Queen's University Chemistry Department

Reminiscences and Recollections

Edited by R.J.C. Brown and E. Buncel

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Preface

The Department of Chemistry has undergone tremendous change since the end of World War 2. It was felt that the memory of a vibrant Department and those who were part of it, should be preserved before they are lost forever.

As a Millennium project it was decided to collect together and record some personal reminiscences of the Chemistry Faculty. The 50-year period since 1945 until 1995 was chosen for our account since the rate of change during the past five years has accelerated even more, with new faculty appointments and the plans for a New Building coming at last to fruition.

It is not our purpose to produce an official historical record, with statistics and official accounts of events. Rather, the intention has been to record our personal memories and impressions, our hopes and regrets.

Our Department can be justly proud of its achievements and its record in teaching of generations of students who have made tremendous contributions in society after graduating from Queen's. At the same time, our research programs have blossomed and we are now ranked as one of the top chemistry departments in the country.

This account, therefore, records our aspirations in both teaching and research. Since the accounts are personal, different faculty have chosen to emphasize different aspects of their activities. . The advent of large classes, the introduction of computer-aided teaching, the organization of departmental facilities such as NMR and other major instruments, the administrative structure of the Department and the University are some of the topics covered.

The programs offered by our department have also undergone changes. A program in Environmental Studies was spearheaded here and has now blossomed into a highly successful inter-faculty effort based in the new Biosciences Complex, with major contribution from Chemistry including joint appointments.

We celebrate the achievements of the Engineering Chemistry program, honour those who passed through it, and give credit to those who devoted their energies over the years in nurturing it. The Engineering Chemistry program remains very much alive though its administration has passed to the Chemical Engineering Department. We continue to be involved in teaching of students in this program. At the same time, new opportunities

for collaboration between our two departments have opened up, which bodes well for inter-disciplinary research in the future.

Preservation of the memories of those colleagues who have passed on is an important part of this account. We have attempted to tap the memories of any individuals who were personally acquainted with these former colleagues, as well as the recorded materials in Queen's Archives. Our account is necessarily incomplete, for which we apologize. An account by Professor Roy Dorrance, which appeared in the Journal of the Royal Institute of Chemistry in 1957, covers the period since the founding of Queen's.

A few of our present faculty were unable to contribute to this account. However, we view the present account as an interim one and hope to fill any gaps in another edition in the not-too-distant future. In the meanwhile, a departmental brochure on the research activities of all current faculty is available.

The project has been warmly supported by the Head of the Department (1995-2002), Stan Brown, and the Current Head of the Department, David Wardlaw. Thanks are due to the many individuals who have freely given us information on events in the 1950's and 60's. The assistance of Mr. George Henderson, the Queen's Archivist, is warmly appreciated. Thanks are also due to the Royal Society of Canada for permission to reproduce the biographies of Professors J.K.N. Jones and L. Munro from the Transactions of the Royal Society.

Julian Brown,
Emeritus Professor

Erwin Buncel,
Emeritus Professor

Contributions

ROY L. DORRANCE

1920-1961

Professor Roy L. Dorrance, B.A. (Toronto, 1915) was appointed Lecturer in Chemistry in 1920 after serving in the Chemistry Division of the Civil Service, Central Experimental Farm, Ottawa (1916 - 20). There he participated in studies on the deposition of atmospheric nitrogen compounds in rain and snow, and on the composition of bran and shorts milled under the regulations of the Canada Food Board. At Queen's Dorrance initiated some electrochemical studies, an area in which he published several papers (1928 - 37), including one on the teaching of electro-chemistry, in the Journal of Chemical Education (1930). He also wrote an account of the development of the Department of Chemistry for the Journal of the Royal Institute of Chemistry (1957).

Professor Dorrance taught General Chemistry to the whole of the first year Applied Science - two sections with 100 students in each - in the large lecture theatre in old Gordon Hall (demolished during the renovation of Gordon Hall in 1961). He also taught Analytical Chemistry to third year students. His text "Experiments and Problems in General Chemistry", first published in 1943, underwent many editions, the last in 1961.

Roy Dorrance died in an automobile accident on 22 June 1961, after 41 years of service. He had just been appointed Vice-Chairman of the Chemistry Department. Professor Munro in his report to the Faculty Board included the following testimonial of Roy Dorrance:

"His special interest was teaching; his firm conviction being that instruction of the first year was MOST important and deserving of the best efforts of senior staff. For many year he was a member of the Examining Board of the Ontario Department of Education for Grade XIII Chemistry. His intimate knowledge of the high school course in Chemistry was used to help the first year student make the required adjustment to university courses and methods. His honorary membership on the executive of various class years attest the appreciation of the students.

During his long association with Queen's he acted as chief proctor and served on senate and various committees of this faculty.

I move that the faculty record its grateful remembrance of a faithful and devoted teacher, Roy L. Dorrance and that copy of the minute be sent to his son, Mr. Graham Dorrance."

E.B.

JOHN A. McRAE
1921 - 1956

John Alexander McRae graduated from Queen's with an M.A. degree in 1909 and in the same year he joined the University staff as lecturer. During 1910-11 he held the same postion at the University of Toronto but returned to teach at Queen's the following year. The years 1919-21 were spent at the University of Manchester where McRae earned the Ph.D. under the tutelage of Professor Lapworth who had by that time distinguished himself for research in organic chemistry. Those years were very formative for McRae; he became acquainted with the work of such luminaries as Perkin, Wallach and Ingold (a hand-written letter from Sir Christopher Ingold to McRae, dated 12 April 1921, is in the possession of Marie Moir). Manchester University subsequently awarded Dr. McRae the D.Sc. degree for research accomplishments. Following the years at Manchester, McRae returned to Queen's where he rose through the ranks, becoming Head of the Department in 1940, a position he held until his retirement in 1956 when he became Emeritus Professor.

Professor McRae was an enthusiastic and highly effective teacher of organic chemistry. His enthusiasm for research carried over to the students and a continuing stream chose to study with him in graduate programs. At least 50 M.Sc. theses were accredited to him over the years. Most of the graduates went on to doctoral study in Canada and the U.S. and became eminent scientists in their own right. The results of their research were published in numerous papers in the Canadian Journal of Research, The Journal of the Chemical Society (London) and The Journal of the American Chemical Society.

