Memory Research: The Convergence of Theory and Practice

John F. Kihlstrom
Yale University

Concern for the practical aspects of memory can be traced back at least as far as Bartlett's (1932) critique of Ebbinghaus (1885). As we all know, Ebbinghaus had hoped to do for memory and the other higher mental processes what Fechner (1860) had done for sensation and the lower ones (frankly, I loathe these terms, because they perpetuate what I consider a false distinction, but they do provide a convenient shorthand). By his invention of the nonsense syllable, and his enforcement of what Bartlett (1932, p. 8) called "a perfectly automatic attitude of repetition in the learner," Ebbinghaus hoped to prove Kant wrong, and to show that the mind could in fact be studied with the tools of modern science. And to some extent, he was successful. The establishment of what amount to psychological laws of repetition and decay was quite an achievement for 1885.

But Bartlett was unhappy, to say the least, with Ebbinghaus' reliance on the nonsense syllable and the method of reproduction. Commenting on Ebbinghaus' attempt to strip his stimulus materials of any possible variation in meaning, he wrote: "Once more [the first time was with Fechner] the remedy is at least as bad as the disease. It means that the results of nonsense syllable experiments begin to be significant only when very special habits of reception and repetition have been set up. They may, then, throw some light upon the mode of establishment and the control of such habits, but it is at least doubtful whether they can help us see how, in general, memory reactions are determined" (1932, p. 3).

Then, after several pages of detailed criticisms of Ebbinghaus's method, Bartlett continued:

> I have dealt at this length with the nonsense syllable experiments, partly because they are generally regarded as occupying a supremely important place in the development of exact method in psychology, and partly because the bulk of this book is concerned with problems of remembering studied throughout by methods which do not appear to approach those of the Ebbinghaus school in rigidity of control. But most of what has been said could be applied, with the necessary change of terminology and reference, to the bulk of experimental psychological work on perceiving, on imaging, on feeling, choosing, willing, judging, and thinking. In it all is the tendency to overstress the determining character of the stimulus or of the situation, the effort to secure isolation of response by ensuring simplicity of external control. (1932, p. 6)

Of course, we now know that to some extent Ebbinghaus got a bad rap (Gorfein & Hoffman, 1987; Roediger, 1985; Slamecka, 1985; Tulving, 1985). Ebbinghaus had a much broader vision of memory, and a fuller appreciation of the constraints he had imposed on his own research, than he is sometimes given credit for. Ebbinghaus' achievement was not the invention of the nonsense syllable or the method of savings or even the discovery of the law of repetition; his real achievement was to show that the mind could be the object of scientific investigation, and that the combination of controlled observation and quantitative analysis could reveal the laws of mental life.

And it is also clear that Bartlett's real target was not poor Ebbinghaus himself, but rather the doctrine of associationism under which he labored. Bartlett wasn't really unhappy with the nonsense syllable. After all, he realized, as indeed Ebbinghaus did as well, that despite what the experimenter did to strip his or her materials of meaning, the subject—who after all was continually engaged in "effort after meaning" (Bartlett, 1932, p. 20)—would just put it right back in again. No, Bartlett's real target was the prevailing emphasis on the overwhelming importance of stimulus determination. Thus we get Bartlett's own doctrine, by which he attempted to save the mental in psychology against the onslaught of associationism and its evil twin, behaviorism: "The psychologist, of all people, must not stand in awe of the stimulus" (1932, p. 3).

Bartlett and his allies lost that fight, as we all know, and psychology very quickly settled down to tracking the functional relations between stimulus and response (a task that is, to some extent, still carried out by our connectionist colleagues). Fechner's Law turned into Stevens's Law. Animal learning was taken to be a satisfactory model for the human case, and was studied with a focus on the effects of different schedules of reinforcement. The study of human memory was converted into the study of verbal learning, with a concentration on interference and transfer in the acquisition of paired associates. And what we now know as the *Journal of Memory & Language* (*JML*) began life as the *Journal of Verbal Learning & Verbal Behavior* (*JVLVB*).

THE LEGACY OF THE LABORATORY

Of course, things began to change in the 1960s. In fact, the signs of change were already evident in the 1950s. From my own point of view, the signal event in the cognitive revolution in psychology—at least so far as the study of memory was concerned—was the discovery of category clustering. Bousfield (1953)—who was also one of the first to rediscover the charms of the method of free recall—observed that subjects tended to recall list items in a different order than that in which they had been presented. This was bad enough for classical association theory, but then Bousfield showed that subjects clustered list items according to superordinate, conceptual relationships that could not be predicted by the associative links between items. Bousfield's subjects were certainly not in awe of the stimulus (and neither was Bousfield, who understood perfectly well the implications of his finding). Rather, they were imposing structure on the stimulus—a structure that resided in their minds, not the environment. Of course, Bousfield built categorical relations into his word lists, and a determined environmentalist could simply say that his subjects were picking up on that structure. It was left to Tulving (1962) to clinch the point, when—in a paper that we now know he had difficulty getting published—he showed that subjects would organize a list of words into some sort of narrative (or possibly image-based) structure even if the experimenter took great pains to make sure that there were no objective interitem relations built into the list. Subjects find structure when it's there, and they impose structure when it's not—precisely the "effort after meaning" of which Bartlett wrote so affectingly.

Of course, organization theory was soon swept aside by levels of processing theory (Craik & Lockhart, 1972), but that doesn't matter. Deep processing is still something that the subject does to the stimulus, and so it only bolsters the basic point I am trying to make. The sort of associationism that Bartlett criticized in the 1930s was pretty much dead by the time the cognitive revolution was consolidated in the mid-1970s.

The cognitive revolution in psychology had quite an impact on the way we thought about memory processes. By the time *JVLVB* turned into *JML*, in 1985, the new cognitive psychology of memory had uncovered seven (plus or minus two) broad principles that characterized what was going on inside people's heads as they remembered and forgot the things that had happened to them. Barnhardt and I recently summarized these principles as follows (for full documentation, see Kihlstrom & Barnhardt, 1993; for another exposition, see Kihlstrom, 1994a; for another set of principles entirely, see Crowder, 1993):

- The Elaboration Principle: The memorability of an event increases when that event is related to preexisting knowledge at the time of encoding.

