

## Chapter 19 LIVING WITH GERONTOLOGY

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This chapter describes how I came to be a gerontologist, or in my case, how I became intrigued with the study of psychological development from young adulthood to advanced old age. Inevitably, it is also an account of how my career became interwoven with a program of scientific inquiry conducted by me, my associates, and my students over the past 40 years that has come to be known as the Seattle Longitudinal Study (SLS; Schaie, 1996).

When I entered the field of gerontology in 1951, few people knew how to spell the name, let alone being able to offer a meaningful definition. Those who did would be most likely to respond to a young student interested in gerontology by asking, "Why do you want to worry about old people, why not do something mainstream?" That was, for the most part, the response I received from my teachers and peers at the time. Hence, this autobiographical account also contributes to the story of how what once was considered an idiosyncratic interest eventually developed into a lifelong career that today nobody would doubt to be in the mainstream. Given the small number of early geropsychologists, I may have been privileged to have had at least some small influence on the progress of our field. For this opportunity I am very grateful to a number of teachers, colleagues, and students whose influences on my own scientific development I will attempt to trace in this chapter.

### Childhood and Adolescence

I was born in 1928 in the town of Stettin, which then was the provincial capital of Pommerania, one of Germany's pre-World War II political subdivisions. My parents were Jewish middle class; my father and mother owned a small outfitters store for the then rapidly

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In 1996 I published an autobiographical account of my pursuits as a developmental psychologist, focusing more specifically on the Seattle Longitudinal Study under the title of "The Natural History of a Longitudinal Study," in M. R. Merrens & G. G. Brannigan (Eds.), *The Developmental Psychologists* (pp. 232-249). New York: McGraw-Hill. The present chapter represents an expansion of the personal autobiographical material as well as a greater emphasis on my career development in gerontology. But one can live only one life, and the reader who is familiar with the earlier work will notice, of course, considerable overlap that was necessary to present a coherent story.

The program of research that has formed the scientific basis of much of my career, including preparation of this chapter, has been supported since 1963 by various grants from the National Institute of Mental Health and the National Institute on Aging. It is currently supported by research grant R37 AG08055 from the National Institute on Aging.

growing crowd of motor bikers. My native town was a sleepy provincial city of about 150,000 inhabitants (involved primarily in the garment industry, ship building, and fish processing) as well as a terminal for transferring grain and coal from the river barges to freighters that went to Scandinavia, Russia, and beyond. It was also a major garrison town, and as Germany rearmed the barracks multiplied and colorful parades were common. The big excitement for me was a visit to Berlin, which was an hour's train ride away. We usually stayed with two of my grandmother's widowed sisters, my first intensive interaction with old people.

The Great Depression began in Europe shortly after I was born, probably the major reason why I remained an only child. Not very long thereafter the unemployment lines lengthened, the Weimar Republic went on a course of self-destruction, and Hitler and his Nazis soon took over. When I was 6 years old and the time came to start elementary school, I therefore attended a private school that had hastily been formed by the local Jewish community to protect its children from the daily harassment experienced in the public schools. I attended that school through the middle of fifth grade, learning enough basic skills such that I can still converse in German and write grammatically correct prose in that language, although my German is studded with archaic colloquialisms that were common in the 1930s.

While in the middle of fifth grade, there came Crystal Night (November 9, 1938), the systematic destruction of Jewish synagogues and stores by Nazi hooligans, as well as the incarceration of most Jewish men in concentration camps. My parents' store was destroyed, but my father was able to avoid being taken to a concentration camp by going into hiding. He now began desperately to seek a way for our family to leave Germany, because the likely consequences of our remaining had become convincingly clear. By that time hardly any country was willing to accept Jewish refugees from Germany. The question thus became primarily one of how to get out, regardless of where one might end up going. My father discovered that it was possible to book passage on an Italian cruise ship that plied a route through the Suez Canal, then around India and Malaysia, ending up in the port city of Shanghai, China.

In June 1939, my parents and I took the train from Stettin to Trieste (the two anchor points in Winston Churchill's famous iron curtain speech) and embarked, not really knowing where we would wind up. After several futile attempts to obtain permission to go ashore along the way we finally were allowed to enter Shanghai. At the time Shanghai was still an international settlement governed by the consular representatives of 17 nations that were signatories to the so-called unequal treaties. Through these treaties, during the 19th century, foreign concessions had been created on Chinese soil that were not subject to Chinese law. The reason we were allowed to land was primarily because of the fact that the amorphous local government had not been able to get its act together to keep us out!

The trip to the Far East and the bustling and exotic streets of Shanghai seemed high adventure to an 11-year-old. Hence, I gave little thought to the uncertain future facing my family. There was a large foreign population in Shanghai, with a substantial Jewish community that had settled there during the expansion of Western trade in China or who had taken refuge from the Bolshevik revolution in Russia after World War I. Some of these people had even acquired great wealth, and they formed charitable organizations that attempted to provide shelter and food for all the refugees and education for the young. I attended a school for refugee children for about two years, acquiring English language competence and completing an educational program that would approximate that of an American junior high school. Then came Pearl Harbor, my English and American teachers were interned by the Japanese authorities, and at age 14 I became an involuntary high school dropout.

