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Sydney Teachers' College, NSW, DPE (1950)
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NSW Department of Secondary Education: Teacher, Hurlstone Agricultural High School, Sydney (1951–1953)
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Douglas Stuart led the charge at the University of Arizona for interdisciplinary programs in neuroscience and physiological sciences and the recruitment of an invertebrate neurobiology group. He also facilitated the recruitment of a learning and memory group, and an interdisciplinary program in biomedical engineering. Throughout Arizona and beyond, he promoted the development of movement neuroscience research and training. His 1959–2006 experimental research focused on spinal motor mechanisms and their relation to tremors, locomotion, muscle fatigue, and the overall control of movement. In recent years, he has continued to provide overviews on motor control and the history of movement neuroscience.

Douglas G. Stuart

Australian Background

My early attitudes and behavior had much to do with being a fourth-generation Australian. My father, Gordon Khartoum Stuart (1900–1933) and his eight siblings were in a family whose activities were traced in recent times back to 1137, when they owned land in the Scottish Highlands north of Inverness. This land was lost in 1746 after the Battle of Culloden near Inverness, where troops of the British government soundly defeated the largely Scottish Jacobean rebels. My ancestors who took part in this uprising were forced to flee down the west coast of Scotland to the Peninsula of Ardnamurchan above the Isle of Mull. They settled there as poor shepherders in a location known to this day as Bourblaige, named by earlier Scandinavian invaders. My family remained there until 1839, when the opportunity arose to have free passage to Sydney, Australia. The British government (and the Presbyterian Church to some extent) made 20 ships available to Scottish Presbyterians and some Roman Catholics. These included poor highlanders like my Ardnamurchan family, which then consisted of a husband, wife, and five children. They arrived without incident in Sydney in January 1840, where they all passed a government-controlled reading and writing test. Such free-passage immigrants were frequently given the same opportunity offered to selected convicts who had completed their sentence there: work for a farmer for up to 10 years and then be granted by the government a generous allotment of arable land. (See chapters one and two in Megalogenis [2015] for the more liberal treatment of “farming” convicts during the earlier days of the food-deficient NSW settlement under Governors Arthur Phillip [1738–1814] and Lachlan Macquarie [1762–1824]).

My Stuart family forbears spent several years working first for a farmer in Raymond Terrace, 16 miles north of Newcastle and 116 miles north of Sydney. They then spread out in the Raymond Terrace, near the Port Stephens area working largely on farms owned by others. In 1857, they rented and later owned (by the late 1890s) a farm on the banks of Carr’s Creek, about 2 miles north of the town of Grafton and 20-plus miles southwest of the village of Maclean and town of Yamba, a Pacific Ocean resort, about 420 miles north of Sydney. The farm was suitable for dairying and for growing maize and sugar cane. I have no record of whether or not they received government assistance for the purchase of this farm. Thus, my father and his eight siblings were born in Maclean or Grafton and raised in an attractive rural area near the Pacific Ocean.

In contrast, my mother, Martha Monteith Stuart (1901–1996), and her eight siblings in the Monteith family were of Northern Irish and English/Australian extraction. Her father, Joseph Monteith (1853–1925), had emigrated with assisted passage from Londonderry, North Ireland to Sydney in 1872. By dint of hard work, probably native common sense and intelligence, and much drive, he developed a successful 1,200-acre property, which he named the Red Bank Farm. It was 3 miles outside Guyra, a small town about 24 miles north of the larger town of Armidale in the Northern Tablelands of New South Wales (NSW) about 300 miles north west of Sydney. It was here that my mother attended primary school in Guyra (riding most happily there and back with two siblings on a horse) and high school in Armidale. Her mother, Elizabeth Willis Monteith (1857–1950), a first-generation Australian, was of English background. Her first husband, Richard Moore, whom she married in 1880, was also of English origin. He died in 1887 leaving her with three children to whom six were added after her second marriage to Joe Monteith in 1888. From all accounts, my Londonderry grandfather was well known in Guyra and Armidale for his colorful personality and strong-mindedness about most issues.

Early Family Life (1931–1947)

I was born in the Richmond Private Hospital, which was located in Casino, a small, attractive town located in the northeast coastal area of NSW. This hospital was owned and run by my aunt, Chrissie Stuart, who was a registered nurse. A local doctor, Dudley Small, delivered me. He was married to Chrissie's sister, my Aunt Jessie, who also had once been a registered nurse. I had much contact with her until leaving for America in late 1953.

My mother was widowed one and a half years after my birth, leaving her to raise her two young children, the other my brother Ian, who was two and a half years older. We lived 20 miles east of Casino in the larger town of Lismore, which is 460 miles north-northeast of Sydney, and 22 miles west of the Pacific Ocean. At that time, Lismore had a population of about 7,000 people, in contrast to the more than 40,000 people who live there today.

My father had been rising rapidly in local government in Lismore before his death at a young age in 1933, due possibly to rheumatic heart disease. In 1927, after his marriage to my mother in Kyogle, a small town 20 miles north of Casino, he had become the shire clerk (equivalent to a U.S. county manager) of the Gundarimba Shire, which extended in rich agricultural land southeast from Lismore toward the ocean. As a youngster, I took pride in what was written in obituaries about him, and what was told to me by his close friends—he was a pleasant, effective, and well-regarded shire clerk; popular with other administrative constituencies and his many friends; an excellent cricketer and good golfer; and clearly headed for a rapid rise in local government to much larger shires or possibly toward a political career in the conservative,

farmer-focused Australian Country Party. My mother dearly loved and admired him and remained that way throughout her entire life, which extended until a few weeks before the age of 96. She had been a graduate of Sydney Teachers' College and a primary school teacher in Kyogle where she met and married my father. After his death, she returned to teaching, much later in Sydney becoming for a short time, albeit reluctantly, a primary school principal.

Our home was about a mile from the center of Lismore, adjacent to the town golf course, which my father had helped found, and with views of attractive green hills, many covered with pineapple and banana plants. Our home was constructed of wood, and like many in Lismore, it was elevated a few feet above the ground to allow circulation for home cooling in the semitropical climate. Multicolored hydrangea plants concealed the space between the ground and our home's base. The front and backyard grass was very green and fast growing. One of my jobs as a youngster was to reluctantly mow the grass twice a week under the watchful eye of my much bigger and stronger brother. Outside the town, there were patches of rain forest. As youngsters, my friends and I bicycled there on weekends for a swim in their rock pools. All in all, it is hard to imagine a more attractive Australian rural town than was Lismore in the 1930s and 1940s, and its nearby beaches.

In the early to late 1930s, a Scottish orphan, Mary, lived in our home, arriving at the age of 17 as an economic migrant, following free ship passage from Scotland. While treated as a relative of ours, Mary's low-salaried responsibilities were to look after my brother and me and to do much of the housework and cooking. My brother and I dearly loved Mary, who was very vivacious and spent much time with us. She later married a local journalist and then lived happily in Lismore with her own family. This turn of events was much to the delight of my mother, Ian, and me. I mention this because Mary's story had a very strong effect on me—the start of a lifelong empathy and support for financially poor immigrants irrespective of the countries involved in their emigration desire, and the process involved in their immigration.

As for my mother, she was an attractive, scholarly, hardworking, and vivacious person, with several close friends, a great sense of humor and usually fun loving, which made our home a popular one for my brother's and my friends from throughout our childhood to young adulthood. Later, she had similar close relationships with my brother's family in Sydney, and much later was greatly admired and liked by my American wife and our four children.

Up to the mid-1940s and particularly on the weekends, the focus in our Lismore home was on reading, with each of the three of us in a different room or on a verandah reading all manner of books. I still remember the full name and conversations with the main librarian at the Lismore Municipal Library in the late 1930s.

When Ian and I were quite young, our mother took us on long hikes in the hills around Lismore, and arranged holidays for us at local beaches during school breaks. Early on, we also had two December weeks in a suburb of Newcastle,

400 miles south of Lismore where one of her married sisters lived. Later, when Ian was in his very early teens, she went alone to Newcastle in December for two or three weeks to see this sister, leaving us to delightfully fend for ourselves in a tent at a local beach. There we body surfed all day with other young teenagers who had similar tents, including some of our Lismore friends.

Superimposed on all of this was our mother's strong desire that we take schoolwork *extremely seriously*, this being of great ease and interest to my brother, but of less concern to me throughout high school. This was a great and permanent disappointment to my mother, even after she visited the University of California, Los Angeles (UCLA) many years later for my PhD thesis defense.

What I admired most about my mother was her strong personal commitment to social fairness and insistence that Ian and I should always behave with societal concern. She was deeply religious and in later years did much volunteer administrative work for the Presbyterian Church in Sydney. However, she never tried to indoctrinate her religious beliefs into her two children. In return, Ian and I attended Sunday school and a church service with her the same day until we graduated from high school in Lismore, because we knew this pleased her.

There was, however, an intermittent sad side to my mother, which had a very strong, lifelong effect on me and influenced my much later behavior as an academician. She greatly missed my father and considered his early demise the one great tragedy in her life. When I was about six years of age, we walked one weekend day to the Lismore churchyard where my father was buried, about two miles from our home. I sat on the concrete bed of his gravestone and watched my mother as she cleared weeds from around it, crying softly and mournfully. Without speaking to her, I contemplated her intermittently sad behavior and vowed to go through life with a consistently cheerful and positive attitude *no matter what was to befall me*. This I have done consistently, despite several reverses, which were invariably of my own making.

My brother Ian was an outstanding high school student, particularly in mathematics, and he topped this subject in the NSW five-year Leaving Certificate, which was the university entrance examination. He also had some close friends in high school, a good sense of humor (some called it odd), was a renowned rugby footballer, and a feared street fighter. He gained notoriety by breaking the nose of a well-known bully, two years ahead of him in high school. Later, much to my mother's horror, Ian thrashed a street thug who had tried to enter a school dance.

In much later years, Ian and I discussed how many good times we had had as youngsters in Lismore, and how much of this was due to our mother's total commitment to us. This included, I am ashamed to now confess, her pleasure in bringing me breakfast in bed until I left for the United States in late 1953 at the age of 22. The reader can imagine my future American wife's reaction when she learned of this behavior.

Kindergarten–Grade 11 Schooling (1935–1947)

The quality of teaching could be very high in rural towns in NSW up to the early 1960s. Many teachers wished to work in Sydney for its cultural and other advantages. Those who wished to do this were rotated through the rural towns before achieving this goal. As a result, one's education in a place like Lismore could be as advanced as that in Sydney. I, for example, was doing differential equations in my fourth year of high school when barely 15. This quality of instruction and demand for advanced student performance in mathematics was matched in my other high school subjects; in all for me, English, French, Latin, history, math I and II, chemistry, and physics for high school years one to three, with two subjects dropped for years four and five (Latin and history by me) when the focus was exclusively on six subjects for university entrance. Strangely and sadly, however, the local culture was for boys not to take biology courses, these being for girls, and girls alone. I have no knowledge of when this absurd fashion changed.

I still remember the name and physical features of my kindergarten teacher, and many of the subsequent elementary (grades one–six) school ones, too. They were strict in a nonthreatening way, and seemed to me at that time to be very committed when imparting their knowledge. The single exception to strictness was my sixth-grade primary school teacher. He was younger, friendlier, and more personable and intimate with his class than was usual at that time. At the beginning of the school year, he told us he was very religious and had “known God for 14 years.” However, by the mid-year, he was writing letters to the local newspaper claiming to be Lismore's only Communist and first public atheist. In class, we had great fun with him when one of us had to stand facing our classmates, and recount a feature of our life, or show the ability to express strong emotions like happiness, fear, and rage. In return, our classmates would comment boisterously on the quality of our presentation. This teacher taught us our academic material quite well, but when we went on to high school, some of my classmates and especially I continued our rowdy ways, which were far less tolerated in the serious precollege environment.

For me, a serious flaw in rural high school education at that time was the lack of career counseling. The number of options seemed very few to me: become a teacher, engineer, or clinician. None appealed to me. From elementary school onward, I was a staunch Republican: that is, a person who believed that Australia should secede from the British Empire and, as a republic, trade with our neighboring countries. In retrospect, I first felt this way based on my Scottish origin, but by high school, I had read much about the United States, its history in becoming a republic, and its multiple ways of life. How I could use my republican beliefs in an interesting career in Australia was a puzzle for me, except for the vague thought that I might eventually become a politician. I discussed this with no one, and it may help explain my casual approach to high school work, which greatly

frustrated my mother, and my teachers, particularly those in mathematics, who thought I would perform as well and be as committed to this subject as my brother, who was two years ahead of me. Indeed, the only teachers who tolerated me were those interested and supportive of my performance as an athlete, which is discussed later. However, I had the strong support of my brother, who always, indeed throughout his lifetime, thought that down the road I would achieve much in something or other. Even in his own case, however, and despite his academic success in high school, he was given bad local advice, and began his university training in engineering, when mathematics and physics would have been far more appropriate for his interests.

Despite my lackadaisical approach to high school education, I did sufficiently well in university entrance exams to be awarded a university scholarship, which could be applied to any field. However, two months before the early December examinations for the five-year High School Certificate, when I was barely 16, a calamity beset our family. My brother had a manic bipolar incident in Sydney near the end of his second-year university courses. I was told nothing about the details, except for the fact that he had been committed to a mental hospital. My mother left immediately for Sydney to grasp the full details of his mental health status, secure a new teaching position there, and find a house for us to live in. I remained in Lismore, studying intensely for my final high school examinations. Then, in early January 1948, I left the nirvana of Lismore and the far north coast of NSW for an uncertain future in Sydney, particularly in relation to the problem that had beset my brother.

Sydney Teachers' College (1948–1950)

My vow to remain cheerful was severely challenged after I arrived in Sydney and visited my brother at the mental hospital where he was incarcerated. It was a truly ghastly place, and he told me he was being given electroconvulsive shock therapy that could ruin his brain. This was true at that time, the refinement of such treatment not occurring until much later.¹ It was also painfully obvious that no one in the Stuart or Monteith family was going to help solve the problem of why and how my brother had been put in a mental hospital, or how to get him out. Like most large former farming families at that time, the adults in the Stuart and Monteith families ranged from uneducated laborers at one end of the economic and social spectrum to doctors, lawyers, and other prominent folk at the other. None of them seemed interested in Ian's calamity. His incarceration was beyond my mother's capability, and my Aunt Jessie, an ex-nurse who could have been of great help, was

¹ Encyclopedia of Mental Disorders, "Electroconvulsive Therapy," <http://www.minddisorders.com/Del-Fi/Electroconvulsive-therapy.html>.

still up north in Casino, managing her business affairs. It would be up to me, at barely the age of 16, to solve Ian's problem, and I quickly developed a strategy to do this. But how could I bring about my strategy while beginning an intense university program?

What seemed best for me academically at that time was a major in economics or political science and a minor in mathematics, but it would be too demanding for the strategy I had in mind. Also, I had to earn my keep, easiest done with some type of college scholarship that gave me adequate time to get my brother out of the hospital for short periods to give his doctors confidence that he could be released from incarceration. Thus, I chose a three-year Diploma in Physical Education (PE), the well-paid scholarship for which was funded by the NSW Department of Education in return for five subsequent years of high school teaching. It was quite competitive, being limited each year at that time to 20 females and 20 males, with 15 of the latter having to be former World War II (WWII) servicemen with repatriation benefits equivalent to the GI bill in the United States. I was certainly attracted to the PE field by virtue of my enthusiasm for the state's support of national fitness camps, which I have discussed previously in the Living History Project of the American Physiological Society.² Ironically, my becoming a neuroscientist and subsequently having a very fulfilling all-round life in the United States was due to my decision to enter a college program in PE.

To get my brother out of hospital for brief periods, I enlisted the help of a much older married cousin with children. He was a welder by trade and on the weekends made extra money in his own welding shop. He was an ex-serviceman who had fought in the demanding New Guinea campaign near the end of WWII and had much sympathy for those of his army unit who had been subject to shell shock. Ian's doctors were quite willing for this cousin to take my brother out on Saturdays, accompanied by me. He, as an ex-serviceman, was widely respected by all, including Ian's doctors. This was in contrast to me, a callow 16-year-old about to become a college student.

After a few Saturdays, my cousin convinced the doctors that I, while only 16, could take Ian out alone. Once this was achieved, I cut college classes as much as was possible and took my brother out several times a week to all kinds of interesting places; historical and science museums, art galleries, harbor rides to the zoo and Manly Beach, sports events, and Chinese and Italian restaurants. This was necessary for just a few weeks and after much harping by me to his doctors, and further support from my welder cousin, Ian was released from hospital. He immediately went to work for a government research agency with which he stayed successfully for his entire professional life, while also returning to the University of Sydney for a BS and MS in mathematics and physics and an MS in applied mathematics

² American Physiological Society, "Douglas G. Stuart," <http://www.the-aps.org/mm/Membership/Living-History/Douglas-Stuart>.

from the University of Cambridge. Despite two or three subsequent minor bipolar hiccups, he also had a fine married life, with a very intelligent, sensible, and friendly wife, and three children who have excelled in their adult professions.

Although my strategy and actions had been a success for Ian, they had a bad effect on me. Without mentioning it to anybody, I became far too confident that I had the ability to solve difficult problems without advice from anybody. This internal arrogance led to many errors of judgment in subsequent years.

With Ian's problem seemingly solved by mid-1948, I elected to stay in the PE program, which was interesting in many ways. First, I appreciated the teaching faculty's primary interest being their subject matter rather than how individual students were reacting to their material, as seemed to be the case in my high school years. Instruction in English was as outstanding as it had been in high school, and even more enjoyable, this also being a feature in physics. In contrast, educational theory seemed trivial despite the efforts of several truly earnest, well meaning, and likeable faculty. A major disappointment was the teaching of exercise science, biomechanics, and kinesiology. This surprised me, because the Australian U.S.-trained instructor who headed the PE program had written an interesting textbook on the physiology of exercise.

In many nonclassroom ways, three college years in PE was a maturing and unforgettable experience. I had two close post-high school friends in the PE program, with whom I had many adventures: hitchhiking for hundreds of miles in different directions while on class breaks; body surfing at the marvelous Sydney beaches; classes at a private sector central Sydney gymnasium under the tutelage of a famous instructor, who taught tumbling and trampoline gyrations to well-known dancers, divers, and gymnasts; and interacting with "White Russian" immigrants of many ages at their lively post WWII club in central Sydney.³

In college, I associated with hard-bitten ex-servicemen, who were my senior by many years. I greatly respected those who had actually fought for Australia in WWII against Nazi Germany, Italy, Vichy France, and Japan. One such person, who was the oldest in my PE class, took a particular interest in one of my college friends and me, and he often found odd jobs for us where we could make some money while attending all of our college classes. He had been a laborer before the start of WWII in 1939. He enlisted immediately and spent the next six years as an infantryman in the volunteer portion of the Australian Imperial Force (2nd AIF; see Lambert 1951), which often had disastrous battles. He told us that the worst part was seeing so many of his army unit die during this long ordeal.

