Editors’ Introduction

The following reminiscence by Cedric Raine is the 10th autobiography in a series published in the Journal of Neuropathology and Experimental Neurology. These have been solicited from senior members of the neuropathology community who have been noted leaders and contributors to neuroscience and to the American Association of Neuropathologists (AANP) and have a historical perspective of the importance of neuropathology in diagnosis, education, and research. His is the first autobiography by a distinguished PhD member (as well as former president) of the AANP, highlighting the contributions and integral roles of basic neuroscientists in our Association. It is hoped that this series will entertain, enlighten, and present members of the AANP with a better sense of the legacy that we have inherited, as well as reintroduce our respected members as humans having interesting lives filled with adventures, joys, and sorrows, and allow them to present their lives in their own words.

MNH, RAS

“A man is the product of his thoughts. What he thinks he becomes.”

Mahatma Gandhi

INTRODUCTION

Lacking a more original arresting opening, let me begin by saying that to be invited to join the list of esteemed neuropathologists featured in the Journal’s Autobiography Series, following icons like Asao Hirano, Robert Terry, and Nicholas Gonatas, is both a privilege and a challenge. Never known for being conventional and sporting a background completely at odds with the standard training of most members of the American Association of Neuropathologists (AANP), I would like to take off on a different foot by thanking my colleagues in the Association for welcoming and accepting me, among the first PhDs, into the fold as an Active Member. When Mike Hart rang me a few months ago asking me to prepare a piece for the Series, my knee-jerk reaction was to decline on the grounds of juvenility and inappropriateness. Recognizing that autobiographies usually represent one’s own self-chosen, self-redacted reminiscences, I was in no hurry to wax solipsistic to the readership and my colleagues. As we spoke, however, I rationalized the issue by recalling how many years had flown by since I became involved with the AANP (actually, 44) and how my own development as an investigator had paralleled and benefited from my affiliation with the Association and its mouthpiece, the Journal of Neuropathology and Experimental Neurology (JNEN). Setting feelings of uncertainty aside and with Mike still on the line, I thought, “this is the academic body that gave me the legs to stand on in neuropathology; of course, I’m qualified; I’ll do it.” So, for an autobiography garnished more with anecdotes than achievements, read on.

EARLY YEARS

I was born in May 1940 in Eastbourne, England, to a single mother; the vivid description of WWII events from her provided me with images difficult to distinguish from actual memories. She used to talk about the Dunkirk evacuation and the dogfights overhead between Spitfires, Hurricanes, and ME 109s, with their contrails slicing the summer sky, all occurring while I was in my push-chair. Moreover, this coastline was also the planned landing site for the German invasion barges waiting to depart Belgium, but thanks to the Royal Air Force and the Valiant Few, it never came to pass. Not surprisingly, Eastbourne residents were advised to evacuate to safer havens, particularly as German bombers driven from the London Blitz fled our airspace and jettisoned unspent bombs along the coast. My mother decided to return to her parents’ home in Carlisle, Cumberland, 400 miles north, 8 miles south of the Scottish border. We arrived in Carlisle in 1942, unannounced and definitely not welcome. Apparently, she had left Carlisle about 10 years earlier under a cloud and had not been heard of since. However, her odyssey had ended; her funds had run dry and she had come home. Aunt Maureen, who is 7 years older than me, answered the doorbell and found her sister who she had never seen, standing on the doorstep with “a beautiful little boy in a green woollen suit with small pink flowers across the front.” We were admitted into the house by my grandmother; my grandfather was summoned from his victory garden and there followed an unpleasant confrontation ending with my grandmother (my life’s best friend and savior), taking me by the hand out of the fray saying “I am going to put this young man through university,” and she did.

 Shortly thereafter, real memories began to form. There were 4 sisters in the house; 3 of them rather like the ugly...
sisters of Cinderella constantly battled with my mother and abused me at their convenience. I was terrified; I used to hide a lot and developed a bad stutter, which stayed with me until I came to the States in the late 1960s. My mother kept my hair long and in a pageboy style (remember Cedric of "Little Lord Fauntleroy?) and had me attend dancing classes at the age of 3 (Fig. 1). About 12 months later, the hair came off, my mother took a war-related job, and I was released to the Carlisle streets to become a grubby member of the populace.

It was wartime and, like many cities in the United Kingdom, Carlisle was in the thick of it. The United States had by now entered the war (thank heavens!), and being a center for about 6 railroad companies and situated on the only road south from Glasgow (the main port for traffic from North America), everything had to come through Carlisle, much to the delight of the local kids. The roadside and railway embankments became our playgrounds as convoys and trains of US and Canadian troops, lines of gray-painted trucks with long trailers carrying airplanes with folded wings, and tanks (oh, the tanks!) trundled through the cobbled streets of Carlisle. Near our street was a long lay-by where tanks would stop as crews took a break. I can still smell the hot oily fumes. We would race like mad to them and climb aboard shouting, "Any gum, chum?" Our reward was usually sticks of Wrigley’s gum and/or a Hershey bar—what a delight to kids from families on severe rationing! On 1 occasion, an American soldier, noticing my dirty face, gave me a bar of perfumed soap with which I ran home to give my mother who had not seen its like for some years.