After the Japanese authorities made all the refugees relocate to a ghetto area, vocational options became quite restricted. I was fortunate enough to find a job as an apprentice in a small print shop, where I learned some typesetting skills. When the war ended in 1945 and the local English-language newspaper reopened, I managed to get a job in their print shop and learned how to use a Linotype machine and to typeset newspaper advertisements. The labor unions were already dominated by the communists, and they did not like a foreigner looking over their shoulder in the print shop; they soon forced me out.

During my final months in Shanghai, I had the opportunity to work as an untrained social worker with the American Joint Distribution Committee working with people about to be resettled in the United States. Here I first became intrigued with the infinite variety of individual differences in life experiences and reactions thereto, as well as in the resilience of adults in adapting to profound stresses and adapting to externally imposed changes of life conditions.

### Young Adulthood

The communist armies were beginning to approach the gates of Shanghai. My father had died of a stroke in early 1947, and my mother was too distraught to actively participate in planning our future. Thus in 1947, I unilaterally decided it was time to resettle myself and my mother to the United States, and to our great relief we were able to leave Shanghai in November of that year on a former troop transporter (the SS *General Gordon*), arriving in San Francisco on December 17. I still vividly remember sailing under the Golden Gate Bridge in the morning fog, wondering what lay in store for me in a new country.

I have often been asked whether coming to San Francisco was a strange and stressful experience. My response has always been that, to the contrary, it seemed much more like a homecoming. Shanghai had presented us with the need to adapt to a totally different culture, within a strange environment, whose language and customs we did not understand, where water and many foods were unsafe, and many familiar foods were unavailable. By contrast, having acquired fluency in English, in San Francisco I could understand what everyone said, I could read all the signs, food and water were safe, and many of the conveniences of life we once knew were once again available to us.

After a week in San Francisco, I met with a caseworker from the agency that had sponsored our immigration to the United States to discuss the future. I was informed that the Eugene, Oregon, Jewish community had agreed to sponsor us and that a job had been found for me as a busboy in a restaurant. I pondered for a minute or two. As a brash 19-year-old, I then revved up my courage to tell the caseworker that this plan didn't quite match what I had expected would be possible for me in America. I thanked her politely and told her that I would first see what I could do for myself during the next few days. Indeed, the next day I had found a minimum-wage job in a small print shop, and the following day I moved my mother and myself into a small apartment; I promptly informed our sponsors that I had completed resettling us and that their help was no longer needed.

My printing experience had served me well in making it easy to find my first American job and it continued to help. By the summer of 1948, I had managed first to move to a better paying job at a suburban newspaper and, after being admitted to the printer's union, was able to find work in the composing room of the *San Francisco Chronicle* typesetting and making up display advertisements. The *Chronicle* was and remains a morning newspaper, which means that printers typically work at night. Hence, there was little to occupy my afternoons. One day, placing some of the display advertisements into a newspaper page, I noticed a story on a high school program for adults at the local community college. Never having completed high school, on the spur of the moment I decided that it might not hurt me to have a high

school diploma. I enrolled at City College of San Francisco, took courses in civics, American history, and chemistry, but was able to test out of most other requirements (including high school English) and obtained a diploma from the San Francisco Unified School District at the end of the first semester.

### Becoming a Geropsychologist

It became very clear to me that I did not wish to seek a lifelong career as a printer and that I wanted somehow to become an educated person. But why did I choose to eventually enter geropsychology? As will become clear from this section, I started college primarily interested to embark in some social service field, but serendipity soon intervened, and by the end of my junior year it was almost certain that I had found my niche in the study of aging. Moreover, another serendipitous choice of the research population studied for my dissertation would point me toward centering on the aging of intellectual competence as my central academic concern.

### *The College Years*

The environment at San Francisco City College was very pleasant. It was a great opportunity to make new friends, the work was stimulating but not unduly demanding, and I was well able to get it done even while holding a full-time job. Having gotten used to and liking the college setting, I decided to go on, building my program of studies primarily around those courses that were offered in the afternoon so that I could sleep in the morning following my night shift as a printer. Because most science labs were offered in the morning, this meant that I was destined to concentrate on social science topics.

At City College I was influenced particularly by my English composition instructor, Donald Snapp, who with great patience helped me hone my writing skills and also exposed me to an understanding of the many metaphors in both classical and modern English and American literature that are the bane of the nonnative English speaker. Even more important was Ralph Granneberg, my instructor in the introductory courses in psychology and sociology, who first exposed me to principles of experimental psychology. He probably single-handedly convinced me that psychology was a science that should be taken seriously.

The California higher education system allowed automatic transfer to the state university system on graduation from junior college with a C average or better. Thus, after obtaining my AA, I transferred to Berkeley as a psychology major. Being a newcomer to the States, I really had not been fully aware of the world-class caliber of the University of California—Berkeley campus. Berkeley was an exciting place to be in the 1950s, and all of a sudden I found myself being taught by the people who had done the research and written the textbooks. Not only was the faculty outstanding and intellectually demanding, but the undergraduates were extremely competitive, and most of my smaller upper division classes had mixtures of graduate students as well. After some hairy times, more intensive work, and lower grades than I had come to expect at junior college, I managed to find my footing and made good use of my time.

The highlight of my first semester at Berkeley was an exciting tests and measurement course from Read Tuddenham, to whom I promptly shifted as my advisor. Once again I was having trouble building a full schedule confined to the afternoon. I therefore asked Tuddenham to do a directed study with him. Discussing various possibilities, I idly mentioned

that I had thought his class discussion of Thurstone's (1938) primary mental abilities (PMA) work interesting, and wondered whether there had been any work done on the PMA in adults. As a good teacher, Tuddenham told me to go to the library and find out.