³ Wikipedia, "Russian Australians," https://en.wikipedia.org/wiki/Russian_Australians#The_fourth_wave_1945-1960.

Nonetheless, he was quite upbeat in college and was looking forward to post-college life as a high school teacher rather than as a laborer. From this fine person, I developed a lifelong enthusiasm for the concept of "Up from the ranks," and I still try to help such people, particularly disadvantaged college students at my university.

The second ex-serviceman for whom I had much empathy was two years ahead of me in college. He had been a tailgate gunner in a bomber, which made raids over Germany in the 1944 to May 1945 stage of WWII. He had been the sole survivor of four consecutive plane crashes on English airfields after returning from such raids. He, when a high school PE teacher, arranged for me to have an after-class job in a Police Boys' Club as a gymnastics instructor for 10- to 14-year-olds. This was the best-paying and most enjoyable after-school job I had while in the PE program. It left me with tremendous respect for Police Boys' Clubs, which are quite prevalent and effective in the United States, and it reinforced the irony of life, me too young to fight in WWII, and the tailgate gunner a victim of its horrors.

Hurlstone Agricultural High School (1951–1953)

My state-supported college funding required that I subsequently teach for three rather than five years because after graduation I was assigned to Hurlstone Agricultural High School on the southwest outskirts of Sydney. This well-known school had 500 students, 250 there by day, and 250 boarders. They ranged in age from 12 to 18 for largely a five-year course of study. While I was in college, there had been many discipline problems among the boarders. A decision was made by the NSW Department of Secondary Education to hire two PE teachers who would work long hours four days per week, with both there on Wednesday, when there was a city-wide sports program in the afternoon. Our combined goal was to (1) give traditional 40-minute PE classes throughout the classroom day, (2) solve the boarder discipline problem with much emphasis on post-class physical activities, (3) improve the Wednesday afternoon sports program of all students not engaged in interschool sports, and (4) get the interschool sports competition back on track.

My partner in this venture was a very likeable and well-known halfback on a renowned rugby football team. We worked together harmoniously and enjoyably for the three years I was at Hurlstone. In addition, we contributed to the camaraderie of our group of four or five teachers who lived at the school in a plain multibedroom house to attend to the needs and discipline of the 250 boarders. Because of this extra work, we all owed the state three rather than five years of teaching. Another benefit was that our room and board was free and available seven days a week for the entire year.

In addition to my PE assignments I took it upon myself to teach folk dancing to the boarders, a shock to these "aggie" students and their visiting

parents, and to produce plays. In 1953, one of my productions with 15- and 16-year-old boys won a citywide competition (my one-act version of Maxwell Anderson's 1935 "Winterset," which addressed the 1920–1927 Saccho-Vanzetti case). This experience is still a vivid and particularly enjoyable memory of my high school teaching days.

Transition from Australia to the United States

Despite the many successes that my colleague and I had in meeting our objectives as high school PE teachers and how enjoyable the student contact and experience were, I realized by late 1952 that my main interest in the students was how they performed their various physical activities—that is, the physiology and biomechanics of their exercise movements. This convinced me that the time had come to secure a track and field scholarship to attend a college in the United States. After WWII, this type of scholarship became known in Australia, particularly for elite swimmers. My plan was to undertake a BS and MS in PE and exercise physiology and then return to Australia to a university position. At that stage, I had no knowledge or concern about the role that research might play in this plan.

My next step involved writing to the presidents of several universities in the United States and explaining my ambitions. A few weeks later, I heard from several track and field coaches in the United States, who sent inviting brochures about their universities and their educational and sports programs. After a few ups and downs, I accepted a scholarship offer from Michigan State College (MSC) to begin classes there in January 1954. I was to be given two and a half years of a four-year undergraduate BS degree for my three-year Sydney Teachers' College transcript and would have one year of undergraduate eligibility for athletic competition. An Australian sprinter two years my junior was already there and his support was valuable in securing the scholarship. We knew each other to some extent having both been members of the NSW Track and Field team in 1950. The NSW Department of Secondary Education provided me with a three-year leave of absence to complete both a BS and an MS or MA in the United States. I accepted this as a form of security rather than thinking of it as a command to return to its governance.

On October 10, 1953, five days after my 22nd birthday, I left Sydney on a 1,000-passenger Italian liner bound for Genoa. My purpose was to first visit my brother, who was then studying at the University of Cambridge, and then visit a friend in London, Ontario, before beginning classes at MSC in early January 1954. In retrospect the one-month trip from Sydney to Genoa was an abrupt jump in my ever-widening academic experience. I spent much time with young men of my own age from largely Germany and also from France and Italy. They were returning to their homelands after saving money in Australia since 1949 by using their skillful work on

the technically advanced Snowy Mountains Hydro-Electric Project.⁴ The intent of these young Europeans was to use their savings to attend various universities in their native countries. I had great respect and interest in their academic intent and the long hours they had worked in the Snowy Mountains of southeast Australia. In academe to this day, I have always preferred colleagues, trainees, and classroom students who have met challenges external to a purely academic environment and these young “ship-mates” were about to become the epitomes of this background.

On this voyage, I also worked out two to three hours a day with two affable professional wrestlers, whose names I still remember. One was from Buffalo, New York, and the other from Hawaii. After completing a tour in Australia, they and their female companions were headed to Europe for a similar tour. These were very tough workouts, and by the time I saw my brother in Cambridge, I was the strongest and supplest I had been to that point.

Visits to London and Cambridge were interesting, to say the least, as were the educationally rich trip to New York City on the *Queen Mary* passenger liner and the week I spent in Canada with students at the University of Western Ontario. The latter experience was the beginning of a lifetime of contact with Canadian universities, and their faculty and student contributors to movement neuroscience.

Move toward Physiology at MSC/MSU (1954–1955)

My stay at MSC, whose name changed to Michigan State University (MSU) in 1955, was eventful in many ways, and it aided my self-imposed, painfully slow conversion to the field of neuroscience.

MSU Coursework and Research

My undergraduate BS requirements included two quarters of compulsory humanities. The teaching was of high quality, particularly social science, which I had not had previously. As an undergraduate, I was most attracted to physiology and its laboratory exercises, particularly as taught by two outstanding instructors: Princeton-trained Professor W. Duane Collings (1914–1981) for cardiorespiratory mechanisms, and a recent German-born University of Chicago graduate, Assistant Professor Hermann Rudenberg (1927–1994), for experimental neurophysiology. Both emphasized the role of research in a university career, which was the beginning of my understanding of this possibility. I also took a superbly taught course in plant biology and *most mistakenly* did not follow up with a course in animal cell biology. This error was to cost me dearly, as I will explain below.

⁴ National Archives of Australia, “60 Years of Snowy Mountains Hydro-Electric Scheme,” <http://naa.gov.au/collection/snapshots/power/index.aspx>.

These academic pluses were countered by the poor quality of courses in PE and the inadequate research possibilities in my major, which foolishly was still PE. Fortunately, I continued to take physiology courses in 1955–1956, and in the last of these, which was on endocrinology, I learned a lesson that greatly aided my subsequent performance as a PhD trainee and subsequent director of my own research laboratory and its various trainees and visiting collaborators. In that endocrinology course I did all of my study with an older classmate, who later also became a neuroscientist. We profited greatly from our extensive interactions because the more we studied together and challenged each other, the better we did. This emphasis in our interactions was on *dialogos*, an ancient Greek term, which I define idiosyncratically as “dialogue without rancor.” It became a feature of all of my subsequent academic activities.

My MA thesis topic was quite naive. However, it required that I review literature about a variety of physiological indices that were being used at that time in the study of physical fitness. Throughout execution of this study, I became fully committed to the field of physiology and the certainty that I would next pursue a PhD in this area.

Part-Time Work

My athletic scholarship was very meager. It provided free tuition and part of my room and board in a dormitory. The moment I arrived I needed a 20 hour per week paying position, which had already been arranged for me. For two quarters I was one of the telephone switchboard operators in my dormitory, a job I actually enjoyed by chatting with the operator on the other end of the line. Their regional accents and my then-strong Australian one was always of interest and amusement to the other operator and me. Fortunately, MSU helped accommodate my financial needs by giving student athletes part-time employment. This work involved largely assembling and disassembling bleacher rows and seats for various college functions. In addition, we worked on the grounds and gardens of this attractive campus. While doing this work, it never occurred to me that my U.S. visa was a student one, which another state might not recognize as legal for part time nonacademic work. This issue became a major problem when I later transferred to UCLA.

Association with a Faculty Couple

Within my first few weeks at MSU, I met John Cowan Messenger (1920–2010) a cultural anthropologist who, with his wife, Betty (who later also became a cultural anthropologist), was engendering much renown. In 1951–1952, John had studied the Annang people of Nigeria and often referred to this experience and his findings. I became a good friend of this lively, friendly, and distinguished couple, and had many discussions with them,

which I found fascinating because of the nature and range of their research. John was a groomsman at my wedding at MSU in 1957. He was an outstanding teacher of undergraduates, which I found out more than a year later from my future wife, who had taken one of his courses while an undergraduate at MSU. John and Betty's love of their research further opened my eyes to the possibility of a research career. Their long-range effect on me was the extent to which knowing them encouraged me to seek contacts in areas other than neuroscience when I later entered this profession, a habit that persists to this day.

My Best Luck at MSU

I met my future wife, Jean Rassbach, in September 1955 when she was a third year undergraduate at MSU. She, as the head of a sorority, and I, as an assistant manager of a large dormitory, were on a panel to meet with a group of principals of Michigan high schools to discuss what beginning students should be aware of when they started their college life at MSU. My initial impression of Jean was how attractive and interesting she was. A few months later I met her by chance at the main library and she agreed to go out with me. On our first date, this sparkling and intelligent young woman with a truly sunny outlook explained to me the nature of her college major, her philosophy about marriage, the number of children she would like to bear, and how she would like to raise them. We only had about five dates before we decided on marriage after she completed her BA degree in June 1957. Within the short epoch of these dates, I had decided that I would indeed try to undertake a PhD in physiology at UCLA, marry Jean if she would agree, and stay subsequently in the United States to pursue a career in physiology. All of this came to fruition, including her acceptance of my selfish desire to make one final trip to Australia in the fall of 1956 before beginning my PhD training at UCLA in January 1957. In other words, as luck would have it, she agreed to a marriage in early June 1957 and barely seeing me throughout the preceding 10 months. Her most likeable parents made no attempt to discourage Jean from her atypical and indeed risky marital plan. In retrospect, this too was quite remarkable.

Break before UCLA (August 1956–January 1957)

At this stage in my story, I have to return to 1946, the year I began to advance as a high school athlete in cricket, swimming, rugby football, and high jumping. The latter was my favorite athletic pastime, even though landing pits (then usually filled with sawdust) were not used in NSW in 1946–1947, even for statewide contests held in Sydney. As a result, one had to use an old-fashioned style of jumping, which permitted landing without injury on a hard grass surface. This denied the possibility of using one of

the two most advanced styles of jumping in that era, the western roll and straddle. With the encouragement and support of three of my high school teachers (all ex-servicemen just returned from WWII), I was given school travel funds to compete in the combined public high schools track and field championships in Sydney. There, I broke the NSW under-15 high jump record in 1946, albeit a higher jumper beat me. The subsequent newspaper accounts emphasized that I had jumped in bare feet on damp grass. After reading about this, my Aunt Jessie immediately sent me funds to buy customized, spiked shoes such that in 1947 I won the NSW competition and set an Australian record. Later, I set an Australian under-19 record, was a member of the NSW Track and Field team in 1950–1953, and the Australian team for the British Empire Games (1950) in Auckland, New Zealand, and British Commonwealth Games (1954) in Vancouver, Canada.

In the United States, I was denied one year of eligibility in university events, because of an argument between the MSU and University of Michigan track and field coaches, but this was a boon in my path to neuroscience. It enabled me to take a four-hour neuroscience laboratory course at MSU on Saturday mornings in the winter of 1955, after which I would fly to various cities for evening Amateur Athletic Union contests, which were mostly on board surfaces on which I jumped more consistently. Interestingly, I won the high jump on the hard cinder surface at the University of Michigan Fieldhouse in 1954 and 1956, setting a record in the latter event. My ambition going back to 1946 was to be the first man to jump 7'. However the highest height I achieved was 6' 8 15/16" (6' 9" would sound much better) in 1956, the year that another jumper leaped 7'. Nonetheless, I wanted one more crack at hopefully the Melbourne Olympic Games in late 1956. I had little money then, and did not wish to ask my mother for financial help. However, with a New York advertising friend and beginning track and field journalist of renown, Jim Dunaway (1928–2015), I had the opportunity to share the owner's cabin with him on an empty Norwegian oil tanker's trip from San Francisco to Indonesia (4 weeks) and another full tanker from there to Sydney (1.5 weeks). This plan was strongly disapproved by the team physician at MSU, James Feurig (1916–1975), whom I greatly respected. He emphasized that I would arrive in Sydney with "dead legs," no matter how hard I practiced on the two ships. In my then-headstrong way, I ignored his advice and set out from San Francisco for Indonesia on September 9, 1956, after a Dunaway-organized car trip across the United States.

Throughout the trip to Indonesia, I practiced physical exercises assiduously, wrote two pages a day to my fiancée, played endless rounds of chess with Dunaway, and had long discussions at our daily meals with our erudite Norwegian ship captain. Our tanker was delayed by bad weather near the Philippines, and we arrived one day late for the next oil tanker to Sydney. Our tanker berthed at a Caltex facility at Sungai Kampai where the Siak River flows into the ocean in the Straits of Malacca, with jungle all around

this small port. While Caltex tracked down my mother in Sydney, we spent two days there before taking a small boat up the Siak River to a much larger Caltex facility in Pekanbaru. The Siak River is very narrow and one could see the wildlife on both sides of the boat, a truly awesome experience.

I spent two or three days in Pekanbaru (still training every day as I had at Sungai Kampai) before taking another small boat back to Sungai Kampai and on to Singapore. The following morning I flew to Sydney, where I was met by my delighted mother, who scolded me for not asking her for funds to fly from MSU to Sydney!

Two days later in Sydney, I resumed high-jump training for the Olympic Trials: that is, the first actual jumping I had done since leaving San Francisco. I was horrified to find that just as Dr. Feurig had predicted, I still had dead legs for jumping, even though I was very fit in other ways. This continued in Melbourne in the high-jump contest at the Melbourne Cricket Ground, which had a temporary hard cinder track for the Olympic track and field events. The selection committee was very sympathetic and arranged for me to stay in Melbourne for a week and then compete in a jump-off for the third and final high-jump spot on the Australian team. I jumped higher in the jump-off, but was again beaten. As I lay in the landing pit after my third and final failing jump, I reflected that I now had to find a way to make some money to return to the United States and begin my PhD courses at UCLA. However, this had already been arranged for me. Jack Metcalfe (1912–1994), a Sydney lawyer and outstanding prior Australian Olympic athlete and coach whom I had known well in 1950–1953, was in charge of all the equipment used or constructed for the 1956 Games. Before explaining what he wanted me to do, he took me up to the bar at the Melbourne Cricket Ground to meet someone who wanted to have a chat with me. This was Richard Casey, a well-known and popular diplomat and Australia's then ambassador to the United States. He had heard good things about me from Metcalfe and others, and he offered me the opportunity to return with him to the United States as a member of the Australian Department of External Affairs, where my job would be to give talks throughout the country, the purpose being to strengthen the already strong relations between Australia and the United States. I thanked him for this opportunity, but told him I was fully committed to a post-PhD career in physiology. He looked at me in amazement and politely asked me, "What the ##### is a physiologist?" I explained, and our conversation continued pleasantly for another hour.

The next day, a Sunday, I began work at 8:00 A.M. with Jack Metcalfe's unit. My job was to design and have built the nonelectronic scoreboards used for over a dozen sports (including basketball, boxing, diving, gymnastics, rowing, swimming, water polo) at the Olympic Games, which would begin a few weeks hence, and to continue throughout the Games as the scoreboard troubleshooter. Amazingly, a person had been hired two years earlier to have all this already prepared, but he was indifferent to his assignment,

albeit doing well on other ones. I was paid a particularly generous salary and worked about 80 hours per week for several weeks, including giving many lunch-hour talks to various service groups about life in the United States. As a result, I had more than sufficient funds to return to the United States by plane, and in addition, bring attractive gifts for my fiancée and her parents.

After the Olympic Games, I returned to Sydney for a few weeks to spend time with my family and await the postal arrival of my UCLA acceptance as a graduate student, which was needed for a new student visa. Mercifully it arrived in time for my arrival back at my fiancée's home in Cleveland, Ohio, on Christmas Day. While waiting for my visa in Sydney, I was requested to meet with a high administrator in the NSW Department of Education. He had assumed that I would return to a new and very attractive position under his jurisdiction. At the suggestion of Gordon Young (1904–1974), the director of physical education in the same Department of Education, this was to develop track and field clubs throughout NSW and coach the more promising of the athletes, who would accrue from my efforts. Such a position would have enhanced my statewide visibility and paved the way for a subsequent political position in the House or Senate of NSW. This administrator and Gordon Young were surprised and quite annoyed that my plans had changed irrevocably. Reflecting on this much later, it is quite obvious that I had been treated most honorably by the NSW Department of Education, at least two of whose directors had my best interests at heart.

In summary on this Australian adventure, I had certainly had a memorable and unusual trip, but it was a stupid thing to do in terms of my ambition of competing in the Olympic Games. Also, it had interrupted what should have been my more important goals of augmenting my fiancée's social activities during her final college year and taking a hard look at my academic deficiencies before leaving MSU to begin a PhD program in physiology at UCLA.

UCLA (February 1957–May 1961) and Wedding at MSU (June 1957)

My selection of UCLA for a PhD program had been the suggestion of my MSU professor, Duane Collings. He felt that I should not give up on my interest in exercise physiology, and he was impressed with the work of an eclectic bioengineer, bio-mathematician, and exercise scientist, Craig Taylor (1909–1958), who had a joint appointment at UCLA in physiology and engineering.

My First UCLA Semester

Before coming to UCLA, the deans of students at MSU and UCLA had arranged for me to have one semester of free room and board at a fraternity

near the UCLA campus. In return, I was to give regular physical exercises to the fraternity members, which I did with much success and the enthusiasm and enjoyment of the participants. I also became by chance the announcer for the UCLA rugby union team, a position I kept and greatly enjoyed throughout my PhD experience. However, there were no meals at the fraternity house between Friday and Monday breakfasts, and I was very short of money because student visa regulations were much stricter in California than in Michigan. As a result, I had no regular part-time work and I struggled financially throughout this first semester.

