to either mood congruence or incongruence) are a key element in how people control and manage everyday mood fluctuations. So we do know that

different affective states produce different processing strategies, and we also know that different processing strategies produce different levels of affect infusion and mood congruence. If we put these two ideas together, we may be on the way toward developing genuinely interactive, dynamic cognition/emotion theories. I think such an interactive cognition/emotion system has obvious evolutionary, adaptive characteristics that we are only just beginning to appreciate.

Gordon Bower

Joe's answer is just fine except I would add that I think the issue of unconscious emotions, which John Kihlstrom and his associates pursued earlier in this book, will come increasingly to the forefront.

Paula Niedenthal

I think that this area will be and has to be informed by, as well as be prepared to inform, advances in the cognitive sciences and neurosciences. We do not have to develop biological models—we can leave that for the neuroscientists—but I think our models have to be informed by their data, just as their progress should be informed by our theories and empirical research. Similarly, computer scientists and other cognitive scientists may lead us down some roads that are dead ends because much of the complexity of emotion cannot be accounted for with their methods (at least for now). But they will engage in the simulation and modeling of large quantities of data that our models and methods cannot handle. In addition, whatever insights that dynamical systems models reveal about emotion is likely to be seriously interesting and important for us to consider. Since emotions have time courses and may be considered self-organizing systems, it is clear that such approaches will be useful.

Cognition/emotion research is plagued by the problem that it is not clear what is the emotion, what is its antecedent, and what are the cognitive consequences. This is a problem that will have to be addressed in future research using these approaches.

A related difficulty is that sometimes stimuli used to measure the cognitive process of interest also temporarily or even permanently affect emotional state. I recently completed some experiments on emotion and the perception of facial expression of emotion with a doctoral student, Ase Innes-Ker. Our finding were influenced by the fact that the face stimuli altered the momentary emotional states of the subjects, something I should have anticipated, given my own past work on emotion and perception (e.g., Niedenthal, 1992). But our theories were not really ready to handle the interactions between emotional state and momentary emotional reaction in a way that could provide us with confident predictions.

5. In terms of clinical issues, it seems that much current research on cognition/emotion interactions is only tangentially related to the sorts of problems that clinicians deal with on a daily basis. As basic-research scientists, do we—should we—have a responsibility to do work that is more directly relevant to clinical concerns? Or should we continue to follow the lines of investigation that are suggested by our latest results? In either case, as basic researchers, what can we offer to clinicians—and their clients—who have to confront real-life, hot-button issues, like exhuming "repressed" memories?

Joseph Forgas

Personally, I don't think that there is anything wrong with pursuing theoretically driven research programs even if these projects have no immediate relevance to practical concerns, such as clinical problems. In any case, whatever we can contribute to understanding the links between affect and cognition will inevitably have some implications for clinical work on mood disorders as well. I think there has already been a pretty impressive record of cross-fertilization of ideas between laboratory-based experimental work—for example, on mood dependence in memory—and clinical research.

Gordon Bower

I think the relevance of our kind of cognition/emotion laboratory research to clinical problems is recognized by some clinical researchers, such as Andrew Mathews, Colin MacLeod, John Teasdale, Richard McNally, Edna Foa, Susan Mineka, David Barlow, Fraser Watts, Chris Brewin, Tim Dalgleish, and Ian Gotlib, to mention just a few (see Barlow, 1997; Brewin, 1997; Brewin, Dalgleish, & Joseph, 1996; Foa & Hearst-Ikeda, 1996; Gotlib & MacLeod, 1997; Kuyken & Dalgleish, 1995; MacLeod, 1996; Mathews & MacLeod, 1994; McNally, 1994; Mineka & Nugent, 1995; Teasdale & Barnard, 1993; Watts, 1995). Some of that work is reviewed in the chapter that Joe Forgas and I wrote for this book. Their research often runs parallel to the laboratory findings, but has the added complication that different "neurotic" populations have considerably different milieu exposures, probably different initial temperaments and upbringing, differential practice using the vocabulary of their disorder, differential elaboration of their conceptual networks, and so on. Moreover, comorbidity between depression and anxiety disorders is a major complication for interpreting results as due to one or the other emotional complex, as are complications introduced by associated drug abuse or medications.