Dr. McRae was elected Fellow of the Royal Society of Canada in 1938. He was also Fellow of the Royal Institute of Chemistry, and a Fellow of the Chemical Institute of Canada.

On his retirement in 1956 Dr. McRae was honoured by Queen's with the degree of Doctor of Laws. The citation read: "distinguished graduate of this University, for forty-four years a member of its family, a scientist whose contributions to organic chemistry have brought respect for his high standards, a teacher whose thoroughness asked no concessions from the exacting discipline of his subject, a colleague as generous in his loyalty as he has been laconic in his comment."

In memory of Professor McRae's outstanding contribution to the development of chemistry at Queen's, his former students established a fund to provide for a "McRae Memorial Lecture."

The first McRae Lecturer was R.H.F. Manske (a former student of J.A.M., mentioned elsewhere in this account). Subsequent McRae Lectures included Kurt Mislow (Princeton), Saul Winstein (UCLA), Herbert Brown (Purdue, Nobel Prize medalist), Karel Wiesner (UNB), James Kutney (UBC), Ronald Breslow (Columbia), Stephen Withers (UBC) and Anthony Kirby (Cambridge, UK).

Professor McRae died on 8 April 1960 after 47 years of service. He was a teacher who left a deep and lasting impression on his students.

E.B.

GRENVILLE B. FROST

1924-1961

Grenville Frost was educated at the University of Toronto and after graduating with a B.A. went on to the University of California where he worked under the famous G.N.Lewis, the "father" of the electronic structure of the atom. Dr. Frost was appointed Lecturer at Queen's in 1924 and Full Professor in 1944. He became Head of the Chemistry Department in 1956 and served in this post until retirement in 1961. He died in 1963.

Professor Frost taught General Chemistry to students in the 3rd year and Physical Chemistry to the 3rd and 4th years. He was especially effective as a teacher and well liked by students (see Bob Bannard's testimonial below). His area of research was thermodynamics, mainly calorimetry. Among his numerous students was the first Ph.D in the Department (H.G.McAdie, 1956). Our Wally Breck and Bob Wheeler were also his students. There are, indeed, many eminent scientists among his former students.

Dr. Frost supervised the renovation of Gordon Hall, completed in 1961. He also planned the new wing of the Chemistry building, which was opened in 1962 by Leslie Frost (his brother), the Premier of Ontario, and named the Frost Wing.

Professor Frost received the Chemical Education Award from the Chemical Institute of Canada in 1963. Following his death a Memorial Lectureship was established in his honour. Of the many eminent scientists who have given the Memorial Lecture, one can mention Professor Robert F. Curl of Rice University who was awarded the Nobel Prize in Chemistry in recognition of the discovery of fullerenes (Buckminsterfullerenes, or "buckyballs").

We think you will enjoy the letter that Bob Bannard sent to Ken Russell of his reminiscences of Professor Frost, dated 11 December, 1973. We thank Bob for allowing us to reproduce the letter.

"I was delighted to learn during my recent visit to Queen's that a Lectureship is to be established as a memorial to the late Prof. G.B. Frost. This is very appropriate since G.B. was highly regarded as a teacher and as an authentic interesting personality by his students.

I had the good fortune to have G.B. as a teacher for four courses, elementary physical chemistry and general Chemistry III in third year, thermodynamics in fourth year and statistical mechanics and more thermo in my graduate year.

I don't think there was much doubt that he was our favorite prof in third year. He seemed to have great facility in presenting concepts, and in fact, one of the difficulties with his courses was that he explained things so simply that one frequently failed to make adequate notes. He was particularly good in teaching General Chemistry III, which dealt with atomic theory, Werner coordination compounds etc. and it was certainly one of the most interesting courses in the whole B course program in those days.

G.B. made his lectures so interesting by interspersing the chemistry with numerous comments on politics (federal, provincial, university and departmental) and many other matters that nobody would have considered missing one of them. He was very relaxed and informal with students and frequently enjoyed smoking a big cigar as he lectured, stopping often to re-light it, for emphasis. In those days, smoking in Gordon Hall was completely verboten and I suppose we liked the fact that he was prepared to break the rules and to turn a blind eye when we did so.

We enjoyed his third and fourth year labs. He had them very well organized but was almost never to be found during lab periods. He obviously wanted us to be thrown onto our own devices and get on with the job ourselves. This was excellent preparation for graduate school and we knew that we could always fill a pipe and smoke it during one of his labs without fear of reprimand if he arrived on the scene.

In fourth year his thermo course was interesting but most of us found parts of it difficult to grasp. G.B. smoked lots of cigars in that course and as time for the final exam approached he told us that he always was in a more benevolent mood marking papers when he had a good cigar to smoke. There were about 20 students in the class and it was arranged among ourselves that we would buy 20 identical jumbo cigars and each enclose one in our examination book. G.B. was delighted, and the Proctors in Grant Hall wondered what was going on when the papers were handed in. We never did establish whether or not the marks were influenced by our attempted bribe.

In graduate school G.B.'s extension of the thermo course to statistical mechanics and reaction kinetics was absorbing but quite demanding. He had us use the library a good deal checking original papers and told us that he didn't see much sense in having a final exam, that what he really wanted was a lengthy term essay from each of us on some aspect of the course which interestcd us, using the original literature as our source material. However, university regulations demanded that we must have a final exam. He told us that he would have to set an exam and that we were to present ourselves at Grant Hall at the appointed time, put our names on the exam books, sit the minimum required time of 30 min and turn the exam books in.

We thought he was kidding us at first but he really meant it and the twelve students taking the course did exactly as he suggested. Can you imagine the consternation of the Proctors when we all got up simultaneously and turned our papers in? He thought it a terrific joke that he had been able to find such a simple mechanism of avoiding the exam.

