
A History of
GEROPSYCHOLOGY
in Autobiography

Edited by
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Chapter 2

AUTOBIOGRAPHICAL REFLECTIONS: From Developmental Methodology and Lifespan Psychology to Gerontology

Paul B. Baltes



The editors of this volume know that I struggled with accepting this assignment. Only after Jim Birren reminded me of collegiality and my commitment to the field did I persuade myself to engage in this task. Nonetheless, I would like to summarize some of the reasons for my many reservations. They are relevant when one reads and evaluates the product that resulted.

Several factors converged to produce resistance. On the one hand, I felt I was still composing my life. The idea of a life review struck me as off time and perhaps as counterproductive. Moreover, I sensed that good autobiographies reflect a mixture of intuitive and rational knowledge. But as I thought about my professional career, what I found foremost in my mind were rationality-based knowledge structures

and associated “abstract” mental scripts, with little room for the intuitive.

For instance, after years of working on theoretical models of lifespan development, the nature of these models seemed to dictate how to think about my own life as well. Two examples will illustrate. One of the lifespan models I have advanced and elaborated (with colleagues, as is true for much of my work) specifies three interacting influences on the sequencing, direction, and differentiation of ontogeny across the lifespan: *age-graded influences*, *history-graded influences*, and *nonnormative influences* (P. B. Baltes, 1987; P. B. Baltes, Lindenberger, & Staudinger, 1998). In addition, with Margret Baltes, I have outlined a model of successful lifespan development labeled *selective optimization with compensation* (P. B. Baltes & Baltes, 1990).

It is difficult for me to distance myself from these theoretical frameworks, to let my life express itself without the mental scripts flowing from lifespan theory. As soon as I begin to reflect on my biography, I ask questions about age-graded events, history-graded factors, and nonnormative events, and about their interactions. Similarly, I ask how in my life the processes of selection, optimization, and compensation operated, actively and passively, consciously and subconsciously, individually and collectively. As I anticipated writing this

This autobiography is dedicated with gratitude and love to my colleague and wife, Margret M. Baltes, who died unexpectedly on January 28, 1999, and after this chapter was written. Her contributions to my life and professional career were immense, unconditional, and without fail.

Note that the present author now prefers the spelling of *lifespan psychology* instead of *life-span* or *life-span developmental psychology* (Baltes & Goulet, 1970).

autobiography, the losses rather than the gains of such mental constraints were at the fore. Perhaps this is so because I was assailed by uncomfortable thoughts about the usefulness of these abstract models for the representation of individual lives.

There was more to feed my resistance. I feared creating a flexible and self-serving reconstruction of the past. It is a special feature of *Homo sapiens* that we act on and transform reality, especially if reality is projected into the past. Autobiographies, it seems to me, seduce their writers to engage in egotism and vanity. Throughout my life, I have consciously fought these vices in myself. Similarly, did I want to confront weaknesses and failures?

Yet, as I read in a commentary on autobiographies, “if autobiographies couldn’t be self-serving, would anybody ever write one?” How was I to solve this motivational and intellectual conundrum, how would I contain egotism and ignore the theoretical frame of lifespan theory as I reflected on my own life? I found no solution except for making, as I do with these observations, this vexing problem transparent, and I use these self-insights as a general disqualifier. Whatever I offer in the following narrative should be taken with a grain of salt.

I will begin with some observations about my childhood and the conditions that, in my view, orchestrated the factors necessary for investing myself in my own development. In a second section, I will reflect on how my wish for advancement was channeled to psychology as a field of study. Subsequently, I will try to locate my own work in the emerging field of gerontology. Mentors, colleagues, students, and supportive institutional contexts are crucial to my assessment. I view myself as someone who has always worked in a context of interactive minds (e.g., P. B. Baltes & Staudinger, 1996) and in reciprocal relationships with people who have enriched and transformed my thinking and orientation in profound ways.

Acquiring a Sense of Proactive Agency and Upward Mobility

I was born in 1939 in Saarlouis, Germany, located near Saarbrücken, the capital of the Saarland, and the Alsace-Lorraine-Luxembourg border. Historically, the city of Saarlouis is of French origin. A liking for France was part of the soul of the Baltes-Haser-Detemple family, although my upbringing was undoubtedly German. This imaginary French connection was further enhanced when, in 1963, I married Margret Labouvie, whose family lineage included French as well.

When Germans of my age reflect back on their childhood, many wonder how and why they survived at all. Childhood in Nazi-era and postwar Germany was lived in miserable conditions. Death, hunger, war, air raids, war-forced migrations, fear of the invading foreign soldiers, an absent soldier-father (who remained a French prisoner of war until 1947)—all these factors suggest deprivation and vulnerability, risk factors en masse, with few economic and social resources. That the risks were real can perhaps be inferred from the fact that up to late childhood, I had a severe stutter. My family was concerned about whether I could enter primary school.

In retrospect, I view these early challenges primarily as opportunities. Why? The historical context taught me that bad circumstances can be transformed into stories of progress. On the one hand, I vividly remember war, hunger, and fear. But I also remember the sense of being a survivor, and I especially remember the warmth and many strengths of my mother and the larger family context that generated an experience of mastery. Given our generally low level of material resources, minor positive events were transformed into major ones and were associated with personal or family agency.

For instance, I think often about the excitement and opportunity that ensued in response to a gift I received for being the best pupil at the end of first grade in 1946. The gift came from an officer of the French occupying force and perhaps paradoxically involved a military toy tank. After having played with the tank for a day or so, I traded it to a neighbor child, whose parents owned a meat factory, for a string of delicious sausages. My mother's response was tears and pride in her son. How often she later told this story to others in my presence, thereby strengthening my belief that I was raised in a tough situation but, through my own behavior, made a difference. Similarly, I remember pieces of chocolate that my mother had acquired for her children in trade for family jewelry. Again, this and similar events were expanded by the magic of subsequent storytelling, and I began to feel that despite my hard circumstances I was special and could bring about changes.