Thurstone in the 1930s had analyzed more than 60 measures of mental ability with large samples of children and adolescents in Chicago. Applying his new method of centroid factor analysis he discovered that individual differences on these measures could be accounted for by no more than 10 factors, which he thought of as the "building blocks of the mind." Thurstone published a formal test of the five most important of these ability factors. They were Verbal Meaning (a measure of recognition vocabulary), Space (a measure of being able to rotate abstract figures in two dimensional space), Reasoning (a measure of the ability to induce rules from common features of an activity), Number (a measure of addition skill), and Word Fluency (a measure of word recall).

A thorough search of *Psychological Abstracts* revealed that there were substantial data on children and adolescents but that nothing had been done with adults. Hence, I proposed a directed study to determine whether the low correlations among the different abilities reported in childhood would also prevail in adults. Tuddenham agreed that this was an interesting and appropriate question for a term project and told me to go ahead.

But where does an undergraduate find adult subjects beyond college age? As serendipity would have it, I was still being treated for the aftereffects of the malnutrition experienced during my Shanghai years. My family physician, Robert M. Perlman, happened to be interested in geriatrics. When I mentioned my subject problem to him, he offered to provide me with testing space in his practice and allowed me to recruit subjects in his waiting room. He also introduced me to Florence Vickery, then director of the San Francisco Senior Citizens' Center, one of the first to be established in the United States, who permitted me to recruit and test subjects at her facility. My first aging study was under way.

I was able to test several dozens of subjects ranging from the 20s to the 70s and found not only that the primary mental abilities remained distinct in adulthood but also that age differences were not identical for all abilities. As compared to the normative data for adolescents, it turned out that young adults and those in early middle age, on average, did better than the high school students. There were significant age differences thereafter, and in particular older adults did less well on Space and Reasoning than they did on their verbal and numeric skills. Administering the test to a subset of study participants under untimed conditions, further showed that the age difference patterns were even more pronounced when the speed restriction was removed.

While the data collection was proceeding, Dr. Perlman received an announcement for the Second International Congress of Gerontology to be held in St. Louis, Missouri. He suggested that I submit a proposal for a convention paper with him as a coauthor. The paper was accepted, but in order to report respectable statistics I now had to recruit a friend. Fred Rosenthal, who was a semester ahead of me, to run the t-tests that I had not yet mastered. Thus, in August of 1951, I mounted the Greyhound bus for my first long American trip to go to St. Louis for the Congress.

Gerontology was still a very small affair and the Second International Congress had about 200 registrants, two thirds of whom were Americans. Perhaps no more than 30 participants were psychologists. I do not remember much about the scientific sessions, but I vividly recall meeting many of the founders of geropsychology, including James Birren, Robert Kleemeier, Irving Lorge, and Robert Havighurst. This was very heady stuff for a college junior, and I was even more excited when the editor of the *Journal of Gerontology*, John Esben Kirk, invited me to submit my paper, titled "Differential Deterioration of Factorially 'Pure' Mental Abilities," as a journal article and promptly accepted it (Schaie,

Rosenthal, & Perlman, 1953). My entry into adult developmental psychology and gerontology was obviously determined by these events.

During my last semester at Berkeley I did some more reading on individual differences and became interested in the concepts of behavioral rigidity and perseveration studied by psychologists such as Kurt Lewin, Abraham Luchins, Jacob Kounin, and Charles Spearman. They suggested that the boundaries between different domains of behavior would rigidify with age, and that there would be increasing interference in shifting away from old and no longer appropriate strategies to the adoption of new and more appropriate problem-solving strategies. If this was the case, I thought that perhaps age differences in the primary mental abilities might well be explained by a progressive reduction in cognitive functions for those who were more rigid to begin with or who became less flexible as they aged. I attempted to test this proposition in another directed study, but although the effort was too ambitious to succeed then, it became the basis of my research in graduate school.

### *Graduate School*

With my Berkeley experience coming to an end, I now turned to apply to graduate programs. Cocky as ever, I unrealistically considered only the top schools. Rejections from Berkeley, Stanford, Michigan, and Harvard put me in my place. But my backup, the University of Washington, came through. I suspect that the article in press in the *Journal of Gerontology* probably helped get me accepted into the University of Washington clinical psychology program. In the fall of 1952, I therefore headed north to Seattle, actually the first time I had been entirely on my own. The psychology department had not committed any financial support, quite usual for the time, and so once again I supported myself by working as a nighttime printer in the composing room of the *Seattle Post-Intelligencer*.

In contrast to most of my classmates, I early on had found an intellectual niche in geropsychology and I also had a set of specific research objectives at the very beginning of my graduate training. In addition to obtaining the necessary clinical training to become an academic clinical psychologist, I wanted to focus my research on the interesting puzzle of why it is that some people maintain their intellectual powers into old age while others begin to decline at an early adult stage. I did not realize at the time, of course, that I was posing a challenge, the response to which would occupy my entire career.