In our final year we tried for weeks to get him to commit himself about attending the Science Formal, which in those days was held about mid-February. We thought he didn't really intend to come but after we dragged him over to the Gym to see all the work which we had been doing on the decorations he bought a ticket and duly arrived in tails with one of the waitresses from the United Cigar Store. Then he told us that he was the only Full Professor at Queen's who had taken two girls to the RMC Ball without either one knowing that he had the other with him. He did it on a bet and apparently was able to work the deception without any trouble.

The last time I met him was in the elevator in the Royal York in Toronto at the C.I.C. Conference at which he was presented with the Chemical Education Award. I told him how pleased I was that he had received some official recognition for his many years of excellent teaching, but in his characteristic way he said that there were many who deserved the Award more than he did, and that he had just been lucky.

I hope some of these reminiscences of G.B. Frost may be of some assistance to you. No doubt Wally Breck and Bob Wheeler will have many other sidelights on his character since they worked for him as graduate students."

E.B.

Lloyd A. Munro

1929-1967

Lloyd Alexander Munro, Emeritus Professor of Chemistry at Queen's University, was born in Pictou County, Nova Scotia in January 1899, and died in Brampton, Ontario in August 1987. With his wife Della, he had moved to Brampton from Kingston just a few months earlier, to be closer to the family of his son Douglas.

Although he was born in Toney Mills, his family migrated to Manitoba when he was quite young, and returned to Pictou County a year later to settle in Stellarton, where he was educated. After Stellarton schools he attended Pictou Academy, graduating in the class of 1917. He immediately enlisted and went overseas with the Canadian Signal Corps, returning in 1919 to enrol at Dalhousie University, from which he graduated in 1921. After receiving his Master's degree in 1922, he was awarded a series of scholarships by the National Research Council, which allowed him to enter a doctoral programme at McGill University. He completed this programme in 1925, in the laboratory of F.M.G. Johnson, Department of Inorganic Chemistry. At the age of twenty-three he was elected to the Nova Scotian Institute of Science, and his first paper, "A Study of Molybdenum Blue", was read to this Institute on January 8, 1923. This paper already contained the seeds of his subsequent scientific and collegial efforts: an interest in the history of chemistry, an enthusiasm for the creation and uses of colours, and a meticulous documentation of the experimental facts. Following the completion of his Ph.D, he joined the University of Manitoba as Assistant Professor of Agricultural Chemistry, and moved to Queen's in 1929, where he remained for 38 years, until his retirement in 1967.

Chemistry had been offered at Queen's, as a part of "Natural Philosophy", from the founding of the Institution in 1842. It became a separate subject in 1854 with the establishment of a Faculty of Medicine, and the first Professor was appointed in 1859. Subsequently a Chair of Chemistry and Mineralogy was created in 1882, as part of the School of Mining and Agriculture. Following an announcement in 1886 that a School of Practical Science was to be established in Toronto, a series of public and private efforts led to the formation of a Faculty of Practical Science at Queen's in 1893. From its inception the Faculty offered a four-year programme consisting of a common first year followed by specialization in a branch of engineering or natural science. To this day, Queen's maintains two parallel programs in Chemistry, one in Applied Science and one in Arts and Science. Because of the common first year, it was a particular challenge for skilled lecturers to try to inspire the best engineering students to follow in their own disciplines.

For many years this responsibility fell to Munro, and led to huge numbers of chemistry and engineering chemistry students in the upper years. He earned the lifelong respect and affection of his students through his mastery of lucid expression, Scottish economy of words, sense of humour, and innate empathy and understanding. Many wrote him with surprise and pleasure following the receipt of book prizes for particular accomplishments in his courses. Towards the end of the 1950's he began to place his ideas on the teaching of chemistry to engineering students on paper and, in 1964, completed his widely acclaimed text "Chemistry in Engineering". Within a short time this text had passed through ten printings, been translated into several foreign languages, and adopted by Universities throughout North America, including the hated University of Toronto.

The formal organizational structure of Chemistry in Canada dates from 1902, when the first Canadian section of the Society of Chemical Industry was organized in Ontario. In 1919, the first Dominion Convention of Chemists was held in Ottawa. This led, two years later, to the chartering of a Canadian Institute of Chemistry. However, the Institute encountered constant organizational and financial problems, because of the continued existence of other, rival, bodies. At the Kingston Convention in 1935, Munro had participated in the organization of a symposium devoted to the problem, but the Chemical Institute of Canada, as we know it today, did not become a single national organization until January 1945. It was quickly recognized that the unification would benefit greatly from the creation of a national journal and newsmagazine, and this task fell to Munro. Almost single-handedly, he undertook the work of assembling, editing and publishing the "Chemical Institute News" during the critical formative period from June 1944 until June 1946, when he became Chairman of the Editorial Board. The journal was renamed "Chemistry in Canada", at Munro's suggestion, in 1948. He continued to serve until 1964 as, variously, Chairman of the Journal Advisory Committee, and Chairman and member of the Editorial Board of "Chemistry in Canada".

At the outbreak of the Second World War he again volunteered to serve his country, and was appointed to the Army Reserve with the rank of Captain. Soon afterwards, he was recruited by Otto Maass for chemical warfare research, which continued until late 1945.

For his contributions to his profession and his country, he was elected to Fellowship in the Canadian Institute of Chemistry in 1937, to Fellowship in the Royal Society of Canada in 1947, and received a Centennial Medal from the Government of Canada in 1967.

During his active scientific career, Munro published some three dozen papers on the chemistry of gels, colloids and resins and on chemical catalysis.

As consultant to the Hendry-Connell Research Foundation, he carried out investigations into the use of colloids in the detection and treatment of cancer.

A seminal 1944 paper in the Journal of Physical Chemistry is entitled "Protective Colloids in Cancer". His enthusiasm for colours was manifested in his garden, and his interest in the history of chemistry was expressed through a series of articles and public lectures on the subject of Chemists and Chemistry on Stamps. At the time of his retirement he was asked to comment on his professional philosophy. His reply was succinct: "...to accept facts and be absolutely honest in interpretation of research". As we approach the twenty-first century, the openness and integrity that once characterized scientific discussion seems increasingly to have been replaced by a rush to publish undocumented and unreproducible claims. It is, therefore, useful to recall the simple credo of a man whose life and work spanned most of the present century.