My sense of efficacy was enhanced by the collective success of a small family business. After World War II, we were poor. Without any financial resources, my parents started a small business. Initially, we ran a small public restaurant along a major roadway. Over the course of a decade it was expanded to include a larger restaurant, a small hotel, and a busy service station. All three children (aside from a beloved older sister who died in 1950 at age 21, I had two older brothers) participated in making a go of the family enterprise: behind the counter as a cashier, as a bartender, as the bellboy taking luggage to the rooms, as a waiter in the restaurant, or as the gas station attendant. For me this experience started when I was 11 years old. My father suffered from a war-related illness and died early (when I was 17); my mother, with primary assistance from my eldest brother, was the household chief. These were years of hard work and serious financial insecurity, as I knew very well. I was often the one who took the daily receipts in my back pocket to school. During the school recess, with official permission, I jogged to the local bank to deposit the funds. I knew how important it was that I be on time so that none of our own checks would bounce; more than once, the cashier waited for my deposit before he presented the checks for payment. In time, however, under the firm leadership of my mother, we as a family developed a sense of accomplishment. We looked forward to, and participated in, significant upward economic mobility.

In prevention and resilience terms, I was well immunized and taught early to adapt to whatever life conditions surrounded me. Perhaps most important, I learned what it meant to have a sense of control and a purpose in life. A primary goal of life was striving toward betterment through ingenuity and hard work. Sociologists would call it a sense of upward mobility, wanting to move beyond one's family background.

The feeling of being motivated for upward mobility, in economic, social, and psychological dimensions, never left me. It is a fountain from which I still drink. I developed a worldview that contained the modifiability of reality as a basic principle, and I would not be surprised if my later interest in the modifiability in the course of aging had its foundation in this belief. This interpretation is consistent with lifespan theory, of course. As Klaus Riegel, one of my intellectual heroes, argued so eloquently in his conception of dialectical psychology (Riegel, 1976), the mastery of crises is among the critical ingredients for developmental advance.

Education as a Vehicle of Individual Development and Academia as an Outcome

Three primary circumstances channeled my sense of personal efficacy and desire for upward mobility into an academic rather than a business career. The first was that I learned

quite early that, in good German tradition, my eldest brother was the heir apparent of the family business, and I accepted this without any negative feelings. I needed to look for another career. A second reason was my parents', especially my mother's, commitment to intellectual matters and her belief in education and education-related domains as better spheres of life than the family business we owned. She encouraged me to become educated and showed unwavering pride in my academic success—in addition, she largely ignored my weaknesses. To some degree I never understood why my mother had such a strong belief in education. To the best of my knowledge, nobody in her family ever attended college. Nonetheless, like her own mother, my mother was interested in cultural affairs: reading, theater, and opera. As an adolescent I became her regular companion at plays and musical performances in the nearby cities.

The third source of my evolving interest in education and becoming an *Akademiker* (university graduate) was located in the family network of my father. Although his parents came from a farming background, one line of the family was academically disposed. An older second cousin (Werner Reinert, one of the noted literary authors of the Saar), in particular, dropped by from time to time during my adolescence and encouraged me to pursue a university education. For me, this cousin and his younger brother, Günther Reinert, were models to emulate, and as inner voices they guided me to the Abitur and a university education. This story of adolescent grooming through my second cousins continued. When I entered the university, Günther Reinert (1929–1979) became my first and probably principal academic mentor. This mentorship began when he persuaded me to study psychology at the University of Saar in Saarbrücken (1959–1967), where he was a faculty member of the Psychological Institute.

Enhancing my choice of psychology as a career was a sibling rivalry with my brother Peter, four years older. He was interested in psychology and had brought home, for instance, tests of intelligence. Like chess, which was the primary field of intellectual rivalry for my father and his three sons, these intelligence tests for a few weeks provided our competitive play. My brother Peter, however, could not study psychology himself for lack of the proper educational degree (he had dropped out of the gymnasium track to become a primary school teacher). This gave me a chance to win out. A decade later, and by way of a detour in the form of a PhD from the University of Lima in Peru, my brother too was able to join the rank of academics as an educational scientist. In fact, some of his educational work on life management (e.g., P. B. Baltes, 1993) is similar in substantive orientation to my own research on wisdom and successful development.

Becoming a Developmental Psychologist at the University of Saar

In 1959, having completed the Abitur in a humanist gymnasium in Saarlouis, I entered the University of Saar (Saarbrücken). At that time, this newly founded university was in its infancy, and the atmosphere for beginning students was friendly but also highly competitive (see P. B. Baltes, 1997a). In the Psychological Institute, we were a small group of students, many of whom ended up in academe. And it was the place, where during the second semester of study, I met my wife.

The chair of the institute was Ernst E. Boesch. At that time, chairs through their status alone dominated German psychology departments. In addition, Boesch possessed a powerful intellect, and his influence on students was intense. He also offered us a prestigious academic lineage: Boesch had been a student of the Genevan psychologists Jean Piaget and Andre Rey. For this reason, I have always considered myself a Piagetian. Boesch added to

the Piagetian framework a deep interest in cultural variation and cultural psychology. It is likely that my commitment to the theoretical orientation of contextualism and the concept of "plasticity" originated here.

Piagetian developmental cognitive structuralism and Boeschean cultural-psychological theory thus were two of the cornerstones in my study of psychology. The third, provided by the guidance of Günther Reinert, was psychometrics and the developmental study of intelligence. Reinert was, next to the chair, the most senior teacher at the institute, and he functioned as an associate director.

My 1963 master's thesis under Reinert's supervision was a factor-analytic study on the differentiation hypothesis of intelligence in childhood (Reinert, Baltes, & Schmidt, 1965). This theme continues to be part of my intellectual agenda, though now with a primary focus on adulthood and old age. What is the structure of intelligence? How does it change with age? How is it related to ability and performance levels? What are its antecedents, correlates, and consequences? What do structural changes in intelligence imply for the measurement of intelligence? (See P. B. Baltes, Cornelius, Spiro, Nesselroade, & Willis, 1980; Lindenberger & Baltes, 1997.)

After my receiving a diploma in psychology (equivalent to a master's degree) and at the age of 24, I left my intellectual home territory for the first time. Primarily to learn English (my humanist education had taught me quite a bit of French, Latin, and Greek, but no English), I obtained a travel stipend from the German Academic Exchange Office (DAAD) to study in the United States. This was a big step for me, and it did not come easily. The enthusiasm of my wife was a critical factor. Margret looked forward to the adventure of spending a year in the United States. Occasionally, she tells me that this opportunity was an important reason for marrying me.

On the day after our wedding in 1963, Margret and I left for a year at the University of Nebraska. We chose that institution because Warner Schaie, who had been a visiting professor at the University of Saar a few years earlier, had invited me to be his research assistant. This was a momentous turning point in my life and career. Intellectually it opened new windows and freed me from the local constraints of one relatively isolated university, and interpersonally it reduced my perhaps all-too-tight family and homeland bonds. I often wonder what would have happened if Margret had not been the adventurous kind who was excited about going to the States and willing to nurture me through a case of homesickness. Her experience, while working as a research secretary with Schaie, marked the beginning of her interest in a career in academe, an aspiration that at that time was not encouraged in women in Germany.

