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## Chapter 7

### AGING MEMORIES: A Career in Cognitive Psychology

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When I was at the University of Liverpool in the early 1960s, our research group was visited by an eminent scholar from London—a Professor Fry as I remember. Our group was made up of young postgraduate students, and when we were introduced as researchers of the aging process, the professor remarked “Hmm, studying it from some distance it seems!” Less true today, alas! However, living through the aging process does at least afford the researcher the opportunity of thinking out his or her earlier pronouncements in a way that is not usually possible for students of other developmental, comparative, and abnormal phenomena. Looking back at my earlier work I am torn between pleasure at being “right” from the start—at least in the limited sense of still holding the same view today—and dismay at my rigidity and lack of progress.

In this essay I will attempt some accounting of the switch/stay ratio in the ideas that have guided my experimental research over the past 40 years.

I was born in Edinburgh, Scotland, in 1935, and later went to high school and university in that chilly but beautiful city. I also met my wife Anne there, so we look on it as our home

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Careers in geropsychology are very varied, as this volume demonstrates, and I suppose mine is no more unusual than many others. In my cognitive aging corner of the field, and in my generation (plus or minus 20 years), it is not uncommon for researchers to be active contributors to the parent discipline of cognitive psychology as well as to cognitive aging—Tim Salthouse, Leah Light, Pat Rabbitt, Denise Park, Lynn Hasher, and Rose Zacks are some of many names that come to mind.

I would like to express my gratitude to the many people who have helped my career at various points; mentors and colleagues in Edinburgh, Liverpool, London, Toronto, and elsewhere. Apart from my difficulties as a medical student I have had it easy, thanks largely to these helping hands. Much of my empirical work and many of my ideas are attributable directly to interactions with my graduate students and postdoctoral fellows over the years, and it is to them that I owe the most. Of the 16 PhDs who worked with me as their primary supervisor and who had qualified by the end of 1997, only five worked primarily in aging research (Mark Byrd, Joan McDowd, Ray Shaw, Karen Li, and Nicole Anderson). In contrast, I have had some 21 postdoctoral fellows, and 17 of these worked on aging topics. I will ponder these data for a while before drawing any major theoretical conclusions.

Finally, I owe an enormous debt of gratitude to my family for putting up with me over the years. My wife Anne and children Lindsay and Neil were often shortchanged while I toiled to meet yet another deadline. I was somewhat taken aback on one occasion by being asked by one of my children’s friends (teenagers at the time) whether I was “actually a workaholic,” but my alarm was reduced when the same youth later commented to my wife (who, like me, has mildly leftist views) that “he had never met a real communist before!” So everything is relative I suppose. And now the children have children of their own, who regard their grandpa with a mixture of affection and tolerant amusement. From the Beatles to babysitting—it didn’t take long! Oh well, still time to write one or two more decent papers with any luck.

town although we have lived and worked happily in Canada for many years. My father is now aged 90, and is a living testament to the beneficial effects of a daily dose of Scotch whisky on successful aging. At the time of my birth he was an ambitious young executive in the Bank of Scotland and was appointed manager of the branch in Lockerbie—a small town in the southwest of Scotland that achieved unhappy fame many years later as the site of the TWA plane disaster. Growing up in a Scottish town was a satisfying experience; my first career aspirations were modeled, naturally enough, on some of the local figures who seemed to spend their lives in interesting ways. Around the age of 4 or 5 I debated whether to become a minister or a joiner (the Scottish term for carpenter). I wonder now whether these two vocations appealed, respectively, to my future interests in teaching and empirical work; whatever their origins, I soon decided that being a joiner would be more fun.

My first school was Lockerbie Academy, and there I learned the basics of arithmetic and grammar. My parents were ambitious for me, and so sent me to an excellent high school in Edinburgh at the age of 12. My best subjects there were English and physics—a slightly unlikely combination. I liked science in general (except for the mathematics) and biology in particular, so a career in medicine seemed a sensible choice. I think I was the first person in my family to go to university, although it seemed entirely appropriate that I should do so—almost all of my friends from school were also going on (most to Edinburgh University), my parents expected and encouraged it, and Scottish attitudes were very much in favor of the best possible education. Although Scotland is a small country, and not a particularly wealthy one, it has had four universities for 500 years or so. The tradition there (certainly in my day) was for students to attend their local university, so I applied to the medical school at the University of Edinburgh (my *only* application) and was duly accepted in 1953.