- The Organization Principle: The memorability of an event increases when that event is related to other events at the time of encoding.
- The Time-Dependency Principle: The memorability of an event declines as the length of the storage interval (i.e., between encoding and retrieval) increases.
- The Cue-Dependency Principle: The memorability of an event increases with the amount of information supplied by the retrieval cue.
- The Encoding Specificity Principle: The memorability of a event increases when the information processed at the time of retrieval was also processed at the time of encoding (or, alternatively, when the information-processing activities performed at the time of encoding are repeated at the time of retrieval).
- The Schematic Processing Principle: The memorability of an event increases when that event is relevant to expectations and beliefs about the event.
- The Reconstruction Principle: The memory of an event reflects a blend of information retrieved from specific traces encoded at the time of that event with knowledge, expectations, and beliefs derived from other sources.

I happen to think that this is not bad for 30 years' work. Only two of these principles, time-dependency and reconstruction, were well understood in Bartlett's time—and, frankly, reconstruction was not that well documented, nor for that matter accepted by anyone other than Bartlett himself. And only one of those principles that emerged subsequently, cue-dependency, even comes close to standing in awe of the stimulus. These principles reflect a thoroughgoing cognitive psychology of memory, because they move us away from stimulus structure and stimulus conditions to mental structure and processing activities—especially when you add in the details. So, for example, Hastie (1980, 1981) produced a careful analysis of the effects of mental schemata on memory, showing—quite surprisingly, I think—that events that were *in*congruent with prevailing schemata were better remembered than those that were *con*gruent. Schema-congruent events are better remembered than schema-irrelevant ones, to be sure, but the U-shaped function relating schema-relevance and memorability was a real surprise. It was definitely not what Bartlett had in mind.

Later, Hastie was able to show that the superiority of incongruent events stemmed from the subjects' attempts to explain why they occurred. And it appears that the superiority of schema-congruent events stems from the fact that the schema can serve as an internally generated retrieval cue. In the final analysis, then, the memorability of schema-incongruent events seems to reflect the elaboration principle, whereas the memorability of schema-congruent items seems to reflect the cue-dependency principle. Thus maybe there are fewer than seven principles after all. On the other hand, we might want to add other principles to characterize how events are represented in memory; thus seven plus or minus two seems like a satisfactory estimate.

Still, just as there had been signs of dissatisfaction with Ebbinghaus, there were signs of dissatisfaction with the new cognitive psychology of memory as well. Reviewing *Über das Gedächtnis* for the journal *Science*, William James was more impressed with Ebbinghaus' enterprise than with his accomplishments, writing that his laws of memory "add nothing to our gross experience of the matter" (James, 1885, p. 299). Similarly, in his keynote address at the first conference on the Practical Aspects of Memory, Neisser (1978, p. 4) offered what might be thought of as an eighth principle of memory:

- The Irrelevance Principle: If *X* is an interesting or socially significant aspect of memory, then psychologists have hardly ever studied *X*.

And then, just to rub it in, he elaborated:

> You need only tell any friend, not himself a psychologist, that you study memory. Given even a little encouragement, your friend will describe all kinds of interesting phenomena: the limitations of his memory for early childhood, his inability to remember appointments, his aunt who could recite poems from memory by the hour, the regrettable recent decline in his ability to recall people's names, how well he could find his way around his home town after a thirty years' absence, the differences between his memory and someone else's. Our research, of course, has virtually nothing to say about any of these topics. (1978, p. 5)

I have to confess that although some of this critique struck a responsive chord with me, I always thought it was too extreme. For example, I have frequently taught the phenomena of infantile and childhood amnesia in my introductory psychology classes, and I have happy memories of my students wrestling with the question of whether in fact it occurred, and if so how it might be explained. They would see immediately that we need to know whether the difficulty which a 25-year-old has in remembering events from birth to age 5 is any different, quantitatively or qualitatively, from the difficulty which a 45-year-old might have in remembering events from age 20 to 25. Assuming that this is the case, I would then remind them of the principles of memory function—the sorts of principles I outlined earlier—and they would generate plausible explanations of the amnesia in terms of them. So, for example, maybe children lack the cognitive capacity, or the knowledge base, to encode retrievable memories. Or maybe they repress them, interfering with their retrieval after they're encoded. Or maybe there is an encoding-specificity effect stemming from developmental shifts in Piagetian stage. Or maybe, as Neisser (1962) himself proposed, the world of the child doesn't supply the sort of information needed to encode memories of specific episodes. My students, then, clearly saw three things: (a) that infantile and childhood amnesia were, at least in principle, explicable in terms of broad precepts developed in the laboratory; (b) that it was possible, again at least in principle, to perform formal experiments that would determine which of these precepts actually explained the effect; and (c) that it was too damn hard to conduct the necessary experiments. But at least they could see the relevance of the theoretical principles which we discussed to practical problems of everyday memory, and they got a good exercise in testing a theoretical hypothesis.

Actually, of course, they—we—were wrong on that last point. Some of the potential explanations can be ruled out by the simple expedient of asking young children what they remember of their short lives so far. If a four-year-old can tell you what he or she did at age three, but an eight-year-old cannot, that rules out some potential explanations and supports others. Such studies are now available—for example, one by Fivush and Hammond (1990)—and they tell us clearly that preschool children are, in fact, able to encode and retain their experiences, at least under certain circumstances. And it takes nothing from the investigators to say that the studies weren't *that* hard to do, after all. All that had to happen, and it turned out to be a pretty big thing, was for someone to think the subject was important enough to devote time and effort to studying it. That it took us so long borders on the criminal, and on this point we can certainly agree with Neisser.

Moreover, the detailed study of the processes underlying young children's autobiographical memory may well tell us something theoretically interesting about memory that we did not know before. For example, pioneering studies by Nelson (1993) and Hudson (1990) have suggested that autobiographical memory develops as children and their caretakers tell each other stories about the past. Findings such as these portend the emergence of yet another theoretical principle:

- The Interpersonal Principle: Remembering is an act of communication as well as of information retrieval, and so our memories of the past are shaped by the interpersonal context in which they are encoded and retrieved.

The Interpersonal Principle is important, because it suggests that memory cannot be studied with the conceptual and methodological apparatus of cognitive psychology alone. Memory is not just a matter of the acquisition, storage, and retrieval of information. When we remember our past we are telling stories about ourselves, to ourselves and to others. These stories serve personal and social purposes, and so individual and interpersonal factors become important in determining what is remembered and what is forgotten. In his wonderful textbook of social psychology, Brown (1965) made a similar point about language: It is not just a tool of thought, it is also a means of communication. Just as linguists and psycholinguists have to pay attention to the pragmatics of language, as well as to phonology, syntax, and semantics, so students of memory must pay attention to the pragmatics of remembering and forgetting, as well as to questions about the representation and processing of knowledge.