Contributed by Saul Wolfe

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Walter Mac F. Smith

1945-1980

Walter Smith spent his early life in Fredericton; he graduated from the University of New Brunswick at the age of 20 in 1935. The following year, he published his first paper with E.W.R. Steacie at McGill and went on to obtain his Ph.D. in 1938. He was awarded an Exhibition of 1851 Science Research Scholarship and used it to do sodium resonance studies with the eminent photochemist, R.G.W. Norrish, at the University of Cambridge. His work was interrupted by the war and in 1940 he began armament research at Woolwich which he continued at Bournemouth. He completed his Cambridge Ph.D. in 1943 and returned to Canada with his English wife, Frances, in 1944. For the last year of the war, Walter was associated with the Canadian Armament Research and Development Establishment near Quebec City.

Walter joined the Queen's Chemistry Department as an Assistant Professor in 1945. At that time the whole Department was housed in Gordon Hall. He was soon immersed in teaching as a flood of ex-service men arrived at Queen's. He and senior colleagues, including Drs. McRae, Frost, Munro and Dorrance, looked after the class of 19481/2 in Applied Science along with their normal heavy teaching duties. Their students in the class of 1950 included ex-service men Breck and Wheeler. Some normality returned to Queen's in the early 1950's and the student body dropped to around 2,300 in 1954.

In 1948, the upper floors of Gordon Hall Annex were completed and Walter moved to a relatively spacious office-lab on the third floor. His teaching duties included lectures and demonstrating to first year Arts students (General Chemistry 1) and a fourth year course in Electrochemistry which he ran on the top floor of Gordon Hall. He lectured to all the first year Arts (and Science) students until 1962, but by then the University was well into its major expansion and he looked after only one of the sections. He continued lecturing to first year Arts and Science until he retired in 1980. His lectures were always extremely well prepared and carefully organized, so much so that one student remarked "Machine-gun Smith. Drop your pencil and you're two weeks behind". Some students had difficulty reading his handwriting on the blackboard and he was once given a Grade 2 book on "How to Write" as a Christmas present. He was nonetheless an extremely effective Chemistry lecturer and an obvious choice to give the more specialized Chem 116 course when it was introduced in Arts and Science. He also got a particular pleasure from his fourth year course in Reaction Kinetics which he gave for some 20 years.

He inspired many able first year students to take Honours Chemistry and he provided a firm foundation in kinetics for those going on to graduate work. He never missed a lecture in his 35 years of teaching at Queen's.

The University underwent considerable expansion in the early 1960's and major changes came to the Chemistry Department under its new Head, Bob McIntosh. Bob appointed Walter Chairman of Undergraduate Studies and Walter quickly became so good at this that successive Heads left him to look after this part of the administrative work in the Department until he retired. He enjoyed advising undergraduates and it gave him an opportunity to use the great storehouse of knowledge he had built up concerning all departments in the Faculty of Arts and Science. He took on the additional responsibility of Acting Head from 1969 to 1971, but he was much relieved when the new Head was appointed and he could spend more time on his preferred job as Chairman of Undergraduate Studies in the Faculty of Arts and Science.

Walter's early research at Queen's involved mechanisms of energy transfer from gaseous alkali metals to various substrates. This work built on the sodium resonance radiation studies he had made at Cambridge. He spent the 1956-57 academic year on sabbatical leave at Oxford working in R.P. Bell's laboratory which reinforced his major interest in the kinetics and mechanisms of rapid reactions in solution. He and a succession of graduate students which included Harry McAdie, Gerry Davis, Dirk Pouli, Rudi Bauer, Raj Pandey and Mohan Sanduja made in-depth studies of the rates of association of ions in solution. Walter's elegant work has stood the test of time and his analyses are still referenced in major reviews. A Nobel laureate once commented that Queen's had so overburdened Walter with teaching and administrative duties that he never became the research star he deserved to be. Be that as it may, Walter enjoyed his work in and out of the Department and rarely complained about the responsibilities that came his way.

Walter made good use of the opportunity that Queen's provided to pursue interests in the arts, theatre and music. Before 1960, the University was small and Walter and Frances knew almost all of the members of staff in the Arts and Science Departments; there was also a strong connection with the Royal Military College. Social gatherings might bring together philosopher English and history professors, the resident artist and the resident musician, chemists and physicists among others, with the result that stimulating conversation was the norm. Frances became Curator of the Art Centre and Walter grew to share some of her passion for art. Their older daughter, Janet, took Honours Chemistry giving Walter some new insights into the Department; Janet went on to high office in NSERC and the Science Council of Canada.

Their younger daughter, Sheila, also graduated from Arts and Science; her husband is now on the Board of Trustees.

Walter retired in 1980 after a lengthy career at Queen's, 19 years as a full professor. He was part of a transition that took Queen's from being a small, largely undergraduate, university to one of the best all-round academic institutions in the country. He took home many of the records of his work at Queen's but he wisely took advantage of his freedom to follow other interests, including his garden and wines, and to travel with Frances.

Contributed by K.E. Russell

Robert Y. Moir

1949-1985

Professor Emeritus Robert Young Moir was born in Estevan, Saskatchewan in 1920 and died on February 13, 1996 in Kingston after a distinguished career at Queen's where he taught chemistry since 1949 to generations of students.

That young Moir was destined for chemistry was apparent at an early age; he prepared indigo from benzene in the laboratory his father (Robert C. Moir, teacher and later School Principal) had built in Govan where the family moved when RY was 7 years old. Robert Moir left his native Saskatchewan for Queen's in 1937 at the age of 16 with the Governor-General's Medal for the highest provincial standing. He graduated in 1941 with the Medal in Chemistry and the Prince of Wales' Prize for highest standing in the Faculty of Arts and Science. He received the M.A. from Queen's in 1942 in Organic Chemistry under the direction of Professor J.A. McRae and the Ph.D. from McGill in 1946 under Professor C.B. Purves. His first employment was at the Dominion Rubber Company (now Uniroyal) in Guelph, which at that time was a centre of research activity under the direction of R.H.F. Manske (a Queen's graduate), a world authority on alkaloids. The microanalyst at the Dominion Rubber Company was an attractive young lady with a beautiful singing voice and when Bob accepted the invitation to join the Chemistry Department at Queen's in 1949, he brought Marie with him as his bride. Marie then traded microanalysis for teaching in the Music Department at Queen's.