At the University of Nebraska, I participated fully in what was then called a proseminar for incoming graduate students. As a result, my Genevan cognitive and developmental approach was complemented by a solid dose of American behaviorism and learning psychology. As part of the main overview seminar, I also had a brief introduction to the study of aging in the form of a couple of lectures presented by Schaie.

I learned more about gerontology in connection with my research assistantship with Schaie, where I worked on the data analysis and write-up of a study on auditory sensitivity in old age. In the process, I began to venture into the gerontological literature and copublished my first article in English (Schaie, Baltes, & Strother, 1964). Most certainly, this one-year contact with Schaie was the primary reason why gerontology became one of my areas of interest and why I joined Schaie at West Virginia University at a later time, where he, as the head of the department of psychology, recruited me as an assistant professor.

*Emerging Interest in Lifespan Theory and Methodology:
My Dissertation on Sequential Strategies*

Any science needs good methodology. A central part of my work on gerontology has been in developmental methodology (e.g., P. B. Baltes, Reese, & Nesselrode, 1977). How did this happen? The principal impetus stems from my dissertation work.

After my year at the University of Nebraska, I returned in the fall of 1964 to the University of Saar to obtain a doctorate. In a position comparable to a junior assistant professor or lecturer (*Wissenschaftlicher Assistent*), I taught courses in developmental (introductory and Piagetian) and clinical psychology, including a seminar on behavior therapy. In addition, I supervised several baccalaureate and master's theses on various topics, such as parent-child relationships, the role of social desirability response sets in personality assessment (jointly with Klaus Eyferth), and on age differences in optimal level of stimulation and the use of testing-the-limits as a method in clinical-developmental psychology (jointly with Lothar Schmidt). Of the students in my undergraduate classes I remember Jochen Brandtstädter and Gisela Labouvie-Vief (discussed later) most. Of fellow colleagues in my own cohort, Leo Montada stands out. Our interactions continue into the present.

Soon after my return to Germany in 1964, I began to seek a topic for my own dissertation research. My first choice, empirical studies on Boesch's developmental model of action psychology, was not received with much enthusiasm. I remember the essence of Boesch's response when I presented my idea to him in an informal conversation: "Dear Mr. Baltes, let me do this kind of research myself. Why don't you find something else?" I was disappointed. From today's perspective, this rejection generated many gains and I feel no regret. It was a turning point and set my course toward becoming a lifespan rather than Piagetian developmental psychologist. On the splendid celebration of his 80th birthday, Boesch and I exchanged our memories of this event. He could not remember it and expressed surprise, perhaps even sadness.

Relatively quickly, I decided to reorient myself and pursue the general topic of developmental methodology. In teaching developmental psychology at the University of Saar, I had covered issues of research design and measurement, including Schaie's general developmental model (Schaie, 1965) and his formulation of three sequential strategies. While at the University of Nebraska, I had already mentioned to Schaie that I was uncomfortable with his theoretical and statistical proposal for defining and unconfounding the effects of age, period, and cohort. At the same time, I was much impressed with his effort and sensed that the questions of how to index age changes, how to study development in a changing society, and how to separate interindividual differences from intraindividual age change were central to developmental psychology.

With Reinert's proximal and Schaie's distal encouragement, I proposed to Boesch to write a dissertation on developmental methodology with a special focus on a critical evaluation of Schaie's general developmental model. Boesch agreed, and in 1967 I submitted to him and the faculty my dissertation titled "Cross-Sectional and Longitudinal Sequences in the Study of Age and Generation Effects." The English article (P. B. Baltes, 1968) resulting from my dissertation became a citation classic. The friendly and supportive editor accepting the article was Hans Thomae, the 20th-century doyen of German lifespan psychology. At the time I did not know Thomae personally, but I had read his work on longitudinal methodology for my dissertation.

Apparently, an initially less than desirable “nonnormative” life event, Boesch’s discouragement of my first dissertation topic, had a positive outcome. Although the substantive course of my work had been directed away from Piagetian-Boeschian psychology, when deciding on the topic of my dissertation I had selected well. Schaie, in his seminal work on cohort effects in human development, had identified a major theoretical and empirical issue; an issue, incidentally, that was of much interest in other quarters of social science research as well (e.g., Elder, Riegel, Ryder, and Riley) and attracted much attention. Any critique would be widely read, especially because Schaie’s initial exposition was rather complicated.

For many, I became the intellectual opponent of Schaie. Specifically, in my dissertation I argued on the one hand that Schaie’s concern with historical and cohort change in the study of development and aging was right on target. On the other hand, I criticized his specific approach and suggested that his theoretical and methodological efforts—namely, to identify age effects primarily as maturational, period effects as environmental, and cohort effects as genetic—were conceptually misguided. I argued that his categorization ignored the fundamental genetic–environmental interaction associated with each of the three components age, period, and cohort. In addition, I criticized Schaie’s statistical solution aimed at unconfounding the effects of age, period, and cohort as inappropriate. To prevent misinterpretations about the causes for the sequential collected age-cohort outcomes, I suggested using Schaie’s sequential strategies as “descriptive” and not “explanatory” methods of data collection. For this purpose, I introduced the labels of *cross-sectional* and *longitudinal sequences* (see also P. B. Baltes, Reese, & Nesselroade, 1977).

Regarding the issue of age- and cohort-sequential methodology, Schaie and I continued to present opposing views in the literature for several years. To the best of my knowledge, however, these exchanges did not alter our close and positive relationship. Where I am concerned at least, the associated dialogues and publications strengthened my intellectual bonds and commitment to the topic of developmental methodology. In the final analysis, in a joint paper published in 1975 in *Human Development*, Schaie and I agreed to disagree about the interpretation of age, period, and cohort effects, but to sort out, as I had argued, the descriptive from the explanatory.

Parallel to my conceptual dissertation, and in cooperation with Günther Reinert, I tested sequential methods empirically (e.g., P. B. Baltes & Reinert, 1969). While Schaie was using his model to study age and cohort-related functioning in adulthood intelligence in the United States, I conducted comparable sequential research on cognitive development in children in Germany. We clarified the so-called season-of-birth effects in IQ and showed that they were simply due to differences in the length of education that same-aged children had received as a result of fixed age cutoffs for school entry. It pleases me that this early research continues to be cited as “one of the best documented studies of the effects of schooling” (Ceci & Williams, 1997). Together with my earlier master’s thesis topic, this cohort research on cognitive development in children also strengthened my lifelong interest in the study of intelligence.