We may sincerely hope that our laboratory research on emotional control would stimulate discovery of novel therapeutic methods for treating mood dis-

orders. However, the history of innovations in psychotherapy suggests otherwise: ingenious clinicians devise one or another novel application of a known principle (e.g., extinction by evoking competing responses; contingency contracts) whose success is then evaluated and received with acclaim if it proves to be somewhat successful in the clinic. For example, techniques of the now-popular cognitive behavior therapy arose from the clinical methods tried out by Albert Ellis (1962) and Tim Beck (1967, 1976) with practically no direct influence from laboratory research. I predict that our future will replicate the past in this regard.

Regarding Eric's question about the nature of "exhumed memories," emotion researchers have made several contributions to those discussions (e.g., Bower & Sivers, 1998; Christianson, 1992; de Rivera, 1998; Kihlstrom, 1996; Lindsay & Read, 1994; Schooler & Eich, in press). But the main issues to be sorted out regard the clinical "facts" of the matter and what mechanisms we have in cognitive theories of memory to explain how memories unavailable at one time may become available later, and why this may be especially likely to happen with negatively charged memories (see Bower, 1990).

Paula Niedenthal

I think we have to be clear what we are basic researchers *of*. As I implied previously, I do not think that people should be quick to generalize from studies of manipulated emotions to clinical issues (e.g., affective disorders) just as we should not draw simple conclusions from work on phobia, depression, or chronic anxiety when trying to model the influences of state emotion on cognition. However, the two areas should proceed in an interacting way because each has something to say to the other.

John Kihlstrom

This is another area that it's really important to pursue. As I noted earlier, the cognitive/emotion interface helped connect cognitive psychology to the real world, and the laboratory to the clinic. But I think that, for the most part, the transfer between laboratory and clinic has been unidirectional. That is to say, there has been a lot of transfer from the lab to the clinic, as any reading of the literature on cognitive-behavioral therapy will show. But now I think it's time for laboratory psychologists to take up, more fully and more deliberately, problems that are raised first and foremost in the clinic.

For example, what is the clinical significance of mood congruent memory? The effect is clearly present in the laboratory, but how important is it, really, in real life—including real life in the clinic? Some researchers and theorists have cautioned that mood congruence may distort autobiographical memory—

for example, biasing depressed patients to remember unpleasant rather than pleasant experiences or, perhaps, to reconstruct events in a negative manner. On the other hand, Brewin and his associates (Brewin, Andrews, & Gotlib, 1993) have concluded that the evidence for this proposition is inconsistent and unconvincing. If they're right, then we need to obtain better evidence on this matter one way or the other. The issue is an important one to resolve, and resolve fairly quickly, because of the effect it can have on clinical practice.

Another critical issue is whether traumatic stress can have special effects on memory. In general, the finding from the laboratory is that emotional valence enhances memory, presumably by making events more salient and distinctive. But at least since the time of Janet and Freud, there have also been claims that emotional trauma can impair memory through a process of dissociation or repression. More recently, the Janet/Freud proposal has been revived by proponents of the trauma-memory argument and recovered-memory therapy. These theorists, such as Bessel van der Kolk (1994; Van der Kolk & van der Hart, 1991), essentially reject the laboratory evidence as irrelevant to clinical practice and rely on clinical studies to claim that traumatic memories fail to be integrated into the normal stream of consciousness and express themselves instead through fragmentary images, inexplicable feelings, involuntary behaviors, and somatic changes.

Katharine Shobe and I recently reviewed a number of the most prominent of these claims and found them to be both internally inconsistent and inadequately supported by the evidence (Shobe & Kihlstrom, 1997). Even the clinical evidence cited in support of them isn't very good. Yet the controversy continues and it has spilled over into the courts, where individuals who claim to have recovered memories of sexual and physical abuse have brought charges against the alleged perpetrators. And so judges and juries increasingly have to sort through the laboratory and clinical evidence and deal with presentations by opposing "experts." It's important to get this issue resolved one way or the other. And the only way to do this, in my view, is to get clinicians to collaborate with experimental psychologists to produce methodologically adequate clinical studies.

QUESTIONS FOR GORDON BOWER AND JOSEPH FORGAS

6. Your chapter begins with a review of the history and current status of Gordon's network theory of emotions, and then moves on to a detailed account of Joe's affect infusion model (AIM). Do you see your respective views as being complementary? To the extent that they're different, can they be reconciled?

