

# 10 **The Future of Cognitive Psychology?**

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My task in this chapter is admirably straightforward: to predict the future. Actually, the authors were given three interwoven tasks: (1) to predict the future of science as a whole, (2) to predict the future of psychology, and (3) to predict the future of our own discipline (which in my case is cognitive psychology, and more particularly human learning and memory). The agenda is ambitious, and the fact that I don't have the foggiest specific idea about the future of any of these three topics will not deter me in the least. After all, it is easier to write science fiction than to write science. In the latter case one is usually constrained by such mundane features as data, theory, the plausibility of one's statements, and so on. For an essay like the present one, these normal moorings on reality can be untied; we are permitted to speculate freely. The fact that most of us probably had given the distant future little thought before being set this task will also not deter us, although it might give pause to the reader. I am reminded of what P. B. Medawar (1969) wrote about scientists' approach to method: "Ask any scientist what he conceives the scientific method to be, and he will adopt an expression that is at once solemn and shifty-eyed; solemn, because he feels he ought to declare an opinion, shifty-eyed because he is wondering how to conceal the fact that he has no opinion to declare. If taunted he would probably mumble something about 'Induction' and 'Establishing the Laws of Nature,' but if anyone working in a laboratory professed to be trying to establish the Laws of Nature by induction we should begin to think he is overdue for leave."

So I begin my essay here in a shifty-eyed, but not particularly solemn, mood. We may be solemn about the scientific method, but there is no reason to be so when contemplating the future. The history of attempts to predict the future is comical, at best, and there is no reason to think that the writers of this volume will do better than those in the past. The future is as unpredictable as it

is unknowable, so we may as well enjoy ourselves when thinking about it. Attempts to predict the future with a solemn mien would only make our efforts look more pathetic in the future, should anyone dip back into this book and its predecessor to see how far off the mark we were.

My aim here is to take a look back and then a look forward. I will first ask how well I (or a scholar of the time) might have predicted the future had I been alive and asked to do so at various periods in the past. Then, with the thoughts aroused by this exercise firmly in mind, I venture to write about the future.

### **A Look Backward in Time**

The title of this book provides grand ambition, as it includes the millenarian theme already being overused in contemplating the year 2000. (This overuse is apparent already, even though I write at the end of 1995.) What does the 21st century hold for us? Before contemplating possible answers to this question, let us consider, briefly, what possibility there would have been to predict where we are today from the past. I will consider three epochs: 1000 years, 100 years, and 25 years.

Of course, the biases of hindsight are well known. As the saying goes, *hindsight is 20/20 and we all are great experts in telling why, after the fact*, some event must have occurred. Experimental psychologists have repeatedly documented that people can easily explain, after the fact, the occurrence of events and then believe that they would have predicted them. However, controlled studies show they could not predict the events, even the ones whose outcomes seem obvious in retrospect (Fischhoff, 1975; Wood, 1978). Because of this knowledge, I doubt that anyone today could have accurately predicted the course of the future in my field—the cognitive psychology of learning and memory—at any point even in the relatively near past (say 25 years ago or more), even in broad terms. But let us first consider longer periods of time, in keeping with the millenarian theme of this book.

Imagine a European scholar, probably a monk or another ecclesiastical scholar, who had some interest in how the mind learns and remembers, writing in A.D. 1000 to predict where scholarship may lie in 1000 years. Who can imagine what this mythical scholar may have thought? If we are charitable, we can imagine that, at best, he (and it probably would have been a male) may have thought that the future would lie in the art of perfecting memory. Be-

cause medieval scholars knew of mnemonic systems, he would probably have bet on their perfection as a main topic for future scholars. That prediction would have been partly right, had it been made. The art of memory did occupy some scholars of at least the Middle Ages with many techniques handed down from Greek and Roman writers and other new ones created (Yates, 1966). (The study of mnemonic systems still occurs, but this topic is not now hotly discussed in cognitive psychology.) Our hypothetical scholar's other probable thoughts may have concerned arguments of free will and determinism, of how God's will and method were unveiled in the human mind. (When reading intellectual history, I am always struck by how religious beliefs permeated all thought on every topic in the past, because the situation is so different in the academy today.) Surely the scholar of a thousand years ago would have been certain that today's thinkers would have been similarly influenced by religious questions and issues. In this he would have been wrong, for the most part. We do occasionally hear scientists referring to discovering the secrets of Mother Nature, who seems to take on (at least metaphorically) some trappings of a knowable and kindly, but elusive, deity. She is the closest most scientists come to making religious statements today.

Could the scholar of a thousand years ago have predicted the rise of modern science beginning in the 1500s, the development of so many specialized branches of knowledge, and the remarkable intellectual achievement in the late 19th century of experimental and scientific methods being applied to the study of mind? Even with our wisdom of hindsight, the answer would have to be no. The scholar a millennium ago would have no chance of even coming close, no matter how well informed and perspicacious he might have been. And change during most of the last millennium was, relative to today's pace, much slower. Therefore, for any authority today to write seriously about the future of his or her field a thousand years hence is surely sheer folly. I won't even try.