During his career at Queen's Professor Moir taught Freshman Medical Chemistry, First Year General Chemistry, Introductory Organic Chemistry, and Intermediate and Advanced Chemistry. For the period 1957-1961 he taught all the undergraduate organic chemistry given at Queen's to students in Arts and Science, Chemical Engineering and Engineering Chemistry. As well, for the period 1949-1953 he taught all the graduate organic chemistry given at Queen's. He continued teaching organic chemistry at all levels until his retirement in 1985. From 1985 until his death he remained active, collaborating with colleagues in the Department of Chemistry in research and the supervision of graduate students.

Bob Moir was a highly creative and original thinker. Rather than being a narrow specialist as most of us are these days, Bob's knowledge ranged far and wide. In fact, he was at the forefront of some of the most important discoveries over the past 50 years. He was one of the first organic chemists who came to understand how nuclear magnetic resonance could be applied to solve complex problems of structures of molecules.

This landmark discovery was first disclosed by Bob in front of a huge audience at a meeting of the American Chemical Society, and published in the *Journal of the American Chemical Society* in 1958, together with Professor Ray Lemieux, one of our great Canadian organic chemists.

Bob was also at the forefront in the application of computerized techniques for solving problems in chemistry, always writing his own computer programs. During his career he supervised the research programs of 40 M.Sc. and doctoral students and published numerous papers in scientific journals, the last one in 1995 just a few months before his death, in the *Canadian Journal of Chemistry*. His graduate students now occupy prominent positions in academia, industry and government throughout the world. They consider him the personification of those ideals and attributes which we ascribe to the finest traditions of Queen's.

Bob Moir loved teaching and had the ability to elicit the best performance from his students because of his very friendly and caring nature. His door was always open to those who needed assistance of any kind, and he saved many from disaster by his sympathetic and caring advice. He constantly challenged his students to strive for excellence. In his words, he aimed "to set minds free". He was held in very high esteem by his students who remained very faithful to him. To quote one of his former students, writing to the *Queen's Alumni Review* in 1987:[1]

> "Chemistry had never held much appeal to me, but in 1972-73 I had the good fortunate to have Dr. Moir for Organic Chemistry. He was a wonderful teacher - he opened my eyes to the magic there is in chemistry, he showed us that chemistry was an art form, not just another science.... I now realize that Dr. Moir "set free the minds" of many people, the effect of which will last our life time. Many professors are able to guide brilliant students. But only the very rare teacher can capture the interest of the "common" student such as myself, and show that we are capable of accomplishments that we would never have thought possible."

Professor Moir's contributions as an outstanding chemistry teacher were recognized by the Chemical Education Division of the Chemical Institute of Canada, which selected him to receive its prestigious Union Carbide Award for Chemical Education in 1984.Bob shook up many of the views I had on chemistry. When I first came to Queen's I was ready to accept the simple definitions that I found in organic texts and I was surprised that Bob found these wanting. But in the coming years that was probably the most important thing I learnt, to question current ideas, accepted thought, and I have to thank him largely for that. I had the good fortune to collaborate with Bob in the teaching of Chem.280, Chem.481 and Chem.980 and the supervision of graduate students for many years, well into his retirement.

Typically, he would happily delve into the most complex kinetic problems; his insight and wide knowledge were quite phenomenal.

The Chemistry Faculty had great affection for Bob, and much appreciated his special sense of humour. Following his death, the Robert Y. Moir Scholarship in Chemistry was founded in his memory.

This testimonial to Bob has benefited greatly from the insightful comments of many colleagues, especially Robert Bannard, a close friend as well as scientific collaborator of Bob for close to 50 years, Frank Maine and George Neville[2], two of Bob's eminent graduate students, Bert Fraser-Reid as a student at Queen's came to have great respect for Bob whom he regarded as mentor, Victor Snieckus who was lastingly inspired by a lecture he heard Bob deliver, and Saul Wolfe who interacted with Bob almost daily for 25 years and had great admiration for Bob's skill and patience as a teacher and the intensely critical way in which he analyzed his own research and that of others.

Our tribute to Bob Moir would not be complete if one did not mention his love of nature, be it the green leaves with the chlorophyll working at fixation of CO_2 and synthesis of sugars, or watching and feeding birds, or gazing and photographing the stars at night, with his telescope and a simple camera attachment with which he nevertheless managed to capture the Milky Way and far away galaxies. He was a kind and a good man.[3]

Erwin Buncel

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[1]Dr. Cathy Younger-Lewis (née Laird), Med's '77, Arts '79, letter to *Queen's Alumni Review*, dated October 4, 1987.

[2]Dr. George Neville authored a delightful article on Bob Moir, following the retirement of RY, in *Queen's Alumni Review*, May-June 1987. Dr. Robert Bannard also wrote about his association with Bob in that issue.

[3]For an excerpt from a tribute given by EB at the Memorial Service in memory and celebration of Robert Y. Moir's life (May 4, 1996), see *Queen's Alumni Review*, June-August ,1996.

J.K.N. Jones

1953-1977

John Kenyon Netherton Jones was born in Birmingham, England, on 28 January 1912. All his university education was obtained from Birmingham University. There he completed his studies towards the B.Sc. degree, with first class honours, in 1933 and received the Frankland Medal. In 1936 he received the Ph.D. degree, having worked under the supervision of the illustrious team of Sir Norman Haworth and Sir Edmund Hirst, and in 1948 he received a D.Sc. He was assistant lecturer and then lecturer at Bristol University from 1936 to 1944. During the Second World War he was engaged in munitions research and training. At the end of the war he resigned with the rank of captain and returned to academic work as senior lecturer at Manchester University from 1945 to 1948, and reader in chemistry at Bristol University from 1948 to 1953. In 1953 he assumed the position that he held until his death on 13 April 1977, as Chown Research Professor of Chemistry at Queen's University in Kingston, Ontario.