Gordon Bower and Joseph Forgas

From our perspective, we see no difficulty at all in reconciling the network theory and the affect infusion model. Indeed, the AIM was developed in direct response to growing evidence in the literature at the time that there are important boundary conditions that regulate mood congruence in cognition. Whereas one of us (Bower) moved toward emphasizing things like causal belonging, the other (Forgas) thought that the critical requirement for the affect priming theory to work is that people must engage in genuinely constructive, generative processing strategies where affectively primed information has a good chance to be incidentally used. Forgas moved in this direction because (a) he was always sensitive to the highly constructive nature of social cognitive tasks, where mood congruence was more reliably obtained, and (b) people like Fiedler (1991) explicitly drew attention to a processing dichotomy, contrasting constructive versus nonconstructive processing. In a sense, all that the AIM does is to emphasize and systematize the contextual circumstances within which substantive, constructive processing is most likely, and when affect priming effects should be most reliably obtained. In short, we see the AIM (Forgas, 1995) as being completely consistent with and complementary to the affect priming principle, as instantiated in the original network theory of emotions (Bower, 1981).

7. My impression is that neither the updated network model nor the AIM would expect reliable mood effects to emerge in the performance of automatic, data-driven, fast-paced tasks like lexical decision or word naming. Yet this is just what Paula and her colleagues have found in a series of recent studies (several of which are reviewed in her chapter). What do you make of this, and are you inclined to revise your theories accordingly?

Joseph Forgas

I don't see Paula's results as necessarily being inconsistent with the fundamental implications of priming theories—and thus, the affect infusion model. Indeed, what is really surprising is that others have not found such effects more reliably. The way I see it, lexical decision and word naming, even though fast-paced and data-driven tasks, nevertheless do involve an element of constructive processing, in the sense of "going beyond the information given" to produce a response. They are also constructive in the sense that performance on similar kinds of tasks has been previously shown to be sensitive to various context effects and even semantic priming effects. The fact that a task is fast and almost automatic does not mean that it cannot at the same time be constructive; that is, I would not define "constructive" as necessarily meaning only high-level, inferential thinking. To take another example, in a study done with Gordon (Forgas, Bower,

& Krantz, 1984), we found significant mood priming effects on rapid on-line judgments of observed behaviors. This task is in some ways similar to Paula's—fast, automatic, and apparently stimulus-driven—yet at the same time, responses are essentially constructive.

Nevertheless, I can understand that you may feel that I am pushing the notion of "constructive" too far here. Perhaps there is a need to distinguish between high-level, inferential construction (as in a social judgment) and the kind of construction involved in front-end cognition as demonstrated by Paula's work. To my mind, the similarities (i.e., needing to go beyond the information given to produce a response) are just as important as the differences.

Gordon Bower

The network theory of spreading activation has always expected a "top-down" influence of strong emotion on perceptual processing of strongly associated words (see Bower, 1981). Its prediction here arises for the same reason that semantic priming arose in John Morton's (1969) old logogen theory (for an update, see Bower, 1996). The emotion spreads a little activation onto those strongly associated words in advance of the input stimulus arriving, thus making them more readily available (to cross threshold) given minimal stimulus input. This theory expects all manner of word priming measures to reveal emotional congruence effects: perceptual identification, reading speed, word-fragment and picture-fragment completion, word associations, lexical decisions, resolving the meaning of ambiguous homophones (e.g., *die* vs. *dye*; *pain* vs. *pane*), and so on.

Postman and Brown (1952) had earlier demonstrated allied effects in the tachistoscopic identification of success versus failure words after subjects had received a success versus failure manipulation. The problem is that the first several attempts we made to verify the prediction failed. Richard Gerrig and I found no mood congruent enhancement in perceptual identification of pleasant versus unpleasant words following the induction of happiness or anger (Gerrig & Bower, 1982). I knew, too, of a similar pattern of partial successes and failures of mood congruence across several experiments on perceptual identification found (though not published) by Professor Michelle Millis of San Jose State University in the mid-1980s. Later, I found no mood congruence when subjects judged whether or not two words agreed in their pleasantness or unpleasantness. David Clark and his colleagues (Clark, Teasdale, Broadbent, & Martin, 1983) found no mood congruence in lexical decisions regarding pleasant and unpleasant words. Ian Gotlib and McCann (1984) found no emotional congruence in Stroop interference (color naming) following the induction of sad moods in normal subjects, confirming earlier unpublished, negative findings that Jerry Clore and I had obtained in 1981. So the problem became one of explaining

why the emotional congruence effect did not arise as predicted. It was in that unrelentingly negative environment that I reluctantly (and tentatively) suggested that perhaps emotional effects did not operate on well-practiced, automatic perceptual processes such as word perception.