The war had a toughening effect on kids: fathers were not around, we lived in a matriarchal society, and we grew up fast. My grandmother, anxious to get me out of the house, walked me down to Brook Street School just after I turned 4 and enrolled me as a 5-year-old in the infants’ class. That worked fine until I was to take the 11-plus examination, a government-required test designed to channel the “brighter” children at the age of 11 toward grammar school and the remainder to a “secondary” education, at which point birth certificates were required. My true age was revealed, and I was made to tread water to take the examination the following year.

**GRAMMAR SCHOOL**

I remember the 11-plus very well, some mathematics and English, a Rorschach test, and a composition on “your favorite film.” For the latter, I selected “The Four Feathers” and began by saying that I had gone to the cinema expecting to see a movie called “The Four Fatheads,” a comedy I thought, but was thoroughly enthralled by a movie about Victorian soldiers fighting in Africa. I still suffer from misreading titles. Nevertheless, I passed (3 of 50 boys from Brook Street made the grade), leading to tough but life-shaping years at Carlisle Grammar School, an all-boys school founded in the ninth century. It was tough mainly because of a poor financial situation at home; life shaping because the gown-draped masters were strict and demanding (Fig. 2). At the age of 16, I scored well on the General Certificate of Education Ordinary “O” level examination and wanted to leave school to become an accountant. My mother no longer worked full time, and I was supported mainly by my grandparents’ old age pension from the government. My grandmother put her foot down and stuck to her guns. “Young man,” she said, “you are going to university.” And so, I reported to grammar school the following September to join the VIth form to be coached for the GCE Advanced “A”
level examinations, which I passed, giving me a scholarship to enter university. I was lounging by a swimming pool in Raunheim-am-Main, Germany, surrounded by local friends (mainly girls), who I had met while hitchhiking around Europe (as I did every summer from the age of 17 to 22, working a while to earn a few pounds then taking off for the rest of the vacation), when I learned that I had been accepted by King’s College, University of Durham (later to become the University of Newcastle). What a drag, I thought, as I packed my rucksack for the long hike home. The following week, I registered at Newcastle to study for a BSc.

**COLLEGE YEARS**

King’s was great. I went mad for about 2 years (parties, rugby, etc.) (Fig. 3) but somehow managed to slow down enough in my third year to concentrate on the dreaded “finals,” which I overcame with a middle-of-the-road grade.

During my time at King’s, I had reestablished a childhood romance with a pretty girl called Maureen (my wife-to-be), who I had met in 1956 in the Northumbrian village of Rothbury where I spent many vacations at the home of an aunt and uncle. Contact with Maureen was lost after she left grammar school and I entered the Vth form, but shortly after I entered King’s, my uncle met her and, forever a matchmaker, gave her my telephone number. Consequently, throughout much of my time at college, I had my lovely Maureen to keep me on the rails as I studied and she went through nursing school to become a state-registered nurse.

Suddenly, in 1962, undergraduate days were over, and armed with a BSc (Hons) Zoology specializing in Limnology (freshwater biology) and a newly acquired desire to revitalize the polluted rivers and lakes in the United Kingdom, I entered the job market. I traveled the length and breadth of Britain on my motorbike to be interviewed for positions as a river board officer, losing out each time to candidates with no university degree but 3 years’ experience. I gave up and took a part-time job as a bartender in the Green Dragon, a public house in a tough area of Carlisle. This went very well for me until the night I unwisely tackled and evicted the local pub-wrecker, a short bearlike man who had sent the domino table flying (and many glasses of beer thereupon). He swore he would be back the next week to get even. He returned as expected; and I was
prepared with bar-flap locked down. I was in the midst of pulling a pint of ale with a large skittle-shaped pump handle surmounted by a brass finial when he lunged at me over the bar. Purposefully, I released the pump handle, it sprung back, and the finial caught him under the chin, drawing blood. On seeing blood on the bar, he growled, lost control, and proceeded to climb over. His hands slipped and he fell, once more banging his chin on the upright pump handle. More blood, more rage. Several times he nearly made it over, but I managed to push him back, sometimes using the spring-loaded pump handle. "This is it," I thought. Just then, the Carlisle police arrived and took him away, his face a bloody mess. They had been looking for him because, earlier the same night, he had trashed another pub, the Pheasant, down the road from the Green Dragon. For many days thereafter, people would come into the Green Dragon asking to see "the student who beat up so-and-so" (name withheld), and I would show them the pump handle that did it. Incidentally, so-and-so was sentenced shortly thereafter to several months in Durham jail, and I gave up my days as a barman.