One of the most impressive aspects of Professor Jones's massive contributions to carbohydrate chemistry was the universality of his interests. His earliest publications (with W.N. Haworth and E.L. Hirst) were concerned with ascorbic acid (vitamin C) and its analogues. In 1937 the rights to a patent (with W.N. Haworth, E.L. Hirst, and F. Smith) on the oxidation of L-sorbose with nitric acid to L-ascorbic acid were sold for a return of £100 sterling to each co-author. When E.L. Hirst was appointed to the chair of organic chemistry at Bristol University in 1936, he brought with him from Birmingham J.K.N. Jones to be part of the nucleus of his carbohydrate research group. The close association of Professor Jones with Professor Hirst, which continued until 1948 at Manchester University, was a tremendously fruitful one; over fifty joint publications resulted from their research on complex polysaccharides such as plant gums, mucilages, and starch. In addition to these pioneering studies on polysaccharide structure Professor Jones made major contributions to synthetic carbohydrate chemistry, stereochemistry, biosynthetic mechanisms and metabolism of carbohydrates, and the application of separational techniques such as paper and gas-liquid chromatography in the carbohydrate field. His extensive studies on the reaction of sulfuryl chloride with carbohydrates, for example, has made available a particularly effective procedure for the preparation of chlorodeoxy sugars, derivatives which have been found to be extremely valuable intermediates in the synthesis of a wide variety of rare sugars.

The results of his researches have been documented in over three hundred scientific publications.

Professor Jones's outstanding achievements in carbohydrate chemistry have been recognized by his receipt of numerous awards and honours. In 1957 he was elected a Fellow of the Royal Society of London, and in 1959 Fellow of the Royal Society of Canada and also the Chemical Institute of Canada. The Division of Carbohydrate Chemistry of the American Chemical Society presented him in 1969 with the Claude S. Hudson Award, and in 1975 he received the Anselme Payen Award from the Cellulose, Paper and Textile Division. In March 1975 he was awarded the third Sir Norman Haworth Memorial Medal of The Chemical Society (London). He served as a member of the editorial advisory board of *Carbohydrate Research,* and was a member of the board of advisers of *Advances in Carbohydrate Chemistry and Biochemistry.* In 1967 he was chairman and chief organizer of the Fourth International Conference on Carbohydrate Chemistry which was held in Kingston, Ontario.

As in the case of his association with E.L. Hirst, Professor Jones throughout his career attracted the close collaboration of a number of university colleagues. Thus, at Bristol University L. Hough collaborated with him, and in Canada at Queen's University M.B. Perry until 1962, and then W.A. Szarek from 1967 until Professor Jones's death in 1977.

Although he derived great pleasure from his work, Ken Jones did have a number of other interests. Together with his wife Marjorie (Ingles Noon), whom he married in 1937, he had an active interest in the cultural affairs of Kingston, such as the promotion of live theatre and the symphony orchestra. He was an experienced and extremely eager traveller. He had a great love of the outdoors and took much pride and joy in cultivating and displaying his flowers and garden at his beautiful home on Treasure Island on the St Lawrence River.

Professor Jones was, at all times, an educator of the highest rank and an inspiration to a large number of graduate students, from whom he evoked, as a result of his enthusiasm, sincerity, and gentle character, tremendous respect and affection. All of his students, former research associates, university colleagues, and friends will long remember this truly fine and outstanding gentleman.

Contributed by Walter Szarek.

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Ken Russell
1954-1990

After taking my M.A. and Ph.D. at Cambridge in 1948, I was appointed Assistant Professor at Pennsylvania State University (1948-50), Assistant Lecturer at Manchester (1950-52) and Fellow at Princeton (1952-54). During this period, I gained research experience in cationic and free radical polymerization, energy transfer using flash photolysis, low temperature thermodynamics and infrared and Raman spectroscopy. I lectured at Penn State and Manchester and demonstrated in physical chemistry laboratories.

I decided to come to Canada in 1954. There were however no openings in physical chemistry at well known Canadian universities – just a position at the lecturer level at Queen's. The Head, Dr. J. A. McRae, had a D.Sc from Manchester and my two years in the Lapworth laboratory clearly impressed him. I was not excited about some aspects of the move. Queen's was small (2300 students including 100 or so graduate students) and my salary was $4000. My office and laboratory were in Gordon Hall, a building virtually unchanged since it was built in 1910. The library, a small room, housed the departmental secretary and a few books and journals. Research facilities were meagre and the technical staff consisted of Paddy Doolan, the first year lecture demonstrator, and Norm Hyland, the storekeeper. I was however free to run my own research with a grant from NRC, and the staff, including the recently appointed Chown Research Professor, J.K.N. Jones, gave me a very friendly welcome.

Almost half of the students at Queen's were in Applied Science and 300 of them were in first year. I lectured to 100 of these in lecture room 310, Gordon Hall. The lecture material consisted of elementary inorganic and physical chemistry; the textbook by Sherwood Taylor, I had first used as a schoolboy in 1935. The lectures were livened enormously by the demonstrations of Paddy Doolan. He had learned a lot from A.C. Neish (Head 1919-1941) and was a great showman; the hydrogen lecture included the hydrogen oxygen explosion in a quart milk bottle. The reaction was initiated by platinized asbestos, a catalyst also used in a demonstration of the contact process. I demonstrated to 25 of the students and marked their lab reports and problem sets.

My other duties included teaching organic chemistry to 50 second year premedical students. I gave 2 lectures a week and with Norm's help ran two 2-hour laboratories acting as both demonstrator and marker. I also demonstrated for three hours a week in the second year analytical laboratories. I introduced a graduate course in polymer chemistry in 1955 and enjoyed giving variations of this to graduates and undergraduates over the next 35 years.