In their chapter in this volume, Paula Niedenthal and Jamin Halberstadt have materially advanced the analysis by noting that perhaps emotion congruence in perception is a genuine phenomenon, but the words have to be selected with far more specificity to the exact emotion induced beforehand. They noted that the earlier failures had used mainly pools of undifferentiated positive and negative words. Accordingly, their experiments used specifically happy and sad words as stimulus materials following happy and sad mood inductions, and indeed found significant mood congruence in word identification and in lexical decision. Thus, a sad mood induction enhanced perceptual processing of a word like *depressed* but not more remote (unpleasant) associates such as *doom* or *debacle*.

While these recent results are most welcome, I am still left to wonder why there weren't at least some small effects in the several earlier experiments (mentioned above) that averaged over pools of affective words which, of course, included synonyms for the induced happy, angry, or sad moods. The new results could be accommodated in the network theory by supposing that activation from the mood spreads appreciably (enough to be noticed in these behavior measures) only for words very highly associated to the specific emotion. But that is indeed a lame explanation of the variety of results.

QUESTION FOR GORDON BOWER

8. Intuitively, it seems that once a particular mood has taken effect, it tends to linger for quite a while and to change much less quickly that any of the specific ideas, images, or recollections that it may trigger by virtue of prior association. Can (or does) the network model accommodate this observation?

Yes, I agree: real arousal of an intense emotion usually produces an aftereffect that lasts for many minutes. The reason is obvious and has been known for at least 75 years: strong emotional reactions are accompanied by neuroendocrinal discharges (e.g., adrenaline, norepinephrine) that circulate in the blood stream and provide a persisting discharge and tonus within the sympathetic nervous system (see Bower, 1992). These endocrine changes probably also lead to short-term changes in the supply of various biogenic amines in the brain (dopamine, epinephrine). These cholinergic biochemicals continue to have a biological impact for many minutes, at least until they are metabolized out of the bloodstream.

This biological observation can be rendered into our psychological model by

a correspondent assumption: once an emotion node is turned on (e.g., by remembering or recognizing a fearful scene), it has a long life due to a very slow decay rate compared to the "cognitive, ideational" nodes that provide cognitive representations of the ideas, images, and propositions that fill the working memory of the mental system. Such emotion nodes are embedded in a cognitive architecture which must assume that activation on nonemotional concepts and propositions in working memory is quenched or decays rapidly (150 ms?), as soon as the flow of ideational thought moves on to another associated idea or proposition. The result of this slower decay rate is that the emotional subject is capable of fleetingly thinking over a collection of many emotionally congruent ideas and memories all during the course of a single episode of emotional arousal. That, at least, is a first rough sketch of how I would try to model the distinction in duration between thoughts and emotional arousal.

QUESTION FOR JOSEPH FORGAS

9. Though AIM is chiefly concerned with mood congruent effects, it seems relevant to mood dependent effects as well. As an example, suppose that happy and sad subjects are asked to read about and form impressions of fictitious individuals who seems rather ordinary or quite odd. From your earlier work, one would expect stronger mood congruence in the subjects' judgments of the odd as opposed to the ordinary targets, because the former are more amenable to high affect infusion than are the latter. Now suppose that the subjects are later asked to freely recall as many of the targets as possible, and that testing takes place in either the original or the alternative mood. My hunch is that a change in mood state would have a greater adverse impact on the recall of the odd targets. More generally, it may be that mood dependent effects are most likely to emerge in a robust and reliable manner when both the encoding and the retrieval task are conducive to high affect infusion; high infusion at either encoding or retrieval alone won't work. What do you think?

I think this is a really interesting issue. It is correct to suggest that the AIM was initially developed to account for the observed presence or absence of mood congruence in social judgmental tasks, so mood dependence was of less importance initially.