Let us move to a more modest unit of time, the century. Could a scholar 100 years ago predict where we are today? As I see it, the task now becomes more manageable; I can even imagine a scenario where the predictions could, in a general sense, be accurate. However, my real belief is that the answer is still no; an experimental psychologist writing in 1900 could not have predicted where we are today. Experimental psychology had begun only in the 1860s and 1870s, mostly by physicists and physiologists who branched into new regions. Progress was uneven in the early years and few universities opened

psychology departments until much later, after the discipline was securely established. (Rice University, where I write these words, did not establish a separate psychology department until the early 1970s, but that was partly because Rice was founded as a science and engineering institute. The California Institute of Technology still does not have a psychology department.) Although our contemporary knowledge of the growth and development of psychology may make us think that this course was inevitable, the path doubtless could have been quite different.

Certainly it would have been difficult a century ago to predict accurately the multifaceted nature of contemporary psychology. Experimental methods had been applied to problems of sensation and perception, but Wilhelm Wundt, one of the early champions of the experimental approach, did not believe that these methods could be extended to higher reaches of cognition—to remembering, or thinking, for example. Rather, he believed that one must study (as he did) the products of cultures, much as a cultural anthropologist or sociologist would, to gain evidence about the more creative aspects of mind. Ebbinghaus's (1885/1964) great experiments on memory showed that at least this topic could be submitted to experimental inquiry, but to extrapolate from his experiments to modern cognitive science (much less modern psychological science) in all its manifestations would have too great a leap for even the brightest minds of 1900.

William James's (1890) monumental two-volume work, *Principles of Psychology*, captured wonderfully what was known at that point, but even James had his doubts about the field he so beautifully ratified in his writings, at one time remarking that psychology was "a nasty little science" as he turned back to philosophy late in his career. Careful experimentation often seemed to bore him. The philosophers were then, as now, asking grand questions about mind. Experimental psychologists were probably seen as providing answers to only some rather lower-level questions. Could anyone have then foretold how creatively experimental methods could be extended to study all manner of perceiving, reading, speaking, remembering, and thinking, to mention just some main topics in cognitive psychology? If we added the startling methods and insights provided by those studying cognitive development, social cognition, and other related fields, we would certainly overpower the ability of any scholar in 1900 to predict the current scene.

If a scholar could not have predicted the future of the field 1000 or even 100 years ago, what about 25 years ago? That seems a manageable number, so

let's take 1970 as a convenient point. Could a cognitive psychologist of 1970 have predicted where the field would be in 1995 or 2000? Even here I think the answer is no, at least in anything less than general form. I am in a bit better position to analyze this scenario, because I began graduate school at Yale University in the 1969–1970 academic year. Most of my undergraduate background had been in what was then called human experimental psychology. However, I applied to Yale to study social psychology (never having taken a course in the topic, but having read Brown's [1965] wonderful textbook). My first semester I took Robert Crowder's excellent course in human learning and memory and decided that my interests lay more in that direction, so I switched programs, which was remarkably easy to do. Wendell Garner, Ruth Day, Rowell Huesmann, and Alex Wearing were also on the faculty in the newly christened program in cognitive psychology, Endel Tulving was soon to arrive (in 1971), and John Anderson was hired the following year. Ulric Neisser's textbook, *Cognitive Psychology*, was used in Crowder's course, along with many articles, and it defined the new field and gave everyone a rallying cry.

If you had asked me in the early 1970s what the future would hold in 25 years, I would doubtless have projected the current topics that were being enthusiastically investigated into the future. What were these topics? I will mention the terms without trying to explain them: sensory memories (iconic and echoic memory, and particularly the study of modality and suffix effects in serial recall, at least in Crowder's laboratory at Yale); the role of imagery in memory, and the study of mnemonic devices by Paivio, Bower, and others; the distinction between primary (or short-term) and secondary (or long-term) memory, especially as studied through serial position analyses of single-trial free recall by Glanzer, Murdock and Craik, and as captured in Atkinson and Shiffrin's [1968] model; the topic of intentional or directed forgetting also received considerable attention and was the focus of my first major project at Yale (Roediger & Crowder, 1972); organization in memory (especially in multitrial free recall, where Tulving [1962] studied how subjective organization developed and Mandler [1967] had people directly sort and form organizational units); experiments I conducted both as an undergraduate and graduate student asked about Tulving's (1966) discovery of part/whole negative transfer in free recall (then a hot topic) and were part of the study of organizational processes; the role of retrieval processes in memory (in Tulving's work on encoding specificity and the recognition failure of recallable words, which was just being published; relatedly, I conducted my dissertation on Slamecka's