Medical school was not a success, unfortunately. The first year was basic science—no problems there—but I had immediate difficulties when I started studying anatomy in the second and third years. I found it tiresome and onerous to learn catalogs such as the 47 anterior relations of the kidney, and I just could not find general principles to make the learning meaningful. I soldiered on grimly for several years despite being a mediocre student and despite a growing realization that I rather disliked the company of sick people; not a good attitude for a potential physician. I did enjoy neurology, however, and was intrigued by the psychiatric cases that I saw. I read some books on psychosomatic medicine and was impressed by the obvious power of the mind to influence apparently mechanistic bodily systems. The interaction between brain and behavior was thus a topic of early interest, and I strongly suspect that if I had graduated in medicine I would have ended up as a researcher in this general area, doing much the same work as I do today.

However, my medical aspirations ended in 1958, partly as a function of my own growing dislike of the program and partly as a function of my failure to satisfy the steely eyed examiners of the Edinburgh Faculty of Medicine (my fault, not theirs, in both cases). I was able to salvage the courses I had passed in medicine (physiology, biochemistry—even anatomy) and transfer them to the science faculty; I graduated from the psychology program in 1960. At that time British university departments were usually quite small, and had a single professor—“the Prof”; other faculty members were lecturers, senior lecturers, and readers. The professor at Edinburgh was James Drever, a pleasant scholarly man whose interests reflected the department’s origins as the Department of Mental Philosophy. He was not an experimentalist, but he encouraged me in my empirical interests and clearly felt that the discipline would make progress by virtue of scientific findings as well as by theoretical analysis. The final-year honors class had a weekly seminar with Drever in which we read a series of books, then critiqued and discussed them. Drever’s selection in 1959 was interesting and forward looking when considered from the vantage point of 40 years later; we

read Gibson's *The Perception of the Visual World* (Drever was a friend and admirer of J. J. Gibson), *The Organization of Behavior* by Hebb, *Learning and Instinct in Animals* by W. H. Thorpe, and *Motivation* by Dalbir Bindra. We also read some Hull, but behaviorism was never a major force in British psychology. I was therefore exposed early on to Canadian biological psychology, in the shape of Hebb's and Bindra's ideas, and also to the evolving views of the ethologists like Lorenz and Tinbergen. Perceptual theory also appealed to me, especially the ideas of transactional functionalism (so much more satisfying to roll round the tongue than phrases like "pulmonary embolism" or "ventricular fibrillation") and the writings of Helson and Brunswick.

Ian Hunter taught a course on memory and learning that I enjoyed but that did not capture me entirely. The article that really knocked me sideways was George Miller's classic paper on the Magical Number Seven, so for my thesis project I decided to merge information theory with the perception of time and devised an experiment in which the participants ("subjects" at that time) made absolute judgments of varying complexity and were later asked to estimate how long they had been working. The study yielded a nice function linking subjective judgments of duration to the rate of information processing—possibly before its time in 1960, although I did not think of publishing it. But the experiment did have a practical spin-off; it seemed acceptable to approach young women to ask them if they could help out by participating in a psychology experiment—I ended up testing 120 participants, possibly the largest scale study that I have ever run. But I did the decent thing and married one of them—my wife, Anne.

Designing, running, and analyzing my experiment on time perception was a formative experience. It confirmed my interest in experimentation and demonstrated the satisfaction to be gained from a pattern of data falling neatly into place. I still love to see a tidy pattern of numbers, even in the simplest experiments. In my lab class on human memory we gather data on free recall of word lists and I plot the results with rising excitement (the primacy effect! the recency effect!) while the students yawn and gossip in the background. Professor Drever approved of my experimental interests and suggested that I apply for two possible postgraduate research positions; one was at an institute of ophthalmology, but the work there seemed a little too narrow for my tastes; the second was at the Medical Research Council (MRC) Unit on Occupational Aspects of Ageing at Liverpool University, and this was definitely more attractive. However, I also felt that I had spent too many years as an undergraduate and was keen to get into "a real job," so I applied for a position in the research department of a large London advertising agency. I was actually offered the job and had provisionally accepted it when I was told that, unfortunately, the budget for the position had not been approved. So, in a way, the MRC unit was my second choice, and it is interesting to reflect on the role of chance in a career. My suspicion is that temperament, interests, and abilities will generally govern the broad outlines of the type of work a person does, but chance may play a major role in determining the exact form the work takes.