However, the logic of the model clearly predicts that all affect priming effects—both mood congruence and mood dependence—should be more reliably obtained whenever more constructive processing is required to deal with a task. So it follows that mood dependent effects should also be enhanced whenever both the encoding and the retrieval tasks are conducive to constructive process-

ing, as Eric suggests. In fact, we have found some evidence indicating just such a pattern in several of our studies, even though we have never attempted to independently manipulate the kind of processing used at the encoding and at the retrieval stage. An ideal experiment to test this hypothesis would be one where the level of substantive processing used is separately manipulated at encoding and retrieval; we could then predict that overall mood dependence in recall should be an additive function of the degree of constructive processing at both stages. As far as I know, this has not been done before. However, we did have some indications that more constructive processing of more complex or problematic targets at the encoding stage does in fact result in superior recall (e.g., Forgas, 1992, 1995, 1998).

QUESTIONS FOR PAULA NIEDENTHAL

10. Referring back to question 7, which was posed to Gordon and Joe, why do you think that you've had success in demonstrating emotional effects on perceptual kinds of tasks, while others haven't?

I think the main reason is the very reason that motivated the research in the first place. We believed (and believe) that the spread of activation from an emotion node is likely to be detectable only for closely related information. The number of, say, positive words is enormous, and we did not think that any reasonable model would predict that a state of happiness would detectably prime all positive words, or that any negative state would detectably prime all negative words. I have talked to a number of psycholinguists who say they would be very suspicious of findings that a state of sadness detectably facilitated recognition of all negative words. Perhaps more sensitive paradigms could show this, but not our present lexical decision and word naming tasks.

Of course, I am not the only one to find emotion congruence in these low-level tasks. The British group has also observed these effects (e.g., Mathews & MacLeod, 1986; Mogg, Mathews, & Weinman, 1989). However, at least two conditions are always met: the emotions and stimuli are closely matched, and the emotional states are strong (and pure).

But there is still a potential problem, which I mentioned in response to another question. Several years ago (Niedenthal, 1992), I reviewed models (e.g., Solley & Murphy, 1960) in which the idea was clearly put forth that the percept can change the emotional state of the perceiver, which in turn can affect operations on the percept in real time. This is where we have to go in understanding emotion and perception, especially when looking at emotional faces and other

social stimuli. There will be changes in or interactions with ambient state caused by the stimuli of interest.

11. Your first set of studies showed strikingly specific *effects of emotional state on word perception: happy subjects responded faster than sad subjects to happy words (either in making lexical decisions or in naming the words aloud), and conversely, sad subjects responded faster than happy subjects to sad words (again in both the lexical decision and word naming tasks). In contrast, your second set of studies showed an equally striking but* general *effect of emotional state on categorization: subjects who are feeling either happy or sad put together both happy and sad items on the basis of their perceived emotional similarity to a much greater extent than do subjects who are experiencing a neutral mood. Would you care to elaborate on why these different patterns might have emerged?*

In the first place, the word perception tasks (especially word naming) used in the previous work are supposed to assess automatic processes (priming in this case). In contrast, the categorization tasks measure probably several processes, some of which are attentional processes (but not necessarily automatic ones). If emotions automatically prime related material, we should expect to see this in word perception tasks—and we did in five studies (involving either lexical decision or word naming). Emotions are also thought to be associated with attention to emotionally evocative elements in the world (according to most emotion theorists). This is not only a brief or automatic effect but a functional consequence of being in an emotional state. The attention to emotionally evocative stimuli and the emotional interpretations or features of external and internal stimuli (and nowhere is the argument that this attention is directed to emotionally congruent information) led us to predict a general attention to emotional features or meanings and thus a general emotion categorization effect.

Furthermore, we were influenced in this prediction by our many categorization colleagues. In their models (e.g., Nosofsky, 1992; Smith & Zarate, 1992), sensitization to a "dimension" yields uniform attention to the entire "dimension" (in quotes because this is based on multidimensional scaling language, not to be confused with the notion of a dimensional vs. categorical account of emotional experience). Thus, if someone's attention is drawn to two colors during categorization training, it is the dimension of color, and not just the particular colors involved in training, that will be used in a subsequent categorization task.