Thus, beginning with my study leave at the University of Nebraska and continuing with my dissertation and dissertation-related research, Schaie’s influence on me (although he was not my teacher and mentor in a technical sense) was strong. Despite our intellectual disagreements about specifics (including, later, the nature of intellectual aging; P. B. Baltes, 1993), I view him as the primary figure directing and supporting my initiation into gerontology.

Developing a Career in Lifespan Psychology

Schaie must have felt similarly positive, for he invited me, a few months before I completed my doctorate, to be a discussant at a conference on aging he arranged in 1967 at West Virginia University, where he had just moved. There I met quite a few of the elite in psychological gerontology for the first time.

West Virginia University (1968–1972)

Largely unknown to me, Warner Schaie used this invitation to persuade his faculty to offer me an assistant professorship. As the new chair of the Department of Psychology, he was committed to developing a graduate program in developmental psychology that covered the entire life span. When I returned to Germany, however, the first offer I received was not from West Virginia University but from the University of Michigan. Klaus Riegel had attended the conference and had arranged for the University of Michigan to offer me an assistant professorship.

For a German developmental psychologist, viewing human development as a lifelong enterprise comes naturally. As I have described in several publications (e.g., P. B. Baltes, 1979), the history of German developmental psychology, since the monumental work of the philosopher-psychologist Tetens published in 1777, was oriented toward covering the entire life span. When teaching at Saarbrücken from 1965 to 1967, I already had made an effort to cover lifespan topics when teaching the introductory course in developmental psychology. Joining an emerging program in the psychology of development across the life span was therefore attractive, and my wife and I knew and liked Schaie from our time at the University of Nebraska. Thus, to the surprise of many who know the hierarchy of American higher education, I chose West Virginia University over the University of Michigan. My position as assistant professor started in January 1968.

The four years I spent at West Virginia University were splendid and exhilarating, especially owing to my discovering there a new colleague, John R. Nesselroade. Nesselroade had joined the West Virginia psychology faculty a semester before me with a recent doctorate from the University of Illinois, where he had worked with Raymond B. Cattell, whom I came to admire while studying in Saarbrücken. In addition, West Virginia University was right for the development of a dual career. Following her master's degree from the University of Saar (where she had worked with Boesch), my wife joined the graduate program at West Virginia University to obtain a doctorate in 1973 in another strong program of that department, experimental-operant psychology.

Meeting, and soon collaborating with, John Nesselroade was arguably the most significant event in my professional career. Not only did we embark on a program of exciting collaborative research, we also enjoyed each other's company. Both on and off campus, we worked on issues of developmental methodology (e.g., P. B. Baltes, Reese, & Nesselroade, 1977) but also on intelligence and personality, where we applied, for instance, age-cohort sequential strategies to the study of adolescent development (Nesselroade & Baltes, 1974). This meeting of the minds continues into the present, a successful professional marriage of two careers. If there is something akin to collective agency, or collective selective optimization with compensation (M. M. Baltes & Carstensen, 1999) this is what John Nesselroade and I produced and enjoy.

An added strength of the context at West Virginia University was that Nesselroade and I had excellent doctoral students to enrich the dyad. In fewer than four years I was the primary dissertation supervisor of six doctoral students, several of whom came from my former home

institution in Germany. In gerontology, Gisela Labouvie-Vief (she and I share the Reinert brothers as second cousins) and Harvey Sterns are the best known. In addition, I worked closely with William Hoyer when he changed his program affiliation from experimental to developmental psychology.

It is difficult to identify the institutional origins of a given field. When it comes to lifespan thinking in developmental psychology, however, it seems fair to argue that the lifespan developmental psychology program that evolved at West Virginia University from 1968 onward was the main catalyst during the 1970s, at least in the United States. This faculty, with its critical and collaborative mass of expertise and concentration on the entire life span, was unique and had immediate impact. Of the other colleagues on the developmental faculty at West Virginia University, Larry Goulet, Stephen Porges, Frank Hooper, and Hayne Reese were especially significant.

The program presented itself in a more public manner for the first time when Larry Goulet and I organized in 1969, with strong administrative support from Warner Schaie, the first West Virginia Conference on Lifespan Developmental Psychology. It was for that conference that we made the first effort (P. B. Baltes & Goulet, 1970) to define and explicate the term *lifespan developmental psychology*. During my tenure at West Virginia University we held two more conferences, one on lifespan methodology organized and published in 1973 by Nesselroade and Reese, the other on personality and socialization organized and published in 1973 by Baltes and Schaie.

This period also laid the foundation for my theoretical work. During my years at West Virginia University, I began to outline a basic framework of lifespan psychology with a view toward a joint understanding of the traditional processes of development and aging (P. B. Baltes, 1973a). During this time I also started to think about the modification and optimization of human development (P. B. Baltes, 1973b; P. B. Baltes & Labouvie, 1973).

A focus on the modification and optimization of human development and aging was much enhanced by the work of my wife, Margret Baltes, and her training and interest in operant psychology and behavior modification. Together with William Hoyer, Gisela Labouvie, and later Sherry Willis as well as Margie Lachman, Margret has continuously reminded me of the potential of intervention, of treating a given developmental trajectory, such as the aging of intelligence, as one of many possible ones, and of keeping in mind that any developmental outcome is strongly conditioned by environmental factors and their constraints (M. M. Baltes & Baltes, 1977). I believe it was also in this collegial and marital context that I developed and nurtured my interest in the plasticity of human aging. Later, my interest in plasticity and intervention research was intensified by other contextual conditions, such as the action focus of Penn State's College of Human Development, where my career path took me next.

The Pennsylvania State University (1972–1979)

The intellectual attractiveness of the lifespan movement, combined with the expansion of American higher education, was a significant reason why many of the faculty associated with the West Virginia program in lifespan developmental psychology were quickly recruited away. As for me, in 1972 I was offered at the age of 33 the position of director of a division with some 25 faculty members in the College of Human Development at The Pennsylvania State University. This program already contained a sizable number of successful developmental and geropsychologists (e.g., Ray Bortner, Joe Britton, Aletha Huston-Stein, and David Hultsch). The college dean, Donald H. Ford, invested with vision in an interdisciplinary conception of human development and offered me the challenge of

building a strong lifespan program in psychology and sociology, combining basic research with application.