#### University of Liverpool, 1960–1965

In postwar Britain, research into old age and the aging process had been sponsored by the Nuffield Foundation, and in 1946 a research unit into problems of aging was established at Cambridge University under the direction of A. T. Welford. This group flourished for 10 years, conducting research into aspects of human skilled performance, including perception, learning, memory, attention, and reaction time. The results of their investigations, framed by a broad theory of human performance, were later published in *Ageing and Human Skill* (Welford, 1958). This book, along with Donald Broadbent's *Perception and Communica-*

tion (1958), was a major inspiration to my early work in the experimental psychology of aging. In 1955 the Medical Research Council decided to support further work on the psychology of aging, but to tilt the research toward applied and occupational aspects. The Liverpool unit was therefore set up in 1955 with Professor Leslie Hearnshaw as the director and Dr. Alastair Heron, first as the deputy director and later as the director.

When I arrived in Liverpool in the autumn of 1960, Dr. Heron had planned four research topics spanning the fields of experimental and occupational studies of aging; these topics were taken on by four young researchers. Roger Wilson was to study flicker fusion frequency and other aspects of visual functioning, Norman Wetherick explored age differences in problem solving and concept attainment, Ken Elliott looked at labor mobility as a function of age, and I studied age-related changes in confidence and decision making. The thinking behind this last topic was that an age-related decline in self-confidence might impose an artificial ceiling on an older person's abilities—especially when learning new skills. My first experiments on this topic were carried out in the framework of Julian Rotter's level of aspiration theory. I inherited an aiming task in which participants estimated their future performance at various levels of practice. Some evidence was found to support the idea of a general trait of confidence, and I also confirmed a tendency for older adults to seek more information before committing themselves to a decision. I found the ideas rather too vague for my tastes, however, and the experimental results were not very clear-cut, so I switched my allegiance to the relatively new ideas stemming from signal detection theory, propounded by Tanner, Swets, Birdsall, and others. This framework suggested more rigorous experiments in which younger and older adults listened for faint auditory signals in a noisy background. Signal detection theory permits the separation of sensory and motivational factors, and my studies in this area provided evidence for greater caution on the part of older adults and a tendency for older people to restrict the range of their decision criteria when using a rating scale—they were less risky at the risky end of the scale and less cautious at the cautious end. I submitted all of these studies as a PhD thesis in the Department of Psychology at the University of Liverpool and was awarded the degree in 1965.

As an MRC employee at Liverpool, it was easy to visit other MRC research units, and Alastair Heron greatly helped my career by sending me on several trips to the applied psychology unit at Cambridge. This group was certainly the strongest team of experimental psychologists in Europe. It was led by Donald Broadbent and also had some promising young researchers: Alan Baddeley, John Morton, and Pat Rabbitt, for example. These visits opened my eyes to the exciting new world of cognitive psychology—to studies of attention and short-term memory, to experiments on stress and vigilance, and to sophisticated theories of reaction times. I proudly returned to Liverpool with a copy of tape-recorded dichotic digits. I still have the tape somewhere—the digits were spoken by Margaret Gregory (later Margaret Broadbent) and the recording was obviously made on a summer evening, as the sounds of birdsong from Cambridge gardens can be heard quite clearly.

In my last two years at Liverpool (1963–1965) I therefore started to conduct experiments on age changes in memory and attention; this is the line that stuck with me and led to later work in London and in Toronto. Apart from the support and encouragement I received from Alastair Heron and Professor Hearnshaw, other colleagues and friends provided ideas, criticism, and suggestions. I should single out Dennis Bromley especially, since he supervised my thesis work after Dr. Heron left to take up a position in Africa. Dennis had broad interests in cognitive and social gerontology and provided wise and careful guidance in a good-humored way. He and I went once a week to the Kardomah Tea Room to eat supper before teaching in an evening class. We were sometimes joined by Roma Grundy, later his wife but then a girlfriend; Dennis and Roma would conduct a genteel flirtation over the

teacups as I munched away embarrassedly at my fish and chips. "Well, you know what my name spells backwards? . . ." said Roma coyly on one occasion; "What—Yd-nurg??" responded Dennis in mock innocence. Terry Rick was another pal from the psychology department; he and I shared the teaching in one of the evening classes. Ann Davies from the MRC unit met one of our students socially, a retired academic with a pronounced Eastern European accent. Hoping to get some gossip, Ann asked him how he was enjoying the course. "I hev two teachers" said the man after a reflective pause, "Krick und Krock. Krick is vonderful, but Krock is ebsolutely awful." Terry and I both loved the story—each convinced that *he* was the vonderful Krick of course.

