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Obituary

G.H. Neil Towers (1923–2004) Phytochemistry Pioneer – In Memoriam

(Photograph courtesy of Laurence B. Davin.)

The 2006 PSNA annual meeting banquet honored the extraordinary and colorful scientific life of Professor G.H. Neil Towers, in which his widow, Elizabeth, was in attendance. The Powerpoint collage showed Neil in his teens, snapshots of his stint in the Royal Navy, various stages covering his scientific career and scientific pursuits, and a celebration of memories from Eloy Rodriguez with a recording of Neil singing in the background. It was a wonderful evening to remember a very close friend and scientific colleague.

I first met Neil in 1973 while a graduate student at the University of British Columbia where we discussed science generally on late Friday afternoons fortified with adult beverages for clearer thought! Our friendship lasted throughout the next three decades until his untimely passing, and our joint interests took us all over the world. The photograph above is taken on the day of my wedding, with Neil previously having given the bride (Laurence) away, and where he has now ''borrowed'' the ceremonial bearskin hat from the piper during the reception following.

Below, we have reproduced Neil's essay ''Some Memories of a Budding Scientist in North America,'' as well as the tribute to him of the ''Farewell to Neil Towers'' that appeared in the PSNA Newsletter. Both give a fleeting glimpse of Neil's natural curiosity and insatiable appetite for the (healing) world of phytochemicals, and for the enormous range of collaborations, joint studies, and friendships he had with so many of us.

It was also a privilege to scatter some of his ashes last year in different parts of the globe in remembrance of this extraordinary world traveler and scientist. Two years later, both his memory and spirit remain very much alive.

Some Memories of a Budding Scientist in North America by Neil Towers

[Reprinted with permission from the PSNA Newsletter]

I grew up in Myanmar (formerly Burma) when it was a British colony. My parents sent me to boarding schools around the country run by Christian brothers who sadly lacked an interest in the natural sciences, particularly natural history. Living and traveling as a schoolboy in perhaps one of the most beautiful tropical countries on this planet, I developed a craze for natural history. I collected snakes, beetles, butterflies, dissected animals for parasites and tried to identify plants from books. It was a happy boyhood. On reflection I think I was lucky not to have lived in our computer and television age. I did not see a television program until I was about twenty two! I spent all of my holiday time escaping prayers and wandering through the enchanting

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countryside exploring nature. I was spellbound by the travels, adventures and ideas of Darwin, Wallace, Bates and many other famous explorers. That is exactly what I wanted to be. World War II intervened.

I came to Canada on a scholarship for ex-naval officers at the end of the war. I had many adventures during the war, quite a number of which would have been called unforced errors of life were they to be compared to a game of tennis! Having escaped from the Japanese and winding up in England and then Canada, my life changed and I was suddenly plunged into the cloisters of academia. My sunny days of adventure were over-perhaps forever.

I was saddened to find that there were very few enthusiastic natural historians in this new life in a university. My fellow undergraduates in fact never seemed to have had time to talk about the excitement of biology they were so busy cramming for exams. I found out also that the world appeared to have been already explored by my arrogant zoology instructors and there was little new to discover other than to climb very tall mountain peaks or dive deep under the sea. I was an Honours Zoology student at McGill University in Montréal at the time and was advised by zoologists that the secrets of the animal world really lay in the realm of statistics! Even genetics was all statistics according to them.

Botanists, in contrast, were fascinated by apparent trivia: they were excited by the shapes of leaves, the hairiness of plant structures (for which there are many unpronounceable names) the geometry of flowers and a phenomenon called 2N versus N. However, these botanists seemed to love what they were doing and I was encouraged to join their ranks. They actually worked with their microscopes in the evenings. They suggested to me that the inner workings of plants e.g. how sugars are manufactured from a gas in light was irrelevant and for Heaven's sake don't spoil things by dragging chemistry into the picture in order to understand how a plant lives. Of course, electron microscopy, the role of nucleic acids, the nature of enzymes, etc. were not even dreamed of at that time. Professor R.D. Gibbs, a feisty botanist at McGill, kindled my interest in plant chemistry. He was considered a crank by other botanists as I found out later because he was fascinated by the chemical relationships between plants. In fact he was a chemotaxonomist at a time when chemists did not know the meaning of the word taxonomy and a botanist might have been embarrassed if accused of understanding anything about chemistry.