Having had excellent preparation in the conventional statistical methods at Berkeley, I was able to skip the usual first-year methods sequence and immerse myself directly into multivariate and factor analysis (Paul Horst) as well as scaling methods (Allen Edwards) and Q-methodology (William Stephenson was a visiting professor that year). I was thus ready to begin instrument development to provide me with formal operations that would measure the rigidity-flexibility concept I had become interested in at Berkeley as a possible explanatory variable for individual differences in cognitive aging. From the research literature I identified a set of 10 potentially appropriate measures of the construct of rigidity-flexibility that I adapted for use with a population ranging in age from young adulthood to old age. I was able to test about 300 subjects in several months' work and was then ready to conduct a multiple group factor analysis (on a Monroe desk calculator!), in which I showed that the different measures of rigidity-flexibility could be represented as a three-factor structure. I replicated the factor solution on another sample and eventually published this material as the Test of Behavioral Rigidity. This work was accepted by the end of 1953 as my MS thesis (directed by Charles Strother, Paul Horst, and Sidney Bijou).

Returning home to San Francisco for the summer of 1953, I married my first wife, Coloma John Harrison, whom I had met at a leap-year party in San Francisco the previous

year. During the summer I also attended sessions of the annual meeting of the Gerontological Society in San Francisco, which I had earlier joined as a student member; that year I also became a student member of the American Psychological Association (APA).

It is important to note here that no one on the Washington psychology faculty was particularly interested in adult development or aging, and it was necessary therefore to create my own academic support system. At the 1953 Gerontological Society of America (GSA) meeting I sought advice from some of the people I had met earlier at the St. Louis congress, notably Harold Jones and a University of Washington academic physician, K. K. Sherwood. Returning to campus in the fall, I also discovered a latent interest in gerontology in a number of other departments, and I was able to convince the dean of the graduate school to sponsor a Committee on Gerontology, which my advisor, Charles R. Strother, the director of clinical training, generously agreed to chair, even though he was not particularly interested in aging. Other active members of this committee were Joseph Cohen (a sociologist interested in elder housing, who later on became the outside member of my dissertation committee); Norman Kunde (an exercise physiologist); Robert Lampman (a labor economist); and Victor Howery (then dean of the School of Social Work). The committee needed an executive secretary, and in the fall of 1953, I was finally able to give up working nights as a newspaper printer, as I now received fellowship support (from one of the first National Institute of Mental Health [NIMH] institutional training grants in clinical psychology) in return for agreeing to staff the new committee as well as pursuing my own research on aging.

To focus the work of the new committee, I proposed an intensive study of a group of well-functioning elders that not only would encompass psychological variables but would include an examination of health status, physical activities, and environmental contexts. A small grant from the University of Washington research council to Charles Strother permitted the recruitment of 25 men and 25 women over the age of 70 years who had completed a college degree or beyond. This work occupied much of my third year of graduate study, as well as the completion of a rigorous set of the then in vogue broad comprehensive examinations across the entire breadth of psychology.

As would not surprise us today, the advantaged group of elderly still maintained high levels of functioning and activity on virtually all of our measures. Several reports emerged from this study, the first presented at the 1955 APA meeting in San Francisco. This meeting was important also because it presented an opportunity to renew my acquaintance with James Birren and to start a friendship and many professional collaborations that have lasted to this day.

Other activities initiated by me under the auspices of the University of Washington Committee on Gerontology included a Northwest Conference on Aging in 1954 that, among others, brought Wilma Donahue and Clark Tibbits to campus. There were also talks on gerontology to local professional groups, and in 1955 I organized and led the first gerontology course ever offered at the University of Washington, supported by the continuing education division and staffed by members of the Committee on Aging. About that time, I also became a full member of the American Psychological Association and of the Gerontological Society.

### *Origins of the Seattle Longitudinal Study*

Having passed my comprehensive examinations, it became time to propose a dissertation project. My mentor tried to interest me in taking a critical incidents approach (a la Flanagan) to the study of the process of psychoanalysis at a pioneering hospital for adolescent schizophrenics at which he was a consultant. After several months it became

clear that whatever observational or descriptive scheme I proposed the analysts perceived as effectively changing the process! I was therefore allowed to return to my primary interest, which was to put together my pilot work on rigidity-flexibility and intelligence. As serendipity would have it, Charles Strother, my advisor, had just been named chair of the lay board of trustees of the Group Health Cooperative of Puget Sound, one of America's first (and now one of the largest) health maintenance organizations (HMOs). The HMO was interested in doing a consumer satisfaction survey but had neither staff nor financial resources to allocate. A deal was struck. I was allowed to collect my dissertation data on a random sample of the adult HMO membership under the condition that I conduct the consumer satisfaction survey at the same time. Other members of my doctoral committee included Paul Horst (who as a student of L. L. Thurstone was very sympathetic to my work and provided most of the methodological guidance), Sydney Bijou, George Horton, and Joseph Cohen as outside member). At the last minute my department head, Roger Brown Loucks, added himself to the committee to make sure, in his words "that I wasn't going to get away with something."

I randomly selected about 3000 persons evenly spaced across the age range from 20 to 70 years and administered the Thurstone Primary Mental Abilities (PMA) test and my own rigidity-flexibility test (the TBR) until I had assessed 25 men and 25 women in each five-year interval. I was able to replicate my earlier findings on differential patterns of age differences in intelligence by ability as well as to show that peak ages of performance had risen since the earlier work by Wechsler and others and were now to be found in the 30s or even later. Substantial positive correlations were also found between rigidity-flexibility and the ability measures, but I did not find the predicted causal relationship; that, as it turned out, required longitudinal data (Schaie, 1958).