MEDICAL RESEARCH COUNCIL DEMYELINATING DISEASE UNIT, NEWCASTLE

About a week after I left bartending, I met a college classmate who told me of a vacancy for an electron microscopist at the Medical Research Council (MRC) Demyelinating Diseases Research Unit in Newcastle. The Unit director was Dr. Ephraim Joshua Field (E.J. to all), and its mission was multiple sclerosis (MS). Because I had read issues of Scientific American that featured cellular ultrastructure and articles on myelin by Betty Ben Geren Uzman and John David Robertson, electron microscope (EM) pioneers in the analysis of myelination, and because I was familiar with MS through an uncle who was a victim of the disease, I considered myself adequately versed to apply and an interview was granted. I arrived at the Unit and was shown up to the third floor of a 4-story Victorian row house, passing en route several open doors through which faces quizzically peered, where I was introduced to my future mentor, E.J. Field (Fig. 4). The interview was tantamount to pure intimidation, with E.J. needling me about my inexperience and belittling my background. He asked where I had learned about myelin and I answered, "Scientific American." He rolled with laughter. Surprisingly, I survived the interview, apparently met muster, and was given a starting date. As I left his office, E.J. asked me to lend him my copies of Scientific American.

Over the coming years, I turned a large empty basement into a busy EM suite. My zoology and technical training stood me in good stead. I was sent to learn the mechanics and electronics of the EM from Zeiss in Germany and Philips in Holland and became highly proficient in tissue and specimen preparation. The Unit was superbly equipped for basic and clinical research but, unfortunately, scientific creativity was stifled by E.J.’s personality problems, which reduced many staff members to nervous wrecks. Bullying, demeaning, profanity, and ridicule were his mantra. His total approach was pure solipsism—no one else’s opinions mattered. Group discussions on scientific topics and presentations on progress via journal clubs or works-in-progress were nonexistent, and staff members were expected to remain penned like sheep in their rooms. E.J. loved publicity and would stop at nothing to promote his concepts. One incident heavily imprinted on my cortex involves a visit to the Unit of a delegation of outside scientists from the research community sent to verify a claim by E.J. that a particular blood test he had devised had diagnostic value for MS. The group assembled in the laboratory of a colleague for a briefing while he set up a series of multiwell plates covering a range of dilutions of test reagent and blood samples from MS and control patients. A code sheet was generated to be held by my colleague and the group left for the day, agreeing to return the following day to inspect the outcome. First thing next morning, before the visitors arrived, E.J. entered my colleague’s laboratory, demanded the code sheet, examined it, methodically rearranged some of the plates, and then returned the code sheet to him. The delegation arrived, the results were examined, the code was broken, and shortly thereafter, being satisfied with the results, they left the
building. Fortunately, attempts by others to reproduce the data were unsuccessful, and the test became history. Although staff members became aware later of what had happened, not one of us dared say a word. Anyway, who would have listened?

E.J.’s problems notwithstanding, the Unit was home to a team of dedicated scientists, including Ernest Caspary, David Hughes, Victor Cunningham, David Adams, Rosemary Piper, and Janet Ball, among others. But because cross fertilization was discouraged and only the director’s ideas mattered, opportunities for individuals to shine were limited. Despite the atmosphere, I spent almost 6 years in the Unit, firstly, because I grew to love my research on MS and, secondly, I needed a PhD to progress. To E.J.’s credit, he did provide me the opportunity to acquire a PhD, not to mention a vast array of skills, access to a valuable stock of MS material, and experience with many animal models. Sadly, my own experience was not typical, and many young scientists quit MS research as a direct result of E.J.’s bullying and bad behavior, whereas others like John Prineas (vide infra) and myself (knowing we lacked the gravitas to challenge the situation) refused to be worn down, said nothing, tolerated the nonsense, gained experience, then moved on. Indeed, it would be fair to say that, most of the time, E.J. was a complete misanthrope and that his aberrant behavior was the norm in the Unit. In the beginning, however, he could display a mentor-like role when he would suggest a topic for my thesis and help me set the experiment up, leaving me to continue the project and sample and prepare the tissues for study. When results began to emerge and interpretation was needed (remember, I was beginning from ground zero), he could be quite helpful. However, as I became more trained in morphology and pathology and more independent, his attitude toward me deteriorated as his unpleasant side raised its ugly head; perhaps, he felt threatened, who knows? In any event, I did manage to learn human neuroanatomy and neuropathology, with E.J. dutifully reminding me that I would need this knowledge for my thesis defense. Difficult though it was, I never allowed his mean side to get me down, but it certainly put a blight on many a day.