[1968] finding of retrieval inhibition from part-list cues); the study of word perception and reading was booming, with the lexical decision task becoming a popular topic of study in the 1970s and the Reicher-Wheeler word superiority effect occupying the interest of many researchers; relatedly, the study of the structure of knowledge, or semantic memory, was coming into focus in Collins and Quillian's (1969) model and the experiments designed to test it were widely discussed; the role of attention, as studied by Posner and Garner, among many others, continued a line of research begun in the 1950s and 1960s; and the elegant item recognition paradigm, developed by Sternberg (1966) and often called the Sternberg paradigm, was being applied to ask many questions. There were many other topics of interest, too, but those mentioned above were some of the most popular in my corner of the world at Yale in the early 1970s.

If you had asked me then to project into the future, I would have doubtless placed the study of sensory memories, the relation among memory stores, the organization of memory, retrieval processes in memory, imagery, and all the rest as secure bets for the next 25 years. I would have been wrong on most counts. All the topics mentioned above enjoyed at least 5 years of intense interest after 1970, but most of these topics are not being studied so enthusiastically today and some hardly receive any attention in contemporary literature (e.g., the organization of memory, as indexed by experimental interest in such topics as part/whole negative transfer).<sup>1</sup>

It seemed clear in the early 1970s that other topics were on the way out. To mention but two, the interference theory of forgetting was at a high-water mark, with many publications in the late 1960s. I think I could have foretold (or, more accurately, I could have expressed the opinions of my mentors) that the conflicting findings and unwieldiness of the theory would make it less attractive as a future topic of research (even though the theory did then and does still address critically important problems). Therefore, I and others might have predicted (correctly) that research on interference theory was due for a decline. (Of course, this belief may only reflect hindsight bias; research related to interference theory was still lively in 1970.) The rapidity and completeness of the decline in studies of traditional interference theory probably stunned everyone.<sup>2</sup> On the other hand, I would have also predicted a decline for the study of associative theories of memory in general, had I been asked in 1970 or so, and there I would have been dead wrong. Associationism in the form of John Anderson's theories (e.g., Anderson, 1972; Anderson & Bower, 1973)

and in connectionism (McClelland & Rumelhart, 1986) was about to rise triumphant.

How well could someone in my field have predicted, in 1970, the research topics that were to excite and occupy researchers for the next 25 or 30 years? I believe the answer is “very poorly,” though perhaps not quite so badly as for someone in 1995 or 2000 trying to predict the scene in 2020–2025. Here I list five important topics that arose in the study of learning and memory and discuss why I think they would not have been predicted in 1970, drawing some general conclusions at the end about how new topics emerge.

1. *The levels-of-processing approach to memory.* In 1972 Fergus Craik and Robert Lockhart published one of the most-cited papers in the history of cognitive psychology, announcing their levels-of-processing approach to human memory. Rather than conceiving of a flow of information through the cognitive system, with box-and-arrow diagrams, they argued that people normally go about the world trying to perceive and comprehend it as well as possible and that memory for events is a byproduct, reflecting the level or depth of processing of the original events. Many of the ideas they proposed were similar to those growing out of the attention literature of the 1950s and 1960s and most of the experimental facts about memory that they marshaled were ones developed from incidental learning experiments already published (Hyde & Jenkins, 1969).<sup>3</sup> I can recall reading their paper for the first time and thinking “So what else is new?,” thereby causing myself to miss out on one of the most important sets of ideas for the next decade, which many other researchers eagerly embraced. The levels-of-processing approach is clearly one of the main currents in the psychology of learning and memory over the past 25 years (Roediger, 1993; see Lockhart & Craik, 1990, for a review of this area).

2. *The rise of neuroscientific approaches.* In 1970 the study of the mind and study of the brain were separate enterprises. I remember happening upon George Talland’s (1965) book, *Deranged Memory*, while I was looking for books to read to satisfy Yale’s (rather minimal) requirements prior to embarking on the dissertation. I asked if I should add it to my reading list and was told that it would be all right, but many others should appear ahead of it. The general attitude, often stated quite explicitly, was that nothing important would ever be learned about normal memory functioning from studying the pathological cases arising from injury to the brain. What could we learn from studying freaks of nature? Within 15 years that attitude had changed completely and the neuropsychology of memory was at the forefront of the field. But nobody I

knew would have predicted that in 1970. When reviewing evidence about the two-store memory theories in his *Annual Review of Psychology* paper, Postman (1975) wrote "We have not considered the results obtained with brain-damaged patients which continue to be cited as evidence for dual-process theory. . . . The existing data do not strike us as unequivocal; more important, extrapolations from pathological deficits to the structure of normal memory are of uncertain validity" (p. 517), and nearly everybody in the field then would likely have agreed with him. In fairness, there was an active research community studying the neuropsychology of memory in 1970, but no one in (what we perceived to be) the mainstream was paying attention. Most of the neuropsychological research then was derived from applying standard paradigms developed with normal subjects to see how various patient groups performed on them. This pattern of research activity was soon to change, as happened in the study of the next topic.