### *Postdoctoral Training*

When I obtained my doctoral degree in 1956 there were no employment opportunities for someone who wanted to specialize in gerontology. My mentor therefore advised me to strengthen my clinical skills through a year of postdoctoral study and then seek employment as an academic clinician. This was accomplished at Washington University in St. Louis, then an important place in the development of gerontology. There I had the opportunity to do some research with James Weiss (later chair of psychiatry at the University of Missouri), who then directed the Washington University Psychiatric Outpatient Clinic, to develop a Q-sort instrument assessing the attributes of complaints that brought older patients to the clinic, reinforcing my interest in older populations.

### *My Academic Career*

It was now time to enter academia. My postdoctoral training had prepared me for then-burgeoning opportunities in academic clinical psychology. As will be described in this section, I began my career focusing on psychological assessment in adults, but soon was able to return to basic research in the development of adult psychological competence as well as the formulation of novel longitudinal research methodologies. Although my first academic position was that of a traditional teacher/researcher, my career has also heavily involved academic and research administration, from the very beginning focused on interdisciplinary efforts related to the study of aging.



*The University of Nebraska, Lincoln*

In the summer of 1957, Marshall R. Jones offered me an appointment as assistant professor at the University of Nebraska to teach adult cognitive and personality assessment and to supervise students in the psychology clinic associated with the clinical training program. In this context my interests turned to issues of objective psychological assessment. A visit by Raymond Cattell to speak at the Nebraska Symposium on Motivation aroused my interest in unobtrusive personality measurement, as well as the equivalence of self-ratings and observer ratings in personality measurement. The work with Weiss on defining symptoms that bring patients to the clinic was also continued off and on through 1960, during which year my son Stephan was born. That year I also passed the ABEPP examination in clinical psychology and was promoted to associate professor.

During my last days in St. Louis I met my successor as a postdoctoral fellow, Otfried Spreen, who returned to Germany to lead a new clinical psychology section at the University of Saarbrücken. The psychology department had received the gift of a first-generation computer (the IBM 650) from a steel company that was upgrading its equipment. Spreen knew that I had some computer skills, and in the summer of 1961 he asked me to help him and his colleagues to think through how to use this computer.

At Saarbrücken I met Günther Reinert (later founding chair of the psychology department at the University of Trier) who was then the chief scientific assistant to Egon Boesch (the department head). Reinert introduced me to his mentor at the University of Freiburg, Robert Heiss, who had done a lot of work with a color preference test, the Color Pyramid Test (Farbpyramiden-test), first introduced by the Swiss psychologist Hans Pfister. This test seemed to offer an unobtrusive method for objective personality assessment via the relation of color and personality. On returning to Nebraska, I began to study schoolchildren as well as mentally retarded and mentally ill persons in state institutions. This work led to my first book, *Color and Personality* (Schaie & Heiss, 1964).

Although the work on color and personality almost let me to abandon my interest in gerontology, it incidentally also led to the inception of my long-standing friendship and collegial association with Paul and Margret Baltes. Günther Reinert wrote me that he had a promising young student who he thought could use some American experience. I was able to get a research assistantship for Paul for the purpose of collecting color pyramid data in Nebraska schools. He and Margret joined me during my final year at Nebraska (1963–1964), and Margret began working for me as an assistant and secretary when the first Seattle Longitudinal Study (SLS) follow-up was funded.

*Converting a Cross-Sectional to a Longitudinal Study*

It took me a long time to convince my Nebraska colleagues to use my training in developmental psychology. But in my fourth year at Nebraska, I was finally asked to teach the developmental section of the departmental proseminar and was allowed to introduce a unit on adult development. In preparing for that seminar, I was confronted with addressing the discrepancies between cross-sectional and longitudinal findings in the study of adult intellectual development. I soon became convinced that this issue needed to be addressed by following a structured cross-sectional sample over time, such as the one that I had collected for my dissertation. I therefore designed a follow-up inquiry that converted my original cross-sectional study of cognitive aging into a series of short-term longitudinal studies, each extending over the same seven-year period. My graduate school mentor, Charles Strother, then at the height of his professional career, graciously agreed to front for me as principal

investigator. Funding for the study was received from the National Institute of Mental Health, and with the continuing cooperation of the HMO, I went into the field in 1963 to conduct this follow-up. Additionally, I drew a new random sample from the HMO membership that permitted comparison of panels tested at the same age but at different times (known as "Schaie's most efficient design"). Thus the Seattle Longitudinal Study (SLS) was now in place and I was once again firmly entrenched in geropsychology!

The second cross-sectional study (1963) essentially replicated the findings of the base study. The short-term longitudinal study, however, disclosed substantially different information about peak levels and rate of decline. Publication of findings was therefore delayed until a theoretical model could be built that accounted for the discrepancy between the longitudinal and cross-sectional data. These analyses suggested that comparisons of age group means needed to be conducted for the repeatedly measured samples as well as for successive independent samples drawn from the same cohort.

Results were reported that called attention to substantial cohort differences and that questioned the universality and significance of intellectual decrement with advancing age in community-dwelling persons. While the cross-sectional data implied peaks in early adulthood with decline beginning in middle age and becoming severe as the 60s are reached, the longitudinal data, by contrast, suggested little age-related decline before the 60s and only modest decline during the 70s.