In 1967, after 4 years of marriage, Maureen announced 1 evening that she had accepted a full-time position as a Night Sister (head nurse) for me to get down to writing my PhD dissertation. It worked and I completed the task in 3 months, but its preparation was a nightmare. E.J. insisted that it should be prepared outside of work hours and that I was not to use anything from the Unit, especially office or darkroom supplies. As luck would have it, E.J. decided to take a 2-week trip to Australia (to obtain human Kuru brain from Carleton Gajdusek’s cases in New Guinea), and I took the same period as vacation, using the time to print the 200 plates (3 copies) for my thesis with my own supplies. Soon after he returned,
I appealed to the MRC and was granted a raise of £80 per annum (it was November) on the condition that I would forgo the upcoming annual increment of £120 the following April. The net effect was that my PhD cost me money. Unfailingly, E.J. continued to grind away, and a few weeks later, he hired my replacement, a college friend with a PhD in botany, Alan Peat, at a salary of £2100, and I was to train him in research neuropathology! I decided to relocate.

In 1967, there was a dearth of positions for medical researchers in the United Kingdom, which was in the midst of a brain drain to the Commonwealth and the United States. I interviewed at 1 research institute to work on scrapie (one of the topics of my dissertation), but the academic climate in the institute and salary prospects were dismal. Maureen and I decided to leave the United Kingdom and, when I received an offer of a 2-year postdoctoral fellowship with Dr. Robert (Bob) Terry, an internationally known neuropathologist at the Albert Einstein College of Medicine in the Bronx, New York, I grabbed it. Bob had probably also heard about me from my friend John Prineas, a neurologist who had left Newcastle for Einstein 1 year earlier. While working in my laboratory as a visiting scientist in the Unit, John had become a target of E.J.’s unpleasantness and had complained about the situation to his supervisor, whereupon he was advised to leave. Maureen and I made plans to come to the United States. I applied for travel funds, prepared the requisite number of application forms with photographs for the Wellcome Foundation, and took them to E.J. for signature, finding him in the library with a visitor. He received my request with abject dis-taste and refused to sign, exclaiming “Why should the UK pay for you and your wife to go to America?” We had no alternative but to put our house on the market (instead of renting it for 2 years); the small profit from the sale going toward travel and start-up funds in New York; and for our last weeks in England, we moved in with Maureen’s parents 30 miles away. (One year later, in New Haven, Connecticut, after I received the Weil Award for Best Presentation at the AANP annual meeting, E.J.’s visitor in the library unexpectedly revealed himself to me. It was Wally Bradley, a neurologist from Newcastle General Hospital who had been about to leave to

FIGURE 9. Celia Brosnan, late 1990s.

I brought the finished product to the laboratory (3 copies, 2 volumes, text and plates) for E.J. to sign a coversheet as my mentor, verifying that I had served my time and fulfilled the requirements as a graduate student. When E.J. next entered the laboratory, I politely asked him to sign the coversheet for me to submit the work to the college registrar for processing. He glanced at the coversheet, refused to sign, cursed, and walked out. This became a daily routine over what appeared an eternity, with E.J. storming out each time, leaving the sheet unsigned. I would go home each evening and tell Maureen, “Still no signature.” On the 10th day, I confronted him and told him what I thought of his behavior. His eyes flashed angrily, then he cursed, scribbled his signature, and left. Ten minutes later, the material was with the registrar.

Unfortunately, the nonsense continued. At that time, the procedure for PhD thesis defense in the United Kingdom mandated a 2-person committee, that is, the supervisor (for me, E.J.) and an outside authority (in my case, Prof. John Cavanagh, Queen Square, London). Two to 3 weeks before the defense, E.J. decided for no reason to resign from my committee, almost scuttling everything. After much angst and scurrying around, I managed to recruit the chairman of Pathology, Prof. Heppleston, to step into the breach. One more hurdle negotiated but it wasn’t over. The evening before the defense, as I was leaving the laboratory, E.J. came in and presented me with a letter stating that I was terminated as of that day. I maintained my composure (tomorrow was a big day) and went home. Not knowing what lay ahead, I came in next morning and was about to attend the defense when a smiling E.J. breezed in with another letter, rescinding its predecessor and reappointing me. The defense went well, and I was awarded a PhD in Medicine, subspecialty Neuropathology. But there was more to come. For years, E.J. had informed me that, on obtaining a PhD, my salary would rise to £2000 per annum (from about £1100), a princely sum in 1967. On reminding him of the £2000 level the day after my defense, he burst out with, “What are you doing different today from what you were doing yesterday?” and scoffed at the idea of a raise. I appealed to the MRC and was granted a raise of

A 1-year position in Boston [he later became Chairman of Neurology at the University of Miami]. Wally congratulated me, recalled the incident in the library, and told me he would take great pleasure in conveying the news of the award to E.J. when he returned the following week to the United Kingdom.