3. *The rise of implicit memory research.* The standard measures of retention for the experimental analyses of learning and memory had been measures of recall (free, serial, or cued) and recognition. In each instance, subjects were asked to remember events, usually from the recent past, which had occurred in a particular time and place. Tulving (1972) referred to this as the study of event memory or episodic memory and contrasted its study with that of semantic memory, or general knowledge. In the late 1970s and early 1980s a new endeavor began, the study of implicit (or indirect) measures of memory. These measures were driven partly by techniques used to study semantic memory (the lexical decision task and some others), but more important in this development were studies from neuropsychology. Patients rendered amnesic from brain damage, who were seemingly incapable of learning and retaining new information, were suddenly shown to behave perfectly normally on these new types of memory measures that indexed retention indirectly. Interestingly, the initial studies of this phenomenon were published during the late 1960s and early 1970s by Warrington and Weiskrantz (1968, 1970), but no one in the mainstream paid attention until years later (and the early attention was usually to claim that their experiments could not be replicated). However, the Warrington and Weiskrantz experiments were replicated when subjects were given the appropriate instructional set, and these techniques were later modified and applied to the study of normal subjects in new and interesting ways (e.g., Jacoby & Dallas, 1981; Graf et al., 1982; Tulving et al., 1982). The three papers just cited reported experiments with normal subjects showing that



variables known to have systematic and powerful effects on explicit memory tests (recall and recognition) could have no effect or even opposite effects (e.g., Jacoby, 1983) on these new implicit (or indirect) measures. This set the field off on a new path that, I would argue, no one could have anticipated in 1970 (even though the seminal studies had already been published).

4. *Metacognition.* In 1970 Tulving and Madigan wrote in the *Annual Review of Psychology* that psychologists had made little true progress in understanding memory. They asked: "What is the solution to the problem of lack of genuine progress in understanding memory? It is not for us to say because we do not know. But one possibility does suggest itself: why not start looking for ways of experimentally studying, and incorporating into theories and models of memory, one of the truly unique characteristics of human memory: its knowledge of its own knowledge." (1970, p. 477). Except for early studies on the tip-of-the-tongue effect (Brown & McNeill, 1966) and the feeling-of-knowing effect (Hart, 1965), largely treated then as isolated curiosities, Tulving and Madigan were correct that no one much worried about the problem of how people assessed and regulated their own memories. But many rose to the challenge and studies of metacognition (of how people know about their own cognitive processes) came soon afterward. They were initiated by Flavell and Wellman's (1977) experiments with children, asking how the ability to monitor memory changes with age. Other types of experiments using techniques of metacognition began to boom later. Subjects were asked to make feeling-of-knowing judgments, judgments of learning, reality monitoring judgments (did I do it or imagine doing it?), judgments of the source of information (who said the information? did I read it?), remember/know judgments, and many others (see Nelson, 1992, for a sample of articles). In 1970 memory was thought to be best measured "objectively," with subjects' intuitions about what they might be accomplishing during a test playing little role in the analysis (except for occasional measures of confidence on recognition tests). There was some analysis of the strategies subjects applied while learning, but these were sporadic. In the 1970s and 1980s these experimental practices changed and the study of metacognition remains a hot topic to this day (Metcalf & Shimamura, 1994).

5. *Memory errors and confusions.* The history of experimental analysis of learning and memory has largely been about how much people could accurately remember. The analysis of errors was mostly restricted to correcting measures of veridical performance for guessing, in attempting to arrive at a

more accurate assessment (or a truer measure) of memory. The idea of systematically studying errors to gain an understanding of how remembering occurred was not much in favor, despite important precursors (Bartlett, 1932). Several different analyses changed this state of affairs in the late 1960s and early 1970s (see Roediger, 1996; Schacter, 1995). To mention a few, Neisser's (1967) important text, *Cognitive Psychology*, resurrected Bartlett's (1932) approach to remembering, and important experiments in the early 1970s showed how this approach could be melded with more modern experimental analyses (e.g., Bransford & Franks, 1971). Loftus and Palmer (1974) initiated the first experiments examining how later misinformation provided about an event could systematically alter retention of the event. Combined with assessments using metamemory techniques, we can now conclude that misinformation can alter one's recollection of the event and that often the rememberer cannot distinguish the "new" recollection that is false from one that is true. Later, Johnson and Raye (1981) began their pioneering series of experiments on reality monitoring, or the ability to distinguish fact from fantasy (and confusions of one with the other).