THE SOCIAL ECOLOGY OF MEMORY

Hence memory isn't just for cognitive psychologists anymore; it's also for personality and social psychologists. Bartlett (1932) knew this, too, which is why the subtitle of *Remembering* is *A Study in Experimental and Social Psychology*. As he put it then (p. 296):

> Social organization gives a persistent framework into which all detailed recall must fit, and it very powerfully influences both the manner and the matter of recall.

Neisser (1988) made a similar point in his address to the second conference on Practical Aspects of Memory. In his view, memory emerges out of social interaction, but it also supports social interaction. Neisser lamented that so little research reported at that second conference considered memory as a social activity, and he hoped to see a lot of it at the third conference, at which we are currently gathered. Based on the presentations at this third conference, I am afraid that this particular gap is still with us. It's too bad that this is the case, because the social function of memory opens up lots of possibilities of doing interesting collaborative work.

Consider, for example, the idea that memories are not just representations of prior actions and experiences, but rather *beliefs* about our past. When we tell stories about our past, we are telling about what we believe happened, partly in order to make sense of what we are now thinking, feeling, wanting, and doing. One of the functions of remembering the past is to explain the present (Ross, 1989). This is dramatically exemplified in the virtual epidemic we are currently experiencing of exhumed memories of childhood incest, sexual abuse, and other trauma. These days, exhumed memory is a common vehicle for Neisser's (1978) nightmare scenario: If we should let slip at a cocktail party that we study memory, we are likely to be surrounded and asked to explain how massive repression, and subsequent exhumation, could occur.

And Neisser's (1978) outcome is played out: We have virtually nothing to say about this topic, for the simple reason that exhumed memory seems to violate everything we know about how memory operates (for reviews, see Kihlstrom, 1994b, 1995a, 1995b; Lindsay & Read, 1994).

Actually, in my view, this is exactly what we *should* say about this topic, and not at all defensively, putting the onus on advocates of exhumed memory to produce methodologically acceptable research to support their claims. The whole point of developing a generalized theory of memory is to have a basis for constructing informed views of phenomena that have not themselves been subject to detailed examination. A good theory is a wonderful thing to have until the experimenter comes.

Now, it could well be that a systematic study of exhumed memories will tell us something that we did not know before about how memory operates. That is certainly the hypothesis of those who are advocates for the accuracy of exhumed memories as representations of the past. These individuals, mostly clinical practitioners (some of whom have no advanced training in psychology) tell us that principles derived from laboratory studies of memory are wholly irrelevant to the case; that emotional trauma changes the principles of memory function. As evidence, they offer uncontrolled clinical anecdotes and uncorroborated self-reports from their patients. But there may be another reason why memory theory has little to say about exhumed memory: The exhumed memories may not be memories at all.

Instead, many (if not most) of the memories exhumed in the clinic appear to be *beliefs* about the past, formed as a result of persuasive communication (by therapists, e.g., or from sources in the media), and firmly held, in the absence of any actual recollection, by virtue of the power of the memories to explain the person's present circumstances—and, I believe, their value as a means of social control. Thus, for example, there is a widespread belief that anorexia, bulimia, and other eating disorders commonly occur as a result of incest or child sexual abuse—a proposition for which the evidence is in fact remarkably thin. Accordingly, individuals suffering such disorders may come to be persuaded that they were, in fact, sexually abused as children and proceed to construct memories—mental representations of the past—around such a belief. The point is that this interesting, and socially significant, phenomenon of memory cannot be explained solely in terms of principles of memory function. The only way such "memories" can be explained is in terms of principles of persuasive communication, identity formation, causal attribution, and impression management; in other words, in terms of principles of personality and social psychology, not cognitive psychology. Social psychologists are experts in how beliefs arise; how they are accepted; how they are transmitted, strengthened, and weakened; and what happens when they are challenged. Personality psychologists are experts in matters of identity and self-concept. If we are to make sense of the epidemic of exhumed memories, then, cognitive psychologists are going to have to make common cause with personality and social psychologists. This is because in the real world outside the laboratory, remembering is an act of communication, of self-presentation, and of social influence at least as much as it is the retrieval of a representation of the past (Kihlstrom, 1981; Singer & Salovey, 1993).

And we will want to go outside of psychology, to other social sciences such as history and sociology. Consider the question, initially raised by Halbwachs (1925/1980), of whether groups as well as individuals could be said to have memories. Bartlett (1932) doubted it, but he did believe that groups created stories about themselves just as individuals do, and for the same reasons: to conserve and reproduce their history, and to define their nature. Sometime later, of course, Orwell's futuristic novel *1984* explored the social and political control of memory in the service of conformity and stability. But it is not only political elites who convert memory into myth. Lifton (1967), in his moving study of the victims of the bombing of Hiroshima, noted that the people he interviewed tended to have very similar accounts of the event, regardless of their distance from the epicenter at the time of the explosion. Something similar might have happened in returning prisoners of war and others who fought in Vietnam.

Our colleagues in academic departments of history are very interested in collective memory and other aspects of social memory, and a few years ago the *Journal of American History* devoted a special issue (Vol. 75, No. 4, March 1989) to the problems of memory and history, which are particularly acute for those historians who rely on oral materials. The issue included an analysis by McGlone (1989) of how John Brown's children reshaped their memories of their father to create a family identity in the decades following the Harper's Ferry raid. In addition, Bodnar (1989) grappled with the differences between workers' and managers' stories of life at the Studebaker plant in South Bend, Indiana. And Thelen (1989b) provided an analysis of the memories of those involved in the discovery of the Watergate tapes, showing how each participant shaped and reshaped his story over time depending on the circumstances of the moment, while believing that his memory was accurate and unchanging. As Thelen (1989a) noted in his editorial introduction to the Special Issue:

> The fresh possibilities in the historical study of memory begin with two starting points, deeply embedded in historians' narrative traditions. . . . The first is that memory, private and individual as much as collective and cultural, is constructed, not reproduced. The second is that this construction is not made in isolation but in conversations with others that occur in the contexts of community, broader politics, and social dynamics. (p. 1119)

Collective memory is something that we psychologists haven't begun to study. But when we get around to it, I am sure that we will discover that a purely psychological analysis—that is, an analysis solely in terms of the individual's mental states—will be completely inadequate. In order to understand collective memory, we are going to have to understand how collectivities operate; and for that, we are going to have to consult our colleagues in sociology, anthropology, history, and political science.

