Autobiography of Division 3 President-Elect

Nelson Cowan, Division 3 President-Elect

It seems to me that, as an experimental psychologist, I have an outer, middle, and inner autobiography to write. The outer autobiography includes the story of my published research and interactions with colleagues and students. The middle autobiography includes the research topics that interest me a great deal but that I cannot research myself, knowing that it takes a tremendous investment of time to branch out, and perhaps some skills that I never developed. Finally, the inner autobiography includes thoughts, feelings, and anecdotes that have led to the choice of experimental psychology, and have in turn been influenced by it. I believe that thinking like an experimental psychologist has a profound influence on just about all aspects of my life journey. I will intertwine these three autobiographies as best I can.

My father, Arthur Cowan, went to Boston Latin High School and had dreams of going to Harvard to study engineering. However, he was discouraged from that by what his high school principal said: that if you are Jewish, you should not even bother to apply, given the quota. (It must be hard for young people today to grasp how different things used to be.) After returning from World War II, my father became an optometrist and moved to Washington, D.C. because he liked the city and being in the swim of political action. He soon met and married my mother, Shirley B. Cowan (then Frankle) from Baltimore, and I was born in 1951 as the oldest child of 4. For many years, my mother worked as my father's receptionist.

There was at first considerable friction between my father, who was secular in orientation, and my mother's Orthodox Jewish family. The continuing family debate related to this topic provided me with an early exposure to point-counterpoint thinking. This was reinforced by the Jewish tradition in which studying and mulling over beliefs are encouraged and in which there are no thought crimes, only crimes of action.

From first grade through high school, we lived in Wheaton, Maryland. Almost everyone's early path meanders a bit, and I will describe this briefly for the curious. Many of my early interests were somehow related to the themes of the mind, mortality, and eternity, including suspended animation, on which I did a research project in high school using rotifers in a glycerin bath cooled with dry ice according to a local medical researcher's instructions; relativity physics, for which I simply read, pondered, and talked to a couple of physicists; and philosophical thoughts inspired by media that pushed the boundaries, such as Twilight Zone episodes and stories by Jorges Luis Borges. My father, the optometrist, taught me how to make telescopes out of trial lenses, and loved to play chess with me. A trip to his office in Washington, D.C. quite often included a detour to the Supreme Court, where he always pointed out the motto on the building, "equal justice under law." In general, though, my caring and intelligent parents were tied up in colorful family relations that included a younger brother who was developmentally "differently abled" and the two still younger siblings. Given that busy environment, I often consulted friends more than parents. In fact, it took me a long time to realize how important it is to consult knowledgeable and older people for advice. (I still hope to learn it better!) A few times during high school, to escape excessive noise and commotion in the house, I studied for an exam by the light inside the car. Now, perhaps as a result, I have no trouble concentrating in any semi-ordinary surrounds.

I think that I first learned about research on the mind when I was 11 years old and read in the *Washington Post* that Watson and Crick won the Nobel Prize for determining the structure of

DNA, after which Crick said he wanted to find out "how the brain works." I also recall an episode of the television show called *Watch Mr. Wizard*, which made the interesting psychological point that it is difficult for people to think up truly random number sequences. Such points intrigued me. During high school, I read an NIH summary of research on sleep and dreams that happened to be on the shelf in the public library. One rainy, summer day after reading it, I sat on our covered porch thinking and resolved that, inasmuch as life is short, the most meaningful thing for me to do in my brief time would be to study human conscious experience scientifically. I pretty much stuck with that, and now aim my research at learning how we can measure the information that can be held in mind at once, termed "working memory" or "the focus of attention"; and this topic has sustained my interest. (The terms "short-term memory" and "working memory" shift in their definition from one researcher to the next, so here I will just use them interchangeably.)

From early on, something that impressed me about the science of the mind and behavior, and continues to impress me, is that humans cannot be sure about their beliefs. There are illusions and imperfections in human thought processes from perception up through memory, attention, reasoning, decision-making, problem-solving, and social interaction. I think that the world would be a much better place if more people were taught about this and therefore could have a little humility about their beliefs. Though I do not always have an opportunity to incorporate the point into my published papers, I always emphasize this aspect of the field in my teaching.

My continued dedication to the field took me through undergraduate school at the University of Michigan in the Vietnam era, where I was allowed to devise an independent major in the neurosciences and do a lot of independent study. My parents lived close enough to the National Institutes of Health (NIH) in Bethesda, Maryland that, in the summers, I could ride a bicycle there through Rock Creek Park to work with brain researchers. I did so after my sophomore and junior years of college, once as a volunteer and once with pay. During college I wrote in-depth papers on the chemistry of schizophrenia and, with some guidance from Monte Buchsbaum at NIH, on language and hemispheric specialization in the brain. I also learned how to program computers at NIH, with the Fortran language on, I think, a PDP-12 computer (about the size of a tall bookshelf). One notable event was that, in my introductory psychology class in 1970, a graduate student, David Pisoni, gave a guest lecture on the topic of verbal short-term memory. I met him personally about 10 years later at a conference, where he saw my poster and mentioned that my data looked a lot like his. At that time, we had fun discussing his early influence on my career.