An even more serious problem was the demands of the class work. Before leaving San Francisco, the preceding August I had hitchhiked down to UCLA to meet with Professor Victor Hall (1901–1981; Field et al. 1981), who then administered the physiology department's graduate program. I explained to him my plan to travel by oil tanker to Australia via Indonesia in the hope of competing in the 1956 Olympic Games, and then returning to UCLA to also hopefully begin my PhD program in physiology. His response was to say sternly, "Mr. Stuart, you neither look, act, nor speak like a physiologist! This did not offend me. From the outset, I liked this short, pugnacious-looking professor, a polio survivor who used crutches when standing and walking, and who was later to play a major role in my training to become a physiologist. Rather, I smiled and replied, "Dr. Hall, I think I am going to surprise you!" When we met again after my return from Australia, he told me that my acceptance into the physiology PhD program was contingent on getting satisfactory (B or better) grades in several courses in my first UCLA semester. I found excellent study partners, and with *intense effort* did well in this challenge, and was then accepted by the department of physiology and told to look around for a research position with one of the faculty. I began with Craig Taylor, who suggested that I work on the steady-state oxygen consumption rate of long-shoreman at the San Pedro Waterfront. My MS thesis research had soured me on chronic studies on humans but not on comparing the human maximum oxygen consumption rate in various groups with different levels of health and physical fitness. Taylor was impressed with my well-informed interest in this latter area, which was about to explode with international studies, but he had no such funded study available, so we parted amicably.

The only other faculty member who offered me a position was Professor Allan Hemingway (1902–1972; Anonymous 1972) who was not opposed to people with a PE background, having had a successful WWII research collaboration with a well-known exercise scientist at Wright Field, a U.S. Air Force (USAF) research facility in Texas. Hemingway wanted me to analyze expired respiratory gas for its oxygen and carbon dioxide content using a Scholander gas analyzer in studies going on in his laboratory group, and in parallel have the opportunity to develop my own PhD thesis project using this among other techniques to study an aspect of the neural control of body temperature, for which area he had a grant from the USAF Arctic

Aeromedical Laboratory, which included a research assistant salary for me. I took this position and told Hemingway that I would start work with him two weeks hence after getting married back at MSU.

Marriage on the MSU Campus

Before flying back to MSU, I had arranged for a new drive-away car for the trip back to Los Angeles. For such cars, one was given free gasoline for a drive from an automobile company to a car dealer at the destination. However, when I arrived at East Lansing, the person organizing the availability of a car had lost his connection to the auto industry. Fortunately, the MSU football coach, Duffy Daugherty (1915–1987) arranged another car for us. Duffy had fought with Australians in New Guinea during WWII, and he greatly liked them, including those who were or had been athletes at MSU.

Jean had been very busy in her final quarter at MSU, which involved her BA completion and making all the arrangements for our wedding and reception at the Alumni Memorial Chapel on the grounds of the MSU campus. She held up admirably, however. At a prewedding Rassbach family reception, Jean's Uncle George from Los Angeles asked me how much money I had. I replied truthfully and dispassionately, "I have 35 dollars to my name." He chuckled and said, "Aha, I knew you were an adventurer!" We instinctively liked each other.

Two days after our wedding, Jean and I took off for Los Angeles. We found an apartment in Westwood and helped pay the first month's rent with money we had been given as wedding presents. Jean immediately began teaching at a Santa Monica elementary school in the Los Angeles School District. This required two bus trips and long walks to and from work, whereas I walked less than a mile to the physiology department in the UCLA School of Medicine. Note that we had no time or funds for a honeymoon, and in reality, we barely knew each other. Thus, began our West-Coast married life, which has now reached 60 years, the platinum duration. For the next year (1957–1958), we were very poor, to say the least, because Jean stopped teaching to begin a family. However, we had several friends who were married couples in the same financial situation, and we look back on that year with much amusement, fondness, and pride in our fortitude.

Work in the Hemingway Laboratory

I worked with Allan Hemingway from late June 1957 until late March 1961. He and his charming and lively wife Claire, who was a biochemist, went out of their way to be hospitable to Jean and me. In addition, Allan gave me an open invitation to bring my local and visiting friends to his nearby Westwood home in the late afternoon for refreshments and spirited conversation. He greatly enjoyed all of my invitees, and particularly the UCLA rugby

players from Australia, New Zealand, and South Africa, and also those at the University of British Columbia. (Both Allan Hemingway and Victor Hall were originally from nearby Victoria on Vancouver Island.)

Hemingway ensured that both locally and at national meetings of the American Physiological Society, I had discussions with as many senior physiologists as he deemed desirable for the advancement of my career. In the laboratory, he left me alone to do my temperature regulation experiments and he edited carefully our coauthored six refereed articles and three book and symposium chapters, which were published between 1959 and 1964. When I left his laboratory, he followed my subsequent appointments with great support and interest. In many ways, he was a model mentor, whose friendship and support I will always respect. However, there was one serious problem. For my PhD research, he wanted me to recheck classical shivering studies using old-fashioned techniques (see Hemingway and Stuart 1963). In contrast, I wanted to continue along the lines of Lucy Birzis (1919–2008), his most recent previous PhD student. She and Hemingway had become the first experimentalists to record the unitary activity of hypothalamic neurons using extracellular microelectrodes. As a former physicist and skillful instrument maker, I thought Hemingway would revel in such work. However, he was politely adamant that he preferred other work for me. I also tried to tempt him with the idea that I should work on the determinants of the motor rhythm of shivering, a topic in which we both had interest. Again, his background in physics made him an ideal mentor for this project, which also would have given me a far better start in movement neuroscience. His counter was that this could be my main postdoctoral project, as subsequently occurred.

Another person in Hemingway's UCLA group also had a strong impact on me. I consider him my comentor in becoming a physiologist. This was Walter Freeman III (1927–2016), a Yale MD, and postdoctoral associate in the Hemingway group until leaving in 1959 for a faculty position at UC-Berkeley. Walter was a fourth-generation U.S. clinician. One of his great-grandfathers, William Williams Keen (1837–1932), was the first brain surgeon in the United States and a particularly successful and innovative one. Walter's father, Walter Jackson Freeman II (1895–1972), was also a prominent, if controversial, clinician. A neurologist among other specialties, he co-coined the term "psychosurgery" during his mentorship under the Portuguese 1949 Nobel Laureate neurologist Egas Moniz (1874–1955). Together, they and Freeman II's American collaborator, the neurosurgeon James W. Watts (1904–1994), were leaders in the development of prefrontal lobotomy (leukotomy), a controversial way to alleviate selected psychiatric disorders.

I greatly enjoyed my association with Walter Freeman III. He had a towering and highly original intellect⁵ and was a great conversationalist.

⁵ Wikipedia, "Walter Jackson Freeman III," https://en.wikipedia.org/wiki/Walter_Jackson_Freeman_III.

We became good friends, and I learned a great deal from him about all manner of neuroscience developments, including importantly how hard one has to work to advance knowledge by even a smidgen.

Other Aspects of PhD Training at UCLA

I relished my experiences as a PhD trainee at UCLA, entering the program with much naiveté in the summer of 1957 and defending my PhD thesis in early 1961 with far more knowledge about the field of physiology, in general, and neuroscience, in particular.

Faculty: During my PhD training, three of my professors were elected to the National Academy of Sciences: Ted Bullock (1915–2005) for invertebrate neurobiology; Horace (“Ted”) Magoun (1907–1991) for neuroanatomy and the reticular activating system; and Donald Lindsley (1908–2003) for physiological neuropsychology. Shortly thereafter, Tom Sawyer (1915–2006) was elected for neuroendocrinology. Several of my other UCLA professors were well known at the international level. They included W. Ross Adey (1922–2004), Bernard Abbott (1920–2006), Earl Eldred (1919–), John Green (1917–1964), Wilfred Mommaerts (1917–1994), Arnold Scheibel (1923–2017), Fritiof Sjostrand (1912–2001), Robert Tschirgi (1925–1997), and Victor Hall, my ever-alert critic and unwavering supporter. They were all very friendly and supportive of the biomedical trainees of my era, and all were excellent classroom teachers, the best being Adey, Hall, and Tschirgi.

Magoun and Hemingway, and many other of these noted faculty, ensured that leading U.S. and foreign neuroscientists, who worked at or visited UCLA, had substantial scientific and social interactions with the pre- and postdoctoral trainees. This was how I developed my lifelong interest in interdisciplinary neuroscience, and the advantages of exposing pre- and postdoctoral trainees to the very best of international neuroscience, both intellectually and socially.

Course Work: In the spring semester 1958, I topped the physiology course taken by 100 medical students and about 20 PhD trainees. Of more local interest was that I co-topped the interdisciplinary neuroscience course taken by the same medical students and even more PhD trainees. The “co-topper” was Don Wilson (1932–1970), whose mentor was Ted Bullock. Don later did brilliant postdoctoral pattern generator work on the locust before further such work at UC-Berkeley followed by Stanford University. Tragically, he died during a river trip at far too young an age.⁶

Overall, the UCLA basic and clinical neuroscience faculty were sufficiently pleased with my academic performance that I was granted for

⁶ UC Riverside, “Donald M. Wilson,” <http://faculty.ucr.edu/~currie/donald-wilson.htm>.

1959–1961 a far better paying predoctoral position in the interdisciplinary National Institutes of Health (NIH)–supported UCLA Mental Health Training Program.

The Tragic 1958 Death of Craig Taylor: Throughout early 1958, I maintained my contact with Craig Taylor, who was tempting me with a PhD project on improving electronically driven artificial arms. However, when the Russians launched Sputnik 1 on October 4, 1957, there was a hue and cry in Washington, DC, about why had we fallen behind the Russians at the beginning of the Cold War space race in the number of competent mathematicians and bioengineers required for our own nascent space program. Craig Taylor's advice was more or less demanded by various think tanks in Washington, DC, along with that of many others with his eclectic bio-mathematical talents. He began flying regularly and quite often to Washington, DC, for such meetings. This involved travel on commercial planes far too many times, these being pre-jet, 10-hour flights between Los Angeles and Washington. The terrible result was that in a moment of exhaustion and despair, he committed suicide on April 24, 1958, by jumping off the top of the UCLA engineering building. Allan Hemingway knew this far too well because before Taylor's next-demanded Washington trip just before his suicide, he was totally exhausted and begged Hemingway to take his place for that final meeting. Hemingway declined on the grounds that he was too busy at that time, a decision that he subsequently regretted with much guilt for the remainder of his life.

A Training Gift from Professor Ross Adey: In 1958, Ross Adey decided he should give four of his trainees and me (a fellow Australian and his good friend) informal (noncredit) training, which he felt would benefit our subsequent careers in research. We met for four hours on Saturday mornings throughout the fall of 1958 and the spring and summer of 1959. The 1958 sessions were devoted to electronics and its application to electrophysiological instrumentation. In 1959, we presented summaries to our fellow classmates on the neurobiology of brain structures from the medulla oblongata to the forebrain, with all five of us carefully studying the reading list for each session such that we could interact optimally with the speaker. We worked far harder for this informal training than for our credited coursework. Adey was an intense and highly motivated taskmaster throughout these sessions, and we all recognized that we were being given a unique and extraordinarily valuable experience. In my own case, it was of inestimable value for my future electrophysiological research and for when I began teaching as a faculty member. Clearly, my classmates and I owed much to Ross Adey for his commitment to our development as neuroscientists.

PhD Written and Oral Exams: I did well in my 1959 PhD written examination, writing for a total of 17 consecutive hours. In the subsequent oral examination, however, it became painfully obvious to the committee that I had

a serious deficiency in fundamental cell biology. This deficiency had not been obvious in my previous UCLA physiology exams because their focus had been at the systems level of understanding. Accordingly, I was given six months to rectify this deficiency before another oral exam would be given. During those months, the faculty in the physiology department gave me three informal but intensive oral exams to establish the full extent of my deficiencies. I took all this quite philosophically because I needed and respected their brutal probing, which had come about by my own making. My problem had begun at MSU by not following an excellent course in plant cell biology with a similarly in-depth one on animal cells. This cavalier approach continued at UCLA, where I had ignored detailed histology and focused on its broad principles as taught quite cleverly by Fritiof Sjostrand. Fortunately, I survived this six-month re-appraisal of my broad biological knowledge and did well on the subsequent oral examination. Several years later, I would tell my own PhD trainees about this experience when they were preparing for their own written and oral examinations. I always exhorted them to consider principles of importance in physiology and neuroscience from the cellular to the systems to the organismic level of understanding. Remarkably, our modern trainees can do this, with the added ability to precede cellular principles with molecular ones.

Association with Professor Youjira Kawamura (1921–2013): This remarkable Japanese neuroscientist, a former neurosurgeon, became the youngest (at 38) full professor of physiology in Japan at the University of Osaka Dental School. Kawamura went on to train more than 40 department heads of clinical dentistry, neuroscience, and physiology in Japan's then-20 schools of dentistry. For this effort, together with his scientific renown, he was honored with a prestigious award from the emperor of Japan and similar awards from many foreign countries.

Kawamura came to UCLA in late 1959 on a Rockefeller Foundation award for one year of research at UCLA and six months visiting laboratories in Europe after which he was to write a report for his sponsoring foundation. He had just published the world's first textbook of oral physiology and neurophysiology. Kawamura had hoped to collaborate at UCLA with a leading neuroscientist. However, there were no open spots for him except in Hemingway's laboratory to work with me, a lowly PhD trainee. He took this letdown in cheerful stride, and we had a great year together. He recently had studied some mechanisms of tremor and was not averse to working with me on the central control of shivering. In addition, Kawamura spent much time with the first UCLA dean of dentistry, Raider Sognnaes (1910–1984), who wanted him to transfer to UCLA as head of physiology in the dental school, which was to take its first class in 1964.

I arranged for Kawamura and his wife, Haru, to rent an apartment near Jean and mine such that Haru could spend most of each day with Jean and our two very young children, who were then tentatively learning their first

words of English from their mother, as did Haru. Thirty-six years later, she told Jean and me in Osaka, Japan, how much she still remembered about this experience and how much she had enjoyed it.

Kawamura taught me how to keep our experimental results in better systematic order and other fine points about the electrical stimulation of brain structures (e.g., see Stuart et al. 1961). In all, we coauthored two refereed articles, a technical report, and two book chapters on the neural control of shivering and, together, we discussed this work at several other institutions. He also presented a talk at the 1960 Fall Meeting of the American Physiology Society at Stanford University. For this talk, a fellow graduate student and I had him practice intensely for his presentation of one of his own Japanese projects, an intracellular iontophoretic stimulation and recording study on hypoglossal motoneurons in the cat, which was no mean feat in 1960. I also improved his written English for several of his subsequent publications, a practice I am still undertaking over 57 years later for many Japanese colleagues.

Finally, I should add that Kawamura and I met several times subsequently in Tucson, India, and Japan. Our wives and we had become great friends at UCLA, this being one of the best features of collaborative research with colleagues in both one's own country and abroad.

PhD Thesis Research: I enjoyed writing my 259-page PhD thesis, with its 260 citations on the prosencephalic modulation of the hypothalamically controlled shivering tremor. Admittedly, it involved use of old-fashioned techniques, but the effort put into it was substantial as was the knowledge I had about this subject by the time of its completion. In all, it resulted in three refereed publications, and five technical reports for the U.S. Arctic Aeromedical Laboratory, the latter being a valuable experience for subsequent progress reports to other extramural granting agencies.

Shortly before defending my thesis, Hemingway was visited by Sir Otto Edholm (1909–1985), a famous and very pleasant (indeed charming) English worker on all-round physiology, including temperature regulation. He was then Head of the Division of Human Physiology at the National Institute of Medical Research in London. He came to my tiny so-called office for a chat about shivering. At the outset, I said, "Sir Otto, before we begin I should tell you that I know more about this topic than any other living person!" He laughed and said, "Proceed." I did for more than two hours, with him adding valuable comments and questions along the way. After this delightful and valuable interaction, my PhD thesis defense was effortless.

Postdoctoral Research at the Long Beach VA Hospital (April 1961–1963)

While completing my PhD thesis, I was awarded a postdoctoral medical research fellowship by the Bank of America–Giannini Foundation. It paid

sufficiently for me to take my family to any laboratory in the United States or abroad. However, I felt it best if we could stay in California such that Jean could have as stress-free time as possible while raising our ever-expanding family, with our third child born the night before my PhD defense. For this reason, I chose to work with Professor Earl Eldred as my PhD mentor in my own laboratory at the Long Beach Veterans Administration Hospital (VAH) in a program supported financially by both the hospital and the UCLA Brain Research Institute (BRI). My laboratory was equipped superbly, and it included the full-time services of two fully paid and very capable technicians; a very creative electronics technician, and a masterful one in experimental animal surgery. On the home front, it was particularly helpful that Jean's parents helped us purchase a fine home in the town of Rossmoor, about four miles east of my laboratory, and on a street where Jean made several friends who had children of the same ages as ours. In summary, the four years spent with my family in Rossmoor and at work at the Long Beach VAH were very happy ones, quite scientifically productive, and very pleasant except for one incident with Ross Adey, as explained below.

Two Then-Atypical Research Collaborators

A High School Collaborator of Great Talent: Shortly after beginning work in Long Beach, I requested technical advice from Lawrence Ott (1905–1978), a PhD physicist at a Hughes Aircraft research facility in Culver City. He gave it willingly and asked if in return I could provide a research experience for one of his twin sons, who was a high school senior in Altadena, California. Thus began the research of Ken Ott (1944–) in my postdoctoral research program. On Saturday mornings, he made the one-hour drive from Altadena to my laboratory in Long Beach, and we then undertook an electrophysiological experiment on surgically reduced spinal cats. These experiments ended in the late afternoon. That summer, he worked full time with me, just as he did in subsequent summers while an undergraduate at Yale University, and for two further summers while a medical student at UC-San Francisco. During medical school, he spent a summer as an ex-tern at the National Hospital for Neurology and Neurosurgery, Queen Square, London, England. After medical school, Ken was an intern and resident in the Harvard program in neurosurgery at the Massachusetts General Hospital. He is still very active in private practice in San Diego. Ken established the Gamma Knife Center for radiosurgery, the first radio-surgical unit in San Diego. Additionally he developed a movement disorders surgery program at Scripps Memorial Hospital and Sharp Memorial Hospital. He is a past president of the California Association of Neurological Surgeons. In all, Ken had seven publications with me on a variety of topics, all of which involved electrophysiological recording in anesthetized cats and conscious humans. Ken is still carrying out stereotactic surgery and laminectomies (in humans) just as he did (in cats) long ago in my labs in

Long Beach and Davis California. People like Ken, who participated in experimental neuroscience in high school, were relatively rare in the early 1960s but are now far more prevalent, and neuroscience has profited accordingly.