All these experiments and many more (see Roediger, 1996) brought the study of memory errors to the forefront. This trend might have been predicted by some in 1970—many of the pieces were there but no one was yet working on the puzzle—but I certainly would not have been a prognosticator with that prediction. (One lesson from this line of research on memory errors and illusions is that it is hard enough to know the past accurately; how much harder must it then be to know the future?)

I have described five main trends in the study of human memory dating since 1970 whose occurrence, I maintain, most observers would have been unlikely to predict in 1970. (I do not think it was my being a graduate student that leads me to conclude that I could not foresee these trends.) A few commonalities exist among these five cases. First, in each case the initial studies that were later to become celebrated existed well before the topic became an important general thrust of research. The early experiments were truly ahead of their time. They were published, but had to be combined later with insights by others to have an impact. Some of the ideas, such as those leading to the levels-of-processing framework, were very much in the air, albeit more in the study of attention than memory. Others, such as the neuropsychological influence and the study of implicit memory, were further removed from the mainstream, and it would have been harder to predict the power with which

such studies gripped the field later on. In short, even when the early hints of new trends were before the eyes of anyone who cared to gaze at them, they were missed. And these case studies illustrate why predicting the future of research, even in a relatively circumscribed area, is so difficult.

### **A Look Forward in Time**

My lessons from the past are intended to give us pause. Surely we cannot predict the fate of science, psychology, or even the study of learning and memory, in the next millennium. Even attempting to discuss what might transpire in the next thousand years is folly (apologies to my coauthors in this volume who may be attempting this feat). Predicting, concretely, for even the next 100 years would be fraught with difficulty. I will try my hand at predicting some trends in my field for the next 25 years. Even though I have just argued that I would have failed if I had done this 25 years ago, and failed badly, I did not try then. I will try here along with the cowriters of this book. Who knows? Perhaps we will succeed. Another good reason to pick 25 years is that I have a reasonable chance of living through them and can see if any of my predicted trends come to pass. (Predicting the next 5 years would be too easy—just predict that the research on the hot topics of the present will continue and usually one will be right over the short term. It takes topics time to die just as it takes them time to build).

Because I am writing at the end of December 1995, newspapers and radios are treating readers and listeners to astrologers' predictions of the future and, less often, looking back to their feeble attempts to predict the past year's events. I do derive some guidance from their efforts, however. Those claiming the best record make the most general predictions, along the lines of "There will be civil wars around the world," or "The President of the United States will face an international crisis," or "Terrorist acts will upset international relations." These are all safe bets for next year, and the following year, and so on. Astrologers often predict assassinations, too, but the form is usually "The leader of an important power will be assassinated in 1996." Some made that prediction last year and, sadly enough, Yitzak Rabin of Israel was assassinated. But no one predicted that specific event. The iron law for making predictions is that the more general the prediction, the more likely it is to be verified by some event in the future. But the truth of the prediction is no cause to believe the predictor truly prescient; with general predictions, many events can be

interpreted as fulfilling the prediction. Conversely, specific predictions (in futurology as in science) are more easily falsified. With this rule in mind, my first prediction:

1. *Most of the specific predictions in this volume will be wrong.* (Let's define *most* as 80%, to be specific here.) Specific predictions include ones that set times—such as my own 25-year limit, or even more refined figures (such as “by the year 2013”). I would thereby exclude from considerations such platitudes as my next two predictions, which sound reasonable and almost certainly have to be true, but are good predictions only because of their generality. My second and third predictions are:

2. *New discoveries will lead research in the psychology of learning and memory (or any other field) in new and unexpected directions.* This has happened in every previous 25-year epoch, so can be confidently predicted for the next quarter century. Specific predictions would state what these directions might be, but I doubt anyone knows. (Some speculations exist below in other predictions.)

3. *New discoveries will undermine currently cherished findings, theories, and assumptions.* Again, this pattern happens in every 25-year period, so seems a certainty. Besides, we would not want to live through the next 25 years of research if it were not true. Once again, the trick would be in specifying which findings, theories, and assumptions would be overthrown, but I cannot confidently do that.

Next I turn to predictions that are more concrete and might actually be falsified.

4. *The psychology of learning and memory will become (even more) interdisciplinary.* This seems another safe bet. The main problem clouding interpretation of the outcome would be a ceiling effect—is the field already as interdisciplinary as it can be? Currently, learning and memory are studied from a dizzying variety of perspectives, from molecular, to cellular, to synaptic, to neural systems, to artificial networks, to behavioral analyses with animals, to ethological analyses, to experimental analyses of humans, to metaphorical models, to mathematical models, to social-psychological concerns, and recently even to social, cultural, and historical analyses. After all, everyone from neurobiologists and animal learning psychologists on the one hand, to cultural anthropologists and historians on the other, worry for one reason or another about how people (and other animals) retain past experiences. The same is true of collections of people, from the passing down of legends in preliterate societies to the writing of recent history, such as about the assassination of John F.