THE REAL WORLD AND THE LABORATORY

In his address to the second Practical Aspects of Memory conference, Neisser (1988) repealed the Irrelevance Principle that he had announced almost a decade earlier. In contrast to the earlier situation, there were now (in his view) quite a few people engaged in studying interesting or socially important phenomena of memory. Based on the talks given at the present conference, one would have to say that the practical aspects of memory constitute a growth industry within psychology.

At the same time, there have arisen the inevitable critiques, of which the most prominent was that of Banaji and Crowder (1989, 1991). These authors agreed that realism is preferable to artificiality, so long as methodological rigor can be preserved. But they also argued that ecological validity neither guaranteed generalizability nor substituted for methodological soundness. They expressed doubt that studies of memory in the real world would provide information that was unavailable in the laboratory, and cautioned investigators against abandoning the precision of the laboratory in favor of mundane realism.

Banaji and Crowder's article unleashed a firestorm of protest, but frankly I think that their fundamental point is incontrovertible: It simply is not possible to learn anything about memory, *qua* memory, unless there is careful control, experimental or statistical, over the conditions of encoding, storage, and retrieval. Let me illustrate with some work from my own laboratory.

My first illustration comes from a study on autobiographical memory in a case of multiple personality disorder (Schacter, Kihlstrom, Canter Kihlstrom, & Berren, 1989). The patient, whom we called I.C., was a 24-year-old college-educated woman with a world-class talent and at least five alter egos, one of whom was a suicidal adolescent. A prominent characteristic of the dominant personality (by which I mean the one that had been known for the longest period of time to the most people) was a very dense amnesia covering the first 10 to 14 years of her life. We were able to confirm this amnesia using the Crovitz-Robinson technique, in which words are used to cue the retrieval of autobiographical memories (Crovitz & Schiffman, 1974; Robinson, 1976). In an unconstrained version of the technique, she showed a strong recency effect, with very few memories from before age 14. And when she was constrained to report memories only from the first 12 years of life, she displayed a huge number of response failures, and produced nothing at all from before age 10. This was all very interesting, and confirmed her therapist's informal assessment of memory, but without control subjects we had no idea how to interpret this effect. It turned out that control subjects, matched to I.C. for sex, age, and education, had plenty of memories from before age 14 in the unconstrained condition, and few response failures and plenty of memories from before age 10, in the constrained condition. So there really was an amnesia there after all.

Still, that was as far as we could go. Without knowledge of encoding conditions, we were left puzzled as to what it all meant. There was some evidence of childhood sexual abuse in this case, and it is possible—if one believed in such things—that I.C.'s amnesia resulted from a massive repression (or dissociation) of childhood experience from conscious recollection. Another, more intriguing possibility, was that I.C., whom we all considered to be the original personality, might actually be an alter ego who emerged when the patient was about 10 years old chronologically. The lack of memories from before age 10 would be consistent with this hypothesis, and the paucity of memories from ages 10 to 14 might reflect normal infantile and childhood amnesia affecting this newly emerging personality. It's an intriguing idea, fun to play with. But without detailed knowledge of what this person was like at that time, we'll never be able to make sense of her pattern of results. So, in the final analysis, both the practical and the theoretical significance of the case was limited by the constraints on our ability to control the conditions of encoding and storage as well as the conditions of retrieval.

Another example comes from work in my laboratory on the phenomenon of posthypnotic amnesia—the inability of highly hypnotizable subjects to remember, after hypnosis, the events and experiences that transpired while they were hypnotized (Kihlstrom, 1985). Posthypnotic amnesia occurs only if it is specifically suggested to the subject, and it can be reversed by a prearranged cue, without reinduction of hypnosis, so it is not an instance of state-dependent memory. The fact that it can be reversed at all indicates that, in contrast to the organic amnesic syndrome, whatever is going on operates at the retrieval stage of memory processing.

In the present context, posthypnotic amnesia is especially interesting because it is a phenomenon of memory that occurs naturally in the laboratory. By using standardized hypnotic procedures, in which subjects receive an induction of hypnosis accompanied by a series of test suggestions, all administered verbatim according to a prepared script and evaluated according to objective behavioral criteria, we know exactly what was said to the subjects, and how they responded, at every moment of the procedure. Thus a subject's memory for his or her experience of hypnosis might be a particularly lifelike laboratory model for studying autobiographical memory. Posthypnotic amnesia is also something that is not easy to understand in terms of the general principles I outlined earlier, so it promises to tell us something new about how memory works.

In one line of research, Evans and I were interested in the organization of memory for hypnotic experiences (Evans & Kihlstrom, 1973; Kihlstrom & Evans, 1979). I had fallen under the spell of organization theory in memory, and Evans and I had the idea that suggestions for posthypnotic amnesia might somehow disrupt the organization of retrieval processes, and thus render the memories temporarily inaccessible. Specifically, we thought that temporal sequencing was the natural form of organization for autobiographical memories, and that it was particularly vulnerable to the amnesic process. We tested this hypothesis by correlating the order in which hypnotic subjects recalled their experiences with the order in which those experiences had actually occurred during the standardized procedure, to get a measure of temporal sequencing in recall. There was a little trick in the study: We could not use our best subjects because they showed a dense amnesia, and one cannot study the organization of recall in subjects who do not remember anything. So we threw these subjects out of the experiment, and looked at temporal sequencing in the rest, testing the hypothesis that the recall of highly hypnotizable subjects, who are at least partially responsive to hypnotic suggestions, will be less organized than that of insusceptible subjects, who do not respond to them at all.

In fact, Evans and I got the temporal disorganization effect in several different studies. When the amnesia suggestion was in effect, there was less temporal sequencing in hypnotizable than insusceptible subjects. And when we eliminated the amnesia suggestion, hypnotizable and insusceptible subjects showed equal levels of temporal sequencing. Still, there were some problems. Some colleagues were highly critical of the experiments, precisely because they lacked certain experimental controls: There was no assessment of initial acquisition, for example; furthermore, the memory task was somewhat ambiguous, so it may have been unclear to the subjects what they were supposed to remember. Because of our reliance on a standardized procedure, which was by definition ecologically valid but had not been devised with this experiment in mind, we were unable to examine the fate of temporal sequencing after the amnesia suggestion was canceled. And finally, temporal sequencing was the only form of organization that we could study in the context of the standardized scales, so we couldn't test our hypothesis about temporal sequencing against alternative hypotheses concerning other forms of organization.