Of course, both processes can operate. We have recently completed experiments in which subjects in happy and sad states performed the triad categorization task, described in our chapter, either under substantial time pressure or under no time pressure. The expectation was that there is a time course to these

two effects, and that emotion congruence in categorization can be observed under conditions of time pressure due to the facilitated encoding of emotion congruent material. Emotion congruent categorization is the tendency by individuals in happy states to group together happy things, but not sad things, and for sad subjects to group together sad things, but not happy things, more than do control subjects.

Under conditions of no time pressure, we replicated the typical result of enhanced emotional response categorization by subjects in induced emotional states. In addition, we found that time-pressured subjects in both emotion conditions also grouped together happy concepts more than did control subjects. However, for the sad concepts, emotion congruence emerged. Under time pressure, sad individuals grouped together sad concepts more than did happy or control subjects (which did not differ between themselves). Although we are pursuing these findings further, we suspect that the difference in the findings for happy and sad triads has to do with the speed with which positive versus negative information is encoded, regardless of emotional state. As the word-recognition results reported in chapter 4 showed, words with a positive meaning are processed very quickly. Perhaps because of this basic feature of positive information, emotion congruence in the categorization of items related to happiness cannot be easily detected; the triad task is not as sensitive to encoding speed as are other tasks. On the other hand, it appeared that sad concepts were more likely to be grouped together by sad subjects. Because sad information is processed more slowly (as our word recognition work also showed), it isn't surprising that we had a greater chance to detect the influence of the automatic spread of activation in the triad task.

In any event, the first study in this new line of research suggests that tasks which assess the automatic spread of activation, and tasks which assess selective attention to features of experimenter-provided stimuli, tap into processes that are influenced in different ways by emotional states.

12. Though you clearly favor a categorical as opposed to a dimensional conceptualization of emotions, is this preference critical for understanding your current data and for planning your future research on emotional coherence?

I think that when appropriate methods are used, categorical and dimensional accounts of the subjective experience of emotion will both be supported (and they are). I also think they are not opposed to each other but reflect different methods, measures, and statistical techniques and are sensitive to different processes.

However, with available methods and tasks, at least those we use, I suspect that often the categorical organization of cognitive material can be detected more reliably. My research is guided by the model because I am convinced by the

data suggesting that there are categorical emotions. However, with regard to conceptual coherence, I think the question of whether emotional response categories are grounded in the basic emotions is an empirical question. In the end, perhaps there will be either situations or people for whom the valence grouping is preferred. I am thinking here of Lisa Feldman Barrett's (Feldman, 1995a, 1995b) work showing that some people represent their emotions internally in terms of categories while others really favor a valence representation. This may or may not be true for emotional response categories, but Lisa and I are addressing this question in collaborative research.

QUESTIONS FOR JOHN KIHLSTROM

13. In your chapter, you cite several studies using the mere exposure effect to alter affective judgments for particular stimuli and interpret their results as evidence of implicit emotion. But there's also evidence that mere exposure strongly influences judgments that are not ostensibly emotional in nature (e.g., stimulus brightness or perceived exposure duration). Does this concern you in any way?

First, that's not exactly what we say. We cite the mere exposure studies as evidence for emotion as an expression of implicit memory or implicit perception. In the mere exposure studies, I am assuming that the feeling state of "liking something" is consciously accessible, even if the current or past events giving rise to that feeling state are not.

Still, your point is well taken, and a lot of my argument relies on the assumption that the mere exposure effects on liking are emotional in nature. In one of the studies you're referring to, George Mandler and his colleagues (Mandler, Nakamura, & Van Zandt, 1987) questioned that assumption. They found that mere exposure enhanced judgments of lightness and darkness as well as of liking. Their point was that there was nothing inherently emotional about the effects of mere exposure, but that exposure produced a priming effect that facilitated performance on any stimulus-relevant task. This argument doesn't bother me too much, because I don't share Zajonc's view that the effects of exposure on emotion are direct, unmediated by "higher level" cognitive processes. And that's the view that Mandler et al. were attacking. Rather, I tend to think that exposure effects are attributional in nature: priming produces an experience of fluency, or availability, that is then interpreted in terms of liking (or whatever). This is in line with the general view, which I think Mandler shares to some degree, that emotional states can occur as a product of cognitive activity. But once you've made the judgment that you like a nonsense polygon, or a