With 17 contact hours a week and a lot of marking, I had little time for research. However, there were always the summers, and a succession of M.A., M.Sc. and summer students, including Jack Hazell and Gary Harpell, did some remarkably good free radical research. I broadened my polymer experience by becoming a member of the NRC committee on High Polymer Research and, in 1959, Secretary of the Canadian High Polymer Forum. I was also Program Chairman for the highly successful 10th Forum in 1960, host of the International Symposium on Macromolecules in 1961 and Chairman of the 11th Forum in 1962.

J.A. McRae retired in 1956 and G.B. Frost took over as the Head for the next five years. G.B. handed a 3rd year course in General Chemistry to me. The engineering chemistry and physics students produced some very interesting essays as part of the requirements for the course but I was much relieved when Geoff Torrible came and introduced some challenging inorganic chemistry. I also joined G.B. as demonstrator in the 4th year physical chemistry laboratory. I introduced a vacuum line experiment just before the advent of our first glassblower. The experiment went well once it was known that a single 15-amp fuse supplied the power for a half dozen benches.

Research in the Department as a whole was in transition. The first Ph.D graduate (1956) was Harry McAdie; he worked on urea inclusion compounds with G.B. Frost. He was quickly followed by a number of Ph.D.'s from J.K.N. Jones's rapidly expanding carbohydrate group. Thanks largely to J.K.N., equipment began to flow in to the department - a liquid nitrogen plant (6L per hour) in 1956 and an IR spectrometer a year or so later. I ran the nitrogen plant for the first year or two and after selecting the Perkin Elmer 21, shared the responsibility of running it with Malcolm Perry. The University was beginning to expand but we were still graduating small classes in Honours Chemistry and the B course. The first graduate brochure (1957) showed the recent additions of R.A. Stairs, W.G. Breck and R.C.Wheeler to the staff.

In 1961, R.L. McIntosh breezed in from Toronto to be the new Head. He quickly expanded the academic and technical staff. By coincidence, the 1961 graduate classes in Engineering and Honours Chemistry totaled 38 students; many of these were first class and were persuaded to stay on for graduate work. R.L. organized firm financial support for these chemistry graduate students. We lost one or two able staff – Bob Stairs to Trent, Malcolm Perry to NRC – but by and large the staff chosen by R.L. remained to determine the course of the department over the next quarter-century.

The physical facilities were rapidly expanded. G.B. Frost had designed the Frost Wing in 1961. He had to fight hard to retain 4 laboratories on each of the three floors and initially the 2nd and 3rd floors were unfurnished. The rapid increase in size of the University – 3500 in 1961 to close to 10,000

at the end of the decade – led to rapid exploitation of the Frost Wing, renovation of old Gordon Hall including a greatly modified 4th floor and to 2 additional floors for the Frost Wing. R.L. used Wally Breck's size report to plan an even greater expansion into Nicol Hall and south towards Electrical Engineering but reality set in and a Senate cap of 10,000 students for Queen's ensured that this was not implemented.

I continued teaching First year Applied Science students until 1967. We introduced some more modern chemistry but I still used a lot of the demonstrations to which I had been introduced by Paddy. In 1967, I switched to Arts & Science and continued with Chem 012/112 for the next 11 years. We tried very hard to get to know our students and one year I brought them all home to dinner in groups of six. We often shared a class and I particularly enjoyed the years with Don Heyding as partner. The lectures and tutorials went very well; attendance at the latter was encouraged by the large boxes of cookies which were provided by our wives. We experimented with a class of 210 students in 1972/73 but there were problems both running the labs and remembering names, and I was glad to get back to normal the next year. One of the additional problems was the large number of letters of recommendation I wrote for the many bright students hoping to enter Medicine.

The 4th year physical chemistry laboratory moved to the Frost Wing in the 60's and gradually broadened in scope. Eventually, research projects were provided in the second term. We had long used first class undergraduates as summer assistants but now we had a new way to influence potential graduate students. I sometimes taught physical chemistry at the third (later second) year level but my main duties outside the 1st year were the polymer lectures now open to 4th year students.

My primary research interest in the early 60's was free radical chemistry and I used my sabbatical in 1964-5 to obtain ESR experience at Leeds University. I learned how to determine concentrations of radical intermediates in hydrogen abstraction reactions.

With the support from R.L. McIntosh here and Jeff Wan at NRC, I obtained an NRC grant for an ESR spectrometer. Bob had been successful in attracting Jeff Wan to Queen's and the instrument was soon well exploited. It was first installed in Dupuis Hall because of projected renovations in the Annex. It was decided however that two more floors on Frost were a better option than renovating the Annex, and the spectrometer and its users moved back to Gordon Annex.

My main use of the instrument (with Heino Lilles and Bob Parnell) was to make quantitative studies of phenoxy radical intermediates in polymer systems.

Investigations of cationic polymerization and related complexes, continued by Ron LaFlair and John Meyer in the early 60's, were given a

boost when Rudi Bauer returned from a spell at the Polymer Corporation to obtain a Ph.D. Rudi introduced me to NMR studies of Lewis acid complexes and to gel permeation chromatography and considerably improved our understanding of the role of the coinitiator in cationic polymerization. I became increasingly burdened with administrative duties and one year I chaired 6 committees including the newly formed Macromolecular Science Division of the CIC, Graduate Studies in the Department, Admissions in Arts & Science and the Salary Committee of the Faculty Association. I travelled a lot and it was difficult to find time for undergraduate and graduate students and my family.

The late 60s saw me with membership of Senate and the Council of the School Of Graduate Studies and with associated committee responsibilities. I was fortunate however to have a group of extremely able Ph.D. students. Bob Parnell from England put our concentration studies of free radicals on a firm foundation. Jim Flood from Scotland talked me into cooperative work with Dave Wiles from NRC on photochemical studies of polymer stabilizers. Lindy Chartier came from Montreal with a 1967 NRC scholarship. She made considerable advances in our understanding of cationic polymerization and also obtained a patent on a polymeric antioxidant. Hiroshi Furue from Japan developed a much deeper understanding of the quenching of singlet oxygen by polymer stabilizers. Their achievements gave a firm base to my NRC grant applications for years to come.