None of these issues could be settled within the (relatively) lifelike context of the standardized scales, and so we were thrown back on our old friend the verbal learning experiment, in which words serve as analogues of episodes of experience. Wilson and I (Kihlstrom & Wilson, 1984; Wilson & Kihlstrom, 1986) hypnotized subjects and then asked them to memorize a list of words. In this experiment we employed an incremental learning technique that virtually guaranteed that subjects would organize list items in temporal order. In another experiment, we used standard free recall learning but with a categorized word list, virtually guaranteeing that category clustering would occur. And in a third experiment, we again used free recall but with a list of unrelated words, thus forcing subjects to impose a subjective organization on the list. The results of the three experiments, taken together, nicely supported our initial hypotheses: When subjects organize experience temporally, temporal sequencing is disrupted during posthypnotic amnesia and restored when it is canceled. However, when subjects organize their experiences by conceptual or other meaning-based relationships, this organization is not disrupted during posthypnotic amnesia (for a fuller discussion, see Kihlstrom & Wilson, 1988). The amnesic process has a particular impact on the temporal relationships among memories—an important clue, I think, to the nature of the amnesic process itself.

The point is that in both cases, the most lifelike settings were not necessarily the most appropriate for addressing questions of theoretical interest. But don't misunderstand me. I'm not saying that the lifelike studies shouldn't have been done. They should have been. The point is that neither traditional nor ecological approaches to the study of memory have any privileged access to virtue. Each has its assets, and each has its liabilities (Winograd, 1988). When we first learned about I.C., we wanted to do a series of fairly traditional experiments, looking for evidence of "ego-state"-dependent memory, dissociations between explicit and implicit memory, and the like. But we couldn't get experimental control over her various personalities, and so we had to settle for a study of autobiographical memory in one of her personalities. The findings were interesting, but our inability to get experimental control over the situation prevented us from going very far. In the case of posthypnotic amnesia, I am certain that if we had begun with a traditional, verbal-learning study we would probably have looked at category clustering first, failed to find an effect, turned our attention somewhere else, and missed entirely the effects on temporal sequencing. By starting out in the naturalistic setting of the standardized scales, we discovered something interesting that we otherwise might have missed. But we were only *sure* it was interesting after we had translated the naturalistic setting into a laboratory analogue where we could get tighter control over potentially confounding variables.

THE LESSONS OF EYEWITNESS MEMORY

I think this is a common scenario. For example, it is replayed constantly in the study of eyewitness memory, one of the undisputed success stories of practical memory.

Consider first the question of memory for faces, as exemplified by the host of very interesting studies of accuracy versus confidence; biases in lineups, showups, and photospreads; cross-race accuracy in identification; and the like (for a review, see Ellis & Shepherd, 1992). The practical problem is this: Can witnesses and victims of crimes reliably identify perpetrators, live or depicted, sometime after a crime has occurred? Or, more prosaically perhaps, how well do people attach names to faces at cocktail parties? Up until 20 years ago, there wasn't much literature on this topic. There was, of course, Shepard's (1967) classic work on picture recognition, showing that memory for visual (as opposed to verbal) materials persists for a pretty long time. And, as far as putting names and faces together goes, there was that 50-year tradition of paired-associate learning. Memory for picture postcards (and for postcard-nonsense syllable paired associates) might have been taken as a satisfactory laboratory analogue of memory for faces, but nobody thought so, and for good reason: Faces are special. They are the primordial social stimulus. Babies seem to be built to find them and look at them. Studies of prosopagnosics indicate that there may be particular brain structures specialized for processing them. Thus we should accept no substitutes: We can only study memory for faces by studying memory for faces.

And in doing so, investigators imposed rigorous experimental controls on the practical question, controls that are the equal of anything that was ever done to a sophomore in a laboratory cubicle. The best studies of face memory conducted in lifelike, ecologically valid settings leave nothing to chance, they are completely controlled from beginning to end. They are, to all intents and purposes, indistinguishable from traditional laboratory experiments, except perhaps that the subjects are clerks at the local convenience store instead of volunteers from a subject pool.

Moreover, investigators turned quite quickly from purely practical questions, such as those that might be raised by judges and attorneys, to theoretical questions that are as esoteric as anything dreamed up by the inventors of ACT* or PDP. Consider, for example, the information-processing model of face recognition, proposed by Bruce and Young (1986) on the basis of both laboratory and neuropsychological studies. This is an interesting theoretical model, but I dare say that there's nothing very *practical* about it. And since this model was proposed, it has generated dozens of traditional laboratory experiments intended to test and revise its details.

Another success story from the annals of eyewitness memory research is the postevent misinformation effect documented by Loftus and her colleagues. In a study that has become a modern classic, Loftus and Palmer (1974) showed subjects a film of an automobile accident, and then asked them questions about details. By means of leading questions, the investigators were able to manipulate subjects' estimates of how fast the cars were moving. But later on, those subjects in the "fast" condition proved more likely to report broken glass than did those in the "slow" condition—when, in fact, no broken glass had been shown in the film at all. Hence subjects' memories were distorted by events occurring over the retention interval. Loftus and others have published lots of other demonstrations of the misinformation effect, usually using very lifelike stimulus materials, and nobody seems to doubt it.

Of course, there has arisen quite a controversy about the precise nature of the misinformation effect. Originally, Loftus (e.g, Loftus & Loftus, 1980) had proposed that the social construction might overwrite the original based on personal experience, which is just lost. McCloskey and Zaragoza (1985), on the other hand, concluded that the misinformation did not displace the original memory, but rather biased the memory reports of subjects who had forgotten the original for other reasons. Tversky and Tuchin (1989) attempted a compromise position, in which the two memories existed side by side, the latter interfering with retrieval of the former. So did Metcalfe (1990), who argued that the two memories are blended into a single representation that permits either one to be retrieved, depending on the circumstances. Zaragoza and Koshmider (1989) and Lindsay and Johnson (1989) suggested that the two memories might become confused because people forget their sources.