Turkish word, or whatever, that judgment, and the feelings arising from it, probably tends to stick. In the "subliminal" case, it's still an emotional response (albeit one based on an attributional judgment) whose source is an event perceived outside of awareness—emotion as implicit perception. Frankly, I think that Mandler et al. scored some points with their study, which may be one reason why there's been so little response to it. One exception has been Seamon, McKenna, and Binder (1998), who attempted to confirm the findings of Mandler and his colleagues. Apparently at the suggestion of Bob Zajonc, Mandler et al. (1987) had also tried to show that priming could enhance judgments of disliking as well as of liking—that would have really clinched the attributional case. That didn't quite work: the priming effect on disliking was in the right direction, but not significant. Seamon et al. (1998) essentially repeated the Mandler et al. (1987) study and got priming effects on both liking and disliking judgments, but not on judgments of brightness and darkness. Their finding qualifies the attributional story somewhat, but the more important point was that it took 7 years, from 1980 to 1987, for anyone to put Zajonc's mere exposure hypothesis to a critical test, and another 11 years, from 1987 to 1998, for anyone to follow up on the results of that test. One of our purposes in writing this chapter is to get more people working on this problem so we can find out what's what.

14. Phil Merikle and his associates (Merikle, 1992; Merikle & Daneman, 1998; Merikle & Joordens, 1997) have argued that the most convincing evidence for unconscious (subliminal) perception comes from studies showing qualitative—not just quantitative—differences in task performance when a stimulus is presented above as opposed to below the subjective threshold of awareness. By extension, is there any evidence that unconscious emotions are qualitatively distinct from their conscious counterparts?

This is a reasonable position to take, though I sometimes think (though not in Merikle's case) that the requirement of a qualitative difference is an act of desperation by researchers and theorists who are made nervous by talk of consciousness, awareness, and other mentalistic constructs. Often, I think, it's a holdover from behaviorism, a reflection of what the philosopher Owen Flanagan (1991) has called "positivistic reserve." And it can lead us into trouble if we make a fetish of it.

Consider a lesson from the history of implicit memory. The distinction between explicit and implicit memory, as it was originally formulated, was essentially phenomenal: implicit memory was reflected in the effect of a past event on experience, thought, or action in the absence of conscious recollection of that event. But cognitive psychologists don't like self-reports of awareness—even though, if you think about it, virtually all of cognitive psychology is based on people's self-reports of what they perceive, think, and remember. And so

they proposed additional criteria for implicit memory, such as independence of level of processing. If explicit memory is affected by level of processing, but implicit memory is not, that's a qualitative difference that makes some people feel more comfortable about using the language of consciousness. But it turns out that implicit memory *is* affected by level of processing, although the effect is weaker than the corresponding effect on explicit memory (Brown & Mitchell, 1994; Challis & Brodbeck, 1992). And even that quantitative dissociation is questionable, because levels-of-processing studies of implicit memory have been almost exclusively confined to repetition priming, which may require only shallow, perceptual processing at the time of encoding. Semantic priming counts as implicit memory, too, and it would be surprising if semantic priming were independent of levels of processing, because it would seem to require semantic processing at the time of encoding.

The same lesson could be drawn from the history of automaticity. In the beginning some theorists had the notion that some perceptual and cognitive processes were executed involuntarily, without any conscious intention on the part of the person. But I suspect that this sole criterion for automaticity, which seems to rely on self-reports of what subjects intended to do, made them nervous. As a result, theorists piled on other criteria—for example, that automatic processes consume no attentional capacity. But again, it turns out that automatic processes do consume attentional capacity (e.g., Logan, 1997; Pashler, 1998). This shouldn't have surprised anyone, because the criteria were unreasonable to begin with. After all, even thermostats consume resources—especially after they've automatically turned on your furnace or air conditioner.

As they might have said in *Ghostbusters*, "I ain't afraid of no unconscious mind," and I'm not embarrassed to use terms like *conscious* and *unconscious*, or *voluntary* and *involuntary*, as if they meant something. It may turn out that the only qualitative difference between explicit and implicit emotion, like explicit and implicit memory, is phenomenological: the former is accessible to conscious awareness, the latter is not.

Still, if consciousness is functional and not just an afterthought, as some cognitive scientists seem to want to argue, then being unaware of something ought to have consequences for experience, thought, and action that are different from those that attend being conscious of it. For example, conscious awareness may be a logical prerequisite for conscious control. So it might be that unconscious (I prefer the term *implicit*, to continue the analogy to cognition) emotions are less subject to self-regulation. Alternatively, it may be that implicit emotion is limited to relatively primitive feeling states, what Ekman (1992) calls "basic emotions," whereas more complex emotional states, like the counterfactual emotions (regret, frustration), require so much prior cognitive analysis that they can't be elicited outside of awareness.