Intellectually, the hallmark of my stay at Penn State was the further evolution of lifespan theory and the implementation, jointly with Sherry Willis, of a major research program on the plasticity of intellectual aging. Here, however, I focus on institutional development. Among my first appointments at Penn State were John Nesselroade, Lynn Liben, and William Looft. Soon we were able to strengthen this group further by adding scholars in child and adolescent development who were friendly to a lifespan approach, such as Richard Lerner, but also similar minds in the sociological study of lifespan family development (e.g., Ted Huston, Graham Spanier). Richard Lerner in particular has become a significant figure in the effort to generate a theoretical field of developmental psychology that reflects the theoretical perspectives of a lifespan approach. As at West Virginia University, we also had gifted graduate students whose dissertations I had the pleasure to supervise, such as Steve Cornelius, Margie Lachman, and Carol Ryff. There were other doctoral students, such as Rosemary Blieszner, Roger Dixon, Brian Hofland, and Ron Spiro, with whom I worked closely, although I was not their primary advisor.

Max Planck Institute for Human Development (1980–Present)

In 1980 I received an offer to become a director with the Max Planck Society in Germany. Specifically, I was offered the opportunity to direct a center for psychology and human development in Berlin. At the same time, Margret Baltes was offered a professorship in psychological gerontology at the Free University Berlin.

These were attractive offers, but the pull to stay in the United States was not weak. We very much liked our new homeland. But there were also more personal reasons to remigrate. In the two years before, two people in Germany had died who were central to our lives and whom I had promised to return to Germany when an opportunity arose: my mother and my first mentor, Günther Reinert.

The Max Planck Society is a dream institution for anyone whose primary commitment is to promoting a long-term program of research and to nurturing younger generations at the international frontiers of science. Berlin enjoys a continuous stream of visiting scientists, the Max Planck Society offers financial and infrastructural support for long-term research, and one has the opportunity to recruit outstanding young scholars as doctoral students, postdoctoral fellows, research scientists, and visiting scientists. Among the visiting scientists, for instance, and restricting names to adult development and aging, Vern Bengtson, Laura Carstensen, David Featherman, John Nesselroade, and Tim Salthouse participated actively in many facets of our research program.

In Berlin my interest in and commitment to gerontology grew stronger. To a large degree this resulted from my work as codirector of the Berlin Aging Study (P. B. Baltes & Mayer, 1999; P. B. Baltes & Smith, 1997) and excellent collaboration with the coleader of the Berlin Aging Study, the sociologist Karl Ulrich Mayer. Mayer is one of my codirectors at the Max Planck Institute. Just as I am interested in advancing a lifespan approach in psychology, he is committed to strengthening the sociological counterpart, life-course sociology.

In my judgment, the affiliation with the Max Planck Society offered a new level of action and support for me and for gerontology as a field. In Germany, having the Max Planck Society devote a significant amount of its resources to a field is a symbol of high peer recognition. Not surprisingly, therefore, and due to the collaboration with a new cohort of first-rate younger research scientists, my research program showed new mental energy. During these years I also began to enjoy a number of peer-based friendly gestures such as

awards, elections to important academies such as the Royal Swedish Academy of Sciences or the American Academy of Arts and Sciences, and honorary doctorates. I mention these not only because they were important to me but also because they symbolize that by the 1990s, for my generation at least, geropsychology had arrived.

Institutional Networks and Progress in Research

This last observation leads me to comment on the role of institutions and the significance of participating in institutional networks beyond one's home institution. I hold in high esteem each of the institutions I was affiliated with. But other so-called institutional or networking institutions significantly fostered my career and my interest in lifespan psychology and aging.

Especially for someone whose first academic homes were not part of the elite system of institutions, these extrauniversity institutions were of paramount importance. For my own career, six such institutions stand out: the U.S. Social Science Research Council (SSRC), the Stanford Center for Advanced Study in the Behavioral Sciences, the Berlin-Brandenburg Academy of Sciences, the John D. and Catherine T. MacArthur Foundation, the Johann Jacobs Foundation, and the European Science Foundation. Each of these institutions has contributed in notable ways to my career. Through these institutions I have also acquired several mentors and friends who have deeply influenced my career and ways of thinking about science and scholarship: Orville Gilbert Brim, David Hamburg, Gardner Lindzey, David Magnusson, Matilda Riley, and Franz Weinert.

For instance, in 1973 Brim invited me to become a member of the SSRC Committee on Work and Personality in the Middle Years. This committee, under Brim's leadership, opened up an entirely new set of perspectives and collegial relations, for instance with Gardner Lindzey and David Hamburg. Brim himself is one of my handful of significant mentors from a distance, as is Lindzey. In 1978, this committee was transformed into an SSRC Life-Course Perspectives on Human Development Committee, initially chaired by Matilda Riley and later jointly by Glen Elder and me. In my work with the SSRC, I sharpened my thinking about the nature of human development and its interdisciplinary as well as policy agenda. These interactions formed a convoy of scholars who in the long run, I am sure, helped me to gain admittance to the "real" academy, including three fellowships (1978–1979, 1990–1991, 1997–1998) at the Center for Advanced Study in the Behavioral Sciences in Stanford, California.

Let me use the Stanford center as one more example of the powerful role of extrauniversity institutions. Regarding my theoretical contributions in the field of geropsychology, my first one-year center fellowship was perhaps the most crucial one. Initiated by Matilda Riley, a seminar convened to examine basic issues of aging and its future research agenda. In this seminar, many of my ideas, especially regarding lifespan theory and the role of plasticity, achieved a new level of precision, integration, and interdisciplinary fertility (e.g., P. B. Baltes, Reese, & Lipsitt, 1980). Conversations and discussions with CASBS fellows from several disciplines, such as James Birren (psychology), David Featherman (sociology), James Fries (medicine), Matilda and Jack Riley (sociology), Martin Seligman (psychology), and George Vaillant (psychiatry), were particularly helpful. Regarding plasticity, for instance, James Fries, David Featherman, Margret Baltes, and I, each in our own way, sharpened this notion for use in intervention research and models of successful aging.

The Emerging Contours of Psychological Gerontology in the 1960s and 1970s

So far I have presented a chronological account of the antecedents and contexts of my career. In the following sections I reflect on my contributions to the field of geropsychology. Here we are most likely to encounter self-serving views of the past. More complete accounts are contained in P. B. Baltes (1987, 1993, 1997b) and P. B. Baltes, Lindenberger, and Staudinger (1998).