Our final weeks in Newcastle were particularly stressful. I was commuting from Rothbury, finishing several manuscripts, training my replacement, and running the EM laboratory. Bob Terry’s letters buoyed me up, advised me not to stir up any dust, and to leave quietly, sage advice that was hard to follow because E.J. remained incorrigible. One day, while working on the Philips EM 200 with the column down to air and the specimen chamber open as I aligned the cold-finger apparatus, he entered the room, saw what I was doing, and demanded that I stop and reassemble the microscope. Unable to comply immediately, I asked him to leave. He began to berate me. I again asked him to leave. He refused and continued to remonstrate, becoming white with rage and trembling. Suddenly, I snapped and, knowing from past experience that E.J.’s Achilles’ heel was an extreme fear of liquid nitrogen and that a flask full of it was sitting on the microscope, I went into attack mode. E.J. had moved around the microscope with his back toward the corner of the room and I have vivid memories of what followed. I took the flask and tipped some of the liquid at his feet, causing much hissing and vapor. He backed into the corner and shrieked. “Dance you (expletive),” said I, tipping more out. He was terrified and began yelling for help. “Dance,” said I, again. He danced. I kept tipping and commanding, and he kept yelling and dancing. I then paused, stepped aside, and asked him to leave me to finish what I was doing. He darted past me and fled, blurting out as he slammed the door. “I’ll get you for this, Raine.” Strangely enough, I heard nothing more about this incident, but he never harassed me again. Many years later, I recounted this incident (and several others) to an audience of young neuroimmunologists as part of a talk entitled “Negative Role Models Have a Role.” They were enthralled. I doubt whether anyone could nominate a better candidate than E.J. for the title.

On my last day in the Unit, I delivered the finished manuscripts to E.J. for final typing and submission (he would subsequently rearrange the authorships at the galley-proof stage, putting himself up front—but that’s another story) and went into town to purchase 2 suitcases for our journey to the States, returning to finish the day and say farewell to everyone. As I left the Unit with my suitcases, E.J. appeared, I thought to say farewell. No, he wanted to check the contents of the two empty suitcases, which he made me open. So ended my training in medical research in the United Kingdom. (In 1972, Professor E.J. Field was relieved of his position as director of the MRC Demyelinating Diseases Unit by an MRC investigative committee based on issues stemming from scientific impropriety and poor interpersonal relations with staff members and never led a research team again. He died in 2002.)

FIGURE 11. Einstein Clinical Neuropathology Division, 1992. Standing, Karen Weidenheim, Ken Hutchins, and Dennis Dickson (director); sitting, Elizabeth Wu and Sunhee Lee. The latter two were also research fellows in my laboratory and were outstanding.

FIGURE 12. Kris Selmaj (now in Lodz, Poland) and Anne Cross (now at Washington University, St. Louis), two of the best clinical fellows I ever helped train, with myself in the background, 1991.

EINSTEIN YEARS, 1968–PRESENT
On a 100°F Friday afternoon, June 21, 1968, I arrived at JFK airport with a worried Maureen and a burning desire to succeed in MS research. My harrowing apprenticeship in Newcastle was behind me; I saw only blue skies ahead. We were met by John Prineas who delivered us to his flat in New Rochelle, where we met his wife Eileen and their 3 children, and where we stayed almost 2 weeks while I settled in at Einstein and Maureen pounded the sidewalks to find an apartment. The next day (Saturday), John brought me into Einstein to see Bob Terry who was coming in to meet me. John took me to the laboratory, one of the smallest I have ever encountered (a narrow room little more than corridor width, maybe 20 ft long, with 2 EMs crammed into small enclaves at either end). Looking back, I still find it incredible how much pioneering cutting-edge neuropathology had emerged from that small space (the square foot value in scientific product must have been immeasurable). And there, sitting, waiting for me, was the maestro himself, Bob Terry (Fig. 5).
Our meeting was brief, probably no more than 20 minutes. John dashed off to attend to something while Bob succinctly outlined his plan for me. I would run the laboratory until a new building, the Kennedy Building, was completed in 1970, whereupon I would move to a new laboratory. I would continue my work on demyelinating diseases and would form a bridge between the Terry laboratory and that of Murray Bornstein, a world authority on cultures of myelinating CNS tissue, a system on which I had also worked and published in England. Having been given my charge, before he left, Bob gave me a mini tour of adjacent laboratories where I met the renowned myelin neurochemist and soon-to-become long-term friend and collaborator, Bill Norton, and Labe Scheinberg, a neurologist who would become my prime source for clinical material in the future. My first week was an eye opener. In addition to 2 full-time technicians cranking out sections for EM, there was an amazing lineup of scientific talent trafficking in and out of that small laboratory: Henryk Wisniewski, Mike Shelanski, Kinuko Suzuki, John Prineas, Carlos Araoz, Ivan Herzog, Roy Weller, John Andrews, Jan Leestma and Isabelle Tellez-Nagel, inter alia. Several of them were about to leave and were putting finishing touches to things, mainly manuscripts, before their year ended on June 30. These were heady times for Bob’s team, probably the most prominent in the field, with a multitude of projects coming to fruition and investigators moving on; they were content, well qualified, and armed with the best training. Entering the scene was this 28-year-old skinny postdoctoral fellow from Carlisle in a 3-piece suit, still wet behind the ears with a brand new PhD, an annoying speech impediment, and little else apart from ambition, trying to get a foot on the conveyor belt. Bemused and not knowing what else to do, I surveyed the scene, found an abandoned desk alongside a Siemens 1A EM, deposited some papers on it, and sat down. Interestingly, after a little more than 1 year at Einstein and much gained self-confidence, the speech impediment left me. Einstein had a reputation for open-door collaboration and, during my first weeks, I established solid working relationships with several colleagues, John, Henryk, Mike, Kinuko, Bob, Labe, Murray, Bill, Bernie Fields, and Bob Katzman (Chairman of Neurology), each of which would bear fruit later. The corridors oozed with science and, during my free time, I would explore the buildings just to read nameplates and discover more and more famous: Purpura, Bloom, Pappas, Novikoff, Eagle, and many more, each quite approachable for advice or discourse. What a difference from Newcastle and the sheep pens!