My First Postdoctoral Research Fellow: In the early 1960s, it was very unusual for a beginning research assistant professor to have the services of a nationally funded postdoctoral research fellow, particularly one from abroad. However, this was my good fortune in 1963 shortly after completing my postdoctoral traineeship. A Japanese neurologist on leave from the department of neuropsychiatry at Chiba University requested that I meet him in my Long Beach office. Koichi Ishikawa (1932–), a former neurophysiology trainee of Professor Sabura Homma (1923–2014) at Chiba University, who himself had trained previously with a later Nobel Laureate, Ragnar Granit (1900–1991) in Stockholm, was working at that time with a senior American neuroscientist at a university in Southern California. Koichi was very unhappy in this position and asked if he could work with me. Homma was a good friend of Eldred, who had also worked with Granit, so I told Koichi that I would try to help him. I considered the very limited number of possibilities for providing a U.S. fellowship to a non-U.S. citizen and together we applied to the Foundations' Fund for Research in Psychiatry. To my great astonishment, the application was successful, and Koichi spent the next four years with me as his supervisor; two at Long Beach, which were very happy ones for Koichi, his wife, and two young sons; and two years at UC-Davis, where he was most unhappy because he felt far more at home in a large city. It was in my Davis laboratory that he met the 1963 Australian Nobel Laureate, Sir John Eccles (1903–1997), who later provided him with a two-year position at SUNY Buffalo, so in subsequent years, he felt much better about his association with me in Davis.

During our four-year collaboration, I learned far more about overall neurobiology from Koichi than he did from me. He was a fourth-generation Japanese clinician, going back to late Samurai times. Together, we coauthored seven refereed articles and one book chapter. These featured my transition from work on the neural control of shivering at UCLA to extracellular neuronal recording in the cat central nervous system (CNS) and in human spinal cord injury subjects at Long Beach, and finally intracellular recording of motoneuron properties in cats at UC-Davis. This progression involved his enthusiastic participation in the experiments and editorial criticisms, delivered with typical Japanese politeness, of our articles and my various seminars and national meeting presentations.

Research Projects at the Long Beach VAH

Work on the Rhythm of Shivering and Other Tremors: This work was my main postdoctoral research project with Eldred as my mentor and collaborator. He participated actively and imaginatively in many of our key

experiments on shivering, which were accomplished on anesthetized cats and conscious humans, the latter using some procedures that were quite novel at that time. The work resulted in three consecutive reports on the control of the shivering rhythm, general sensory, proprioceptive, and central contributions and a chapter written for a symposium volume. The findings were summarized as follows:

It was concluded that under experimental conditions, the contributions to the shivering tremor of general sensory inflow, proprioceptive input, mechanical factors and central influences could be recognized and each even shown to be essential. In the intact animal and human the rhythmicity of shivering would appear to involve a complex yet smooth interaction between all these contributions. (Stuart et al. 1966c)

These findings should have been of general interest to an emerging group of movement neuroscientists, who were beginning to study the overall control of movements by blending “inside-out” and “outside-in” approaches. Inside-out involves determining the properties of single cells within the CNS and then ascertaining how these properties influence the operation of CNS microcircuits, single reflexes, groups of reflexes, and generators of central patterns of movement. This information is then used to theorize about CNS control of overt motor behavior. In contrast, the outside-in approach begins with analysis of the biomechanics of posture and movement and then uses this information to theorize on how the mechanics are solved by the CNS and its pathways, circuitry, and even single cells (Stuart 2007; see also Wetzel and Stuart 1976). Sadly, our shivering rhythm articles were largely ignored for two reasons. Shivering movements had less general appeal than other forms of movement, with breathing and locomotion being the most popular. Also, our three articles were published in the then relatively insignificant *American Journal of Physical Medicine*, as urged by Magoun, who at that time was trying to help the field of physical medicine advance its scientific visibility. I was too naive at that time to see the disadvantage of not submitting our work to a leading journal where publication would occur only following rigorous peer review. This would have improved our own thinking on the topic, and there would have been a far wider readership. Later, this point was drilled into my own pre- and postdoctoral trainees.

Extracellular Recording in the Hypothalamus: Using steel microelectrodes, I did a great deal of extracellular recording of the firing patterns of hypothalamic neurons in response to local changes in temperature (with one collaborator), visceral and somatic inputs from lower brain structures (two collaborators), and the relation between such firing patterns and direct current (DC) shifts in the hypothalamus and cerebral cortex (two other collabo-

rators). Among the collaborators, Ross Adey was most helpful, teaching me several nuances of extracellular microelectrode recording that greatly aided my subsequent work using intracellular recording in the spinal cord, which ironically was never undertaken by him. Fortunately, I was given the opportunity to summarize all of this work in a single unifying chapter (Stuart et al. 1963), which was written for an international symposium to which I was invited, as held in Villahermosa, Mexico, in 1963. Admittedly, I wrote this chapter with tongue in cheek because I knew that I would not stake my subsequent neuroscience career on the hypothalamus, even though other of my articles on this structure would have to be written following their completion. Rather, shortly after I began my postdoctoral research with Eldred, I became fully committed to research on the neural control of movement using both inside-out and outside-in approaches, as explained previously. Interestingly, however, our 1963 summary of my work on the hypothalamus was well received, probably because the neuronal patterns of its discharge were still largely unexplored.

An Impromptu 1963 Talk in San Francisco: By late 1963, I had given several fully prepared reports at national meetings of the American Physiological Society, and at three international symposia. However, the talk I remember with the most pleasure was a totally impromptu one-hour talk given at the Mt. Zion Hospital in San Francisco, where the audience included Ben Libet (1916–2007). Many consider Ben to have been not only a Nobel-standard cognitive neuroscientist but also a far better neurophysiologist for his work on the mammalian cervical ganglion than he was usually given credit for. We met several times subsequently, and when Ben was almost 90, he provided a chapter for one of the publications I arranged and edited.

Work on Spinal Cord Injury Patients: During and after WWII, three clinicians were well known for their attention to the clinical needs of allied servicemen and ex-serviceman with a spinal cord injury (SCI). Ludwig Gutmann (1899–1980) was a German-born neurosurgeon who became a British subject in 1939 when he began directing a SCI unit at Stoke Mandeville Hospital. Donald Munro (1889–1973) was an American neurosurgeon at the Boston City Hospital, and Ernest Bors (1900–1990) was a Czech urologist until 1938, when he became a U.S. citizen. Bors led a major SCI center first at the Birmingham General Army Hospital in Van Nuys, California (1945–1958), and then at the Long Beach VAH (1958–1970). I had the good fortune to interact with Bors on two aspects of his Long Beach SCI program. First, he selected the patients for a study I led on fluctuating spinal reflex patterns in SCI patients. Second, I spent many hours with him observing the unusual reflex patterns of many of our test subjects and his other SCI patients. This experience reinforced my intent, which was supported by Bors, to study spinal cord mechanisms in the cat using intracellular recording from spinal

motoneurons and, hopefully, later from spinal interneurons. I greatly admired Bors, who was one of the finest and most motivated clinicians I was to meet throughout my time as an experimental movement neuroscientist.

A Blow Up on the Alleged Measurement of Impedance in the CNS: It was well known at UCLA and indeed internationally that Ross Adey had an explosive temper. Even as a graduate student, when I was known to be his close friend, I was often asked to mediate with him about a faculty member or student who had been the victim of his wrath. Eventually I, too, became such a potential victim, but I chose to combat Adey rather than ask someone else to intercede on my behalf.

In mid-1963 Adey provided my laboratory with a recording device, which he claimed was appropriate for measuring impedance in the CNS, such that I and two other research assistant professors could compare this measurement to DC shifts in the hypothalamus and cerebral cortex in response to stimulation of the midbrain reticular formation. The three of us came to the conclusion that his method for the measurement of impedance was fallacious. This opinion was shared by some in Adey's UCLA group who spoke with us, including a very bright English physicist, who returned to England in disgust about this issue, and a similarly upset Australian neurophysiologist, who returned to his homeland where he had a long-standing and distinguished subsequent career.

We urged Adey to publish a retraction of his recent two publications using this instrument, which was an adaptation of one that had been used successfully and legitimately for the measurement of peripheral blood flow responses (Kolin and Kado 1959). Adey's response was to try to get me fired from the BRI and the Long Beach VAH and in his words drive me "out of neuroscience." His ire required the support of Jack French, the director at the main BRI component at UCLA and Bob Porter, a Jack French-trained neurosurgeon who was also the head of neurosurgery and director of the smaller Long Beach VAH component of the UCLA BRI. Porter was not prepared to get involved. Rather, he would have liked me to again become a colleague of Adey. French, too, was not prepared to get involved once I told him that my two colleagues and I had the theoretical support on the impedance issue of Don Jenden (1926–2013), a professor of pharmacology who many recognized as the leading quantitative neuroscientist at the UCLA School of Medicine and its BRI (Jenden 1996). French did not intercede, however, when Adey succeeded in having the BRI component of my salary abolished. This cut my overall salary by 10 percent, and it occurred just a few weeks before our fourth child was born, when Jean and I were very short of money. The cut was not for long—just the few minutes it took for me to go from the seventh floor BRI business office to the fourth floor of the adjacent medical school where my former PhD mentor had his office. After telling Allan Hemingway my tale of financial woe, he picked up his phone, called an administrator, and

told her to put me on one of his grants as an advisor, at a stipend level identical to what the BRI had taken from me. These hijinks became widely known throughout the BRI, the Long Beach VAH, and far beyond. Indeed, I received phone calls and had personal discussions with some leading neurophysiologists across the entire United States about this issue, these being neuroscientists who doubted the validity of Adey's work on impedance.

My two colleagues and I never published our impedance results, of course, but we did publish the work we had done on DC shifts in the hypothalamus and cerebral cortex because we were sure of the validity of our measurements. As for Adey, he never did publish a retraction. Rather, he published 10 further reports on alleged impedance up to 1971, but I do not know whether he made changes to his recording instrument or whether his subsequent measurements have been critically evaluated in the overall impedance literature. Certainly, he made his mark in other areas of neurobiology, including in particular the effects of electromagnetic fields on biological tissues. Adey had many other battles at UCLA, both scientific and political, but he retained the support of French and Porter for several more years. Eventually, however, his fight with a UCLA chancellor forced him in the late 1970s to leave UCLA and take a position at Loma Linda University and set up a laboratory at the Loma Linda VAH where he stayed until his demise. He appears to have undertaken sound work at Loma Linda on the role of power frequency of electromotive forces for the alleviation of some cancers, but he again became beset in controversy when he promulgated the possibility of potential cancer risks following exposure to cell phone radiation. Nonetheless, I still owe Adey a great debt of gratitude for his role in my PhD and early postdoctoral training.

The impedance brouhaha soured me on the BRI at UCLA, and I planned to leave there once an appropriate faculty position became available in the UC system, which did not occur until 1965.

Continued Contact with the Department of Physiology at UCLA: In 1963–1965, my title was “assistant professor of physiology in residence.” This was a non-tenure-track title, which at other state universities was equivalent to “assistant research professor.” I taught the full 12 one-hour lectures on respiration to the medical school and graduate student classes of 1963 and 1964. Victor Hall attended many of these lectures and gave me valuable critiques just as he had done when I was a PhD trainee. I also became friends with two new assistant professors of physiology. Together, we taught the new first-year dental school class of 24 members in 1964 and 1965.

UC-Davis (1965–1967)

Our transfer to UC-Davis had many pluses and minuses for our family life and largely pluses on the work front for both Jean and me.

The Home Front

We were “house poor” in Davis, because we found but one home large enough for our family, and it was quite expensive to purchase. Mitigating this problem was that the elementary school for our three older children had excellent teachers, and it was just around the corner from our home. At that time, our older son of seven was already committed to becoming accomplished at horseback riding. At Davis, he had a superb instructor who taught him how to make a horse jump over obstacles, a talent he progressively refined in his subsequent outdoor action career in movies and TV.

On Sunday mornings, it was a tradition for me to take our three older children to see the wide variety of animals that were housed on the campus, always ending at the veterinary school’s surgery recovery barn where one could see championship horses with their grooms, who slept close to their charges. There were several other social advantages. We frequently drove over to Napa Valley to taste wines at various vineyards. Our route was via Lake Berryessa, as attractive a car trip as can be imagined. Amusingly, our home became a bed and breakfast stop for many of our LA friends who found it convenient to stay overnight with us on skiing and summer trips to nearby Lake Tahoe. Most important, Jean began teaching again, and she greatly liked her young students whose parents were Mexican agricultural workers. These children’s kindergarten to sixth-grade school was in the town of Dixon, just a nine-mile car ride southwest of Davis, with most of the drive on a freeway. In 1966, Jean’s older sister and her husband and four children transferred from Columbus, Ohio, to San Francisco such as to enhance our family activities and possibilities. However, despite these pluses, by mid-1966 our departure from Davis was inevitable. The climate, with its damp Tule fogs in the winter and excessive agriculturally produced pollen counts in the spring, were too harsh for the respiratory system of our six-year-old daughter. Clinicians at UC-San Francisco were adamant that she would have to leave Davis as soon as was possible, and live in a dryer, less pollinated environment. Fortunately, this would be possible if I could secure a position in physiology at the University of Arizona (UA), where a new medical school was to open in the fall semester of 1967.

Hostility of the UC-Davis Chancellor

While at UC-Davis, I had an extremely heavy teaching load in the department of physiological sciences, which after my arrival was composed of nine faculty, three each in biochemistry, pharmacology, and physiology. Shortly after I arrived, it was announced that the size of the veterinary school class would be immediately doubled from 50 to 100 students. This came as a complete surprise and shock to the entire veterinary school faculty, except for the dean, who lost much face by his lack of honesty about this issue. For the first-year physiology course, it meant that we would have to increase

our number of four-hour laboratory exercise sessions in the spring semester from three to six per week—that is, five on Monday to Friday afternoons and a session on Saturday morning. My two new physiology colleagues were furious and asked me to cosign a letter to the dean with a copy to the chancellor in which we demanded that a salary be provided for an electronics specialist to maintain the electronic recording equipment used in these laboratory sessions. Naturally I complied and thereby gained the enmity of the chancellor, Emil Mrak (1901–1987), a short, rotund, somewhat unkempt-looking person, who was known affectionately to the faculty as “Fats.” He provided us with the requested salary but was annoyed by our aggressiveness. Before the beginning of the fall semester 1965, he held a social function one Saturday afternoon for the new faculty and their significant others. When Jean and I met him in his reception line he snarled and said, “Ah Stuart, you have just arrived and you are already complaining.” Jean was not impressed. She said rather coldly, “Dr. Mrak, my husband does not discuss business on the weekends.” He muttered under his breath and from then on referred to me as “that young son of a ### from UCLA.” However, I admired Mrak. He was respected throughout the overall UC system because of the proximity of UC-Davis to the state legislature in nearby Sacramento. This helped make him a valuable lobbyist for the UC system as a whole and indeed an advocate for state-supported higher education and research throughout all of California. Also, he was the driving force in the conversion of UC-Davis from a first-class agricultural and veterinary science campus to a similarly first-class all-round one like those at UCLA, UC-Berkeley, and UC-San Diego. He had begun his academic career at Berkeley as a food scientist and microbiologist and had developed an international reputation on the biology of yeasts.

I never again interacted personally with Mrak, but his antipathy to me was further enhanced a few months later, when the planned new dean of medicine decided not to leave the University of Washington. Mrak panicked, and without consulting his original search committee for this position, he offered it to a person who the committee had ranked as 37th on their original list of potential candidates. With encouragement from the dean of the veterinary school, I made enquiries about this person and foolishly relayed my concerns to one of my former UCLA professors, Robert Tschirgi, who had become the vice president for academic planning of the entire UC system. He shared my reservations and relayed them to the president of the UC system, Clark Kerr (1911–2003). Tschirgi’s and my concerns did not impress Kerr, so he phoned Mrak and told him that I should be “restrained” from attempting to play a role in the issue. The reader can imagine Mrak’s reaction: “That young son of a ### from UCLA had done it again!” From then on, I would hear from various Davis colleagues how Mrak had castigated me in various meetings. This was always told to me with great humor and laughter, and it did not worry me in the slightest because I was a tenured associate professor. The affair was a valuable lesson, however and I never again

came on too strongly or too quickly when confronting a difficult academic issue. My future strategy was to give much thought to how to accomplish a difficult goal in academic politics, rather than simply stir the pot of contention. Interestingly, I got on quite well with the new dean of medicine, who did a good job in getting his new school off to a sensible start. This, too, was a valuable lesson for me, as I will discuss.

Recruitment of Bob Reinking

The electronics expert hired to assist our teaching program was Bob Reinking (1941–), who was superbly trained and experienced for what we needed him to do in our department. As a bonus, Bob was young, imaginative, inventive, and hard working, as well as being naturally pleasant with a great sense of humor. My department decided he should work more or less exclusively with one of the pharmacologists and me, and within a few months this changed to virtually me alone.

One of Bob's and my joint laboratory exercise triumphs was to have the veterinary students chronically implant electrodes into cat brains for subsequent stimulation of the awake animal. Bob also played a major role in our development of an electronically sophisticated electrophysiological laboratory for spinal cord research. With the help of Professor Sabura Homma, when on a visit from Japan, Koichi Ishikawa, Bob Reinking, and I began to record intracellularly from mammalian spinal motoneurons (Homma et al. 1970).

Meeting Ted Goslow

Shortly after my beginning at UC-Davis, Ted Goslow came to my office to discuss measurements of muscle length-tension during dynamic movements. He was a PhD graduate student in zoology working on a most imaginative thesis project. It involved analyzing the lower limb and foot movements of six species of raptors (birds of prey) as they descended to strike a mouse on the ground. These movements were photographed in Ted's backyard against a scaled backdrop for subsequent kinematic measurements of the movements. I was amazed by the originality of the project. Later, I emphasized that we should collaborate when we both came to Arizona at the same time: he to Northern Arizona University (NAU) in Flagstaff, and me to the UA in Tucson. We had a very successful collaboration from 1970 to 1984, I, a late-trained neurophysiologist learning much from an exceptionally well-trained and imaginative comparative morphologist and vice versa (e.g., see Goslow et al. 1973).