Kennedy and the fall of the Nixon presidency (to pick two events that seem open to multiple historical re-creations). One can imagine a future in which historians team with experimental psychologists to ask how (say) the records and reporting of different events become transfigured as they are passed from one generation (of scholars in history, or subjects in psychology) to the next. Factors such as the scholars' or subjects' interests and beliefs in the topic at hand could be manipulated, along with many others.

Although many disciplines currently worry about the problems of learning and memory in their own domain, with their particular field's own assumptions, viewpoints, and theories, there is great room for cross-fertilization of thought. Not long ago I was asked to review a paper for the *Journal of Southern History* on the topic of what could be learned about the institution of slavery from accounts provided by slaves themselves. Briefly, most current accounts of slavery in the American South have come from records and reminiscences of the slave owners, using their interpretations of events. This fact might account for the relatively benign picture of slavery that appears in some history books: yes, slaves led a hard life, but generally they were well treated because they were valuable personal property. Recently, some historians have tried to systematically evaluate the relatively infrequent narratives of the slaves themselves, but these are quite scattered by region, by the gender and age of the writer, and so on. These historians sometimes consult with psychologists, asking questions that most psychologists are unprepared to answer. How accurately might memory for an episode from, early in a slave's life (e.g., the family being sold at auction and broken up when the rememberer was 6 years old) be retained, if it were not retold or written down until the person was 75 and asked to do so by an historian? How might the tale change? This question and many others like it are common among historians studying this issue, because it was mainly between 1915 and 1935 that historians began to collect reminiscences from former slaves, who were then quite old. How good might such evidence be as history?

Psychologists cannot answer this question now, but future longitudinal research could conceivably provide some information that would aid historians. In general, one profitable future development in the psychology of learning and memory would be to take a life-span approach through longitudinal research. This type of research would doubtless have to be carried out by teams of researchers, over generations, much like the famous studies of gifted children initiated by Lewis Terman in the 1920s and still ongoing today. But such

studies would open up the field to many fascinating questions that have not yet even been raised, much less answered. Many other interdisciplinary collaborations would probably address similarly new questions, ones not even being considered now.

5. To become more specific, *I predict that at least a fledgling program of life-span memory research will begin in the next quarter century.* At the moment there is, even among experimentally oriented psychologists, a rather sharp division of labor. Many researchers such as myself study learning and memory in young adults, largely college students. Other researchers study learning and memory in children; another group of investigators studies learning and memory in older adults. Still others are interested in learning and memory in various specialized populations: patients with specific patterns of brain injury, people with Alzheimer's disease, people diagnosed as schizophrenic or clinically depressed, and so on. Although researchers interested in the populations mentioned above overlap a bit and to some degree are informed of one another's findings, none really examines the person over the life span.

A few researchers have worried about retention of specific bodies of knowledge over long periods of time (such as Bahrck's [1984; Bahrck et al., 1975] studies of the retention function of names and faces and Spanish vocabulary learned in college), but these studies were conducted long after the fact, when there could be no control or manipulation of conditions of learning or of the intervening experiences. In the future, I predict that a true life-span psychology of learning and memory will develop, wherein research participants will be recruited as infants (with permission of their parents), given various experiences under controlled conditions, and then tested at various periods during their lives. Intervening events could be manipulated to some degree, too. In addition, parents may be induced to record important events accurately at the time of their occurrence (accidents, birthday parties, deaths of relatives, first dates) and the knowledge about these events can be assessed later under varying retrieval conditions. If records of textbooks and school grades are maintained, this would represent another rich source of information to be tested in later life. A true life-span approach to learning and memory, through longitudinal studies, might serve to increase our knowledge of many important properties of memory. Although such a field is only a dream now, it could (and should) become a reality in the future.

6. *The "everyday memory movement" will pay important scientific dividends, by being combined with the traditional experimental logic of cognitive psychology to*

*lead to important new insights.* Neisser (1976, 1978) critiqued the psychology of learning and memory and found it sadly wanting, too tied to sterile laboratory paradigms. He advocated abandoning laboratory methods for a more ecologically valid psychology of memory, using ethological observational methods and the like. Many psychologists rallied to his cry and began studies of ecological memory or everyday memory. Banaji and Crowder (1989) surveyed the scene some years later and found little to cheer about; they announced the ecological or everyday memory approach “bankrupt.” Their article, in conjunction with those of Neisser, ignited a debate that continues today. The essence of Banaji and Crowder’s (1989) argument is that without experimental control over conditions of learning and testing, no firm conclusions about memory (inside or outside the laboratory) can be made. Any attempt to favor external validity over internal validity is a mistake; one must first achieve internal validity to have any hope of producing generalizable (externally valid) knowledge. Yet the ecological movement eschewed experimental control for a purely observational approach. I weighed in on the side of Banaji and Crowder (1989) in this little debate (Roediger, 1991), noting that the studies claimed as the best of the everyday memory movement (such as Loftus and Palmer’s [1974] study of eyewitness memory) actually used traditional laboratory methods, experimental control, and the usual logic of experimental, cognitive psychology. The triumphs of a true observational approach were relatively few (although I would certainly include Neisser’s [1981] fascinating study of John Dean’s memory in this group).