Here was a botanist who actually knew some phytochemistry and, Good Lord, this chap could actually draw chemical structures! We became good friends and I obtained an M.Sc. under his supervision. The research involved the chemotaxonomy of plant lignins and was published in Nature. Perhaps I was the first person at McGill to use the new technique of paper chromatography. Certainly, the chemists and biochemists at McGill seemed to be as yet unfamiliar with the use of this technology. Later on during a sabbatical leave with the enzymologist D.D. Davis at the University of East Anglia, I used to drink beer every afternoon with Dick Synge, one of the discoverers of paper chromatography, a Nobel Laureate, a seasoned beer drinker, and a very dangerous cyclist (after drinking of course). Curiously Gibbs suggested that paper chromatography would never work and that I should use fractional sublimation instead to separate my products of alkaline nitrobenzene oxidation, namely *p*-hydroxybenzaldehyde, vanillin and syringaldehyde. Clearly this was not good advice because his total NRC budget for the year was about $115! Gibbs was old fashioned by current standards. It was good for me. It made me more of an independent scientist. When I asked him if he would be kind enough to read a draft of my thesis he was astounded. "I am here to examine you Towers, not to help you. It is your thesis – not mine!" Nowadays, of course, there are many rules in Canadian universities to chaperone graduates in thesis writing so that in the end I feel we sometimes produce the well-known camel instead of the desired racehorse.

I went to Cornell for my Ph.D. studies with Professor F.C. Steward, a distinguished English plant physiologist who boasted that he had never taken a course in botany in his life. Needless to say he thought that he was the founder of botany. He had achieved fame for his work on ion accumulation in plants. His students called him the Golden Bantam because he was small, a fearless fighter, and rather arrogant. He once told me that he regretted the fact that dueling was no longer encouraged as a means of resolving departmental quarrels among faculty. He meant it!

Steward had become interested in the use of paper chromatography for separating and identifying the many unidentified non-protein amino acids in plants. My Ph.D. thesis was concerned with designing new methods for the identification of α-keto acids in plants. Quite boring actually. It was a wonderful period of study, however, because Steward had attracted extremely knowledgeable postdocs, such as John F. Thompson, and clever graduate students to his lab. He was also a research supervisor who was so busy chasing research dollars that we had complete freedom in our own programs. I think that this is still the case in many universities.

At Cornell I took Botany and Biochemistry and a course on enzymes by J.B. Sumner. After many years of tedious research Sumner had discovered that the fewer steps involved, the better were his yields of the hydrolytic enzyme urease which he was studying in *Canavalia ensiformis* (Jack bean). In fact, he discovered one day that a 32% acetone extract heated to about 60 °C, filtered overnight through Whatman paper into a graduate cylinder, and placed in a refrigerator yielded a precipitate which, when examined under a microscope, was found to consist of "octahedral" crystals. The crystals had tremendous urease activity. Repeated analyses showed that it was a protein. This was in 1926. Sumner wrote in his lab notebook about this momentous day: "That night I slept but little". At that time of course the true nature of enzymes was unknown and the leaders in the field, among them the very famous

German biochemists, Willstatter and Waldschmidt-Leitz, refused to acknowledge that a 26-year-old American had actually isolated an enzyme and proven that it was nothing more than a protein. His discovery was treated with some ridicule which unfortunately made him rather bitter. Four years later when Northrup crystallized the proteolytic enzymes pepsin and trypsin from animal sources at the Rockefeller Institute and showed that they are also proteins, Sumner's achievement was acknowledged – they shared a Nobel prize. We had the privilege of repeating Sumner's work in our laboratory course and even of recrystallizing urease. Of course, like Sumner, we made the entry "That night I slept but little" in our laboratory notebooks.

I was offered a job as Assistant Professor in the Botany Department at McGill and assigned to teach plant anatomy, plant physiology, plant biochemistry and help run introductory botany labs. I inherited an old physics lab which could only be accessed through a men's urinal, a bit of an annoyance to my women graduate students. After four years of working in this "lab" we discovered an open pool of about 40 kg of mercury under the wooden floor. It must have been "lost" by the physicists during their war research years. Also contributing to the poor working conditions were the feral cats that had taken up residence in the dark corners of this medieval set of rooms, occasionally emerging to produce a litter of young kittens on our chromatograms which had to be stored on the floor. The lab was cold and we often had to use gloves and overcoats to stay warm during the winter months. At that time, university startup money for research was unthinkable. Besides, there were tons of microscopes and herbarium sheets lying around. What more could a botanist want? Roy Waygood, the plant physiologist at McGill was most encouraging, allowing me access to his lab equipment and his knowledge of plant physiology and biochemistry.

Among the many wonderful graduate students in my lab was our illustrious PSNA stalwart Ragai Ibrahim. Seichi Yoshida of Tokyo Metropolitan University also joined me as a postdoc. Later his student, Minamikawa, joined my lab and much later on Minamikawa's student, Etsuo Yamamoto, came to my lab as a postdoc. That's three generations of great Japanese phytochemists! We spent a lot of time making 2D chromatograms of plant extracts, cutting out spots, eluting them, and so on. I remember my eight year old son spending an afternoon in my lab. After watching me for half an hour he asked "What are you going to be when you grow up Dad?" It seems that in his eyes I have never really grown up.