Interactions with Leo Bustad

Within a few days of beginning work at UC-Davis, I met Leo Bustad, DVM, PhD (1920–1998), a former American prisoner of war in Germany during WWII,

and later the dean of veterinary medicine at Washington State University, where the veterinary school building is named in his honor.⁷ At UC-Davis, Leo was the director of a long-standing research program on the effects of radiation on beagle puppies. He attended virtually every lecture I gave at Davis to veterinary students, and he gave me advice on innumerable issues confronting the university as it developed its new academic programs. Leo was very shrewd politically, which was combined with a wonderful sense of humor and true *bon vivantism*. We became close friends, and I attribute much of my later subsequent success at the UA in dealing with contentious issues to his advice, which was always sound.

A Visit by John Eccles

As mentioned earlier, my quarrel with Ross Adey over the validity of recording impedance in the hypothalamus and cerebral cortex attracted much attention, both in the United States and abroad. The Australian Nobel Laureate John Eccles heard about it and wrote me a letter suggesting we should meet. I arranged for him to spend a week at UC-Davis in 1966, where he was to present two seminars and interact with faculty in the veterinary school. On a Saturday morning, Koichi Ishikawa and I met him at the San Francisco Airport after his flight down from Vancouver, where he had been visiting the University of British Columbia. With Koichi driving the car, Eccles and I sat in the back seat for the trip back to Davis. His first words were, "Now tell me about Adey." I summarized the controversy and he then gripped my arm and said, "Well done, your Australian toughness got you through that business." He then explained that years earlier in Australia, he had been the external examiner on Adey's PhD thesis committee, and he had refused to sign off on the thesis, about which he had scientific and perhaps ethical concerns. From that car conversation until our last meeting in Helsinki Finland in 1989 we remained close friends, and this accounts for my subsequent publications about him (Stuart and Pierce 2006; Stuart and Zigmond 2006; Stuart and Brownstone 2011; Callister et al. 2018; in preparation).

After closely examining my Davis laboratory, Eccles requested that we send him the details on how to acquire or have constructed our then unique (for its size) electromagnetic, servo-controlled muscle-stretching apparatus. He wanted it for one of his own new laboratory setups at the American Medical Association's short-lived Institute for Biomedical Research in Chicago (Stuart and Pierce 2006). He also emphasized *quite strongly* that I had insufficient training in the nuances of spinal cord neurobiology, particularly aspects introduced by Charles Sherrington (1857–1952) and refined subsequently by his trainees, including Eccles. Accordingly, he personally

⁷ Washington State University, "Dr. Bustad, Founder of PPP," <http://tinyurl.com/hmb35ho>.

recommended me to Anders Lundberg (1920–2009) in Göteborg, Sweden (see the section *Latter Day Mentors*). He also began describing to me his perception of the history and strategy of neurophysiological research and, in particular, the impact of Sherrington on the field. This continued until our last meeting in 1989.

Bioengineering Graduate Students and Veterinary Postdoctoral Trainees

At that time, the NIH wanted to attract more bioengineers into neuroscience, and I was successful in securing very attractive fellowships for four such people. In addition, two veterinary graduates from other schools came to my laboratory with postdoctoral fellowships arranged by the dean of the veterinary school. These were all first-class young people. If we had stayed at Davis, I would certainly have had an active research group. However, none of them transferred with me to the UA. They wanted to remain at UC-Davis rather than join what they thought would be a risky academic environment in a state not known for its financial support of the overall academic enterprise.

Intramural Service

I greatly enjoyed all of my interactions with faculty in my own department, and many faculty in other departments of not only the School of Veterinary Medicine but also in the long-established School of Agriculture and the newly developing School of Engineering, particularly its department of electrical engineering. Many of these associations came about as a result of my finding time for a type of university service that became a feature of my subsequent academic goals: the pioneering of new multidisciplinary ventures. For example, I served on the first committees at UC-Davis for the establishment of university-wide programs in physiology and biomedical engineering, and the incorporation of computers into campus-wide research. For these efforts and the success of several research article submissions, UC-Davis rewarded me with a double-step promotion to step III associate professor, barely two years after my arrival. Interestingly, this promotion could have been blocked by Fats Mrak, but he chose not to do so, either out of respect for his promotion and tenure committee or his pleasure that “the young son of a ### from UCLA” was about to leave his beloved university.

In summary about UC-Davis, our two years there were a maturing experience not only for me as an academician but also for Jean in her return to teaching, and our children adapting to a new environment, countering some irritating obstacles, and supporting our older daughter as she contended with her health problems there.

University of Arizona (1967–present)

This article is being submitted 50 years after I joined the faculty of the UA as a nontenured associate professor on August 15, 1967. Neither Jean (originally a Philadelphian), our children, nor I have ever regretted the decision to come to Tucson nor have we ever lost our enthusiasm for the physical beauty of Arizona, and Tucson's Spanish and Native American history and culture, which are strongly influenced by the city's proximity to Mexico and the relatively high percentage (~36 percent) of Hispanics and other Latinos who live here, the majority being Mexican Americans. Ironically, this enthusiasm coexists with our distaste for the abject failure of the state legislature and many of the governors over the years to support public education from kindergarten to the postdoctoral level. Indeed, Arizona currently ranks among the worst of our 50 states for its support of public education, a situation that seems bound to continue for many more years.

The Home Front

Jean went down to Tucson in the early summer of 1967 to find a home for us. She chose one on a street with friendly and enjoyable neighbors, many having children the same age as ours. This made for very neighborly living on the home front, which was greatly appreciated by all six of us. Jean's career and those of our four children have blossomed here, as has their enthusiasm for the city and its overall lifestyle, and the physical beauty and uniqueness of the entire state. Our older daughter's respiratory problems immediately abated to some extent and finally disappeared after she reached puberty, suggesting that her initial problem was more due to allergies than to a respiratory defect. Interestingly, I have little more to add here about the home front because all has gone well and most enjoyably, as described at the end of this autobiography. It can be summarized by stating that all of our family members have exploited the charms and advantages of Tucson in a manner that suited our individual needs and aspirations, while also having as maximum a set of interfamily interactions as was possible for six strong-minded individualists who nonetheless believe implacably in the concept of a united family.

In all, we had 16 years in our original home, the next 16 years in a nearby townhouse once all four of our children had left to pursue their own careers, and the next 18 years to the present in a small attractive home for just Jean and me on the far east side of Tucson with awe-inspiring views of the Rincon and Catalina mountain ranges.

My one regret is that I did not appreciate for many years the extent to which I overloaded Jean with requests that enhanced my university goals, particularly in home entertainment, much of which could have been undertaken in restaurants. This occurred not so much because I was self-obsessed

but rather because I truly believed she was an all-conquering person when it came to taking on so many functions and responsibilities.

Acquiring a UA Position and Early Colleagues in Physiology and Other Areas

In late 1966, I asked my Phoenix colleague and good friend, Eduardo Eideleberg (1930–2003), to submit my CV and his recommendation to the dean of the new medical college at the UA that I be considered for a position in the department of physiology when it was appropriate to do so. A few months later, I received a letter from the new head of physiology, Paul Johnson (1928–), suggesting that I fly down to Tucson for a weekend chat about a potential position in his new department. That initial meeting in early 1967 was very pleasant and thoughtful, and all of our subsequent interactions have remained so up to the present, with Paul now retired in California but still visiting Tucson and still interested in the progress of his former department and its history.⁸ Similarly, interactions were and remain optimal with the other new faculty members of the physiology department who included, in the chronological order of their arrival, Bob Gore (1939–), appointed in December 1967 on one of Paul Johnson's grants as a research associate; Bill Dantzler (1935–), Paul Johnson's first actual pick, who arrived as an associate professor in February 1968; and George Hedge and Raffi Gruener (both 1939–), who were appointed as tenure-track assistant professors in August 1968. All four of these colleagues are now emeritus professors after very successful careers as experimental physiologists. They also excelled or continue to excel in other endeavors—for example, Gore scaling the Mt. McKinley Denali peak in Alaska when he first became emeritus; Dantzler as head of physiology at the UA (1991–2005) and president of the American Physiological Society (1993–1994); Hedge as chair of physiology at the University of West Virginia (1977–1998) and vice president for research at Washington State University (1998–2002); and Gruener, not only an emeritus professor of physiology at the UA but also a research professor in our College of Agriculture and Life Science, where he is undertaking research on crop production under hydroponic conditions as supported by his own U.S. Department of Agriculture grant.

In the beginning years, I also had very close, pleasant, and stimulating contact with Jay Angevine (1928–2011), a neuroanatomist from Harvard University, and Bill Sibley (1925–2015), a neurologist from Case Western Reserve University. For several years, we three taught an interdisciplinary neuroscience course to first-year medical students and some PhD trainees in relevant disciplines.

⁸ American Physiological Society, "Paul C. Johnson," <http://www.the-aps.org/mm/Membership/Living-History/Johnson>.

Academic Responsibilities

When I joined the UA Department of Physiology as an associate professor, I clearly understood the responsibilities of an academician at a research university, as based on my prior experience at UCLA and UC-Davis. I knew that my job would be to contribute enthusiastically to teaching; research, including research training; and service, the latter having both extramural and intramural components. As I will emphasize, our research universities would be in much better shape today if a far higher percentage of their faculty took on all five of these commitments with enthusiasm.

Teaching

Getting Started: Bob Reinking came with me to the UA, where he was paid on my primary NIH grant until June 30, 2000. This meant giving up the security of his state-funded position at UC-Davis, which he claimed recently he has never regretted. Together, we spent most of the fall semester of 1967 preparing laboratory exercises for the physiology course we would give to a class of 32 medical students in the spring semester of 1968. These exercises included the electrical stimulation of a cat brain, which we had undertaken previously for veterinary students at UC-Davis; design and use of a muscle-stimulating device, which we improved progressively in subsequent years (Reinking et al. 1971); and, to the best of our knowledge, the first intracellular recording by medical students, using the abdominal ganglion of *Aplysia Californica* as the experimental preparation. For the latter exercise, which required eight recording setups for four students per setup, we received much help from José Segundo (1922–) at UCLA and Felix Strumwasser (1934–2007), who was then at the California Institute of Technology. We repeated the latter exercise in the spring semester of 1969, but when the class grew to 64 students in 1970, we had to discard it for logistical reasons. Similarly, we had to discard the cat brain stimulation experiment after but one year because the cats' postoperative maintenance was too burdensome and worrisome for our veterinary staff, an understandable problem, which we had not had at UC-Davis in the veterinary school.

Subsequent Teaching Contributions: From 1968 onward, I taught virtually all aspects of cellular and systems neuroscience to a wide variety of constituencies (e.g., upper and lower division undergraduates; graduate students in engineering and the life sciences; professional students in medicine, occupational, and physical therapy; clinical residents in neurology, neurosurgery, and orthopedics; teachers of special education; and members of the lay public). In addition to this substantial teaching at the UA, I remain the only Regents' Professor in the State University System who has had voluntary regular teaching commitments statewide. I was a yearly instructor for 10 hours of lectures at NAU for 16 years (1991–2000, biology

and exercise-science students; 2001–2007, physical therapy students) and I also began a 16-hour graduate course at Arizona State University (ASU) in 1993–1998 for largely bioengineering students and also some in biology and exercise science. I finally turned this over to the bioengineering faculty at ASU with the suggestion that they make it a full-length, one-semester course. This occurred and the course still prospers.

Research and Research Mentoring

Technical Help from Ed Perl and Motoy Kuno 1967: Shortly after arriving in Tucson and still conscious of Eccles' 1966 criticisms of aspects of Bob Reinking's and my recording setup at UC-Davis, Bob and I took advantage of our relatively short flying time between Tucson and Salt Lake City to visit Ed Perl (1926–2014) and Motoy Kuno (1928–2009) for a few hours in the department of physiology at the University of Utah. Ed had graciously complied with my request that we have an afternoon visit with them to review their setup for experimental electrophysiological spinal cord research, which was internationally renowned. During our visit, Ed and Motoy were most helpful, and we took back to Tucson several ways to improve our own recording setup. This proved to be most advantageous for our subsequent intracellular research with Bill Willis in Tucson and Anders Lundberg and Elzbieta Jankowska in Göteborg.

A Valuable Collaboration with Bill Willis 1970: Bill Willis (1934–2015) was one of the most intelligent all-round biologists it was my privilege to know and have as a good friend from 1964 until bad health overtook him a few years before his death. He completed his MD in 1960 at the University of Texas Southwestern Medical School and earned his PhD in physiology in 1963 under Eccles at the Australian National University. While there, Bill became a fully trained and skillful spinal neurophysiologist in the Eccles' tradition. He then spent a few months in Italy as a postdoctoral research fellow under Professor Giuseppe Moruzzi (1910–1986) at the University of Pisa before returning to his Dallas medical school as an assistant professor of anatomy in 1963. One year later, Bill became a full professor and chairman of this department. This was the year I met and began a series of conversations with him, which culminated in Bill spending the summer of 1970 in my Tucson laboratory when he was between positions in Dallas and Galveston, where his career continued to flourish. While with us, Bill showed us how to undertake an intracellular recording spinal cord study in the Eccles' tradition. Eccles heard me summarize this work in 1971 at an international meeting in Munich. Bill was most impressed with our recording setup for spinal cord recording, thereby giving me confidence that we were now technically "competitive" for the work I would next undertake with Lundberg and Jankowska in Göteborg.

Latter Day Mentors: Anders Lundberg and Elzbieta Jankowska (1971–1972). In 1971–1972, I, as a full professor, had the opportunity to work for seven months at the University of Göteborg with Anders Lundberg and his main collaborator Elzbieta Jankowska (1930–). At that time, they were clearly the world's leading workers in the field of spinal cord neurobiology, and I profited greatly from our coexecuted research (Jankowska et al. 1974). As predicted by Eccles in 1966, I was also able to return from Göteborg to the UA with a far-more-complete armamentarium of spinal cord techniques, as taught to me largely by Jankowska for surgical techniques and Erling Eide (1934–) for instrumentation and data processing. Following this experience, my UA laboratory changed little in structural arrangement, albeit our data processing continued to improve.

Just as important was Lundberg's generous allotment of time (three to four hours every Saturday morning for six months) to review the complete history of motor control science, and its prospects and possibilities. In 2008, I paid tribute to Lundberg's stellar career in a review article written in collaboration with one of Lundberg's former PhD trainees, Hans Hultborn (1943–; University of Copenhagen).

Interactions with the London group of Tony Taylor: It was purely by a chance meeting in Paris in 1971 that I came in contact with Tony Taylor (1928–) and his recent PhD graduate John Stephens (1946–) from the Sherrington School of Physiology at the St. Thomas's Hospital Medical School in central London.⁹

Subsequently, they both undertook research in Tucson where their lively, friendly personalities were much appreciated in both my laboratory and the entire department of physiology. John came first for two very productive years of research in 1972–1974 on the properties and interactions between mammalian motor units and Golgi tendon organs (e.g., Reinking et al. 1975). Tony came next for six months in 1975. It was with him; my recent PhD graduate Edward Stauffer; Doug Watt, a postdoctoral trainee from McGill University; and the ever-valuable Bob Reinking that I put into good effect all that I had learned in Göteborg about intracellular recording. Our work, using spike-triggered averaging to record very small motoneuronal excitatory postsynaptic potential and inhibitory postsynaptic potential responses to the input from single spindle Ia and group II afferents and Golgi Ib afferents, created much initial discussion as to its validity (e.g., see Watt et al. 1976). Within a few years, however, our results were all confirmed in other laboratories, and even from the beginning, we had strong supporters among the motor control community. If one is to do all-night experiments of extreme technical difficulty it is well to have urbane, good-humored coworkers like these.

⁹ For more details on this meeting, see American Physiological Society, "Douglas G. Stuart," <http://www.the-aps.org/mm/Membership/Living-History/Douglas-Stuart>.

Another International Research Experience (1977): A few years later (1977), when supported by a Guggenheim Fellowship, I undertook six months of microneurographic neurophysiological research with David Burke (1944–) in the Division of Neurology, University of New South Wales (UNSW), Sydney, Australia. This division was headed at that time by James Lance (1926–), with whom I had once shared an interest in the genesis of various normal and abnormal tremors in humans. I had met Jim and his research group in late 1970 on a quick two-day trip to Australia for a family issue. My brief visit included a four-hour interaction with the Lance group at the Prince Henry Hospital just north of Botany Bay in Sydney. Several months later, I had extensive discussions with David Burke in Brussels and Göteborg, which have continued until the present.

With Burke and Lance in 1977, I honed my experience in the optimization of interactions between fundamental and applied (clinical) neuroscientists. Burke was later head and professor of neurology, UNSW, followed by professor of neurology and dean of research, faculty of medicine, University of Sydney. When my family and I arrived in Sydney in December 1971, he had just returned from an intensive two-year experience with Karl-Erik Hagbarth (1926–2005) at the University of Uppsala. I helped Burke restart his Hagbarth-improved microneurography research (Burke et al. 1979) in his new Australian laboratory, which subsequently became world renowned.

Recruitment of Pat Pierce (1980): I was most fortunate in having the opportunity in 1980 to hire Pat Pierce as a research assistant and technical editor. At that time, she was a substitute science teacher in the secondary schools, and she asked whether she could bring her skills to my research group. I complied immediately, the result being her 26 years of first-class work in both areas. Pat's first-ever learning to become an excellent experimental surgeon, preparing surgically reduced anesthetized *in vivo* cats and later turtles, required much disciplined time and effort on her part. In parallel, however, her technical editing was first class from the start, and greatly appreciated by my coauthors and me. Our laboratory's experimental work actually ended in 1998, after which her focus was solely on technical editing. The NIH grant that supported Pat's salary ended in mid-2002, after which I paid her a meager salary from my private funds until all of our experimental work had been submitted for publication, which occurred in August 2006. Clearly, I owe a great deal to Pat for her dedicated and talented effort, just as I do to Bob Reinking.

My Lineage in the Field of Neuroscience

Table 1 summarizes my lineage in neuroscience, including those who trained me, whom I then trained, with whom I collaborated for refereed articles, and symposium volumes many of which I helped referee, as well

as the best-known positions these various collaborators held. For the latter, the majority continued in research. Some of this group of more than 150 individuals held administrative titles, which are largely not provided in the table, where my focus is on their research. (Some comments on administrative positions are provided in the following sections.) With but three exceptions for local UA colleagues, the collaborations are limited to people with whom I had one or more published interactions, that is, either an article or review, or a publication involving me as an editor or coeditor and reviewing a person's submission before a symposium volume was published or vice versa with me being the article submitter. Some other people who greatly influenced my understanding of some aspect of neuroscience are discussed in the following sections.

In preparing this table, I was struck by how much I enjoyed collaborative research and other academic interactive activities, how much I learned from my collaborators, and among those who are living, how much I know about their current activities, and how often we communicate with each other.