As is usually the case, there is no clear line dividing a category of people who were influential scientifically from those that were simply friendly and helpful, but I certainly profited greatly (and in some cases continue to profit) from conversations with Ann Davies, Sid Tune, and Norman Wetherick at the Liverpool unit. We had a visit from Patrick Rabbitt, and I remember being vastly impressed by Pat's prolific list of suggestions for possible future research topics. Alastair Heron was a major help and influence; apart from advice on experimental issues, he facilitated contacts with the Cambridge group, and funded trips to the International Congress of Gerontology in Vienna and to the APA annual meeting in Washington, DC. Liverpool in the 1960s was an interesting and satisfying place to live and work; I remember those days with affection.

### Birkbeck College, 1965–1971

After receiving the PhD from Liverpool I decided to look around for another position. I would have dearly liked to move to the Applied Psychology Unit at Cambridge, but they had no openings at that time. A faculty position in a university department seemed the next best thing, so I applied for a post as a lecturer at Birkbeck College in London and was appointed there in 1965. Birkbeck is part of the federated University of London, and the Department of Psychology was chaired by Arthur Summerfield, with Peter Venables as the second professor. Peter was a big influence, especially; his work was in schizophrenia and in electrophysiology. Although clearly the cognitive difficulties experienced by older adults must have a different cause from those underlying schizophrenia, there are some interesting parallels nonetheless, and my thinking benefited from considering the methods and theories used by Venables and his students.

At Birkbeck I was under no obligation to pursue research in aging, and in fact I became progressively more interested and involved in research on short-term memory in the late 1960s. The age variable played a part in some studies, however; for example, in experiments concerned with the identification and measurement of a "pure" primary memory component in free recall. We found that age had no effect on primary memory but that the secondary memory component declined with advancing age. These experiments were published in a book edited by George Talland and also as one of my first international journal articles in the *Journal of Verbal Learning and Verbal Behavior* (edited at that time by Leo Postman) in 1968.

George Talland played a crucial, if inadvertent, role in my career by offering me a position with his Boston group around that time. I had met George at various conferences, and he had been supportive and encouraging; for my part I greatly admired the manner in which he blended theory and experiments. I was keen to spend a year in an American lab, but had no thoughts of leaving Britain permanently, so Talland's offer posed a real dilemma. My boss at Birkbeck, Arthur Summerfield, offered a solution by promising me a year's leave of absence in the near future if I could find a congenial (and sufficiently wealthy) temporary

haven in the new world. (I should mention that it *was* unusual for junior faculty members to be granted a leave so early in their careers.) As it happened, Donald Broadbent had invited me to a NATO-sponsored conference on memory at Cambridge in the summer of 1967, so I took the train to Cambridge armed with Summerfield's offer.

The meeting lasted two weeks and involved many of the major figures in memory research such as the diplomatic Arthur Melton, the enigmatic Donald Norman, and the charismatic Endel Tulving. I hesitantly approached Tulving to see if there was any chance of spending a year in Toronto, and to my mild amazement he responded that it might well be possible if I was prepared to teach a couple of courses. Was I prepared to? Absolutely! And so it came about that I spent the 1968–1969 year at the University of Toronto—instantly transformed from a mere lecturer in the United Kingdom to a visiting associate professor in Canada. I ended up teaching a course in memory and a lab class in human and animal learning, despite my feeble protestations that I had never taken a course in animal learning and had never so much as handled a laboratory rat in my life. No problem—here's your white lab coat, a pair of gloves, and a manual of possible experiments; let me know if you have any questions, OK? (This was Glenn Macdonald, the dryly humorous chair of the Toronto department.)

I enjoyed the year in Toronto. The Department of Psychology was very active in research, with several meetings a week devoted to problems of memory. Also, I found the general attitude to research in Canada and the United States to be very positive and supportive; new ideas were welcomed and encouraged, unlike in Britain where they were questioned and disputed (reflecting perhaps the difference between an empirically based intellectual culture in North America and a rationally based culture in Britain). Or was it attributable to the fact that psychology in Britain was still loosely connected to philosophy and thus to advances by argument and rhetoric rather than by experimental demonstrations? I'm not sure, but I know that I relished the empirical-theoretical atmosphere in Toronto and other North American centers. My family also liked living in Toronto; we went back to London in 1969 after my year as a visitor but returned to Toronto as permanent residents in 1971.