The first longitudinal follow-up of the SLS provided some answers but it also raised sufficient methodological and substantive questions to initiate a continuing program of studies (by now including seven major and several collateral data collections) that is still in progress. The longitudinal research program was first supported by the NIMH, has been continuously supported by the National Institute on Aging since 1970 and is currently funded to continue through 2004. The initial follow-up was also instrumental in forming my methodological efforts in understanding the relationship between cross-sectional and longitudinal data sets, which led to an influential *Psychological Bulletin* article (Schaie, 1965) that for many years has been required reading for geropsychology graduate students.

#### *West Virginia University*

An opportunity arose for me in 1964 to use my academic clinical skills to organize a clinical training program at West Virginia University. I was rather skeptical at first about a move to Appalachia. But a visit in May, when the grime of the coal mining communities is hidden by the lush greenery of the Appalachian spring, and a university president (Paul Miller, who later became assistant secretary of education) with a vision to move his sleepy state university into modern times convinced me. After a summer spent on the University of Washington campus to tie up the longitudinal follow-up, I thus moved to Morgantown, West Virginia. With the help of an NIMH development grant, I was able to bring the clinical psychology effort forward to APA accreditation, helped the first set of PhD candidates finish, and put in place a working relationship with the region's VA hospitals. I was also able to bring in some contract research with the National Center for Health Statistics to help support graduate students.

In 1965 Stanley Ikenberry (until recently president of the University of Illinois and now president of the American Council on Education), who had just become dean of a new College of Human Resources and Education, asked me to be the founding director of a Human Resources Research Institute whose mission was to provide intellectual links between his college and the traditional social science disciplines in the College of Arts and Sciences. In the context of this institute I oversaw research on the effects of the community

action programs sponsored by Lyndon Johnson's "war on poverty" as well as statewide evaluations of the effects of early Headstart programs. With respect to adult development I was able to organize an international seminar for the study of social change in mining communities with meetings in Morgantown and Saarbrücken.

More important with respect to gerontology, I was able to conceptualize and receive funding for one of the first institutional training grants awarded by the National Institute on Child and Human Development (NICHD) to develop the concept of training in life-span developmental psychology. I had talked earlier with James Birren (then the aging section program officer in NICHD) about developing a training grant in geropsychology, but he had cautioned me on the need to first gain greater faculty depth. Thinking back to my early conversations with the developmentalists at Berkeley as well as having read some of the work of Charlotte Bühler, I thought that it might be propitious to reintroduce the concept of life-span development in the United States. Moreover, if I could combine faculty interested in child, adolescent, and adult development, I would then have a critical faculty mass on which to base a credible application.

I also wrote a conference grant application (modeled after my experience with the Nebraska Symposium on Motivation) with the support of APA's Division 20 (Adult Development and Aging). This conference, held in 1967, had specialists in geropsychology review the literature in core topics of the field, with critiques provided by substantively relevant psychologists who were studying children or adolescents. The conference and the publication arising therefrom was the predecessor of the series of conferences and monographs known as the West Virginia Life-Span Series, which is still continuing under the guidance of Hayne Reese. That year I was also promoted to full professor.

In 1968 I was prevailed on to "simplify" my life by becoming chair of the Department of Psychology, in which role I served until 1973. One of my first acts as chair was to recruit Paul Baltes and John Nesselrode to join the departmental faculty and to take an active interest in geropsychology. Both Paul Baltes (now a director of the Berlin Max Planck Institute for Education and Development) and John Nesselrode (now professor of psychology at the University of Virginia) have continued this interest, both men eventually serving as presidents of APA's Division 20.

Next I once again returned to the study of adult cognitive development. Soon after the completion of the first longitudinal follow-up it had become evident that conclusions based on data covering a single seven-year interval required further replication, if only because two occasions of measurement permit the examination of cross-sectional but not of longitudinal sequences (the latter requiring a minimum of three measurement occasions). Only longitudinal sequences allow designs that permit contrasting age and cohort effects. Hence, plans were made for a third data collection, conducted in 1970. The results from the third data collection seemed rather definitive in replicating the short-term longitudinal findings, but they also showed further progression of the ability-related cohort trends discovered earlier (Schaie & Labouvie-Vief, 1974). This research marked a close association with Barbara Buech, whom I had recruited during the Saarbrücken mining conference and who became my Seattle field office coordinator, and with Gisela Labouvie-Vief, my chief research assistant (now professor of psychology at Wayne State University).

### *The University of Southern California*

The heady expansion days financed by the Great Society programs and other endeavors to develop the Appalachian region had come to a close, and a new conservative university president began to talk about retrenchment and his perception that West Virginia could not

afford a first-rate university. Having brought the psychology program to a nationally recognized level, I was not willing to preside over its return to mediocrity and decided that it was time to move on. At the same time, my first marriage had deteriorated to the point where a decision to bring it to an end had become inevitable, and a new beginning was needed as well to reinvigorate my personal life.

My old friend James Birren had founded the Andrus Gerontology Center at the University of Southern California in 1965. In 1973 he invited me to join him as associate director for research (later director of the Gerontology Research Institute) and as professor of psychology. At USC, I directed the interdisciplinary doctoral training program in aging and was instrumental in developing and overseeing a number of project-program efforts to bring to bear the skills of scientists in the biological, behavioral, and social sciences on major basic issues in the aging process.