So why do I now think the everyday memory movement will lead to great insights? Although I think the methods advocated by this approach were generally wrong-headed, the beneficial effect is to open the study of learning and memory to new topics, ones not previously in its purview. The everyday memory movement had a liberating influence on the topics and content considered legitimate areas of inquiry in the psychology of learning and memory. This influence, and the emphasis on applied problems (such as the current focus of psychologists on the accuracy or inaccuracy of memories recovered during therapy) will have an important impact in the future. Because one group of researchers now keeps its sights focused outside the laboratory, new phenomena may be identified and, initially, described through observational study. However, such identification and description should not be an end in itself (as the original proponents of this movement seemed to imply), but

rather a beginning to careful experimental study. In the most satisfying case laboratory methods can help unravel paradoxes arising between what psychologists have learned from laboratory research and what everyday memory researchers have found. The recent work by Koriat and Goldsmith (in press) represents an excellent example. I predict we shall see many more like it in the future.

7. *Neuroscientific approaches to learning and memory, already prevalent, will become even more dominant.* Cognitive psychologists have tended to live in relative ignorance of advances in neuroscience, at least until lately. Their experimental analyses have been conducted largely on intact adult subjects, with normal working brains, and their theoretical analyses have been in the form of verbal descriptions of assumptions, ideas, and hypotheses (e.g., the levels-of-processing framework), metaphorical models (likening semantic memory to a giant dictionary, for example), or to mathematical models with numerous parameters and explanatory constructs. Until lately, cognitive psychologists have rarely considered neural underpinnings of their constructs or sought explanations of phenomena in brain mechanisms. All this will change dramatically in the future, continuing a trend already in evidence.

Cognitive psychologists have often defended their abstract level and mode of explanation by appealing to the computer metaphor of mind: electrical engineers and other physical scientists may be interested in the hardware that runs computers, whereas computer scientists, designers, and programmers develop and understand the software. Yes, the software may have to operate within the constraints of the hardware, but the rules programmed into the computer can be studied and understood at their own level of analysis, without a programmer needing to know much about the hardware. Correspondingly, the argument goes, cognitive scientists may operate at the behavioral level of analysis in trying to discover the programs that run the mind while (in parallel) neuroscientists learn about the brain's hardware that supports the program. I first heard this defense of the cognitive level of analysis in graduate school and it has become something of an article of faith among cognitive psychologists, but it is now clear to me that we should (and must) give up this idea that the study of cognition can be (and maybe even should be) divorced from the study of the brain. We give up too much and get little in return.

Robert Bjork (1989) argued not long ago that the computer metaphor is all but dead in cognitive psychology. He wrote:



At the current stage of research on human memory, we are poorly served by the computer metaphor. Thirty years ago, as an alternative to the stimulus-response approach, the information-processing approach was invaluable. . . . We have come to realize, however, that in virtually every important respect, the human information processor is functionally nothing like the standard digital computer. From the standpoint of a computer scientist, there appears to be considerable value in drawing upon modern cognitive neuroscience and cognitive science to reconfigure the processing architecture of the next generation of computers. From the standpoint of a cognitive psychologist, there is no remaining value in the standard computer metaphor (p. 310).

Later in the same chapter, which was on inhibitory processes in memory, Bjork commented that "A new metaphor has emerged to influence the thinking of memory researchers: the brain metaphor. . . . Ideas about memory are being shaped by the accelerating knowledge of the possible functions of certain brain structures in human memory" (p. 328). Bjork was clearly right in 1989, although not everyone seems to have gotten on board the new bandwagon. There even seems to be stiff resistance in some quarters. But I predict that the future holds great promise for alliance of traditional cognitive psychologists and others with the prefix *neuro-* in their disciplinary description: neuropsychologists, neurologists, neuroradiologists, neuroscientists, etc. Some corollary predictions flow from assuming this one is true.