Aging research was thus not my primary interest while in London between 1965 and 1971, but I remained active in the field. One topic, mentioned already, concerned age differences in short-term and long-term memory; another was the interface between attention and short-term (primary) memory. Dichotic listening was a dominant methodology in Britain, following Broadbent's pioneering work in the 1950s, and I carried out some experiments using this paradigm. I also continued working with signal detection theory measures; one suggestion that caught *my* attention was that the sum of the squared signal strengths ( $d'^2$ ) from two or more simultaneous discriminations (e.g., shared attention between the two ears) gives a measure of total processing capacity for the discriminations in question. Some of the available capacity is lost in the divided attention situation—possibly because it is needed to control the sharing procedure—and I found that more capacity is needed for this purpose in the older listener. This work is based on an interesting paper by Martin Taylor and colleagues and was never really followed up, although the topic of divided attention retains its interest for me today. One last important event around that time was an invitation to another NATO conference—this time in Thessaloniki, Greece, in 1966. The conference was organized by James Birren and Alan Welford and dealt with age-related changes in decision making in various theoretical and practical situations. It was stimulating and agreeable to share a relaxed week or so with some big names in the field. As I remember, I lectured on the use of signal-detection theory in studies of aging to a large group of polite

but somewhat bemused students from Turkey and Greece. It was a very pleasant few days; I do hope that NATO's objectives were advanced in some small way.

### University of Toronto, 1971–2000

I took up my position as an associate professor of psychology at the Erindale Campus of the University of Toronto in the fall of 1971, and I have been at the university since then. The Erindale Campus is situated about 15 miles west of downtown Toronto in the booming suburb of Mississauga. The buildings are pleasantly situated among trees and parkland (well, car-parkland these days), and in 1971 the faculty members were young, bright, and research oriented (now they are middle-aged, bright, and research oriented). I had decent research space, reasonable funding, and excellent colleagues, both at Erindale and on the downtown campus where I retained my close research ties with Endel Tulving, Bennet Murdock, and Bob Lockhart, among others. The highlight of each week was the departmental memory seminar—the Ebbinghaus Empire—set up by Endel Tulving and George Mandler in the early 1960s and still going today.

My first few years in Toronto were dominated by the development of the levels of processing framework for memory research. While still in London I had been impressed by the attentional theories of Anne Treisman, then at Oxford. She had suggested that perceptual information is subjected to a series of analyses, with each successive level acting as a kind of pass-fail test. The tests work on signal detection theory principles, such that an incoming signal can “pass the test” and proceed to higher level analyses *either* on the basis of its signal strength ( $d'$ ) *or* because the criterion for a passing grade ( $\beta$ ) is set at a lenient level. This system modified and elaborated Broadbent's original filter theory of attention, allowing for both bottom-up, perceptual ( $d'$ ) and top-down, conceptual ( $\beta$ ) factors to play a role in what gets through to conscious awareness. Hierarchical models of this sort have always appealed to me, as have models involving gradual and continuous changes as opposed to sharp categorical divisions, and it seemed to me that memory (or at least the encoding aspects of memory) might be thought of as the products of these various levels of analysis. Thus both the amount and the qualitative nature of incoming information encoded into memory would depend on the attention paid to an event and how the event was processed. These ideas were quite vaguely thought through when I returned to Toronto in 1971, but I was delighted to discover that my colleague Bob Lockhart had been thinking along the same lines. We had mentioned these notions to Endel Tulving, who had been encouraging and who suggested that we write them up as an article for the *Journal of Verbal Learning and Verbal Behavior* ( Craik & Lockhart, 1972). Tulving was the editor of the journal at that time and was enormously helpful in shaping the Craik and Lockhart piece. He also became interested in the empirical aspects of levels of processing, so he and I collaborated on an experimental article that came out in the *Journal of Experimental Psychology: General* in 1975.

Meanwhile my interests had turned back to problems of aging. The key event here was an invitation by Jim Birren and Warner Schaie to contribute a chapter on age-related differences in memory to the first edition of their *Handbook of the Psychology of Aging* (Craik, 1977). When Birren called me about this in the summer of 1973 I was initially quite surprised as I had not been heavily involved in aging research in recent years. However, he persuaded me that this distance from the field would give my views some perspective and objectivity, so I started reading the latest articles and thinking about the issues again. At that time I think it is fair to say that the field of cognitive aging lagged a few years behind its parent fields of “straight” research in perception, attention, memory, learning, and thinking. I had that impression, at least, and thus was emboldened to offer various gratuitous

suggestions on how research on memory and aging should proceed. Having made these insightful suggestions, of course, I was then tempted to follow them up myself—so I was drawn back into the field.