Four other people greatly influenced my development in the field of neuroscience, in general, and movement neuroscience, in particular. The following vignettes on each of these individuals are provided in the chronological order of our first interaction.

Pablo Rudomin (1934–): I first met Pablo in 1963 at an international symposium held in Villahermosa, Tabasco, Mexico. At that meeting, Pablo presented a sophisticated talk on a novel way to study baroreceptor function. He received excellent PhD mentoring in Mexico City from the renowned cyberneticist, Arturo Rosenblueth (1900–1970; Monnier 1975). Pablo is still delving deeper and deeper into the phenomenon of spinal presynaptic inhibition, which he has explored for more than 50 years, using advanced intracellular recording techniques and in recent years taking advantage of computer and mathematical simulations of its neuronal basis.

Don Maynard (1929–1973): In the mid-1960s, I had several detailed conversations with Don, a former UCLA PhD trainee of Ted Bullock, who had very advanced and sophisticated views on what invertebrate neurobiology could and could not contribute to overall neuroscience. My nascent interest in invertebrate neurobiology was greatly widened and deepened by these spontaneous “tutorials” provided by Don. It was a great tragedy that he died at such a young age.

V. Reggie Edgerton (1940–): Reggie and I met at a meeting in Birmingham, England, in mid-1971 and had the first of a detailed set of still ongoing interactions at UCLA in the summer of 1972. He has worked for almost 50 years on the adaptive properties of muscle and the spinal motor system, going from strength to strength on strategies to increase the mobility and

Table 1 Academic lineage of Douglas G. Stuart (1931–), including mentors, trainees, visiting scientists, and other collaborators. *Column 1:* Life Spans unavailable for 2/151 entries. *Column 2:* Origins are for country of birth or in which pregraduate school training was undertaken. *Column 3:* Contact year(s) for duration of training or collaboration. Citations provided in footnotes when collaborative articles not readily available on the Internet. *Column 4:* University positions presented do not usually include administrative titles. Nor do they include university subdivisions unless they are required for further clarification of the address. For country (three-letter) and U.S. state (two-letter) codes, see http://kirste.userpage.fu-berlin.de/diverse/doc/ISO_3166-1 & 2.html. Minimal addresses are provided, there being none for well-known institutions in the United States and abroad.

Scientist (Life Span)	Origin	Contact Year(s)	Best-Known or Current Position
(A) Mentors			
Allan Hemingway (1902–1972)	CAN	1957–64	Professor of Physiology, UCLA
Walter J. Freeman (1927–2016)	USA	1957–60	Professor of Physiology, UC-Berkeley
Earl Eldred (1919–)	USA	1961–66	Professor of Anatomy and BRI member, UCLA
(B) Latter-day mentors and collaborators			
Anders Lundberg (1920–2009)	SWE	1971–72 ¹	Professor of Physiology, University of Göteborg
Elzbieta Jankowska (1930–)	POL	1971–72 ¹	Professor of Physiology, University of Göteborg
(C) High school student and subsequent collaborator			
Kenneth Ott (1944–)	USA	1962–66 ²	Neurosurgeon with Neurosurgical Medical Clinic, Inc., San Diego
(D) University of Arizona undergraduates			
Thomas P. Withey (1954–)	USA	1971–73 ³	Computer Music Engineer, Department of Music, UCLA ⁴
Richard P. Donnelly (1965–)	USA	1992–93	Nurse Practitioner, University of Arizona Medical Center, Tucson
(E) MS trainee and an advanced Australian medical student			
Rebecca L. Gerlach (1942–)	USA	1967–74 ⁵	Radiology Administrative Assistant, University of Washington
James G. Colebatch (1955–)	AUS	1977 ⁶	Professor (Neurology, Conjoint ⁷), University of New South Wales

(continued)

Table 1 (Continued)

Scientist (Life Span)	Origin	Contact Year(s)	Best-Known or Current Position
<i>(F) Veterinary, medical and PhD students</i>			
Kenneth B. Campbell (1940–)	USA	1965–67 ⁸	Professor of Physiology and Biophysics, Washington State University
William C. Nemeth (1945–)	USA	1969–75 ⁹	Private-sector Orthopedic Surgeon, San Marcos, TX ¹⁰
William E. Cameron (1950–)	USA	1975–79	Assoc. Prof. Behavioral Neurosci., Oregon Health and Sciences Univ., Portland
Dennis D. Roscoe (1946–)	USA	1977–80	Founder and Co-CEO, MPACS-LLC, ¹¹ Madison, WI
Debra A. Gordon (1959–)	USA	1982–88	Vice Pres., Chief Legal Counsel, HTG Molecular Diagnostics, ¹² Tucson
Michael J. Joyner (1958–)	USA	1983–87 ¹³	Professor of Anesthesiol. and Physiol., Mayo Med. Sch., Rochester, MN
Lucinda L. Rankin (1951–)	USA	1983–Pr	Assoc. Director of Physiology Undergraduate Program, ¹⁴ University of Arizona
John M. Spielmann (1959–)	USA	1986–91 ¹⁵	Staff Anesthesiologist, White Oak Anesthesiology Associates, Minneapolis
Leslie Bevan (1959–)	USA	1988–91	Assoc. Dean, Sch. Nursing, Oregon Health and Science University, Portland
T. George Hornby (1970–)	USA	1994–98	Associate Professor of Physical Therapy, University of Illinois, Chicago ¹⁶
<i>(G) PhD trainees continuing for one year as postdoctoral research collaborators¹⁷</i>			
Edward K. Stauffer (1941–)	USA	1970–75 ¹⁸	Associate Professor of Physiology, University of Minnesota, Duluth
Jennifer C. McDonagh (1949–2016)	USA	1975–80 ¹⁹	Assoc. Prof. of Physical Therapy, Arizona School of Health Sciences, Phoenix
Sharyn Vanden Noven (1951–2005)	USA	1979–85	Assist. Prof. of Physical Therapy, McGill Univ. and Hong Kong Polytechnic Univ. ²⁰
Chun-Su Yuan (1951–)	CHN	1980–87	Professor of Anesthesia and Critical Care, ²¹ University of Chicago
<i>(H) Postdoctoral trainees and coauthors</i>			
Koichi Ishikawa (1932–)	JPN	1964–67	Associate Professor of Neurology, University of Southern California
John A. Stephens (1946–)	GBR	1972–74	Prof. Physiol., Middlesex Hospital Medical School and University College London
Douglas G.D. Watt (1944–)	CAN	1974–75	Professor of Physiology, McGill University ²²
Marc D. Binder (1949–)	USA	1974–82 ²³	Professor of Physiology and Biophysics, University of Washington
Barry R. Botterman (1947–)	USA	1977–80	Prof. Cell Biology, University of Texas Southwestern Medical Center, Dallas

Thomas M. Hamm (1949–)	USA	1979–Pr ²⁴	Professor of Neurobiology, Barrow Neurological Institute, Phoenix
Walther Kochler (1952–)	GER	1981–83	Neurophysiologist, Neurosurgical University Hospital, Bonn, GER ²⁵
Grant A. Robinson (1958–)	USA	1986–91 ²⁶	Associate Research Professor of Neurosurgery, Duke University
S. Jayne Garland (1958–)	CAN	1988–89	Professor of Physical Therapy, University of British Columbia ²⁷
Yiannakis C. Laouris (1958–)	CYP	1988–91 ²⁸	Director/Researcher, Cyprus Neuroscience and Technology Institute, Nicosia
Michael A. Nordstrom (1957–)	AUS	1989–91	Associate Professor of Physiology, University of Adelaide, AUS
Robert J. Callister (1954–)	AUS	1990–03 ²⁹	Professor of Biomedical Science, University of Newcastle, ³⁰ AUS
Edwin E. Gilliam (1959–)	USA	1994–97	Neurosci. Nurse Pract., Carondelet Neurol. Inst., St. Joseph's Hosp., Tucson
<i>(I) Trainees of colleagues</i>			
William M. Price (1932–)	USA	1960–64 ³²	Businessman, Los Angeles
Kiyoo Kamikawa (1929–N/A)	JPN	1962–63 ³³	Professor of Clinical Surgery, Meiji University of Oriental Medicine, JPN ³⁴
William O. Wild (1929–)	USA	1965 ³⁵	Private-Sector Neurosurgeon, Long Beach, CA
Jeffrey S. Kroin (1944–)	USA	1974 ³⁶	Professor of Anesthesiology, Rush University Medical Center, Chicago
Robert B. Gorman (1965–)	AUS	1993–95	Field Clinical Manager, Saluda Medical ³⁷ , Sydney, AUS
Douglas H. Laidlaw (1960–)	USA	1994 ³⁸	President of Medical Affairs, OPKO Health, ³⁹ Miami, FL
<i>(II) Electronics/computer engineer and a senior technician/technical editor</i>			
Robert M. Reinking (1941–)	USA	1965–00 ⁴⁰	Senior Research Engineer, University of Arizona
Patricia A. Pierce (1934–)	USA	1980–06	Senior Research Assistant, and Technical Editor, University of Arizona
Aaron Cohen (1915–1964)	USA	1958	Pulmonary Function Lab., San Fernando VAH, CA
W. Duane Collings (1914–1981)	USA	1958	Professor of Physiology, Michigan State University
Robert George (1923–2006)	USA	1960–61	Professor of Pharmacology and BRI member, UCLA
David S. Maxwell (1931–1991)	USA	1961–62	Professor of Anatomy and BRI member, UCLA
Robert W. Porter (1926–)	USA	1961–64	Professor of Neurosurgery and BRI member, UCLA
W. Ross Adey (1922–2004)	AUS	1961–64	Professor of Anatomy and BRI member, UCLA

(continued)

Table 1 (Continued)

Scientist (Life Span)	Origin	Contact Year(s)	Best-Known or Current Position
Lawrence H. Ott (1904–1978)	USA	1961–64	Research Physicist, Hughes Aircraft Company, Culver City, CA
Frank C. Cheshire (1921–)	USA	1961–64	Electronics engineer, Barrow Neurological Institute, Phoenix
M. David Fairchild (1923–2004)	USA	1963–65	Associate Professor of Molecular and Medical Pharmacology, UCLA
James N. Hayward (1928–2014)	USA	1963–65	Professor of Neurology, University of North Carolina, Chapel Hill
(K) Visiting faculty collaborator at UCLA			
Yojiro Kawamura (1921–2013)	JPN	1959–60	Professor of Physiology, University of Osaka Dental School
(L) Visiting faculty collaborator at UC–Davis			
Saburo Homma (1923–2014)	JPN	1966	Professor of Physiology, Chiba University
(M) Faculty collaborators at University of Arizona			
Shiroh Watanabe (1923–)	JPN	1968	Professor of Physiology, Kyoran University, Mitaka, JPN
Raphael Gruener (1939–)	ISR	1968–71 ⁴¹	Professor of Physiology, University of Arizona ⁴²
Robert E. Burke (1934–)	USA	1968–06 ⁴³	Chief Investigator, ⁴⁴ Laboratory of Motor Control, NINDS, Bethesda, MD
Carter G. Mosher (1935–)	USA	1969–72	Neurologist, Permanente Medical Group, Sacramento
George E. Goslow Jr. (1939–)	USA	1969–72	Professor of Biology, Northern Arizona University, ⁴⁵ Flagstaff
Paul C. Johnson (1926–)	USA	1969–87	Professor of Physiology, University of Arizona ⁴⁶
William H. Dantzler (1935–)	USA	1969–06	Professor of Physiology, University of Arizona ⁴⁶
William D. Willis (1934–2010)	USA	1970–06	Professor of Anatomy and Physiology, University of Texas, Galveston
Mary C. Wetzel (1935–)	USA	1971–76	Professor of Psychology, University of Arizona
Sten Grillner (1941–)	SWE	1971–Pr ⁴⁷	Professor of Physiology, Karolinska Institute, University of Stockholm
Anne E. Atwater (1937–)	USA	1972–74	Director, Physiology Undergraduate Program, University of Arizona
John V. Wait (1932–03)	USA	1972–74	Professor of Electrical Engineering, University of Arizona
Richard M. Herman (1928–)	USA	1972–77 ⁴⁸	Prof. Physical Med., Temple Univ., Chicago and Good Samaritan Hosp., Phoenix

Paul S.G. Stein (1943-)	USA	1972-Pr ⁴⁹	Professor of Biology, Washington University in St. Louis
Anthony Taylor (1928-)	GBR	1974-75 ⁵⁰	Professor of Physiology, STH Medical School, University of London
George P. Moore (1932-)	USA	1974-80	Professor of Biomedical Engineering, University of Southern California
Judith L. Smith (1942-)	USA	1976 ⁵¹	Professor of Physiological Science, UCLA
Eberhard E. Fetz (1940-)	USA	1979 ⁵²	Professor of Physiology and Biophysics, University of Washington
Elwood Henneman (1915-1996)	USA	1979 ⁵²	Professor of Physiology, Harvard University
Richard B. Stein (1940-)	USA	1979 ⁵²	Professor of Physiology and Neuroscience, University of Alberta
Lorne M. Mendell (1941-)	CAN	1979 ⁵³	Distinguished Professor of Neurobiology and Behavior, SUNY-Stony Brook
Uner Tan (1937-)	TUR	1980	Senior Scientist, Cukurova University, Adana, TUR ⁵⁴
Ziaul Hasan (1945-)	IND	1980-92 ⁵⁵	Professor of Physiology, University of Arizona ⁵⁶
Roger M. Enoka (1949-)	NZL	1981-93 ⁵⁷	Professor of Integrative Physiology, University of Colorado
James C. Houk (1939-)	USA	1982	Professor of Physiology, Northwestern University, Chicago Campus
T. Richard Nichols (1947-)	USA	1982	Prof. of Physiol. and Biomed. Engr., ⁵⁸ Georgia Institute of Technology, Atlanta
William Z. Rymer (1939-)	AUS	1982	Senior Investigator in Rehabilitation Research, Rehabilitation Inst. of Chicago ⁵⁹
Thomas J. Hixon (1940-2009)	USA	1982-10 ⁶⁰	Professor of Speech, Language, and Hearing Sciences, University of Arizona
Jeannette D. Hoit (1954-)	USA	1982-Pr ⁶¹	Professor of Speech, Language, and Hearing Sciences, University of Arizona
Robert W. Lansing (1926-)	USA	1982-Pr ⁶²	Professor of Psychology, University of Arizona
Uwe Windhorst (1946-)	GER	1983 ⁶³	Adjunct Professor of Physiology, University of Goettingen ⁶⁴
Richard A. Satterlie (1950-)	USA	1983-04	Professor of Zoology, Arizona State University ⁶⁵
Jack L. Lewis (1940-)	USA	1984	Prof. Mechanical Engineering and Orthopedic Surgery, University of Minnesota
Frances J. Richmond (1951-)	CAN	1984	Professor of Clinical Pharmacy, University of Southern California
Patti M. Nemeth (1946-)	USA	1984-86 ⁶⁶	Assoc. Res. Prof. Neurol./Anat. and Neurobiol., Washington Univ. in St. Louis
Sei-ichi Sasaki (1951-)	JPN	1984-86	Prof. Physiology, Ibaraki Prefectural University of Health Sciences, Ami, JPN
A. Terry Bahill (1946-)	USA	1984-89 ⁶⁷	Professor of Systems and Biomedical Engineering, University of Arizona

(continued)

Table 1 (Continued)

Scientist (Life Span)	Origin	Contact Year(s)	Best-Known or Current Position
James R. Bloedel (1940–)	USA	1984–00 ⁶⁸	Senior Staff Scientist in Neurobiol., Barrow Neurological Institute, Phoenix
Alan R. Gibson (1943–)	USA	1984–03 ⁷⁰	Professor of Neurobiology, Barrow Neurological Institute, Phoenix ⁶⁹
John G. Hildebrand (1942–)	USA	1985–Pr ⁷¹	Regents' Professor of Neuroscience, University of Arizona ⁷²
Hans Forsberg (1949–)	SWE	1986–87 ⁷³	Professor of Neuroscience, Karolinska Institute, University of Stockholm ⁷⁴
Andon R. Kossev (1950–)	BUL	1986–87	Professor of Biophysics, Bulgarian Academy of Sciences, Sophia
Edward A. Arbas (1951–95)	USA	1986–95 ⁷⁵	Associate Professor of Neuroscience, University of Arizona
Alexander Gydikov (1929–1989)	BUL	1987	Professor of Biophysics, Bulgarian Academy of Sciences, Sophia
Oliver H. Lowry (1910–1996)	USA	1987–88	Professor of Pharmacology, Washington University in St. Louis
Natalia A. Trayanova (1956–)	BUL	1987–88	Professor of Biomedical Engineering, Johns Hopkins University
Konstantin G. Kostov (1946–)	BUL	1987–88	Bioengineer in Biophysics Unit, Bulgarian Academy of Sciences, Sophia
Andrew B. Schwartz (1956–)	USA	1987–88 ⁷⁵	Distinguished Professor of Neurobiology, University of Pittsburgh
Vlastilav Bracha (1957–)	CZE	1987–00 ⁷⁵	Professor of Biomedical Science, Iowa State University
Bud Craig (1951–)	USA	1987–03 ⁷⁵	Senior Neurobiologist, Barrow Neurological Institute, Phoenix
Valerie A. Cwik (1955–)	USA	1987–03 ⁷⁵	Medical Director and Vice President for Research, MDA, Tucson
Jiping He (1955–)	CHN	1987–03 ⁷⁵	Prof. Bioengineering and Electrical Engineering, Arizona State University
Marco Santello (1967–)	USA	1987–03 ⁷⁵	Professor of Biological and Health Systems Engr., Arizona State University
Paul A. St. John (1950–)	USA	1987–03 ⁷⁵	Associate Professor of Cellular and Molecular Medicine, University of Arizona
Mark A. Willis (1955–)	USA	1987–03 ⁷⁵	Professor of Biology, Case Western Reserve University
Eric T. Vu (1963–)	USA	1987–03 ⁷⁶	Co-Director, ASU-BNI Interdisciplinary Graduate Program in Neuroscience
George E. Stelmach (1939–)	USA	1987–10 ⁷⁷	Professor of Motor Control, Arizona State University
Nicholas J. Strausfeld (1942–)	GBR	1987–10 ⁷⁷	Regents' Professor of Neuroscience, University of Arizona
Richard B. Levine (1951–)	USA	1987–15 ⁷⁷	Professor of Neuroscience and Physiology (Joint), University of Arizona