8. *Neuroimaging techniques will assume increased importance and many great discoveries of the future will result from the marriage of cognitive, experimental techniques applied to subjects whose brains are being imaged.* This trend is already apparent, especially in the work concerned with positron emission tomographic (PET) analyses of cognitive function (Posner & Raichle, 1994). Many studies have already appeared that have tentatively identified brain areas responsible for perceiving words, faces, and music and for studies of encoding and retrieval processes in remembering. In some instances, the results confirm findings obtained from neuropsychological work with brain-damaged patients, but in other cases novel findings have emerged which were unanticipated by patient findings. An example of the last outcome is the relatively consistent finding from PET and MRI (magnetic resonance imaging) studies that the right frontal

lobe plays an important role in episodic memory retrieval (Nyberg et al., 1996), although other memorial functions are subserved by the frontal lobes, too (Buckner, 1996). Neuroimaging techniques will, I predict, gain in ascendancy in the cognitive neuroscientists' methodological armamentarium. These techniques may overshadow, but not replace, the typical strategy used in cognitive neuropsychology of studying single cases or studying patient groups (such as patients with Korsakoff's syndrome or diseases of the Alzheimer's type). Although steady advances in technology are responsible in part for these exciting developments, equally important (in my opinion) is the application of well-developed techniques and methods employed by cognitive psychologists over the years. The best work in the future will probably come from technically sophisticated imaging facilities (PET, functional MRI, event-related potentials, and others) combined with the experimental logic and methods of cognitive psychologists, who can adapt their methods to the needs of the imagers to ask interesting and important questions.

9. *The greater influence of cognitive neuroscience will change the theoretical landscape of psychologists interested in learning and memory.* Metaphorical models of memory, those likening memory to a storehouse, or to a library, or to other concrete storage devices (see Roediger, 1980, for a review), will decrease in importance and will seem even more misleading (or beside the point) than they do now. Why speculate about metaphors that might work like the mind, when we can root our theoretical understanding of behavior in the neural mechanisms that are responsible? There will still be much room for controversy, competing theories, and so on. Witness the controversy that has occupied researchers in neuroscience over the past 15 to 20 years over the role of the hippocampus and surrounding structures in modulating memory.

10. This next prediction for the future of theory flows from the prior one and can be stated succinctly: *Cognitive psychologists will begin pursuing a reductionist theoretical approach, in collaboration with neuroscientists.* Reductionism has been mostly regarded as a wrong approach in cognitive psychology during the past 25 years, and for good reason: many neurological discussions attempted in the past (such as some of those by the Gestalt psychologists in the 1930s and 1940s) seem not just wrong but completely far-fetched today. Nonetheless, reductionist approaches help constrain speculation and provide theories with a firm foundation. Now that relatively noninvasive techniques for the study of brains of intact people have become available, reductionism—

explaining behavior in neuroscientific terms—seems both inevitable and desirable.

11. I end with a surprise prediction: *Some form of behaviorism will return and be reintroduced into psychology as a strong force to be reckoned with.* This will occur grudgingly at first, but then enthusiastically. I do not mean the Skinnerian form of behaviorism that eschews all talk of mental constructs and all reference to internal workings of the mind or brain (although I think this approach also has its place). But, for cognitive psychology, I mean the behaviorism of Hull or Spence or Tolman, in which theoretical constructs had to be grounded in observable behavior. Those theorists did propose constructs rooted in concrete observations, a practice that has been all but eliminated in contemporary cognitive psychology, as I have noted before (Roediger, 1980, 1993). Currently, theorists are literally permitted to propose nonsense constructs—models of mind have many constructs that are not moored to observable behavior. Rather, the entire model produces predictions that can be checked against aggregate behavior, but the individual constructs that compose the model have no external markers. I predict (or imagine) a world in the future in which psychology returns to the idea that hypothetical constructs should make contact with observable behavior, even if indirectly. The trend toward neuroscientific explanation may hasten the return of this form of behaviorism. The study of observable behavior should always be central to psychology, even cognitive psychology. (On the other hand, I made a prediction similar to this in my 1980 paper on memory metaphors and the theoretical scene is unchanged 15 years later, except perhaps to be worse on this dimension.)

## Conclusions

In this chapter I cast a backward glance at where we have been and argued that even the most prescient scholar, whose knowledge encompassed the field of psychology, could not have predicted the future course of the field at various vantage points in the past (even from the relatively short distance of a quarter century). Next I made 11 predictions for our progress for the next 25 years. Two were generic predictions—ones true for every 25-year epoch—but the others can be tested against the future. I hope Professor Solso will arrange another volume in 25 years to assess how well we predicted the future of our disciplines. Perhaps some of us will even be there to write for him.

## Acknowledgments

I appreciate comments by Robert L. Solso and Kathleen B. McDermott.

## Notes

1. Many of these topics may simply have been mined (experimentally and theoretically) for all they were worth at the time. They will lie dormant until a new generation of researchers has creative insights to renew their investigation.
2. An interesting challenge that might be set today's researchers is to predict which of the current hot topics will last the longest, which will fade, and which will lead (whether fading or not) to enduring contributions to our understanding.
3. Similar observations were made by a revision psychologist, P. I. Zinchenko, in 1962, but his work was generally unknown in the West until it was translated into English and published in a 1981 volume edited by J. V. Wertsch. Solso (1995) provided an accessible summary.

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