I completed the first draft of the *Handbook* chapter at the end of 1974 and sent off the final revision in May 1975, commenting in a letter to Jim Birren that I had *enjoyed* writing the review and that it had rekindled my interest in the research on aging. I had received helpful suggestions on the first draft from several people, but I might single out the Canadian psychologist David Schonfield in particular—he wrote me a four-page letter with detailed and insightful points. In his letter Schonfield mentioned his concept of “remembering to remember”—a notion that grew into the current research topic of prospective memory, that of remembering to carry out an action at some future time. It is interesting to reflect that Schonfield was thinking about such issues in the early 1970s. One other letter I received in connection with the *Handbook* chapter was from Alan Welford. In reply to my inquiry he said he had nothing new to report on the topic of memory but added that he was surprised to see my address and sorry that I had been “lost to Britain.” The pangs of guilt I might otherwise have felt at this mild admonishment from a much-respected senior figure were mitigated, however, by the fact that Welford’s letter came from Adelaide, Australia, where he had moved as head of department some years before.

The conjunction of the *Handbook* chapter and my experimental work on levels of processing set me thinking about possible age differences in depth of processing. Was it possible that part of the typical age-related drop in memory performance is due to a failure of older people to process words and other events in a sufficiently deep and elaborate fashion? An exploratory study was carried out by Sharon White, a senior undergraduate at Erindale, and this yielded the interesting result that older people recalled and recognized fewer words than their younger counterparts when instructed simply to learn a list, but that the age difference was eliminated by the combination of a semantic orienting task at encoding and a recognition test at retrieval. That is, the simple device of asking a semantic question at the time of acquisition (e.g., Is the word a jungle animal? TIGER) coupled with a recognition test appeared to “repair” the deficiencies in older participants’ encoding and retrieval processes. Although it was only an undergraduate thesis we thought that this interesting result was worth including in the *Handbook* chapter.

This basic result and the ideas that it engendered were developed over the next five or six years in conjunction with various postdocs and graduate students—Eileen Simon, Jan Rabinowitz, Brian Ackerman, and Mark Byrd were some of my main collaborators. We postulated that the aging process was associated with a reduction in “processing resources”—essentially the amount of attention that could be devoted to information processing—and that this reduction curtailed the rich and meaningful analysis of perceived events that occurs spontaneously in younger people. We pointed out that when attentional resources are experimentally curtailed in young adults by having them perform two tasks at once (a memory task and some further attention-demanding task), the resulting pattern of memory performance is essentially the same as that seen in older people working under full attention conditions. These findings bolstered our faith in the notion of depleted resources, although (for reasons I have never fully understood) the idea of a general reduction in processing resources has never met with much favor. The further idea was that whereas depleted resources would typically be associated with inefficient encoding and retrieval processes, these impairments can be alleviated by guiding appropriate processing at encoding by means of orienting tasks, and guiding appropriate processing at retrieval by providing cues, context, or re-providing the event itself in a recognition test ( Craik & Byrd, 1982; Craik & Simon, 1980; Rabinowitz, Craik, & Ackerman, 1982).

The salient events around this time included a sabbatical year spent in the Department of Psychology at Stanford University in 1977–1978. The Stanford department had a stimulating group of visitors that year—nobody in aging as such, but a number of researchers in perception, cognition, and developmental psychology. I shared an office with Jim Cutting and talked a lot to Ann Brown and Rochel Gelman, as well as to various locals including Gordon Bower, Ellen Markman, and John Flavell. In the summer of 1978 I attended the first Talland conference in Boston, organized by Lennie Poon and others. I found this meeting enormously rewarding and enjoyed making some new research contacts, including Tim Salthouse. A couple of years later Sandra Trehub and I organized a conference on aging at the Erindale Campus of the University of Toronto. We tried to cover a range of topics related broadly to age differences in cognitive processing—from the biological basis to social implications. Thanks to the support of various groups such as our local Program in Gerontology headed by Blossom Wigdor we were able to attract a galaxy of aging stars (as it were) including Pat Rabbitt, Marion Perlmutter, John Horn, Nancy Waugh, David Arenberg, Marcel Kinsbourne, and Paul Baltes. The papers were uniformly excellent, and Trehub and I later brought out an edited version of them in book form (Craik & Trehub, 1982).

Like any involved researcher I have served on the editorial boards of a number of journals over the years—British, Canadian, American, and European journals dealing both with mainstream cognition and with cognitive aging. My one stint as main editor was with the *Journal of Verbal Learning and Verbal Behavior* from 1980 to 1984. This journal, known popularly as *JVLVB*, was probably the major journal of human memory and learning in the 1960s and 1970s. By 1980, however, the emphasis was switching to experimental studies of language, and when Marcel Just and Patricia Carpenter took over the editorship in 1985 they changed the name to its present title, *Journal of Memory and Language*, to reflect this change. I checked back through the contents of the four volumes that I edited to see whether my presence as editor had encouraged a wave of articles on aging. The numbers for the four years were, zero, zero, one, and two for 1981, 1982, 1983, and 1984, respectively. An optimist might perceive a trend here, but it is hardly a flood.