Ralph F. Fregosi (1954–)	USA	1987–Pr ⁷⁷	Professor of Physiology and Neuroscience (Joint), University of Arizona
Andrew J. Fuglevand (1958–)	USA	1987–Pr ⁷⁷	Professor of Physiology and Neuroscience (Joint), University of Arizona
Nicolina Radicheva (1937–2014)	BUL	1988–89	Professor of Biophysics, Bulgarian Academy of Sciences, Sophia
Lubomir V. Gerilovsky (1936–)	BUL	1988–89	Professor of Physiology, Bulgarian Academy of Sciences, Sophia
Gail F. Koshland (1951–)	USA	1988–10 ⁷⁷	Associate Professor of Physiology, University of Arizona
Leslie P. Tolbert (1951–)	USA	1988–Pr ⁷⁸	Regents' Professor of Neuroscience, University of Arizona
Kazuaki Sugiura (N/A)	JPN	1989	Private-sector Neurosurgeon, Tokyo
Gregory M. Karst (1954–)	USA	1989–90	Professor of Physical Therapy, University of Nebraska Medical Center, Omaha
Masayuki Yamashita (1957–)	JPN	1992 ⁷⁹	Professor of Physiology, Nara Medical University, Kashihara, JPN ⁸⁰
Michael A. Cusanovich (1942–2010)	USA	1991–10 ⁸¹	Regents' Professor of Biochemistry, University of Arizona
Mont Hubbard (1943–)	USA	1992	Professor of Mechanical and Aerospace Engineering, UC–Davis
Arthur Prochazka (1946–)	CZE	1992	Professor of Physiology and Neuroscience, University of Alberta
Simon C. Gandevia (1953–)	AUS	1994–95 ⁸²	Prof., Neuroscience Research Australia, Sydney and Univ. New South Wales
Alan J. McComas (1934–)	AUS	1994–95	Professor of Medicine (Neurology), McMaster University
Christine K. Thomas (1957–)	NZL	1994–95	Res. Prof., Miami Project to Cure Paralysis and Neurosurgery, Univ. of Miami
Gantcho N. Gantchev (1929–)	BUL	1996	Professor of Physiology, Bulgarian Academy of Sciences, Sophia
Victor S. Gurfinkel (1923–)	RUS	1996	Professor, Institute for Information Transmission Problems, RAS, Moscow ⁸³
Mario Wiesendanger (1931–2017)	CHE	1996 ⁸⁴	Professor of Physiology, University of Fribourg
Shigemori Mori (1936–)	JPN	1996 ⁸⁴	Professor of Biological Control System, NIPS, Okazaki, JPN
Julie M. Barkmeier-Kraemer (1962–)	USA	1997–03 ⁸⁵	Professor of Otolaryngology, University of Utah, Salt Lake City
Alan Brichta (1958–)	GBR	1999	Professor of Anatomy and Physiology, University of Newcastle, AUS
Uwe Proske (1939–)	GER	2001–02	Professor of Physiology, Monash University
E. Fiona Bailey (1961–)	AUS	2007–Pr ⁸⁶	Associate Professor of Physiology, University of Arizona
Masao Ito (1928–)	JPN	2010–11 ⁸⁷	Professor of Physiology, University of Tokyo ⁸⁸

(continued)

Table 1 (Continued)

Scientist (Life Span)	Origin	Contact Year(s)	Best-Known or Current Position
<i>(N) Sabbatical leave host</i>			
David Burke (1944–)	AUS	1977	Prof. of Neurology, University of New South Wales then University of Sydney
<i>(O) Collaborators for historical research published while DGS emeritus at the University of Arizona⁸⁹</i>			
Michael J. Zigmond (1941–)	USA	2004–06 ⁹⁰	Professor of Neuroscience, University of Pittsburgh
Robert M. Brownstone (1960–)	CAN	2004–Pr ⁹¹	Professor of Neurosurgery, University College London
Per Andersen (1930–)	NOR	2004–06	Professor of Physiology, Inst. Basic Biomed. Sciences, University of Oslo
Benjamin Libet (1916–2007)	USA	2004–06	Professor of Physiology, UC–San Francisco
Jonathan S. Carp (1953–)	USA	2004–06	Sr. Scientist, Natl. Ctr. for Adapt. Technol., Wadsworth Ctr., NYS Dept. Health ⁹²
Jonathan R Wolpaw (1944–)	USA	2004–06	Director, Natl. Ctr. for Adaptive Technology, Wadsworth Ctr., NYS Dept. Health ⁹³
Hans Hultborn (1943–)	SWE	2004–08	Professor of Physiology, University of Copenhagen
François Clarac (1938–)	FRA	2008–11 ⁹⁴	Researcher, CNRS Laboratory of Neurobiol. and Movement, Marseille FRA
Jean Massion (1930–)	BEL	2008–09 ⁹⁵	Researcher, CNRS Inst. of Neurophysiol./Psychophysiology, Marseille FRA
J. Gareth Jones (1936–)	GBR	2009–10	Professor of Anaesthesia, University of Cambridge
E. M. (Tilli) Tansey (1953–)	GBR	2009–10	Professor of Modern Medical Sciences, University of London
Jean-Gaël Barbara (1968–)	FRA	2010–11	CNRS Research Fellow, Université Pierre et Marie Curie, Paris
Jacques Duchateau (1954–)	BEL	2010–Pr ⁹⁶	Professor of Neurophysiology, Free University of Brussels ⁹⁷
Andreas T. Schaefer (1976–)	GER	2013–Pr ⁹⁸	Professor of Neuroscience, University College London
Brett A. Graham (1975–)	AUS	2013–Pr ⁹⁸	Associate Professor of Biomedical Science, University of Newcastle, AUS

Summary: DGS was himself trained, and then trained and collaborated with 155 people born in 20 countries, including Australia (11), Belgium (2), Bulgaria (7), Canada (6), Switzerland (1), Republic of China (2), Cyprus (1), Czech Republic (2), France (2), Great Britain (6), Germany (4), India (1) Israel (1), Japan (10), New Zealand (2), Norway (1) Poland (1), Russia (1), Sweden (4), Turkey (1), and the United States (89).

Abbreviations: ASU-BNI, Arizona State University-Barrow Neurological Institute; BRI, UCLA Brain Research Institute; CNRS, French abbreviation for National Center for Scientific Research; MDA, Muscular Dystrophy Association; N/A, Not available; NINDS, National Institute of Neurological Disorders and Stroke;

NHNN, National Hospital of Neurology and Neurosurgery; NIPS, National Institute of Physiological Sciences; NYS, New York State; Pr, present; RAS, Russian Academy of Science; S/A, See above; STH, St. Thomas's Hospital; SUNY, State University of New York; VAH, Veterans' Administration Hospital.

Footnotes (with citations appearing in the reference section of the text): ¹Also in 1983. ²Continued training and research collaboration throughout undergraduate and medical school years. ³Research assistant in 1967–71. ⁴Undergraduate major in electrical engineering and minor in music. ⁵Spent 32 years at UCLA and the Los Angeles film and recording industry advancing electronic music, computer-aided music instruction, psychoacoustics research, film scoring, and audio archive preservation and restoration. ⁶This mentoring was unofficial while on a sabbatical leave at the University of New South Wales, when Colebatch was a 2nd year medical student undertaking an undergraduate honors degree in physiology. His official Australian mentor, Dr. J. David Gillies (1940–), a Senior Lecturer in Medicine, was on medical leave for the first half of 1977. ⁷Conjoint[†] means salary not paid by the university. ⁸While a first- and second-year veterinary medical student at UC-Davis. ⁹MD awarded in 1973. ¹⁰Currently medical director of an interdisciplinary pain control practice in Austin TX. ¹¹A private-sector medical imaging company. In 2012 created and directs RoscoeSkies Observatory and teaches astrophotography and astronomy at the University of Wisconsin in Waukesha. ¹²A private high-tech translational medicine company including molecular diagnostics. ¹³While a medical student. ¹⁴Also Director of Undergraduate Honors Program (2005–). ¹⁵Completed PhD in 1991 then spent 2 years as postdoctoral research assistant of Bill Cameron at the University of Pittsburgh before beginning medical school there. ¹⁶Currently Professor of Physical Medicine and Rehabilitation, University of Indiana, Indianapolis. ¹⁷After PhD spent one further year w/ DGS as a postdoctoral research collaborator. ¹⁸A research collaborator in 2004–07. ¹⁹A research collaborator in 1996–2006. ²⁰Later appointment required declining promotion to Associate Professor at McGill University. ²¹Also Director, Tang Center for Herbal Medicine Research, University of Chicago. ²²Also undertook research in the Aerospace Medical Research Unit at McGill University. ²³Generated his raw PhD data in DCS lab in 1974 while a predoctoral trainee with George Moore at the University of Southern California. A postdoctoral trainee with DGS in 1975–78. See also Binder and Mendell (1990). ²⁴Postdoctoral trainee in 1979–82. ²⁵Previously, a private-sector neurologist in Bonn for 16 years. ²⁶Postdoctoral trainee in 1986–89. ²⁷Currently Professor of Physical Therapy, University of Western Ontario. ²⁸Also 2004 and 2006. ²⁹Further collaboration in 1999–05 and 2013–Pr. ³⁰Currently responsible for research and research training as Deputy Head of the Faculty of Health and Medicine, University of Newcastle, AUS. ³¹Subsequent appointment as Neurology Nurse Practitioner with Western Neurosurgery, Tucson. ³²PhD trainee of Allan Hemingway. ³³Post MD researcher with Ross Adey. ³⁴Previously Professor of Surgery, University of Osaka. ³⁵Senior Resident in Neurosurgery, Long Beach VAH. ³⁶Like Marc Binder, generated his raw PhD thesis data in DGS lab in 1974 while a predoctoral trainee with George Moore at the University of Southern California. ³⁷A private-sector pain relief company, using spinal–cord stimulation. ³⁸An MS trainee with Roger Enoka at the University of Colorado. ³⁹A health-care company. ⁴⁰Initially worked with DGS as an electronics engineer at UC-Davis. ⁴¹Also in 2016. ⁴²Subsequently an Emeritus Professor of Physiology and Research Professor of Agriculture and Life Science. ⁴³Detailed discussions with DGS helped latter advance his understanding of spinal and neuromuscular systems. For editorial interactions with DGS see Burke chapters in Grillner et al. (1985), Stein et al. (1985, 1987), and Stuart and Zigmund (2006). ⁴⁴Head of this NIH laboratory for 30 years. ⁴⁵Subsequently Professor of Biology and Medicine, Brown University. ⁴⁶Collaborated in PhD training programs in physiology. ⁴⁷Substantial discussions in 1971–Pr. See also Herman et al. (1976), Grillner et al. (1986), and Stein et al. (1997). ⁴⁸See first two volumes in footnote 44. ⁴⁹Discussions as detailed as those with Sten Grillner in 1972–Pr. See also Stein (1972, 1985) and symposium volumes in footnote 47. ⁵⁰Also in 1985 and 1986. ⁵¹Also in 2001–02. ⁵²See Fetz et al. (1979). ⁵³See footnote 52 article and Binder and Mendell (1990). ⁵⁴Formerly Professor of Physiology at Universities of Ataturk (1975–1999), Trabzon (1999–2002), and Adana (2002–2004), TUR. ⁵⁵Founding member of the Arizona Movement Neuroscience Group. ⁵⁶Subsequently Professor of Movement Neurosciences, University of Illinois-Chicago. ⁵⁷As in footnote 55. Also collaborated with DGS in 2004–07. ⁵⁸Biomedical Engineering is a joint program with Emory University. ⁵⁹Also Professor of Biomedical Engineering and Physiology, Northwestern University and its Chicago Campus, respectively. ⁶⁰As in footnote 55. ⁶¹First University of Arizona PhD trainee to undertake interdisciplinary movement neuroscience training along with her standard speech and hearing science courses. ⁶²As in footnote 52. ⁶³Also in 1987 and 1989. ⁶⁴Subsequent positions as Professor of Physiology included the Universities of Calgary and the University of Guelph, Umea, SWE. ⁶⁵Subsequently, Professor of Marine Biology, University of North Carolina, Wilmington. ⁶⁶Also in 2004. ⁶⁷Co-I in

(continued)

footnote 68 NIH award. ⁶⁸Co-PI of NIH motor control award for PhD and postdoctoral trainees. ⁶⁹Also Research Professor of Physiology, University of Arizona, and later Professor of Biomedical Sciences and Health and Human Performance, Iowa State University. ⁷⁰As in footnote 67. ⁷¹Regularly spaced updates and advice to DGS on developments in invertebrate neurobiology. ⁷²Previously Professor of Biological Sciences, Columbia University. ⁷³As in Grillner et al. (1986). ⁷⁴Also a consultant in Neuropediatrics. ⁷⁵As in footnote 67. ⁷⁶As in footnote 67 followed by an interdisciplinary administrative research training position. ⁷⁷As in footnote 67 followed by continued similar student training. ⁷⁸Regularly spaced helpful discussions with DGS on statewide developments in experimental science in general, and neurobiology, in particular. ⁷⁹Provided advice in Tucson to DGS laboratory group on methods to study the in vivo turtle spinal cord. ⁸⁰Currently Professor of Medical Science, International University of Health and Welfare, Ohtawara (one of the seven sites of this university in JPN). ⁸¹Collaborated with DGS on development of new interdisciplinary biomedical research programs at the University of Arizona. ⁸²Also in 2001–02. ⁸³Subsequently Professor in Neurological Sciences Institute, Oregon Health and Sciences University, Portland. ⁸⁴Also in 2003–04. ⁸⁵As in footnote 67. ⁸⁶Near-weekly discussions with DGS on strategy of motor control research and research training. ⁸⁷See Ito (2011; p. ix) followed by his comment that the editorial help he received from DGS “was not simply for English but included broad aspects of motor system physiology. It was a valuable interaction for me.” ⁸⁸Also commented that his “most unique (*presumably meaning important*) subsequent administrative function was as “founding director of the Riken Brain Science Institute (launched in 1997).” ⁸⁹Collaborators on previous strictly historical articles included in their chronological order Roger Enoka (1990), Jennifer McDonagh (1998, 2001), Alan Brichta and Robert Callister (2001). ⁹⁰See Stuart and Zigmund (2006). ⁹¹Shares with DGS passion for the history of neuroscience; e.g., Stuart and Brownstone (2011), Brownstone and Stuart (2011). ⁹²Also Research Assistant Professor, Dept. Biomedical Sciences, Sch. Public Health, SUNY-Albany. ⁹³Professor, Dept. Biomedical Sciences, Sch. Public Health, SUNY-Albany. ⁹⁴See, e.g., Clarac, Massion, and Stuart (2009). ⁹⁵Played a prominent role in Stuart, Schaefer, Massion, Graham, and Callister (2014). ⁹⁶Also in 2013–14 with Roger Enoka a coauthor. ⁹⁷Also researcher in the Laboratory of Applied Biology and Neurophysiology. ⁹⁸Robert Callister is the primary coauthor on this project, which is a review of the contributions of Sherrington and Eccles to central inhibition.

movement quality of SCI patients. His work spans and integrates fundamental and applied clinical research, and includes several advances in computerized control of movement and its robotic assistance.

Daniel Kernell (1940–): Daniel is a Swede by birth. His research and conceptual ideas, like those of Bob Burke, who appears in my lineage table, led the way throughout the 32 years (1966–1998) during which I undertook intracellular recording experiments on the spinal motor control system. I first met Daniel in late 1971 at the Karolinska Institute in Stockholm while he was still working with Ragnar Granit. We have had innumerable interactions since then in several countries and in our respective laboratories. After his retirement from the University of Gronenberg in The Netherlands, he wrote a valuable monograph, which summarized much of the work on the spinal motor system, both classic and throughout his career in this field. Recently, at the age of 80, he wrote a monograph on colors and color vision, an inspiration to all of us to keep our minds sharp.

Moscow Motor Control School: The post-WWII Moscow Motor Control School also had a powerful effect on my development as a movement neuroscientist. For me, this school was epitomized by Nicolai Bernstein (1896–1966); Israel Gelfand (1913–2009); Michael Tsetlin (1924–1966); Bernstein's last PhD student, Victor Gurfinkel (1922–); and about 22 others. Working under far-from-optimal circumstances, this largely Jewish group's accomplishments and emphasis on interactions between invertebrate and vertebrate neuroscientists, life- and physical scientists, and basic and clinical scientists have been an inspiration to those of us who have attempted to emulate their interdisciplinary efforts in our own institutions' research and training programs in motor control neurobiology.

The Arizona Motor Control Group: With this group in mind, four colleagues and I founded the Arizona Motor Control Group in 1982 (see Table 1). Our initial group was appropriately interdisciplinary with expertise available among us in advanced neurophysiological recording, biomechanics, comparative vertebrate motor control, mathematical modeling, normal and abnormal speech motor control, neuropsychology, and human respiratory control. Many more areas were added as our group increased to 33 members by 2000, including more faculty from not only the UA but also state-wide, including ASU in Tempe, the Barrow Neurological Institute (BNI) in Phoenix, and NAU in Flagstaff. Our training faculty were more or less evenly divided among those institutions whose experimental animal models were invertebrates, nonmammalian vertebrates, and mammals, including humans. To aid our activities, Jim Bloedel from the BNI and I codirected an NIH-funded pre- and postdoctoral program in interdisciplinary motor control neurobiology. Between 1987 and mid-2002, this program mentored

more than 100 American PhD students and 120 postdoctoral trainees from the United States and several other countries.

A key feature of our training program was a Friday afternoon gathering at the UA, which we entitled “Doings in Motor Control.” At this function, the attendees, including visiting speakers, presented their ideas and research, with “out-of-town” faculty and trainees frequently presenting their work and ideas. “Doings” emphasized *dialogos* (mentioned earlier as “dialogue without rancor”) throughout the presentations. Many found this disconcerting because there were occasions on which the speaker did not get far because of the vigorous discussion of the appropriateness of the title of her or his talk. Because of its free-wheeling, intense-discussion approach, our “Doings” became widely known and discussed in the United States and abroad.

Summary of Main Research Foci (1967–Present)

Experimental neuroscience is done by human beings, each one an individual with some unique characteristics. As a result, the reasons that some go into neuroscience are manifold, as are their contributions. Personally, I admire the most those who blaze a new trail and stick to it throughout their lifetime, with several such people in Table 1. Others, for some reason or another are like me, a player in what I call “the ultimate game.” We make contributions that help advance our field but we are not its stars. In my case, I started late and to have any success in movement neuroscience, with an emphasis on the spinal cord, I had to learn techniques whose mastery brought me great personal satisfaction. Where I was strongest was in the people I was able to attract to my laboratory, either as a trainee or an experienced collaborator. Now, at the age of almost 86+ years, I am as intrigued by the field of movement science to the same extent as more than 60 years ago, with my current interest expressed largely in the form of historical articles and reviews. Several of these have been coauthored by close, long-standing friends made while I was still undertaking experiments. As I reflect on this, it would seem that the human side of movement neuroscience has been my emphasis, expressed in experimental work, reviews, and symposium volumes.