Toward the end of college, I did temporarily have some doubts about an academic future and took a year off from school, in 1973-74. For a few months I worked in a treatment center for psychotic teens in the suburbs of Berkeley, California. Other counselors there heard my musings about a vague intention to apply to graduate school and alarmed me about the timetable, provoking timely action. Consequently, I went to graduate school the next year -- at the University of Wisconsin in an infant speech perception laboratory under Philip A. Morse and in collaboration with a pediatrician, Lewis A. Leavitt. At the Waisman Center, an interdisciplinary facility to study mental retardation and human development, Morse and Leavitt maintained a friendly, dynamic, spirited environment, with lots of teamwork and a well-equipped and well-funded laboratory.

Early in graduate school, one strong interest of mine was in the basic elements of conscious human experience, such as the largest amount of time during which two events seem

simultaneous. It was called the psychological moment and is one of those topics on which you don't see much work anymore, but it naturally led me to topics that I still study: sensory memory, working memory, and attention. During graduate school, I also developed an appreciation for how development could serve as an interesting framework from which to observe how the adult structures of the mind are constructed. However, applying the topics of my interest to infant research proved to challenging. My advisor was brilliant but he was young; about 30. He had developed some infant perceptual discrimination procedures that, as we finally learned, were quick but not very sensitive in detecting fine-grained distinctions that infants can pick up from the environment. I ran about 500 infants, 4 and 5 months old, through two series of experiments that did not really work out and were never published. (One was on hemispheric laterality; Catherine Best, who studied at Michigan State University, soon published almost the identical study, but with an apparently more sensitive version of the heart rate deceleration procedure than we were using.) That was a tough time for me because I wondered whether I was cut out for research, having not had much record of publishing success.

Within a few years, we did publish a study with a different technique that suggested that infants have a longer auditory sensory afterimage than adults do. This would give infants more time for perceiving sounds, but less temporal acuity than adults. In that sense, it seemed to indicate that there is a grain of truth after all in William James' suggestion, which is generally dismissed, that infants live in a world of "blooming, buzzing confusion." I also picked up an interest in language development in graduate school, and especially the topic of language universals, those aspects of language that are determined by our human heritage. I wanted to see how much of the system of grammar was innate (a lot, according to Noam Chomsky) and how much stemmed from logical constraints on language, such as the need to mention the thing you are talking about before describing it.

With few publications coming out during graduate school, it is lucky that I was able to get an NIMH postdoctoral fellowship, which took me to New York University with Martin D.S. Braine in psycholinguistics during 1981-1982. During this time, as well, some work from my graduate school years and some new work began to come to fruition. I wrote an article on two types of auditory sensory memory, published in *Psychological Bulletin* in 1984.

Lewis Leavitt, Martin Braine and I worked on a particularly fun and unusual scholarly detour. Leavitt had found a professor with the ability to talk backward fluently. We were able to get him onto a radio show, after which other people with that skill came forward. We eventually had a sample of 50 people from around the world who could talk backward fluently. It turned out that about half of them did this on the basis of the sounds in the words, whereas the other half did it using letters. They all recollected that they started this practice in the early elementary school years. We also studied two children with this skill. They were not as fast as the adults and, when we revisited them 5 years later, when they were teens, they had not practiced during the interim and were rusty. We never found out why some people can talk backward fluently and continue to do so in adulthood, but the project taught us a few things about psycholinguistics. The sound-based backward speakers did not really operate like a reversed tape recorder; instead they broke the words up into smaller building blocks called phonemes, and reversed the order of those. For example, the reversal of the word *fine* [faIn] was not [nIaf] but [naIf] because [aI] is a single phonemic unit, a diphthong. The word *garage* [guhradz] in reverse was [dzaruhg] because the two instances of *g* have different sounds. This project showed that the abstract phonological units that linguists discuss have some psychological reality, though the method of talking backward was also influenced by the speakers' naive analysis of language. I

stopped working on this topic in the late 1980s, but I hope to put a backward talker in an fMRI scanner some day.

My first academic position was non-tenure-track, at the University of Massachusetts at Amherst, where I got a New Investigator Award in 1984. (It evolved into an R01 grant for a project that is still active, with funding for all but a few lapses throughout the years.) During a divorce but not because of it, I moved to the University of Missouri in 1985 and that same year I met my second wife, Jean Ispa, who fortunately is still willing to put up with me. We were married in 1987 and have raised her two children, Simone and Zachary, and our son, Alexander, together quite happily. Jean is a professor of Human Development and Family Studies with interests in effects of child care, social development, and culture. On many occasions, we talk about psychology and help each other in various ways, although we have not published together. We have had two occasions to take simultaneous sabbaticals, living for 3 months in Helsinki, Finland (in 1991) and 4 months in Bristol, England (in 2000) as well as traveling together to Europe, Japan, and Australia. Such are the fringe benefits of dual professorship. I became an associate professor with tenure in 1989 and a full professor in 1994. I am grateful that my interest in the mind has stayed intact throughout my education and career, and hope to awaken or sharpen such an interest in students.