I spent the 1982–1983 year back at Stanford, this time at the Center for Advanced Studies in the Behavioral Sciences. Several other memory researchers were there at the same time, including Matthew Erdelyi, Bobby Klatzky, and Bob Crowder, so we formed a stimulating and useful discussion group. Someone, it may have been Tom Trabasso, urged me to read Herbert Simon's book *The Sciences of the Artificial*, and I was very taken with Simon's stress on the role of the external environment in both engineering and biology. That is, to be adaptive, a device or an organism must function well in its particular set of external circumstances, and in that sense the external environment shapes, constrains, and supports the inner environment of the system in question. It can be argued that babies and very young animals are quite dependent on a benign external environment and react rather passively to environmental changes. As the baby matures into infancy and childhood, he or she becomes progressively less tied to the here and now, and begins to act *on* the external world rather than react to it (this is one of Piaget's fundamental ideas on cognitive development). It seemed to me that older adulthood might be associated with some reversal of this trend, so that the older person's mental processes are again acted on by the external environment rather than acting to control the outside world.

These ideas can help us understand the varying patterns of age-related losses in memory. A retrieval test that is well supported by environmental context or by the representation of stimuli (as in recognition memory) will show only slight age-related losses, whereas a test with little environmental support (like free recall of a list of items) will require

a lot of self-initiated activity and will show substantial losses with age. This dissociation between recognition and recall had been demonstrated by David Schonfield and was shown again in a more complex experiment published in 1987 by Joan McDowd and myself ( Craik & McDowd, 1987). At the time of acquisition, a task requiring self-initiated processing is one in which participants are simply instructed to "learn this material," whereas environmental support can be provided by, for example, organizing the material into a form compatible with the learner's current store of schematic knowledge. I presented these ideas first at a symposium on memory organized by Donald Broadbent at the Royal Society in London and subsequently at a meeting in Berlin commemorating the centenary of the publication of Ebbinghaus's groundbreaking book *On Memory*. The written versions of these talks were published later in the proceedings of the two meetings (Craik, 1983, 1986).

I have always been interested in the biological mechanisms underlying behavior, and my research in the past 10 years has been progressively concerned with the neural correlates of memory. Following the pioneering work of Brenda Milner and her colleagues in Montreal, the hippocampus and its surrounding structures in the medial temporal lobes have been viewed as the crucial structures for the formation of new memories. More recent work has suggested that the frontal lobes also play an important role; for example, my colleagues Morris Moscovitch and Gordon Winocur have proposed that the medial temporal structures function in a rather automatic way during acquisition and retrieval, whereas the frontal lobes function in a more strategic fashion to select and control what is encoded and what is retrieved. These ideas are of great interest to cognitive aging researchers, because there is evidence for a similarity between normal aging and frontal lobe pathology—older people behave in some ways like patients with frontal lobe damage. One function of the frontal lobes appears to be the integration of information about an event with its temporal and spatial context. Thus, frontal lobe patients may exhibit "source amnesia"—that is, they can remember facts but are unable to recollect where and when they learned these facts. John McIntyre is a colleague from Winnipeg who spent some months in Toronto, and he and I carried out some experiments to show that normal aging is also associated with a mild form of source amnesia; older people show a disproportionate loss of contextual information relative to focal or factual information (McIntyre & Craik, 1987). My colleagues and I later followed up this finding by showing that, within a group of older people, those who showed the greatest loss of contextual information also tended to score poorly on neuropsychological tests of frontal lobe functions.

The recent emergence of neuroimaging techniques has allowed neuroscientists to look rather directly at these problems. The University of Toronto acquired a Positron Emission Tomography (PET) scanner in 1992, and I have been involved in some exciting studies using this new methodology. A group led by Shitij Kapur found that "deep" processing of words was associated with specific activation of an area in the left dorsolateral prefrontal cortex, for example; thus our 1972 ideas on levels of processing were given some neurological justification in 1994. Endel Tulving then pointed out that several PET studies had shown that encoding processes were consistently associated with *left* frontal activation, whereas retrieval of the same material was associated with *right* frontal activation. At first it seems strange that information should "go in one lobe and out the other," but it seems likely that these frontal structures are mediating *control* processes for encoding and retrieval and are not the actual storage sites, which are probably located in posterior regions of the cortex.