Experimental Work Published in Refereed Journals

My areas of research emphasis were more or less undertaken and published in chronological order: the hypothalamus and temperature regulation, the rhythm of various tremors, the nature and neural control of locomotion, the segmental motor system (my main lifetime focus), and, finally, the history of neuroscience, in general, and movement neuroscience, in particular. Among these, the experiments that excited me the most were those involving intracellular recording from spinal motoneurons in surgically reduced spinal cats (usually requiring at least 24-hour experiments) and from spinal

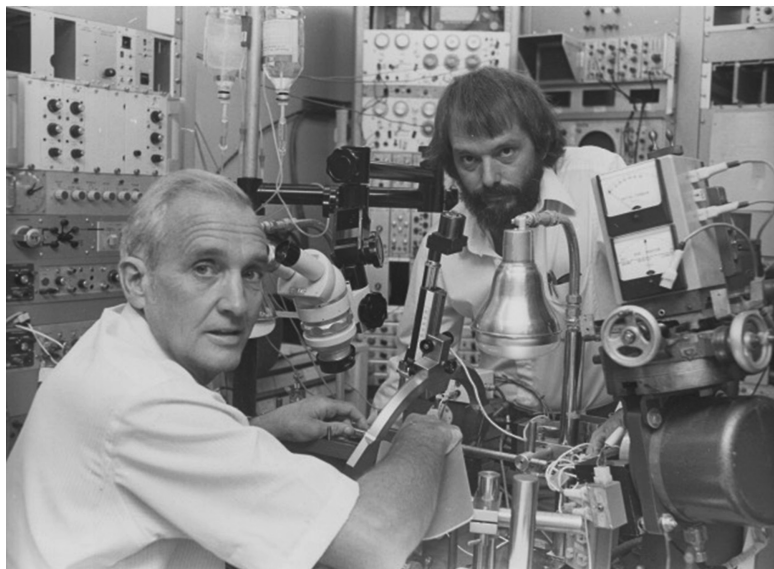
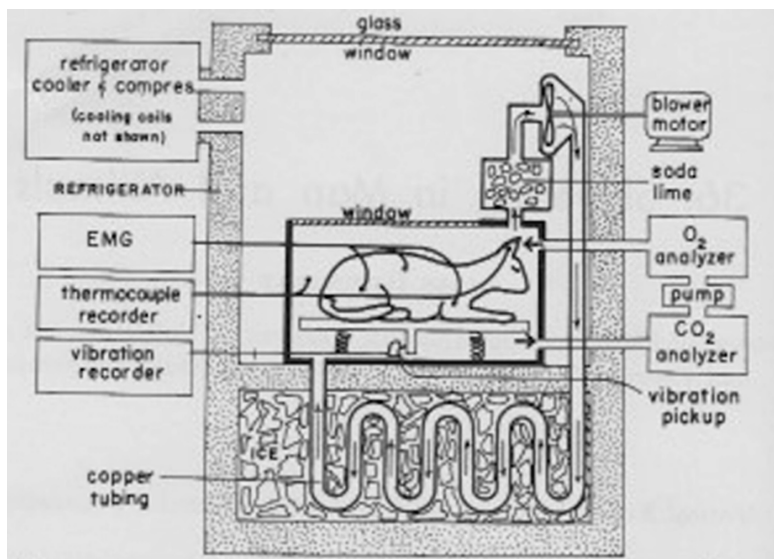


Fig. 1. Transition of experimental recording setup from late-19th-century respiratory and movement monitoring to late-20th-century electrophysiological recording. The top panel shows equipment used for my PhD research, whereas the bottom panel shows my late-20th-century setup for intracellular recording from spinal motoneurons in surgically reduced anesthetized cats and spinal interneurons in slices of turtle spinal cord. Bob Reinking is shown in the rear of the bottom panel. He was primarily responsible for all of the improvements made in my laboratory's electrophysiological and data processing instrumentation.

interneurons in slices of turtle spinal cord. Figure 1 shows my progression from the 19th-century-like equipment used in my PhD thesis to the late-20th-century electronic and computer equipment used in my intracellular recording work, which was progressively improved by Bob Reinking between 1965 and the early 1990s.

Coauthors of Publications

Many of my contributions have been coauthored by colleagues in several countries, the most born in America (77), and collaborating authors from 19 other countries, including Australia (11), Belgium (1), Bulgaria (2), Canada (5), China (1), Crete (1), Cyprus (1), the Czech Republic (1), France (2), Germany (3), India (3), Israel (1), Japan (7), New Zealand (2), Russia (1), South Korea (1), Sweden (4), Switzerland (1), and the United Kingdom (5). Despite the fewness of coauthors from these latter countries, they (even Australians) taught me much about the world, and the details of our discussions come instantly to mind.

Contributions in Reviews

My reviews, chapters in symposium volumes, and research monographs, which I usually also wrote with colleagues, have possibly had at least as much impact on the field of segmental motor control as my research contributions. In these conceptual contributions, my coauthors and I challenged our peers and ourselves to address key unresolved issues, and employed a “user-friendly” style to the benefit of pre- and postdoctoral trainees, the next generation of movement neuroscientists. Among these contributions, the best-known are on (1) the need to effect a stronger interface between neurophysiology and biomechanics (Hasan et al. 1995); (2) aspects of the proprioceptive contribution to the control of movement, which are still largely unexplored (Hasan and Stuart 1988); (3) a research monograph on the neurobiology of muscle fatigue, which was dedicated to the research contributions of Brenda Bigland-Ritchie (1927–; see Gandevia et al. 1995); (4) a symposium volume on sensorimotor control of posture and movement (Gandevia et al. 2002); and (5) another symposium volume on overall brain mechanisms for the integration of posture and movement (Mori et al. 2002). The Gandevia and colleagues (1995) and Mori and colleagues (2002) volumes were compiled and edited electronically in my UA laboratory and required the skillful editorial and technical editing of Pat Pierce.

Current Research Focus: The History of Movement Neuroscience

Since writing my PhD thesis, which began in the fall of 1960, I have greatly enjoyed the history of my research foci. This led to 31 solely historical

publications between 1962 and August 2017, including an unusually detailed article yet to be submitted for peer review. Among this group, there were four publications in which the history was of a broad scale, whereas the other 27 focused on a specific issue, a single person, or a group of people. The latter to this point include in their original citizenship and alphabetical order four Americans (Alexander Forbes, Elwood Henneman, Harry Patton, and Walter Woodbury), four Australians (Lawrence Brock, John Coombs, David Curtis, and John Eccles), many Russians (Nicolai Bernstein, Ivan Sechenov, Fyodor Severin, Mark Shik, Grigori Orlovsky, and several others in the Moscow Motor Control Group), three British subjects (Brenda Bigland-Ritchie, Thomas Graham Brown, and Charles Sherrington), one Frenchman (Jacques Paillard), two Swedes (Sten Grillner and Anders Lundberg), and one Swiss subject (Walter Hess). In retrospect, I felt honored to write about all of them.

Faculty Collaborators for Historical Research: My collaborators for this work came from close and far. Some were my research mentors, both formal and informal; some were from my later research group; and a large group were coeditors or coauthors of relevant volumes and articles. In addition to the people I have noted, all of whom I knew quite well, I coauthored a widely read historical article with two authors I had not met at the time of our collaboration (I have still not met one of them). They were great fun to work with, like all the others. In retrospect, it amazes me how many collaborators I have had for my history projects.

Essential Help of Reference Librarians: I owe a great debt of gratitude to the five UA reference librarians who have ferreted out details for my history publications since 1976. First were two in the Arizona Health Sciences Library: Nga Nguyen, who has continued voluntarily since her retirement, and Kathy Tower. They both insisted that they relished the challenge of a difficult assignment. Next came three in the main UA library: first Ellen Knight and then joined by Joan Schlimgen and Mary Feeney. Their quiet confidence in taking on the most difficult of assignments has been a boon to my endeavors.

In addition to these stalwarts, I have interacted with many highly competent reference librarians and library administrators in several countries, including Australia, Canada, Germany, Japan, New Zealand, the United Kingdom, and the United States. In all cases I received the utmost of cooperation.

Service: Traditional Extramural Contributions

Like most researchers at major research universities I was a member of an NIH Study Section, mine having been Applied Physiology and Biomedical Engineering in 1974–1978, an impressive and enjoyable group of engineers

and physiologists who were truly dedicated to assisting beginning applicants get funded to assist their emerging careers in orthopedics, biomedical engineering, and advanced forms of exercise science. This was followed by several ad hoc NIH assignments spread over many years. I was also a member of the Scientific Advisory Committee, Muscular Dystrophy Association (1987–1989); and the editorial boards of the *American Journal of Physical Medicine* (1975–1987), *Journal of Neurophysiology* (1979–1984), and *Experimental Neurology* (1982–1987). For the International Union of Physiological Sciences (IUPS), I was a member of the U.S. National Committee (chair, 1989–1992) and, for many years, I also was a member of the four-person IUPS Motor Control Commission, which was chaired by my close colleague, Sten Grillner. These latter appointments illustrate my interest in fostering international neuroscience, in general, and movement neuroscience, in particular. Since the early 1970s, I have undertaken a substantial amount of North American and international reviewing of physiology and neuroscience research articles and programs, and I have played a supportive role in the careers of innumerable young motor control scientists in North America and abroad.

Locomotion Research: Sten Grillner, Paul Stein (Washington University at St. Louis), and I have long believed that for study of locomotion there is need to incorporate findings made on invertebrates, nonmammalian vertebrates, mammalian tetrapods, nonhuman primates, and humans (for this approach, I coined the term, “interphyletic awareness”). We were co-organizers of three international conferences held about 10 years apart, with each followed by a symposium volume: Herman and colleagues (1976), Grillner and colleagues (1986), and Stein and colleagues (1997). The local organizers for these conferences were Richard Herman (also conference chair; now with the Good Samaritan Medical Center, Phoenix) for the Valley Forge, Pennsylvania, meeting in 1975; Grillner (also conference chair) for the Stockholm meeting in 1985; and, myself for the Tucson meeting in 1995 (conference chair, Stein). The preface to Stein and colleagues (1997) states tellingly, “The three volumes share common concepts: neuronal networks generate motor behavior, and comparisons of model systems distributed throughout the animal kingdom provide insights into general principles of motor control.” I retired from the planning group for the subsequent meeting in Stockholm in 2006 but attended the meeting, which featured a subsequent publication in a research journal to garner wider readership now that PDF files have become utilitarian.

Bulgarian International Symposia on Motor Control: These symposia were held nine times between 1969 and 2004. Their main purpose was to foster collegiality, cooperation, and scientific interactions among the international motor control community, including the means for Eastern European and Soviet faculty and trainees to have close, person-to-person contact with

leading investigators from the West. I attended these meetings from 1981 to 2000, and I made four contributions to help ensure the continued success of the venture: (1) I secured U.S. National Academy of Science funds to lead a nine-person delegation of U.S. workers to the 1985 symposium. (2) I also secured an NSF award to undertake a six-person exchange (1986–1989) of motor control scientists between the UA/BNI and the Bulgarian Academy of Sciences. (3) I was a member of the four- to five-person international advisory commission, which provided input for the 1985–2000 symposia. (4) In an extraordinarily time-consuming effort, I had the 1993 symposium expanded into a volume that was edited and compiled in my UA laboratory, again working with Pierce, but in addition, soliciting the editorial help of the entire UA/BNI motor control group and many extramural peers. The result was a volume in which all of the Eastern Bloc authors' contributions (63 of 94 chapters) were presented in polished English to optimize these investigators subsequent interactive possibilities with their Western colleagues.

The Moscow Motor Control Group: I had studied the work of some of the Moscow Motor Control School (Victor Gurfinkel, Mark Shik, Grigori Orlovsky, Yuri Arshavsky, and several others) prior to meeting Gurfinkel and Shik in person in Munich, Germany, in 1971. Since then, I have written much about their stellar accomplishments.

Aiding Japanese Colleagues: From 1960 onward, I have helped many Japanese colleagues with their usage of the English language. This emphasis was more formalized when I coedited with Shigemi Mori (1936–) and Mario Wiesendanger (1931–2017) a symposium volume entitled *Brain Mechanisms for the Integration of Posture and Movement*. This symposium was held in Mori's institute in Okazaki, with the subsequent symposium volume prepared in my Tucson laboratory. All 49 chapters were subject to the technical editing of Pat Pierce, and I paid particular attention to the 22 chapters written by Japanese workers. Most recently, and at his request, I edited the third and last 2011 volume of Masao Ito (1928–) on the cerebellum. This was a challenging and enjoyable assignment requested by a remarkably gifted neuroscientist, which is discussed in detail in an upcoming review by Calister and colleagues (2018; in preparation).

Service: Intramural Contributions

My interest in intramural service extended far beyond my departmental and medical college obligations, valuable and essential as these were and will always be. I had 17 years of graduate college experience, eight as a representative (1970–1975, 1999–2002), and nine as a member of the Graduate Council. These positions strengthened my all-around interest and knowledge about the functioning of the entire university. While on the Council,

we approved the beginning of a program in applied mathematics over the objections of the head of the department of mathematics and several of his henchmen. Applied mathematics became and remains one of the strongest programs of the university. Membership on the Graduate Council also indirectly gave me a way to successfully strategize and bring about a well-funded invertebrate neurobiology program and university-wide programs in neuroscience and physiological sciences. These successes also led to me gaining considerable central administrative support for a program in biomedical engineering and aiding the support for a new learning and memory group.¹⁰

Camaraderie in the Field of Movement Neuroscience

The movement neuroscientists of my and the next generation have been a remarkably affable group, indeed their interactions are a model of how international science should be conducted. This was not always the case among the generation of movement neuroscientists that preceded mine when there were far fewer participants and research groups. Also, among some other areas of current emphasis there is less affability, particularly in molecular biology. One reason for this may be the way data are generated and discussed in my generation's movement neuroscience. Most of us use or at least keep abreast of both inside-out and outside-in approaches, such that those focusing on human studies show interest in invertebrate results and vice versa. This is not totally the case: a degree of "test-subject tribalism" still exists in movement neuroscience, but it does not extend to not giving credit when it is clearly due. Furthermore, those who have incorporated molecular biology into movement neuroscience have done a particularly good job in explaining the nature and significance of their results.

Faculty Attitudes at Research Universities

All of my published research and other academic activities have been undertaken at research universities: UCLA, UC-Davis, and the UA in the United States; the University of Göteborg in Sweden; and the University of New South Wales in Australia. Focusing on the United States, in general and the UA in particular, I believe that too many faculty are so imbued in their research and teaching responsibilities that they neglect the need and value of making a contribution to the type of on-campus service that strengthens their university's academic enterprises, like new programs necessary for a balanced curriculum, and those desired by the students, themselves, and their local communities. A few years ago, I expressed this sentiment to a very wise and successful UA president. He rejected my complaint. His experience

¹⁰ For more details on these developments, see American Physiological Society, "Douglas G. Stuart," <http://www.the-aps.org/mm/Membership/Living-History/Douglas-Stuart>.

was that only about 15 percent of the faculty at a research university in the United States or abroad really care about their institution. He believed that it is up to this 15 percent to guard the “intramural service freedom” of the remaining 85 percent because among the latter so many are making research contributions of great societal benefit. For many years, I accepted this president’s point of view. Now, however, at the age of 86 and having discussed the issue with many colleagues in the United State and abroad, I believe that a 15 percent fully committed faculty are too few to keep a research university at the forefront and suggest that 40 percent with the best interests of the university at heart would be a sounder situation.

Another faculty attitude of concern is the amount of support they give to their students, both those they teach at the undergraduate and graduate level, and those they mentor in research in their own laboratories. Donald Kennedy (1931–) of Stanford University addressed this issue in his thoughtful 1997 book *Academic Duty*. An outstanding motor control researcher throughout his academic career, Kennedy emphasized the value of “putting students and their needs first” (1998, p. 287), an adage as appropriate for his own institution as it is for all other research universities.

Good Fortune in Combining Family and Academic Life

Finally, it is a great pleasure to end this autobiography with comments on my family life, which, together with the laboratory efforts of Bob Reinking and Pat Pierce, has been the mainstay of my academic endeavors. My wife, Jean (née Rassbach) was born (1935) and raised (until 15) in Philadelphia. Her mother, Evelyn (1901–1992), a member of the Daughters of the American Revolution, was a native of Gettysburg, Pennsylvania, and a home economics graduate (1924) of the Carnegie Institute of Technology in Pittsburgh. Her father, Phillip (1901–1970) was raised in Washington, DC, before graduating from the same institution as his future wife (1925; BS in metallurgical engineering). He subsequently had a distinguished career in the steel industry: first (1925–1950) with Midvale Steel (Philadelphia), where he rose to general manager and developed a U.S. patent on low-chromium steel; and subsequently with Union Carbide (1951–1970), where he became director of the Metals Division, and provided substantial service to the post-WWII rebuilding of the Japanese steel industry.

Jean Rassbach Stuart is a 1953 graduate of New Trier High School, Winnetka, Illinois, followed by a BA in child guidance and development from MSU (1957) and an MA in educational counseling from the UA (1972) and a certificate in gerontology counseling also from the UA (1990). For 24 years, she was a teacher and guidance counselor in California (Santa Monica, 1957–1958; Dixon, 1965–1967) and Arizona (Pima County Guidance Project, 1972–1978; Tucson Unified School District I, 1978–1993). Our four children have intriguingly diverse occupations. Michael (Monty)

Stuart (1957–; married to Deborah Murphy, a former office manager; one high school daughter) is a former stuntman, and now horse-trainer, wrangler, and stunt coordinator in the Los Angeles television and movie industry, where he is still having considerable success.¹¹ Kathy (1959–; married to Thomas Lohse, a Tucson businessman; three adult children) is a UA graduate (BA, 1980; MA, 1998), a former bilingual (English/Spanish) grade-school teacher, and now a coordinator of problem-based learning with the Tucson Unified School District. Daniel (1961–) is a well-known song and music writer, concert performer, former bandleader, and author who lives in Mexico City.¹² His son, a third-year college student, lives in New York City with his Barcelona-born mother, Nuria Morgado, an associate professor of Spanish literature, at the State University New York, Staten Island and Manhattan campuses. Cynthia (1963–; married to Michael Sadowsky, a Tucson businessman; two adult children) was a preschool teacher who now writes books and poems for young children. Jean and I have a particularly active family life in Tucson with our two daughters and their spouses, and the two grandchildren still in Tucson.

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