Three weeks into my fellowship, just as Henryk, John, and I (Fig. 6) were beginning a series of studies on experimental demyelination and remyelination (which earned us the AANP Weil Award in 1969), Bob walked into the laboratory and handed me an application kit for an RO1 research grant from the National Institutes of Health (NIH), instructing me to prepare the Specific Aims and Experimental Procedures sections and he would complete the other sections. He did a wonderful job (seemingly effortlessly) and submitted the proposal to NIH that September. The budget was enormous and covered 3 full salaries (my own included), an EM, and enough equipment to furnish the new laboratory opening in 1970. To our delight, the application was approved and fully funded and, equally surprising, Bob passed the award over to me to manage, at the same time (April 1969) moving me up to Assistant Professor, with a hefty increase in salary. How
often does one hear of a senior mentor willing to relinquish an award to a young investigator? Such was my beginning as an independent investigator at Einstein.

Looking back, thanks largely to Bob and my colleagues at Einstein, my takeoff was meteoric, and after moving into the new laboratory, I was fortunate enough to secure additional NIH and NMSS long-term funding plus an NIH training grant for predocs and postdocs. For almost 40 years, these funds covered personal salary plus salaries for 3 full-time technicians, a junior faculty member, several predocs and postdocs, and a secretary. My research strategy was to provide a stable home base with strong technical and administrative support that would be conducive to attracting trainees and visiting scientists who would be able to focus on their individual research projects rather than technical issues. A superb technical team (Earl Swanson, Miriam Pakingan [Fig. 7], and Howard Finch, each of whom spent more than 30 years in my team) was something that stood my laboratory apart from others, and because I was at the bench teaching or performing surgery, perfusions, sampling or trimming tissue, or scanning on the microscope (Fig. 8), trainees received maximum attention and top-quality material. This background support, combined with my personal network of collaborators with whom I shared an open-door policy (quintessentially Einstein), made for a fertile training ground and a contented environment to which my track record of successful trainees will bear testimony.

From the outset, I was most fortunate to share a marvelous working relationship with Henryk and John that resulted in several significant publications on demyelination and remyelination. Although Bob left the 3 of us very much to our own devices (exercising his policy of “benign neglect”), he was always there when advice was needed and we never felt neglected. Simultaneously, I also enjoyed an enviable symbiosis with Murray Bornstein, and our partnership spawned many novel contributions to the understanding of the effects of humoral factors on CNS myelin. Later, as immunology became more sophisticated, this would blossom into the neuroimmunology of the phenomena and the International Society of Neuroimmunology in 1988. Rachel Sheppard, a PhD student, was largely responsible for bringing virology into my program at a time when paramyxoviruses such as measles were suspected etiologic agents in MS. My entry into neurovirology was even more facilitated by my teaming up with Bernie Fields, and our landmark work on temperature-sensitive mutant viruses in the early 1970s landed us much acclaim (including a gold medal). Other in-house close collaborators included Bill Norton and Celia Brosnan, one of the brightest and most pleasant persons I have known in science (Fig. 9).

Over the years, shared research relationships beyond the walls of Einstein also formed important milestones in my career, chief of which was with one of my closest friends, Dale McFarlin, of the Neuroimmunology Branch at NIH from its inception in 1973 until his untimely death in 1992 (Fig. 10). Dale was a consummate teacher and collaborator and an outstanding immunologist. Between the two of us and our fellows, we established a number of novel animal models including, for the first time, several chronic relapsing forms of experimental autoimmune encephalomyelitis in the mouse and the isolation of HTLV-1 from tropical spastic paraparesis. Moreover, we worked harmoniously together and jointly trained many investigators, several of whom are today’s leading lights in the field.

FIGURE 15. My administrative strength, Pat Cobban-Bond, 2007. Where would I be without her?

FIGURE 16. I have just given the first Gabrielle ZuRhein Lecture, Madison, Wisconsin, November 2002. Gabi is in the center with Mike Hart (left).
would go with him; they both left, leaving a great void. Almost simultaneously, Neurosurgery was moved off-campus, further diminishing Neuropathology’s “raison d’être.” Because Bob was no longer present, our political clout was weakened and previously less-noted faculty members became elevated and began to make demands to undermine our prior prominence.