I have been fortunate throughout my career to have had excellent students, postdoctoral fellows, and faculty colleagues; my own thinking has been influenced greatly by the suggestions and theoretical ideas of these various collaborators. In the mid-1980s, for example, Robin Morris came over from Britain to work with me, and he, I, and Mary Gick

collaborated on a series of studies on working memory and age. Robin had been one of Alan Baddeley's students and so came primed with the latest ideas from Cambridge. This joint collaboration resulted in a series of studies illustrating age-related changes in the ability to hold and manipulate information held in the mind; older people are poorer at this, especially if they have to carry out a second operation while attempting to perform the primary task. As Welford had suggested in 1958, this type of age-related difficulty may lie at the heart of many cognitive difficulties experienced by older people.

Another important idea, whose implications we are investigating at present, was proposed by my friend and colleague Larry Jacoby. His suggestion is that most cognitive operations reflect a mixture of specific, consciously controlled processes and more general, unconscious, automatic processes. Normally it is impossible to specify the proportions of each type of processing involved in a complex task, but Jacoby has devised an ingenious opposition procedure to dissociate the processes and measure them separately. This procedure has been used by Jacoby and his students to study age differences, with the finding that age-related losses in memory are confined to consciously controlled process; it seems probable in fact, that older people make *greater* use of stereotyped automatic processes, in compensation for their loss of specific control. These ideas and preliminary findings have exciting implications for tying together notions of frontal lobe function and dysfunction, the underlying neurology, and the consequences of reduced cognitive control in perception, attention, memory, and thinking.

In the past 10 to 12 years, two events that were significant for my involvement in research on aging were, first, the formation of the Canadian Aging Research Network (CARNET) and, second, the foundation of the Rotman Research Institute. In the late 1980s the Canadian government decided to allocate a large sum of money to research; not to the existing research councils but in the form of a new competitive program for research coalitions or networks. Its idea was to involve collaborative groups of researchers from across Canada in various research endeavors and also to steer this research in ways that would benefit the country by requiring that each research network should involve partners from the private sector. These conditions were not immediately appealing to a theoretically oriented university researcher like myself, but Blossom Wigdor persuaded a group of us that a network addressing the social and psychological aspects of aging might well be attractive to the politicians who ran the program. And so CARNET was born. After some initial reversals, the network was funded with Victor Marshall as director. Victor is a sociologist of aging who was at the University of Toronto at that time, and so two of the three groups that coalesced (loosely) to form CARNET were made up of sociologists and the third, headed by myself, was composed of cognitive aging researchers from across Canada. One initial hope was that the sociologists and cognitive psychologists would join forces to tackle problems of mutual interest (the effects of different living arrangements on cognitive performance, for example), but this happened to a very limited extent, I must confess. The cultural divisions between community-oriented sociologists and biologically oriented cognitive psychologists are surprisingly impermeable. In other respects, the CARNET venture (which ran from 1990 to 1995) was quite successful; the funds enabled Canadian researchers of cognitive aging to meet and discuss issues on a regular basis, and the funding philosophy also pushed a number of us to apply our ideas to real-world problems. The bureaucracy of the federal program was a major downside, on the other hand, and (as the PI of the psychology group) I was not sad to see the funding end in 1995.

The Rotman Research Institute was set up in 1988 owing to the efforts and vision of Joseph L. Rotman, a Toronto-based businessman and entrepreneur. The institute is part of the Baycrest Centre for Geriatric Care; its director is Donald Stuss, a neuropsychologist

specializing in frontal lobe function, and its mission is to study normal and abnormal cognitive processes as they occur in normal aging, pathological aging, and as consequences of brain damage. I was initially involved in planning the institute when I chaired the psychology department at the University of Toronto (1985–1990) and have since been an associate scientist with the group. In 1997 I was honored to be appointed to the Glassman Chair in Neuropsychology—a University of Toronto position but designed to strengthen the links between the university and Baycrest. So at present I spend one or two days each week at the Rotman Institute, carrying on my research into cognitive changes in normal aging but also becoming involved in studies with brain damaged patients and in the development of rehabilitative techniques. So I am now using neuropsychological tests routinely, as well as traditional laboratory methods, and (as mentioned previously) I am getting involved in the exciting new world of neuroimaging. Perhaps my early medical school training will come in useful after all.

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A History of  
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*in* Autobiography

*Edited by*  
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*Washington, DC*

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