Discrepancies between findings in the repeated-measurement and independent-sampling studies suggested the need for a replication of the 14-year longitudinal sequences, and it also seemed useful to follow the original sample over as long as 21 years. A fourth data collection was therefore conducted in 1977. Continuous funding also made possible addressing a number of other bothersome questions. These included analyses of the consequences of shifting from a sampling without replacement model to a sampling with replacement paradigm, an analysis of the effects of monetary incentives on participant characteristics, an examination of the aging of tests, as well as causal analyses of health and environmental factors on change or maintenance of adult intellectual performance. Doctoral students who participated in this round of the SLS and who have continued to be active in geropsychology included Christopher Hertzog (now professor of psychology at the Georgia Institute of Technology), Margaret Quayhagen, and Michael Gilewski.

At USC I also started a new longitudinal study of cognitive aging (including memory functioning), which I followed only over a three-year period but which is now being continued by another of my former USC students, Elizabeth Zelinski. Beyond the research area, I was able to make an impact on many budding gerontologists by routinely teaching the course on research methods in aging that was part of the annual USC gerontology summer institutes. I was also involved in helping organize the Leonard Davis School of Gerontology, the first of its kind, and was active chairing committees for the recruitment of its first director and psychology faculty. In many ways, both the environment at the Andrus Gerontology Center and the gentle but intellectually stimulating leadership of Jim Birren helped me broaden my understanding of the role of geropsychology within the larger context of the study of aging and convinced me even more that interdisciplinary efforts in our field are a necessity, rather than a luxury.

### *The Pennsylvania State University*

While at USC I had met Sherry Willis, who taught at the Pennsylvania State University. As our personal and professional interests began to merge, we decided that we should give up transcontinental commuting and be at the same institution. I therefore left USC at the end of 1981 to accept an appointment as professor of Human Development and Psychology at Penn State and to marry Sherry. Since 1985 I have directed the Penn State Gerontology Center, and in 1986 I was honored by the university with an appointment as the Evan Pugh Professor of Human Development and Psychology.

The fifth (1984) SLS cycle also marked the assumption of a major role in the study by Sherry Willis, who brought her skills in designing and implementing cognitive training paradigms. A major part of the fifth cycle was therefore devoted to the implementation of a

cognitive training study with our long-term participants aged 64 years or older. This study was designed to determine whether cognitive training in the elderly remediates cognitive decline or whether it increases levels of skill beyond those attained at earlier ages. In this study we found that almost two thirds of all subjects benefited significantly from a five-hour cognitive training program and that 40% of those who had reliably declined could be brought back to the performance level they had shown 14 years earlier. Training was also shown to remove the so reliably demonstrated gender difference on spatial orientation.

From the beginning of the SLS we had followed what was then the conventional wisdom of assessing each primary ability with that observable marker variable, which was thought to be the most reliable and valid measure of a particular ability. With the widespread introduction of modern methods of confirmatory (restricted) factor analysis, it became obvious that we needed to extend our concern with changes in level of intellectual functioning in adulthood to the assessment of structural relationships within the ability domain. This concern argued for collecting further data with a much expanded battery in which each ability would be multiply marked. Finally, this cycle saw the introduction of measures of practical intelligence, analyses of marital assortativity using data on married couples followed over as long as 21 years, and the application of event history methods to hazard analysis of cognitive change with age.

Penn State has had a long history of scientific and educational efforts in gerontology. The Penn State Gerontology Center was founded by Joseph Britton in 1967, with an interdisciplinary training grant supported successively by NIMH, NICHD, and since its inception by the National Institute on Aging (NIA), which has now been continuously in place for more than 30 years. The Gerontology Center had concentrated for a number of years on training service providers for local and state agencies. My early efforts were to refocus on research and education. These efforts included putting in place a pilot study support program for new faculty, the development of an annual research conference (known as the Social Structures and Aging series) with published proceedings, the showcasing of faculty by means of a reprint-preprint service, and by encouraging efforts to broaden the extramural funding for research related to aging. Of great help and continuing collegial support has been my assistant director Steven Zarit, whom I had originally recruited to USC and persuaded to follow me to Penn State.

In the education area we put in place graduate and undergraduate minors in gerontology and have been supportive of training teachers at small colleges by encouraging visiting appointments and being active in support of teaching workshops sponsored by Association of Gerontology in Higher Education (AGHE). I have also been active in encouraging regional cooperation through efforts such as the joint exhibit of Pennsylvania gerontology centers at professional meetings and participating in a consortium with Temple University and the University of Pittsburgh in operating the Pennsylvania Geriatric Education Center, which offer continuing professional education over a wide spectrum of health-related professions. Current efforts include entry into the world of the Internet, trying to share Penn State's aging-related resources with the broader community.

Returning to my own scientific odyssey, I began a new cycle in 1991 that markedly expanded the scope of the SLS. First, with the collaboration of Robert Plomin, a noted developmental behavior geneticist, we began a study of cognitive family resemblance in adulthood. We did this by recruiting the participation of a large number of adult offspring and siblings of our longitudinal panel members. Second, we abstracted health histories on our panel members and have conducted detailed investigations of the relationship between health and maintenance of intellectual functioning, showing both the influence of chronic disease on maintenance of intellectual functioning and the importance of intellectual

competence in postponing the onset of chronic disease. Third, we conducted a seven-year follow-up on the cognitive training study, showing continuing effects of the training intervention, and replicated the initial findings with a more recent cohort of older persons. Fourth, with the first longitudinal replication of our expanded test battery, we were able to conduct longitudinal analyses of cognitive ability structures, demonstrating the greater stability of longitudinal data, and further update our normative data.