From then on, Neuropathology became a collection of individual laboratories lacking a cohesive leader. Dik Horoupian led the clinical division for a while but received dwindling support and wisely departed to join the Stanford faculty in 1986. Kinuko Suzuki left for UNC, and the baton was passed to Dennis Dickson, another star, who held the fort for a few years, but because of the politics and the ever-decreasing clinical load, he decamped to Jacksonville in the mid-90s, leaving Karen Weidenheim and Sunhee Lee to run a service, a shadow of its former self (Fig. 11). Because my own program on MS had always been linked to the clinical service, it pained me to watch my colleagues move on. Fortunately, I continued to attract outstanding clinical fellows (Anne Cross, Kris Selmaj, Wolfgang Liedtke, Wayne Moore, Luca Battistini, Bruno Bonetti, David Pitt, inter alia) (Fig. 12). I maintained a collegial relationship with Herb Schaumburg, the new chair of Neurology, and had the support of a far-sighted CNS disease-savvy Dean, Dom Purpura who helped me relocate my large laboratory away from the embers of the clinical division to firmer ground in another building in 1998. Thus, I was able to weather the storm of change. Combined with a wealth of clinical material from MS cases for molecular and genome-related topics and a plethora of mouse models afforded by the advent of DNA/genetic technologies, we entered the 21st century with a powerful armamentarium of projects, which continues to expand.

**TRAINEES AND BACKGROUND HEROES**

While my personal research program and interactions with investigators within and beyond the walls of Einstein led to a wealth of publications and books, the most rewarding achievement of my 40+ years at Einstein came through my ability to attract, train, and launch an armada of predoctoral and postdoctoral research fellows into the field of MS. Furthermore, facilitated by a gregarious nature and personal friendships with other laboratory-sharing intramural and extramural investigators, trainees were able to travel between laboratories to learn new skills. Therefore, mentoring of fellows became a strong feature of my program, the latter broadening the fellows’ horizons considerably. In lieu of an exhaustive litany of trainees passing through my laboratory, I shall present them in list form, hoping they will forgive me for the brevity: Rachel Sheppard, Jim Powers, Herb Schaumburg, Jack Stern, Peter Spencer, Bob Cook, Mauro Dal Canto, David Snyder, Howard Weinberg, Cecile DeBaecque, Marion Ecob-Johnson, Ute Traugott, Ken Lipow, Matias Roytta, Dov Soffer, Hervé Fleury, Evelyne Bennaud-Touze, Ellen Weingberg, Bill Lyman, John Heath, Satoshi Ueda, Joy Zagoren, Amy Schönfeld, Wayne Moore, Hideo Kusaka, German Roth, Anne Cross, Kris Selmaj, Barbara Cannella, Rogier Hintzen, Sunhee Lee, Dusan Pekovic, Diego Ponzin, Elizabeth Wu, Giovanni Monastra, Dusanka Skundric, Yan-Ling Gao, Joan He, Wolfgang...
Liedtke, Luca Battistini, Elly Capello, Bruno Bonetti, Jurgen Pohl, Marcin Mycko, Carolyn Hoban, David Pitt, Rana Zeine, Steffi Gaupp, Susanna Weerth, Heather Wilson, Klaus Ohnischke, Iris Nagelmeier, Kakuri Omari, Richard Lango, Anna Lüttemann, Shana Marmon, Kathrin Müller-Wielisch, George DeVries, Irene Georgieff, Mohammed Farooq, Steve Snyder (the latter 4 with Bill Norton), Gareth John, Luz Claudio, Alice Rajan, Sara Lutz (the latter 4 with Celia Brosnan), and Vladislav Tshiperson (with Bridget Shaﬁt-Zagardo). Finally, in a comen-tering relationship with Dale McFarlin, the following also spent training time with me: Leslie Barnett, Avery Brown, Forouzan Mokhtarian, Dick McCarron, Anne Cross, Mike Racke, David Katz, Ben Segal, Rhonda Voskuhl, and Betsy Smith. To all the above, I extend my sincere thanks for the purpose and excitement they brought into my career. I hope their gift was reciprocated and I enjoy hearing from them at regular intervals. As for some of my close Einstein colleagues, I see John Prineas annually when he comes with Eileen from his native Australia to visit his daughter Sara and family in New Jersey (Fig. 13). As for Bob Terry, in November 2013, 3 of us, Jim Powers, Dik Horoupian, and I, traveled to San Diego to meet up with him, a memorable event orchestrated with some secrecy by Jim (Fig. 14). Bob was in fine fettle and I think was delighted to meet his ex-fellows. Despite the many in-tervening years, the bonds forged at Einstein were as strong as ever.