During the decades when I joined the field of gerontology, the 1960s and early 1970s, the field was in a *status nascendi*. For my generation, Birren's 1959 *Handbook* had set the stage. Nevertheless, there was a dearth of theory and methodology. As scholars engaged themselves, they had to ask how to open a new field of scientific inquiry. What are the interesting questions about old age and aging? What are the proper methods? What can one expect of old age as a stage in life? How is aging related to earlier phases of life?

Researchers were struggling to identify geropsychology as a field with its own theoretical and empirical Gestalt. Because of the strong developmental and psychometric training I received at my home institution in Germany, my approach to these questions was guided more by mainstream developmental psychology and methodological frameworks than by gerontology. I wanted geropsychology to be a part of psychology, not its own discipline. In fact, it is fair to conclude that I journeyed into gerontology by being pushed, not by being pulled. What interested me were fairly abstract issues of theory and method. Only a decade or so later did I become interested in aging as a substantive phenomenon.

Advancing Developmental Methodology

The triangulation of research design, measurement, and theory was one cornerstone of my psychological training. Not surprisingly, therefore, when "pushed" toward gerontology I became something like a developmental methodologist. This opportunity was enhanced because at that time there was much debate about issues of measurement and the degree to which our data were afflicted by inadequate methods. How to study change, how to identify interindividual differences and separate them from intraindividual change, how to move from description to explanation or from quasi-experimental designs to experimental designs, how to generate a body of knowledge that is both theoretically and empirically sound—these were the questions that I (together with others) attempted to clarify and move into the foreground (P. B. Baltes, Reese, & Nesselroade, 1977).

Historically first was, as I mentioned earlier, my critical evaluation and reformulation of Schaie's age-cohort-sequential designs in my dissertation work (P. B. Baltes, 1968). Subsequently, however—primarily in collaboration with John Nesselroade—my interest expanded to include a search for better convergence between theory and method in developmental research in general. My zealous interest in developmental theory and Nesselroade's deep competence in psychometrics and multivariate research made a wonderful coalition. Moreover, fed by discussions with experimental child psychologists Larry Goulet and Hayne Reese, I argued for an integration of descriptive, quasi-experimental, and experimental approaches in the study of development using the notion of *simulation of development* as a metaphor (P. B. Baltes & Goulet, 1971; Lindenberger & Baltes, 1995).

Lifespan Theory and Aging

The second niche that presented itself during the 1960s and 1970s was the sharpening and articulation of theory. One of the central questions concerned developmental versus gerontological frameworks of theory. What are the origins of aging, do they lie primarily in

the ontogenetic past or in the conditions of old age itself, how much continuity is there in lifespan development? Is aging a transformation of development, or does it constitute the outcome of a new set of determining factors and processes that are superimposed on and interact with development?

In general, the field of gerontology had two primary options. One was to study aging as the status of being old; the second was to view aging as an outcome and part of the life course. Primarily for reasons of my psychological training in Germany, I selected the second, the lifespan option. By letting aging enter my conception of childhood-based development, I became a lifespan psychologist (P. B. Baltes, 1973a, 1987, 1997a; Baltes & Goulet, 1970). By refusing to be a gerontologist in the narrow sense, I maintained strong connections to more historically advanced fields such as child-developmental and cognitive psychometric psychology. Thus I joined those who embedded aging in a larger substantive and theoretical context and who thereby were somewhat immunized against the intellectual vulnerability of a newly emerging field. This view lingers into the present. Often I refuse to label myself as a gerontologist.

Participating in the development and elaboration of a lifespan developmental orientation was significant because it permitted me to be innovative in a dual sense. On the one hand, I contributed to the study of the precursors of aging. At the same time, however, I kept in touch with child developmentalists, thereby helping to create a new field, lifespan development (P. B. Baltes, 1987; P. B. Baltes, Reese, & Lipsitt, 1980). From this vantage point, aging is foremost an integral part of lifespan ontogenesis.

Eventually a new metatheory of development resulted (P. B. Baltes, 1997b), with a perspective that from birth onward made the simultaneous existence of gains and losses, multidimensionality, multidirectionality, and multifunctionality the centerpieces of ontogeny. In addition, I argued that selection, optimization, and compensation are the three foundational processes from which development flows. This view of the nature of ontogenetic change, which has parallels in developmental biology, is a radical departure from traditional theories of development, where either growth (early life) or decline (late life) is considered as the defining characteristic of ontogenetic change in adaptive capacity. Others, of course, joined in this effort and proposed alternative but similarly motivated theoretical conceptions; one example is the important work of my former student Gisela Labouvie-Vief (1980, 1982).

Contrary to my initiation into gerontology, the origins for my interest in the metatheory of lifespan psychology are more difficult to identify where individuals are concerned. None of my immediate mentors or colleagues was a lifespan psychologist, nor can I remember an educational experience that stimulated me to think in that direction. Most likely, aside from adding the topic of old age to my interest in childhood, this enthusiasm about a lifespan approach evolved primarily in interactions with people whom I met in diverse institutional settings and in interactions with my own colleagues and students in writing and teaching.

As I mentioned, the German tradition of developmental psychology going back to the work of Tetens and Thomae was another contributing context. As I invested myself in the historical study of lifespan psychology (P. B. Baltes, 1979), I realized that Germany had a long and distinguished tradition in this field dating back to the 18th century (Tetens). In time, of course, I became familiar with the works of 20th-century lifespan scholars such as Charlotte Bühler, Erik Erikson, Robert Havighurst, Bernice Neugarten, and Hans Thomae. But my interactions with them and my reading of them were minimal. Today I consider their conceptions (developmental task, age grading, cognitive mental representations of the life course, etc.) as very relevant as I work on articulating theoretical frameworks with substance.

Also operative were reinforcement schedules and the building of institutions, including publication outlets. In the late 1970s, for instance, I cocreated an annual research series to promote the visibility of lifespan work. From a sociological perspective, Brim had copublished two essays on socialization after childhood with Wheeler in 1966, essays that opened for me more than any other the institutional structuring of lifespan continuity and change. After I had edited the first volume in 1978, Brim joined me to edit the next five volumes of the annual series on lifespan development and behavior. During this period, and with much guidance from Brim, I developed a better understanding of the substantive processes of lifespan development and its contextual contingencies associated with individual and institutional processes of social differentiation (e.g., ethnicity, gender, social class) and social-historical change.