The point is that Loftus began with a very practical question that was, in fact, settled quite quickly. Yes, eyewitness memory can be distorted by leading questions. I think that everyone accepts this conclusion. Of course, the reason that everyone accepts this conclusion is that Loftus constructed her experiments very carefully, according to the traditional canons of experimental design. Her experiments were compelling because all the proper controls were in place. Put another way, she imposed laboratory conditions on a lifelike setting. Then the field turned its attention to strictly theoretical propositions about how this distortion occurred. That's what the Loftus–McCloskey debate is all about. And in the process of addressing these purely theoretical questions, the field turned to laboratory experiments of a quite traditional sort. Experiments in the McCloskey–Zaragoza vein, for example, are formally indistinguishable from the studies of modified and modified-modified free recall by which Postman and Underwood explored interference processes in paired-associate verbal learning. What goes around, comes around, in psychology as in the rest of life.

THE PARALLEL, DISTRIBUTED STUDY OF MEMORY

In some sense, the debate among memory researchers between theory and practice, and between the laboratory and the real world, is reminiscent of the "crisis" that pervaded social psychology two decades ago (e.g., Elms, 1975; Gergen, 1973; Smith, 1972). The causes of this crisis were complex, including such factors as mundane as the discovery of demand characteristics and experimenter bias and as monumental as racism and the Vietnam War, but there too the debate revolved around questions of relevance, the comparative assets and liabilities of laboratory and field research, the overreliance on college students as subjects, the question of experimentation versus description, and whether it was possible to produce a general account of social behavior that would transcend time and place (for an overview, see Jones, 1985). McGuire (1973, p. 447) captured the essence of the debate as follows:

> During the past several years both the creative and the critical aspects of [experimental social psychology] have come under increasing attack. The creative aspect of formulating hypotheses for their relevance to theory has been denounced as a mandarin activity out of phase with the needs of our time. It has been argued that hypotheses should be formulated for their relevance to social problems rather than for their relevance to theoretical issues. . . .
>
> At least as strong and successful an assault has been launched on . . . the notion that hypotheses should be tested by manipulational laboratory experiments. . . .
>
> In place of the laboratory manipulational experiment, there has been a definite trend toward experiments conducted in field settings and toward correlational analysis of data from naturalistic situations. . . .

McGuire (1969, 1973), for his part, foresaw a future paradigm for social psychology that would involve a greater balance between laboratory and field research, but that would still be oriented toward general theory rather than practical action. As he put it (1969, p. 22):

> What I am urging and predicting is that we correct the current, almost exclusive emphasis on this method by continuing the present level of laboratory manipulational work, but in addition upgrade in quantity and quality the use of natural field settings to test our basic, theoretically derived hypotheses. I am not suggesting that we abandon the physical science paradigm and stop acting like physicists. I am urging that occasionally we also start acting a bit like astronomers.

If, as Lewin argued, there is nothing so practical as a good theory, there is also nothing as good for theory as a little practicality.

One can say that as in the psychology of interpersonal relations, so in the psychology of memory. The practical memory movement, which began a decade and a half ago as a breakaway faction, or perhaps an insurgent force, has contributed much to our knowledge of memory. Investigators who were once exclusively concerned with theoretical issues and satisfied with studies of college students are now more aware of, and more concerned with, problems of practical application than they were before. And the study of memory has been opened up to new settings and new populations, compared to the norms of a decade or two ago. The practical memory movement can't claim exclusive responsibility for these changes—cognitive neuroscience, itself pretty esoteric and, except in the hands of a few investigators, not very practical, has also played an important role in these developments, as has personality and social psychology. But it can claim its fair share, and there is plenty of honor to go around.

At the same time, I sense that the breakaway or insurgent aspects of the movement are diminishing in intensity. There is real convergence occurring, in my view, and it is the result of movement on both sides. Traditional laboratory researchers are more open than they were before to what can be learned from real-life settings, special populations, and practical questions. Practical memory researchers are more interested in observing the methodological niceties, and in connecting their phenomena to more general theories, than they ever have been before.

This has got to be good for the field, in both the short and long run, because, frankly, I don't think we're headed for a situation where we'll have one set of theoretical principles to explain memory performance in the laboratory and another set to explain memory performance in the field—or, worse yet, a sort of situated memory theory in which there is a different set of principles for every different situation in which remembering occurs. Rather, I suspect that we are heading toward a situation characterized by the parallel, distributed study of memory. That is to say, theoretical and practical research, conducted in the laboratory and the real world, will proceed forward in parallel. But where once these goals and venues might have appealed to different constituencies, I think that now individual investigators will be more interested in distributing their attention more evenly across the two streams, working now on some theoretical issue, now on some practical one, now in the laboratory, now in the world outside. The result will be, I think, not an eschewing of theory but rather a real contribution of practical studies to theoretical principles.

And if I had to make a bet, it would be that the new principles would look like the Interpersonal Principle. In my view, the greatest achievement of practical memory is to remind us that the individual's mind operates in a social and cultural context. Social factors do not alter the basic principles of memory function, but as Bartlett (1932) suggested, they do affect how those principles will be instantiated. Practical memory is memory in action, and social psychologists are experts in studying mind in action. And, so, I think the greatest theoretical contribution of practical memory will come from linking cognitive psychology to personality and social psychology, and linking psychology to the other social sciences, including those concerned with the empirical evaluation of public policy (because, e.g., the cognitive and social psychology of eyewitness memory has obvious bearing on the rules of evidence pertaining to memory and testimony). When we come together for the Fourth Conference, some years hence, I hope that we will see some of those principles emerging.

ACKNOWLEDGMENTS

Closing Lecture presented at the Third Practical Aspects of Memory Conference, University of Maryland, College Park, August 5, 1994. The point of view represented here is based on research supported by Grant #MH-35856 from the National Institute of Mental Health. I thank Lawrence Couture, Elizabeth Glisky, Martha Glisky, Tim Hubbard, Katherine Insell, Shelagh Mulvaney, Victor Shames, Susan Valdiserri, and Michael Valdiserri for their comments. In this regard, special thanks are due to Chris Herzog for his detailed comments on an earlier draft.

REFERENCES

Banaji, M. R., & Crowder, R. G. (1989). The bankruptcy of everyday memory. *American Psychologist, 44,* 1185–1193.