Having already acknowledged my major collaborators, trainees, and technical team, I am left with expressing my extreme appreciation to my administrative/secretarial staff who tolerated daily my eccentricities, my handwriting, and my voluminous typing and laboratory management needs for almost 45 years. Mary Palumbo was my ﬁrst trustworthy and patient secretary as I carved a niche for myself from 1968 through the early 1980s; Michelle Briggs (later Graebner), a wonderfully faithful secretary, was with me from the late 1980s until 1993; JoAnn Leone painstakingly ran the ofﬁce of the Journal of Neuroimmunology, which I launched in 1981, from 1996 to 2012, and watched it go online; and ﬁnally, my personal secretary/administrator par excellence, Patricia Cobb-Bond (Fig. 15), to whom I shall be eternally grateful, held my program and ofﬁce together in flawless fashion from 1997 to the present day.

THE AANP AND ITS JOURNAL

During my early training years, a mark of success and the gold standard for me were to have publications in JNEN. About 12 months into my tenure at Einstein, Murray and I submitted our ﬁrst paper together on the ultrastructure of de-myelination in vitro to JNEN and I was ecstatic when it was accepted. On the day I received the letter of acceptance, Murray was hosting a celebratory party for a recently promoted colleague to which I was invited. In those days (1969), I wore 3-piece suits and shirts with detachable collars and before I appeared at the party, I reversed my vest (waistcoat) to show the black silk up front and rotated my white collar, thus transforming me into an Episcopalian minister—quite a sight in a Jewish medical school. I then mingled with the crowd, blessing people much to their bewilderment, so pleased was I with myself and my ﬁrst paper in JNEN.

The AANP has always been the prime outlet for my academic energies and seminar schedule (Fig. 16), and its annual meeting was usually the pinnacle of my calendar of events. From the very outset, at business meetings, I was drawn into animated exchanges from the ﬂoor at a time when PhDs were ineligible for Active Membership. The membership was divided equally into 2 camps, the most vociferous being the contingent wishing the Association to remain elitist and purely clinical. Backed by successive insightful presidents, I was “commissioned” to lead the campaign for the pro-PhD element, and after about 3 years of feisty exchanges and 2 very narrow defeats, we succeeded in convincing the AANP mem-
bership that PhDs should be admitted as Active Members. For many years thereafter, attendance was strong and the program embraced basic science presentations by PhDs who would otherwise probably have appeared on the programs of other groups. I maintained a very active AANP citizenship and served on and/or chaired several committees (e.g. Program, Membership, Constitution, Council), organized symposia, delivered keynote presentations, and for 1 year (1993) was honored to be president. Other honors include being the recipient of 2 Weil Awards (1969 and 1975), the Moore Award (1976), and the Award for Meritorious Contributions to Neuropathology (2007). In addition, I served on the Editorial Board of JNEN from 1987 to 1994. For any academic acclaim I may have achieved, I owe more to the AANP than to any other society.

EPILOGUE

For the sake of completeness, a few lines on personal life seem appropriate. Maureen and I were never blessed with children of our own, but in 1985, while working as a volunteer in a hospital for special children, Maureen was assigned to Frank, a young wheelchair-bound boy with spina bida who had been medically abused and rejected by his family. She became fond of Frank and, because he was eligible for foster care, she decided we should offer him a home. So, in 1985, at the age of 12, he entered our lives (and our hearts) and, within a couple of years, we legally adopted him. Through us, Frank entered high school, learned to read and write, had outside friends and interests, obtained a high school diploma, acquired a home life, and developed a beautiful personality. After graduation, he worked as an assistant teacher in a special education center, a task at which he excelled. Frank always said that life for him began when he met Maureen, and he never let his medical condition get in the way of his ambitions. He was with us for 12 wonderful years and became a well-known and much-loved member of our community; however, the irreversible kidney damage in flicted on him in his early years and his many years on dialysis ﬁnally caught up with him, and he passed away in 1997. Equally sadly, shortly thereafter, in 2000, Maureen was diagnosed as having ovarian cancer and, as a consequence, my hours at Einstein became truncated. I began to tool down, allowed my team time to disband, and ofﬁcially retired in 2008, hanging around part time until 2012. Maureen fought her disease like a tiger, but after a 13-year battle, she succumbed in March 2013. Only 7 months earlier, we had moved from New York to southern New Jersey because
Maureen wanted to participate as a surrogate grandmother in the growing young family of Sara Prineas (John and Eileen having returned to Australia in 1999), a pediatrician, and her husband, Jim Wurzer, an oncologist. Maureen lived long enough to see Sara’s sixth child arrive but was never well enough to benefit from the joy these children would have given her. Looking back, I owe Maureen so much in addition to the 50 years we were happily married; it was entirely because of her support that I was able to make it along the road to Neuropathology (Fig. 17).

Fortunately, as a professor emeritus, Einstein has graciously permitted me to retain my office, which I visit sporadically, and a part-time secretary (Pat), with whom I am in daily contact. There are several yet-to-be-completed collaborative studies on my desk, manuscripts arrive regularly for review, colleagues send me slides and images for interpretation, letters of recommendation are still needed, and I am beginning once more to attend scientific meetings. So, if you see me on the roadside, do pull over and say “Hello.”