15. Is your hypothesis of unconscious emotional states falsifiable?

Oh, absolutely, and in precisely the same terms as the hypothesis of unconscious perceptual or memorial states is falsifiable. We know precisely how to recognize implicit memory when we see it: a change in experience, thought, or action that is attributable to a past event, in the absence of conscious recollection of that event. Once we've overcome our initial nervousness about the fact that the essential distinction between the explicit and the implicit is phenomenological—which is something I just think we have to get used to, despite our tendencies toward positivistic reserve—then we can go ahead and take whatever further methodological steps are necessary. In the case of implicit memory, that further step was to expand our definition of memory from verbal self-reports of recall and recognition to other verbal reports, such as reports of perceptual identification, or judgments of lexicality or pleasantness, or even to physiological indices like changes in skin conductance, which don't require conscious recollection on the part of the subject. We recognized these as valid, if implicit, indicators of memory because they were reliably tied to prior stimulus events.

The same thing goes for implicit emotion. We need to expand our definition of an emotional response from verbal statements like "I like it" or "it's disgusting" to other things, like facial expressions or other overt behaviors, or covert psychophysiological responses such as changes in skin conductance or heart rate. If these kinds of behaviors are reliably related to the presentation of emotional stimuli, and if they're correlated with phenomenological self-reports of emotion under ordinary circumstances, then they're candidates for implicit expressions of emotion. Then the task is to determine whether these implicit expressions of emotion can be dissociated from explicit expressions in various ways. So, for example, are there brain-damaged patients who don't feel emotions? If so, do they still respond behaviorally or physiologically in a way that we recognize as emotional? That's the hypothesis, and it's easy to see how it can be disconfirmed.

If, if, . . . there's always an "if." In order to test the hypothesis about implicit emotion, we have to have reliable and valid behavioral and physiological indices of specific emotional states—or, failing that, indices of positive or negative emotional arousal, or at least indices of general emotional arousal. In the chapter, we suggest that facial expressions of the sort studied by Ekman and his colleagues (Ekman, 1989; Ekman & Friesen, 1975) might work on the behavioral side; on the physiological side, studies by Levenson and his colleagues (Levenson, 1992; Levenson, Ekman, & Friesen, 1990) have suggested that, despite what we have been told since Cannon and Bard, and since Schachter and Singer, there might be autonomic indices of emotions after all. The more specific we can get, the better we can test the hypothesis.

I should say in conclusion that there will be some people who will seize on a chapter with a title like "The Emotional Unconscious" to say that Freud had it right all along, and that our conscious experience, thought, and action is, after all, determined by unconscious emotional states. This actually happened when I wrote my piece on "The Cognitive Unconscious" (Kihlstrom, 1987). But that's not a legitimate conclusion. The emotional unconscious revealed by laboratory research is not the psychodynamic unconscious of Freud: it's not seething with repressed sex and aggression. The only way this research can be claimed by psychoanalysis is to abandon whatever it was that is uniquely Freudian in the Freudian theory of unconscious mental life. It's a situation reminiscent of Vietnam—destroying a village in order to save it.

Far from coming from Freud, the inspiration for our chapter came from behavioral theory—from the very positivistic reserve that Owen Flanagan described. In an important article that came out after we completed our chapter, Richard Zinbarg (1998) has argued, I think correctly, that Lang's (1984) three-systems model and Rachman's (1990) notion of desynchrony were products of behavior therapists who, having read their Watson and Skinner, were extremely suspicious of self-reports and who wanted to avoid "mentalistic" references to conscious states of fear and anxiety whenever possible. And so they proposed measuring emotion "objectively," in terms of behavioral and physiological changes. And they found that behavioral and physiological changes were dissociable, to some extent, from self-reported emotional states. Thus, the radical attempt to produce a strictly behavioral and physiological analysis of emotion ends up providing the rationale for a program of research on dissociations between conscious and unconscious emotional life. It's an ironic situation, and in some respects the irony is delicious.

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The Science of Self-Report

Implications for Research and Practice

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