Most recently, I have been able (with support from the NIA) to broaden my interdisciplinary interest even farther through new collaborative studies with the University of Washington Alzheimer Center and Department of Pathology, to study genetic markers in our longitudinal subjects, to investigate the relationship between our measurement system for the study of normal aging with the diagnostic procedures used by neuropsychologists interested in diagnosing dementias, and the relation of predeath behavioral correlates of eventual structural changes in the brain.

### *Other Influences*

I should be remiss in not acknowledging the important role of international experiences in my professional development. Given the international scope of gerontology I always learned much from attending international congresses, such as the International Congress of Gerontology or the International Society for the Study of Behavioral Development. My German language skills were useful in accessing important professional relationships in the German-speaking countries, once I was able to resolve my feelings about the injuries of the past. I spent interesting sabbatical years at the University of Trier and the University of Bern, as well as at the Gerontological Center of Lund, Sweden. Most stimulating also was a year at the Center for Advanced Studies in the Behavioral Sciences at Stanford, California.

### What Have I Learned as a Geropsychologist?

As part of my scientific work, I have been able to chart the course of selected psychometric abilities from young adulthood through old age. An important contribution of this work has been the detection of substantial generational differences in intellectual performance. Also identified were a number of contextual, health, and personality variables that offer explanations for differential age change and that provide a basis for possible interventions. Cognitive interventions were designed that have been successful in remediating carefully documented declines and that have improved the cognitive functions of many older persons who have remained stable. I have also studied changes in cognitive ability structures across age and different cohorts, have conducted analyses of the relative effect of speed and accuracy in age decline and training gain, have investigated the relevance of cognitive training to real-life tasks, and have studied parent/offspring and sibling similarity in adult cognitive performance. The dialectic process between data collection and model building that has been characteristic of my work has both increased our knowledge base and led to a number of methodological advances in the design and analysis of studies of human development and aging.

Over the course of my research career I have focused on five major questions, which I have attempted to ask with greater clarity and increasingly more sophisticated methodology as time progressed. These questions are the following:

1. *What is the differential life course of intellectual abilities?* Our work has shown that there is no uniform pattern of age-related changes across all intellectual abilities.

Hence, studies using an overall index of intellectual ability (IQ) are of only limited usefulness for an understanding of age changes and age differences in intellectual functioning in individuals or in groups.

2. *At what age can we observe a reliable decline in intellectual abilities and how large is the decline?* Our general finding has been that reliable average decline in mental abilities does not occur before age 60 but that reliable average decline may be found for all abilities by age 74. Detailed analyses of individual differences in intellectual change demonstrated that even at age 81 fewer than half of all observed individuals experienced reliable decline over the preceding seven years. These findings provide a normative base that can help determine at what ages declines reach practically significant levels of importance for public policy related to issues such as ages for retirement eligibility, age discrimination in employment, or the determination of the population proportions that can live independently in the community.
3. *How do successive generations differ in intellectual performance?* The prevalence of substantial generational (cohort) differences in psychometric abilities has been conclusively demonstrated. When cross-sectional data are used as a first estimate of age changes within individuals, they tend to overestimate age changes before the 60s for those abilities that show negative cohort gradients and underestimate age changes for those abilities with positive cohort gradients.
4. *What are the causes of individual differences in age-related ability change in adulthood?* The most unique contribution of a longitudinal program of research on adult development stems from the fact that one can investigate individual differences in antecedent variables that lead to early decline for some persons and maintenance of high levels of functioning for others well into very advanced age. Variables that we have identified as being important in reducing the risk of cognitive decline include (a) The absence of cardiovascular and other chronic diseases; (b) a favorable environment that is often a consequence of high socioeconomic status; (c) involvement in a complex and intellectually stimulating environment; (d) flexible personality style at midlife; (e) marrying an intelligent spouse; and (f) maintaining high levels of perceptual processing speed.
5. *Can age-related intellectual decline be reversed through educational intervention?* Findings from our cognitive training studies suggest that intellectual decline observed in many community-dwelling older people is likely to be a function of disuse and is therefore reversible for many persons.

### What Lies Ahead?

Life as a professional gerontologist encourages one to believe that scientific productivity can be maintained well into advanced old age. Consequently, because mandatory retirement for academics has ended, my future plans do not include formal professional retirement. Current work in my laboratory has just begun to examine the rate of intellectual aging in families and a seventh SLS cycle began in 1997, which includes a further follow-up on the effects of cognitive training and another set of longitudinal data waves. We have also begun to study the relationship between the psychometric measures of cognitive behavior and neuropsychological assessments to explore the possibility of earlier identification of risk for dementia, the relationship between cognitive change and prevalence of the high-risk allele of the Apo-E gene, and recruitment of participants who will allow us to conduct a postmortem to study directly anatomical and cellular features of the normal aging brain and their relations to cognitive behavior.

Longitudinal studies have a life of their own; they involve multiple generations of students and investigators. As all serious researchers know, there are no final answers or critical experiments. We continue to build on the work of those who came before us, and we hope our students will continue the quest. For those of us whose science also serves to help make meaning of our own lives, the study of gerontology is immensely rewarding; I could not have chosen a better or more intellectually exciting vehicle for my professional odyssey.

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