Geropsychology and Interdisciplinarity

In their early evolutionary states, emerging specialties reflect a holistic approach. Disciplinary differentiation comes later. This certainly was true for gerontology in the 1960s and 1970s. The study of aging is more than psychology, and psychological research on aging will profit if it recognizes the biological, social, institutional, and historical forces that shape human development. In this spirit, another marker of my intellectual history is my firm embeddedness in transdisciplinary dialogue.

I am a psychologist, of course, and would like to contribute primarily to the science and profession of psychology using psychological principles and methods. On the other hand, I recognize that psychological analysis is but part of the scientific enterprise. Therefore, I have engaged myself routinely and with enthusiasm in interdisciplinary dialogues with colleagues from neighboring disciplines such as anthropology, sociology, and biology.

Because of this spirit, for instance, I became involved in the Berlin Aging Study, in which about 40 scientists from internal medicine, psychiatry, psychology, and sociology collaborate. And this interdisciplinarity esprit led me more recently to propose an overall theory of lifespan development. This metatheory reflects two decades of interdisciplinary discourse and in its foundation is inherently biocultural; it emphasizes the conjoint action of biological-genetic, institutional, social, and individual factors (P. B. Baltes, 1997a).

Making Developmental Theory Contextualistic

Theoretical orientations in geropsychology reflect variations in emphasis associated with individualistic (organismic) and contextualist (environmental) factors and processes. Some would claim that in the history of psychological gerontology, this dynamic during the past few decades expressed itself in a movement from more individualistic theory toward more contextualistic theory. Within developmental psychology, lifespan psychology was a key player. In this instance, my primary intellectual benchmarks were Urie Bronfenbrenner and Richard Lerner.

While recognizing the defining significance of genetic factors, I have argued that the expression of human development is equally strongly conditioned by cultural and individually based agency factors. Each developmental or aging outcome is but one of many possibilities. I trace this theoretical stance to early writings (M. M. Baltes & Baltes, 1977), although more refined frameworks evolved later and in collaboration with new colleagues at the Berlin Max Planck Institute (e.g., Reinhold Kliegl, Ulman Lindenberger, Jacqui Smith, and Ursula Staudinger). In social-science disciplines, such a contextualist orientation, of course, is the norm. This, however, was not true for psychology.

*From Description to Explanation and Modification:
Plasticity and Intervention Research*

One theme associated with contextualism is plasticity (see also Lerner, 1984). For me, information about the range and conditions of plasticity as well as its age-related changes is the cornerstone of developmental and aging theory (P. B. Baltes, 1993, 1997a). As gerontology evolved, such an approach was less prevalent than it is today; now most of us think that knowing “what is possible and not possible” is the quintessential question of developmental and gerontological theory.

How variable, how modifiable is aging? What is optimal aging? Such questions form another stone in the mosaic of issues that shaped my intellectual motivation and contributions to geropsychology. My enduring commitment has been to use science to change the contextual and behavioral world of old age for the better (P. B. Baltes, 1973b; M. M. Baltes & Baltes, 1977; P. B. Baltes & Labouvie, 1973). In the 1960s and 1970s, such a focus was present but not at the fore. The science of aging, so my colleagues and I argued, is more than the counting of wrinkles and charting decline.

In this spirit of using science for the betterment of human functioning, I have made *intervention research* and the *optimization of human development* a significant part of my intellectual agenda since the early 1970s. Intervention, of course, is relevant to causal explanation and therefore is part of experimentation. But, for me, the significance of intervention research also has a human optimization and human policy dimension. As I have said recently (P. B. Baltes 1997a; see also M. M. Baltes & Baltes, 1977), I consider the architecture of cultural and biological evolution as essentially incomplete. Of all periods of the life span, the period of old age is the most incomplete and therefore subject to most risks and negative outcomes.

As an intervention-oriented psychologist, I have attempted to provide agency to society and individuals in the task of optimization of old age. Here, aside from collaboration with Margret Baltes, historically I owe much to a close cooperation with Sherry Willis, an educational child developmentalist at Penn State. In the mid-1970s I invited her to develop with me research on the modifiability of intelligence in old age. To this end we initiated a large program of cognitive training research aimed at exploring human potential in old age, selecting explicitly a domain (fluid intelligence) where decline was the rule (Baltes & Willis, 1982). When I left for Germany in 1980, Willis joined Schaie in matrimony and research collaboration on the same topic involving the Seattle Longitudinal Study.

The initial findings of this research program, in which Rosemary Blieszner, Steve Cornelius, Brian Hofland, Margie Lachman, and Ron Spiro played critical roles as advanced graduate students, led to much optimism. Later, as I transformed the program of research at the Berlin Max Planck Institute (P. B. Baltes, 1993) and, with the input from others such as Reinhold Kliegl and Jacqui Smith, considered methods of testing the limits, we obtained new findings on age-associated losses that spawned less optimistic considerations, as described in the next section.

*Dual-Process Theory of Lifespan Intelligence and the Search
For Positive Aging (Wisdom)*

Theory, method, intervention research, and models of optimal aging coalesced in one additional effort to make a contribution to geropsychology. One of the hotly debated areas of gerontological research in the 1960s and 1970s was whether intellectual decline is inevitable

and ubiquitous. With verve, researchers took opposing views. Articles by Horn, Donaldson, Botwinick, Schaie, Willis, and me clashed on an intellectual battleground (e.g., P. B. Baltes & Schaie, 1976, versus Horn & Donaldson, 1977). This was an exciting time, in part because as the field advanced, it soon became clear that the questions were not phrased well, and that new research needed to be conducted to make progress.

In my case, the new work (conducted at the Berlin Max Planck Institute) made me change my thinking, which originally tended to argue for an optimistic, no-decline view. One line of my work, on the psychology of wisdom, continued this tradition of optimism. Other work, however, suggested a move toward a less optimistic and more balanced, more differentiated and multidimensional view of intellectual aging (P. B. Baltes, 1993). This change in theoretical and empirical perspectives put me, for some years at least, in opposition to some of my closest colleagues, notably Warner Schaie and Sherry Willis.

I mention this research perspective because it permits me to highlight a profound transformation in my thinking that had precursors (e.g., P. B. Baltes, 1979; P. B. Baltes et al., 1980), but largely coincided with my appointment at the Berlin Max Planck Institute and working with a new group of scholars with additional areas of expertise. In this newly configured context of minds, I worked toward developing a conception of lifespan intelligence, one that explicitly involves gains and losses, links psychometric intelligence with cognitive psychology and the study of expert systems, and furthermore is reflective of Tetens's original outlook on human development.