I feel fortunate about the students and colleagues that I have been able to work with throughout my career. My department at the University of Missouri has been quite supportive of research, and does a lot to help the professors avoid becoming overwhelmed with teaching and service responsibilities. Most of my research has been on working memory and selective attention, and their development in childhood. The research on working memory has been designed primarily to try to understand what the limits are in the amount of information that can be saved in mind at once. This is important not only because working memory provides a window into conscious experience, but also because most of our complex thought processes depend on the information in working memory. There are only a few ways in which working memory could be limited. Early in my career, I tried to determine what the time limits of working memory may be, a classic question that as never been resolved. I have not been able to answer that question, although some useful information about immediate memory of verbal lists has come from that research. For example, consider the observation that you can get an advantage for the most recent items in the list even if you put a distracting task after each item, which has been taken to negate the existence of a time-limited working memory. Our research from the 1990s, and some other research, has shown that the pattern of results is nevertheless much different when the distracting tasks are present, which tends to reinstate the usefulness of the short-term working memory concept.

Compared to time limits, I think that more progress has been made with another sort of limit, the limit in how many items can be held in the focus of attention at once. It was commonly assumed that this limit was determined by George Miller in his famous 1956 paper, with a title that begins, "The magical number seven plus or minus two." However, a 2001 review that I published seems to indicate that this is a compound limit that can be reached only if the core capacity limit is supplemented by strategies such as rehearsal and grouping, and sometimes by a vivid sensory memory. Without those aids, people can remember about 3 to 5 items, no matter whether they are multiple items on a computer screen or a series of spoken items. The individual differences in capacity, though numerically small, do seem to be rather strongly related to individual differences in cognitive aptitudes. Recent work with a graduate student, Zhijian Chen,

and two former students, J. Scott Saults and Candice Morey, has strengthened the argument for a fixed capacity limit.

Attention seems to be used not only to hold information, but also to process that information and shut out irrelevant information. Work with two former students, Noelle Wood and Emily Elliott, was on that topic. A collaborative 2001 paper with Andrew Conway interestingly showed that people with relatively high working memory are much less likely than other people to notice their name when it is presented in an unattended auditory channel. Noticing too much of the environment is, at least in some ways, a weakness rather than a strength because it clutters up the limited working memory capacity.

Although most of my work has gone into empirical studies, what other researchers seem to refer to most often is my attempts to reach a synthesis of findings across studies from my own laboratory and others. I have summarized a simple model of the processing system, in a 1988 *Psychological Bulletin* article and subsequent books published in 1995 and 2005. In this model, or modeling framework, working memory is not a single mechanism; it is the sum of activated portions of long-term memory and, within that activated memory, the subset that is in the focus of attention. New information automatically activates some features of long-term memory but, for a rich perceptual analysis and memory storage of particular stimuli, attention needs to be recruited. This recruitment of attention to certain stimuli occurs either deliberately on the subject's part, or automatically in response to abrupt changes in the environment. Some investigators have found this conceptualization natural and easy to use.

My developmental work has been aimed at determining whether the basic abilities of working memory, including its persistence and its capacity, increase with development over childhood. The research suggests that these abilities do moderately increase. However, a more salient change that we have seen is in the benefits that accrue from the development of mnemonic strategies. In one recent study of name-location associations in working memory, there was a dramatic change in the pattern of responding with development that was due mostly to the improvement of rehearsal skills. When adults were required to repeat the word "the" while doing the task, preventing verbal rehearsal, their pattern of performance looked very similar to that of third-grade children.

Recently, I have extended my work in collaborations with a mathematical researcher, Jeffrey Rouder; a researcher of cognitive aging, Moshe Naveh-Benjamin; and researchers of amnesia, Sergio Della Sala and Michaela Dewar. In the latter collaboration, for example, we found that many densely amnesic individuals can remember a lot more if all interference is removed from their environment for a few minutes after learning something.

If I had my way, I would build more bridges between sub-disciplines in scientific psychology. Many of the human and animal researchers study similar issues with different terminology. Social psychologists often use experimental methods and learn things that should be of great interest to us. Many brain imagers are looking for ways to continue to be experimental psychologists also, such as by using transcranial magnetic stimulation to affect cognitive processes. Psychologists studying cognition and those studying emotion are finding that they can no longer remain in separate camps, given that the brain research is suggesting a strong and natural bridge between the two topics.

The history of experimental psychology is also still germane, and I hope to bring a bit of it into future columns. I feel that experimental psychology has been an incredible influence in my life, and has even made me feel more humble in my interactions with other people. I hope

that my observations will inspire younger academics. I am quite pleased and honored to have been elected president of Division 3 of APA and I hope I be of service to the division.