Banaji, M. R., & Crowder, R. G. (1991). Some everyday thoughts on ecologically valid methods. *American Psychologist, 46,* 78–79.

Bartlett, F. C. (1932). *Remembering: A study in experimental and social psychology.* Cambridge, England: Cambridge University Press.

Bodnar, J. (1989). Power and memory in oral history: Workers and managers at Studebaker. *Journal of American History, 75,* 1201–1221.

Bousfield, A. K. (1953). The occurrence of clusterings in the recall of randomly arranged associates. *Journal of General Psychology, 49,* 229–240.

Brown, R. (1965). *Social psychology.* New York: Free Press.

Bruce, V., & Young, A. (1986). Understanding face recognition. *British Journal of Psychology, 77,* 305–327.

Craik, F. I. M., & Lockhart, R. S. (1972). Levels of processing: A framework for memory research. *Journal of Verbal Learning & Verbal Behavior, 11,* 671–684.

Crovitz, H. F., & Schiffman, H. (1974). Frequency of episodic memories as a function of their age. *Bulletin of the Psychonomic Society, 4,* 517–518.

Crowder, R. G. (1993). Systems and principles in memory theory: Another critique of pure memory. In A. F. Collins, S. E. Gathercole, M. A. Conway, & P. E. Morris (Eds.), *Theories of memory* (pp. 139–161). Hove, England: Lawrence Erlbaum Associates.

Ebbinghaus, H. (1885). *Memory: A contribution to experimental psychology.* Leipzig: Duncker & Humblot.

Ellis, H. D., & Shepherd, J. W. (1992). Face memory: Theory and practice. In M. M. Gruneberg & P. Morris (Eds.), *Aspects of memory: Vol. 1. The practical aspects* (pp. 51–85). London: Routledge.

Elms, A. C. (1975). The crisis of confidence in social psychology. *American Psychologist, 30,* 967–976.

Evans, F. J., & Kihlstrom, J. F. (1973). Posthypnotic amnesia as disrupted retrieval. *Journal of Abnormal Psychology, 82,* 317–323.

Fechner, G. T. (1860). *Elements of psychophysics* (2 vols.). Leipzig, Germany: Breithaus & Hartel.

Fivush, R., & Hammond, N. R. (1990). Autobiographical memory across the preschool years: Toward reconceptualizing childhood amnesia. In R. Fivush & J. A. Hudson (Eds.), *Knowing and remembering in young children* (pp. 223–248). New York: Cambridge University Press.

Gergen, K. J. (1973). Social psychology as history. *Journal of Personality and Social Psychology, 26*, 309–320.

Gorfein, D. S., & Hoffman, R. R. (Eds.). (1987). *Memory and learning: The Ebbinghaus Centennial Conference.* Hillsdale, NJ: Lawrence Erlbaum Associates.

Halbwachs, M. (1980). *The collective memory.* New York: Harper & Row. (Original work published 1925)

Hastie, R. (1980). Memory for behavioral information that confirms or contradicts a personality impression. In R. Hastie, T. M. Ostrom, E. B. Ebbesen, R. S. Wyer, D. L. Hamilton, & D. E. Carlston (Eds.), *Person memory: The cognitive basis of social perception* (pp. 155–178). Hillsdale, NJ: Lawrence Erlbaum Associates.

Hastie, R. (1981). Schematic principles in human memory. In E. T. Higgins, C. P. Herman, & M. P. Zanna (Eds.), *Social cognition* (pp. 39–88). Hillsdale, NJ: Lawrence Erlbaum Associates.

Hudson, J. A. (1990). The emergence of autobiographical memory in mother–child conversation. In R. Fivush & J. A. Hudson (Eds.), *Knowing and remembering in young children* (pp. 166–196). New York: Cambridge University Press.

James, W. (1885). Experiments in memory. *Science, 6*, 198–199.

Jones, E. E. (1985). Major developments in social psychology during the past five decades. In G. Lindzey & E. Aronson (Eds.), *Handbook of social psychology*, 3rd ed. (Vol. 1, pp. 47–107). New York: Random House.

Kihlstrom, J. F. (1981). On personality and memory. In N. Cantor & J. F. Kihlstrom (Eds.), *Personality, cognition, and social interaction.* Hillsdale, NJ: Lawrence Erlbaum Associates.

Kihlstrom, J. F. (1985). Posthypnotic amnesia and the dissociation of memory. In G. H. Bower (Ed.), *The psychology of learning and motivation* (Vol. 19, pp. 131–178). New York: Academic Press.

Kihlstrom, J. F. (1994a). Delayed recall and the principles of memory. *International Journal of Clinical & Experimental Hypnosis, 42*, 337–345.

Kihlstrom, J. F. (1994b). Exhumed memory. In S. J. Lynn & N. P. Spanos (Eds.), *Truth in memory.* New York: Guilford.

Kihlstrom, J. F. (1995a). Suffering from reminiscences: Exhumed memory, implicit memory, and the return of the repressed. In M. A. Conway (Ed.), *Recovered memories and false memories.* Oxford: Oxford University Press.

Kihlstrom, J. F. (1995b). The trauma-memory argument. *Consciousness & Cognition, 4*, 63–67.

Kihlstrom, J. F., & Barnhardt, T. M. (1993). The self-regulation of memory, for better and for worse, with and without hypnosis. In D. M. Wegner & J. W. Pennebaker (Eds.), *Handbook of mental control* (pp. 88–125). Englewood Cliffs, NJ: Prentice-Hall.

Kihlstrom, J. F., & Evans, F. J. (1979). Memory retrieval processes in posthypnotic amnesia. In J. F. Kihlstrom & F. J. Evans (Eds.), *Functional disorders of memory* (pp. 179–218). Hillsdale, NJ: Lawrence Erlbaum Associates.

Kihlstrom, J. F., & Wilson, L. (1984). Temporal organization of recall during posthypnotic amnesia. *Journal of Abnormal Psychology, 93*, 200–208.

Kihlstrom, J. F., & Wilson, L. (1988). Rejoinder to Spanos, Bertrand, and Perlini. *Journal of Abnormal Psychology, 97*, 381–383.

Lifton, R. J. (1967). *Death in life: Survivors of Hiroshima.* New York: Random House.

Lindsay, D. S., & Johnson, M. K. (1989). The eyewitness suggestibility effect and memory for source. *Memory & Cognition, 17*, 349–358.