I was delighted to learn about the Birch and Donovan hypothesis in relation to flavonoids. Instead of being neatly derived (on paper) from two hexoses and a triose according to Geissman and Hinreiner, they now appeared to be derived from a hydroxycinnamate and acetate! We resolved to test this with the dihydrochalcone glucoside, phloridzin. Alas! We were beaten by Neish's group at the National Research Council (NRC) of Canada in Saskatoon who proved this hypothesis with quercetin, and Grisebach's group in Germany who proved the hypothesis with an anthocyanin. It seems silly now but we were dreadfully disappointed not to have been the first to have proven that Birch and Donovan were right.

I spent a summer at the NRC laboratories in Ottawa with D.C. Mortimer and Paul Gorham, learning radiotracer techniques and carrying out ^{14}C photosynthesis studies. The following summer I spent with Stewart "Coumarin" Brown and Arthur Neish at the NRC laboratory (then called the Prairie Regional Laboratory) in Saskatoon studying coumarin biosynthesis. When I returned to McGill at the end of the summer Ibrahim and I prepared two-directional chromatograms of the phenolic acids from a range of plants. Sprayed with diazotized nitroaniline or diatized sulfanilic acid they gave a range of beautiful colors. We had a special room set up with these large chromatograms adorning the walls for participants of the IXth Botanical Congress which was held at McGill, the Université de Montréal, and Sir George Williams College (now Concordia University) that year. These chromatograms were works of art and admired by all who visited us.

After enjoying more than nine years at McGill I was invited by Art Neish to head up the Plant Biochemistry section of the NRC's Atlantic Regional Laboratory in Halifax, Nova Scotia to which he had been appointed Director. Neish was considered to be one of the outstanding phytochemists in Canada and I was delighted to join his institute as I had a great admiration for him as a scientist and also because I was jointly appointed as an Associate Professor to Dalhousie University in Halifax where Neish and I taught a course in comparative biochemistry. My graduate students at McGill accompanied me there and had the advantage of working in the well-equipped NRC labs and alongside distinguished Canadian scientists in chemistry (Gavin McInnis) and biochemistry (Leo Vining). We published many papers especially on the biosynthesis of interesting lichen compounds as well as on comparative phenylpropanoid metabolism in lycopods and fungi. We showed clearly that L-tyrosine is metabolized quite differently from L-phenylalanine, especially in vascular plants. Tyrosine is metabolized to acetate and its derivatives when introduced into plant tissues and phenylalanine is the gateway to phenylpropanoid metabolism. We also identified a new cyanogen from Taxus and studied its biosynthesis showing that both the nitrogen and carbon are derived from L-phenylalanine. We discovered psilotin, a glucoside derived from a hydroxycinnamate and one equivalent of acetate, in the primitive ferns *Psilotum* and *Tmesipteris*.

I next moved to the University of British Columbia in Vancouver as Head of Biology and Botany, an administrative position which tore me away from thinking time and plunged me into the petty life of administration in

a then impoverished Canadian university. After five years, a sabbatical leave in England where I studied enzymes with D.D. Davies at the University of East Anglia, convinced me to resign as Head and settle down again to research and teaching. As most biology students at our universities do not enjoy chemistry my classes were small and were therefore a great pleasure to teach. Many more graduate students and postdocs passed through my research program and it would take many more pages to describe our further achievements in phytochemistry.

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Farewell to Neil Towers (PSNA Newsletter)

We learned of the sad news that Dr. Neil Towers, Professor Emeritus, from the University of British Columbia died quietly in his sleep on the morning of Monday, November 15, 2004. Those of us, who knew Neil, were aware that he had experienced a serious life-threatening disease a couple of years ago and that he had recovered sufficiently to continue his research activities until very recently when other complications took him from us. Many of us who attended the 2004 annual meeting in Ottawa heard a visibly frail Neil give a remarkably lucid lecture that summarized various unique events that marked his life together with his well known and original contributions to the field of phytochemistry and its importance to human activities. I remember well the humanity and originality of the presentation together with Neil's tremendous enthusiasm for our chosen discipline of research. It did have a revitalizing effect on those of us who might have been experiencing frustrations with our own particular research projects.

Neil Towers was one of the founding fathers of the PSNA, the idea of which seems to have germinated in discussions held in Montreal in 1959 on the occasion of the 9th International Botanical Congress [Brown, 1992. Recent Advances in Phytochemistry, Volume 12 Phenolic Metabolism in Plants; Chapter 12, pp. 378–393]. The names of Stewart Brown, Ted Geisman, Gestur Johnson, Arthur Neish, Vic Runeckles come to mind when considering those early days and the efforts that gave rise to the first two day symposium of the Plant Phenolics Group of North America (PPGNA) that was held on August 31 and September 1, 1961! As part of the natural evolution to broaden the appeal of the organization and to expand its membership base, the PPGNA was formally and legally converted into the Phytochemical Society of North America on January 1, 1967. Neil Towers served the PSNA well where he was the only individual who served two separate terms as elected president in 1973 and in 1986.

It is with great sadness that we bid farewell to Neil Towers, but we are confident that he will remain in our memories through the several generations of people he has touched, including students that he trained into a way of life that is research and through his unique vision of the world that is apparent in his many research publications.

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