My principal collaborators in this newly evolving research program were (in alphabetical order) Freya Dittmann-Kohli, Roger Dixon, Reinhold Kliegl, Ulman Lindenberger, Jacqui Smith, and Ursula Staudinger. Expanding the work of Cattell and Horn on the theory of fluid and crystallized intelligence, we added new theoretical perspectives. Specifically, we considered models of expertise and the differentiation between the largely brain-biology-based "fluid mechanics" and the knowledge-based "crystallized pragmatics" of the mind. A so-called dual-process theory of intelligence resulted (P. B. Baltes, Dittmann-Kohli, & Dixon, 1984; P. B. Baltes, 1993). In addition, we developed model research paradigms of the mechanics (serial memory as operationalized by the method of loci) and the pragmatics (wisdom), and used expertise and testing-the-limits paradigms to explore zones and limits (asymptotes) of functioning in these two analogues of the mechanics and the pragmatics (P. B. Baltes, Smith, & Staudinger, 1992; Kliegl, Smith, & Baltes, 1989, 1990).

This approach permitted us, we think, to offer a new integrative conception of lifespan intelligence that is neither pessimistic nor optimistic (P. B. Baltes, 1997, P. B. Baltes et al., 1998), but that contains a modulated view of gains and losses in intelligence over the lifespan. Regarding the mechanics of the mind, the evidence is definite: decline starts early in adulthood (Lindenberger & Baltes, 1997). Regarding the cognitive pragmatics, where wisdom served as sample case, there is a potential for advance as long as the mechanics do not fall below a certain threshold. It is in the pragmatics of the mind where the interpenetration of intelligence and personality, of biology and culture, is strongest, and where culture can outwit biology (Staudinger & Baltes, 1996).

Toward Theories of Successful Aging: Selection, Optimization, and Compensation

Throughout human history, science has looked beyond, using scientific knowledge to extend our visions and improve the human condition. This is true also for old age. For millennia, philosophers and humanists have wrestled with identifying the positive in human

aging; their efforts were largely based on focusing the mind on the meaning and enjoyable experiences of life (P. B. Baltes, 1991).

In gerontology, since the 1960s, empirically minded scientists have taken up the challenge to search for a good life and optimal aging. The concept of successful aging became the mental guidepost for these efforts (M. M. Baltes & Carstensen, 1996; P. B. Baltes & Baltes, 1990; see also Rowe & Kahn, 1987). Beginning in the late 1970s, Margret Baltes and I have participated in this effort. Interrelating the work of others (e.g., Brandtstädter, Brim, Fries, Kahn, Labouvie-Vief, Lachman, Lawton, Riley, Rowe, Ryff) and our own, we have developed a theory of successful aging that reflects our best knowledge about the nature of lifespan development, the biological and social constraints of aging, and the mechanisms and factors that operate for selection, optimization, and compensation (SOC) to orchestrate an effective and good life.

The theory that Margret Baltes and I developed, *selective optimization with compensation*, is a general theory of development that can be applied to many levels of aggregation and analysis, including psychological aging. For each of the main objectives of lifespan development (growth, maintenance, and management of loss), we submit that it is important to select goals, to optimize the means for reaching these goals, and to compensate when extant means to reach goals disappear. In my view, this theory is the most direct expression of my scholarly commitment to optimization, of articulating life-management strategies that are conducive to maximizing gains and minimizing losses as we develop and age. Together with new colleagues, such as Alexandra Freund, Jutta Heckhausen, Frieder Lang, Ulman Lindenberger, and Michael Marsiske, we are currently at work, collaboratively and individually, to test the theory in a variety of settings and with differing formulations.

Observations

Throughout my life, I have been involved in collaborative relationships. More than the ones I have mentioned here were relevant at one point or another in my career. This is especially true for the fields of child and adolescent development, where an entirely different cohort of colleagues would be involved. Of close collaborators at the Berlin Max Planck Institute regarding questions of child development, for instance, the late Michael Chapman, Todd Little, Gabriele Oettingen, and Ellen Skinner come to mind.

Lifespan continuity and change also applies to professional colleagues and friends, in particular, however, were "significant others" throughout most of my career, as colleagues, cothinkers, or inner voices. In order of their entry into my life and using up to 1970 as a criterion, these were Günther Reinert, Margret Baltes, Ernst Boesch, Warner Schaie, John Nesselroade, and Orville Gilbert Brim, whose contributions to my career cannot be overestimated.

My acknowledgments of significant others would be incomplete, however, if I were not to highlight the powerful influence that some of my former students or postdocs, and by now accomplished gerontological or lifespan scholars, have had on my intellectual biography. Over the years, and beginning in the early 1970s with Gisela Labouvie-Vief, many of them have changed the nature of my thinking about development and aging in critical ways. Alphabetically and with a focus on lifespan psychology and the study of aging, I want to acknowledge especially the contributions of Jochen Brandtstädter, Steven Cornelius, Freya Dittmann-Kohli, Roger Dixon, Jutta Heckhausen, Reinhold Kliegl, Margie Lachman, Ulman Lindenberger, Michael Marsiske, Carol Ryff, Jacqui Smith, and Ursula Staudinger. Each of them has succeeded in making me think differently about one or the other of my

intellectual concerns. I expect them to be among those who shape the future contours of lifespan psychology and keep geropsychology in the news of science.

In another institutional context and befitting a lifespan psychologist, as a trustee of the youth-oriented Johann Jacobs Foundation (e.g., *Johann Jacobs Foundation: Annual Report 1997*), I have the opportunity to express a commitment to another period of life: childhood and adolescence. In this context, we discuss lifespan connections between youth and old age, and occasionally I am confronted with the argument that too much societal and scientific concern for and investment into old age detracts from the future welfare of children and youth.

Finally, mentioning and thanking only in the closing paragraph my children Boris (born 1965) and Anushka (born 1971) for their many contributions to family life, reciprocal socialization, and patience with an all too often preoccupied and career-oriented father carries symbolic meaning. It is bound to give the impression of an afterthought. As it turns out and in the best spirit of reciprocal generational transfer, my children are very important to me. Moreover, they seem to understand their father very well, often better than he does himself. I hope that the fatherly neglect they may have experienced in their childhood, and the fact that we uprooted them when returning to Germany and presented our Black adopted daughter with an all-too-White environment, is compensated by a more present and resourceful late-midlife father. Indeed, my wife has remarked more than once that my fatherhood has grown in leaps and bounds as our children have become adults.

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