Lindsay, D. S., & Read, J. D. (1994). Psychotherapy and memories of childhood sexual abuse: A cognitive perspective. *Applied Cognitive Psychology, 8*, 281–338.

Loftus, E. F., & Loftus, G. R. (1980). On the permanence of stored information in the human brain. *American Psychologist, 35*, 409–420.

Loftus, E. F., & Palmer, J. C. (1974). Reconstruction of automobile destruction: An example of the interaction between memory and language. *Journal of Verbal Learning & Verbal Behavior, 13*, 585–589.

McCloskey, M., & Zaragoza. M. S. (1985). Misleading postevent information and memory for events: Arguments and evidence against memory impairment hypothesis. *Journal of Experimental Psychology: General, 114*, 381–387.

McGlone, R. E. (1989). Rescripting a troubled past: John Brown's family and the Harpers Ferry conspiracy. *Journal of American History, 75*, 1179–1200.

McGuire, W. J. (1969). Theory-oriented research in natural settings: The best of both worlds for social psychology. In M. Sherif & C. W. Sherif (Eds.), *Interdisciplinary relationships in the social sciences* (pp. 21–51). Chicago: Aldine.

McGuire, W. J. (1973). The yin and yang of progress in social psychology: Seven koan. *Journal of Personality & Social Psychology, 26*, 446–456.

Metcalfe, J. (1990). Composite holographic associative recall model (CHARM) and blended memories in eyewitness testimony. *Journal of Experimental Psychology, 119*, 145–160.

Nelson, K. (1993). The psychological and social origins of autobiographical memory. *Psychological Science, 4*, 7–14.

Neisser, U. (1962). Cultural and cognitive discontinuity. In T. E. Gladwin & W. Sturtevant (Eds.), *Anthropology and human behavior* (pp. 54–71). Washington, D.C.: Anthropological Society of Washington.

Neisser, U. (1978). Memory: What are the important questions? In M. M. Gruneberg, P. E. Morris, & R. N. Sykes (Eds.), *Practical aspects of memory* (pp. 3–24). London: Academic Press.

Neisser, U. (1988). Time present and time past. In M. M. Gruneberg, P. E. Morris, & R. N. Sykes (Eds.), *Practical aspects of memory: Current research and issues. Vol. 2. Clinical and educational implications* (pp. 545–560). Chichester, England: Wiley.

Robinson, J. A. (1976). Sampling autobiographical memory. *Cognitive Psychology, 8*, 578–595.

Roediger, H. L. (1985). Remembering Ebbinghaus. *Contemporary Psychology, 30*, 519–523.

Ross, M. (1989). Relation of implicit theories to the construction of personal histories. *Psychological Review, 96*, 341–357.

Schacter, D. L., Kihlstrom, J. F., Canter Kihlstrom, L., & Berren, M. B. (1989). Autobiographical memory in a case of multiple personality disorder. *Journal of Abnormal Psychology, 98*, 508–514.

Shepard, R. N. (1967). Recognition memory for words, sentences, and pictures. *Journal of Verbal Learning & Verbal Behavior, 6*, 156–163.

Singer, J. A., & Salovey, P. (1993). *The remembered self: Emotion and memory in personality.* New York: Free Press.

Slamecka, N. J. (1985). Ebbinghaus: Some associations. *Journal of Experimental Psychology: Learning, Memory, & Cognition, 11*, 414–435.

Smith, M. B. (1972). Is experimental social psychology advancing? *Journal of Experimental Social Psychology, 8*, 86–96.

Thelen, D. (1989a). Memory and American history. *Journal of American History, 75*, 117–129.

Thelen, D. (1989b). Remembering the discovery of the Watergate tapes: Introduction. *Journal of American History, 75*, 1222–1227.

Tulving, E. (1962). Subjective organization in free recall of "unrelated" words. *Psychological Review, 69*, 344–354.

Tulving, E. (1985). Hermann Ebbinghaus's memory: What did he learn and remember? *Journal of Experimental Psychology: Learning, Memory, & Cognition, 11*, 485–490.

Tversky, B., & Tuchin, M. (1989). A reconciliation of the evidence on eyewitness testimony: Comments on McCloskey and Zaragoza. *Journal of Experimental Psychology: General, 118*, 142–147.

Wilson, L., & Kihlstrom, J. F. (1986). Subjective and categorical organization of recall in posthypnotic amnesia. *Journal of Abnormal Psychology, 95*, 264–273.

Winograd, E. (1988). Continuities between ecological and laboratory approaches to memory. In U. Neisser & E. Winograd (Eds.), *Remembering reconsidered: Ecological and traditional approaches to the study of memory* (pp. 11–20). Cambridge, England: Cambridge University Press.

Zaragoza, M. S., & Koshmider, J. W. (1989). Misled subjects may know more than their performance implies. *Journal of Experimental Psychology: Learning, Memory, & Cognition, 118*, 246–255.

Basic and Applied Memory Research Theory in Context Volume 1

Edited by

Douglas J. Herrmann
Indiana State University

Cathy McEvoy
University of South Florida

Christopher Hertzog
Georgia Institute of Technology

Paula Hertel
Trinity University

Marcia K. Johnson
Princeton University

LEA LAWRENCE ERLBAUM ASSOCIATES, PUBLISHERS
1996 Mahwah, New Jersey

Copyright © 1996 by Lawrence Erlbaum Associates, Inc.
All rights reserved. No part of this book may be reproduced in any form, by photostat, microfilm, retrieval system, or any other means, without the prior written permission of the publisher.

Lawrence Erlbaum Associates, Inc., Publishers
10 Industrial Avenue
Mahwah, New Jersey 07430

Cover design by Gail Silverman

Library of Congress Cataloging-in-Publication Data

Basic and applied memory research / Douglas Herrmann . . . [et al.], editors.
p. cm.
Includes bibliographical references and index.
Contents: v. 1. Theory in context — v. 2. Practical applications.
ISBN 0-8058-1542-2 (cloth : alk. paper). — ISBN 0-8058-1543-0 (pbk. : alk. paper)
1. Memory. 2. Memory—Research. I. Herrmann, Douglas J.
BF371.B27 1996
153.1'2—dc20 95-40765
CIP

Books published by Lawrence Erlbaum Associates are printed on acid-free paper, and their bindings are chosen for strength and durability.

Printed in the United States of America
10 9 8 7 6 5 4 3 2 1