In the 70s the ACAP assessment took up much of our time. Initially it was a fair operation and we were relieved to be rated fourth in the province. Then politics took over. We also spent a lot of time discussing where the cutbacks should be made but I think the head was wise to educate us in this way. In 1976, Don Heyding took a 6 month sabbatical and I became acting head of the Department. I enjoyed this brief stay in the head's office even though it made me more aware of the sharpness of departmental politics. At the same time I was elected Chairman of the Faculty Board of Arts & Science. It was a great experience working with Dean Sinclair but once again I found myself overloaded with committee work. Added to this, Don Heyding asked me in 1977 to introduce a course on Industrial Chemistry in Canada as recommended by the Eng. Chem. Advisory Committee.

I was to bring together speakers from industry, at no cost to the Department, who would lecture to 3rd and 4th year Eng. Chem. Students on industrial processes and conduct informal evening seminars on current technology, research, economic factors determining scale and processes of the industry, etc. I was fortunate to get good speakers form Du Pont and Alcan and the petrochemicals/polymer segment of the course built up very well. Chem 408 was offered every other year, latterly with much assistance from colleagues in the department. My wife and I made many

friends among the visiting speakers, amply compensating for the extra effort required.

Nonetheless, I was glad to get away from it all during the 1978-79 academic year. We lived in Cambridge and I spent a number of weeks in Leeds exploiting Ayscough's flash photolysis/ESR apparatus. The year provided me with a much-needed opportunity to write up past work and plan future research. Before it ended I was back at Queen's on a visit taking part in a bid to NSERC to obtain a 200 MHz spectrometer.

The acquisition of the Bruker CXP-200 grew out of research at Queen's conducted by Brian Hunter for Murray Phibbs of Du Pont Canada. The grant application took advantage of the interest of NSERC in industry – university cooperative research. The NMR experts at the forefront of the application were Brian Hunter and Dave Axelson (then at Du Pont). As chief (and only) polymer chemist, I promised to initiate a program on the chemistry and physics of model copolymers of ethylene and 1-alkenes. The NSERC grant, together with a large contribution from Du Pont and a smaller one from Queen's, enabled us to install the CXP in 1980. The instrument was checked out in Germany by Brian Hunter, Dave Axelson and Stan Woodman and was virtually trouble-free. Initially Stan was associated with the instrument but when he returned to Toronto, Sue Thompson, who had prepared many of the copolymers required in the joint project with Du Pont, began using the instrument and eventually took over as operator-in-charge.

I knew little of polymer physics and with Maryanne Scammell producing copolymer sample after sample I needed help with their characterization and solid state analyses. Fortunately, Dave Axelson used the Bruker almost every weekend and gave much help and advice. Du Pont provided size exclusion chromatography analyses, Don Heyding determined X-ray crystallinities and Maryanne used Gus Shurvell's equipment for Raman analysis to augment the solid-state NMR measurements. S.-D. Clas and Doug McFaddin at Queen's, with considerable assistance from Eric Kelusky at Du Pont rapidly broadened the whole investigation and I became an expert on the phase structure of ethylene copolymers.

I spent most of my third sabbatical in 1985-86 at the Du Pont Research Centre investigating, at Ron Zelonka's suggestion, the grafting of maleic anhydride to polyethylene. With Eric Kelusky's help, I used model hydrocarbons to delineate the basic mechanism of this high-temperature free-radical grafting process. And so began the last of my major research investigations. Over the next ten years, cooperative work with Warren Baker, Ralph Whitney and their students has established the main features of the grafting of a number of vinyl monomers to polylefins.

I returned to teaching First Year Applied Science students in 1979 and continued with them until I retired. The collaboration with Don Heyding, Jim McCowan, Brian Hunter and others was very stimulating. I found

myself teaching material that I had once taught in second year physical chemistry to first year students and much to my surprise, it worked. I often thought that my colleagues were too demanding in their setting and marking of examinations but they certainly attracted a lot of outstanding students into Engineering Chemistry. In 1984, it was decided that the department should offer a half course in Polymer Engineering. Once again I was selected to give the new course but I needed great assistance in covering its more applied aspects. Gerry Dyer and Du Pont provided a host of lecturers; on the whole they did an excellent job even though there were raised eyebrows when one of the lecturers provided 140 pages of hand written notes to go along with his two lectures. We offered the course again in 1985 (during my sabbatical!) but by then Warren Baker had arrived and he took over in later years.

When I retired in 1990, I could look back on 36 satisfying years at Queen's. I had seen the University grow from a small institution with poor research facilities to one with an excellent international reputation. My research horizons were broadened by some very good graduate students and collaboration at one time or another with Warren Baker, Erwin Buncel, Don Heyding, Brian Hunter, Albert Norris, Jeff Wan and Ralph Whitney in our department and with outsiders including P.B. Ayscough at Leeds, Dave Carllson at NRC and industrial friends particularly from Du Pont. I occasionally disagreed with Heads of Department but I respected them all – from John A. McRae to John A. Stone – and I enjoyed working with them. Of the many firm friends I have had in the Department I should particularly mention Walter Smith, Wally Breck and Bob Wheeler; I have also been fortunate to have the help and friendship of many secretaries and technicians. If I have any regrets, it is that I allowed myself to be talked into taking on too many administrative duties. I would have liked more time to spend with students, my research and with my family.

My main criticism of Queen's is that it has become much more impersonal and is losing those very qualities that made it so attractive to students in the 70's and 80's.

Robert A. Stairs

1955-1964

Indiscreet Reminiscences of Queen's

I came to Queen's with a background of two years at the Royal Canadian Naval College (now Royal Roads University), a year at sea in Coastal Forces, then McGill, Western and Cornell. There I finished with two years as a part-time Instructor, in charge of the introductory course in Analytical Chemistry, while completing my doctoral research. My last Summer at Cornell was spent working on a small project under Professor Debye. Curiously, it was of a semi-secret nature, and because I was a foreigner without security clearance, I was allowed to do the work, but was not told why. (Debye afterwards encouraged me to apply for a position as founding director of a laboratory being set up by Avco in Cambridge, Mass. I declined, preferring to come to Queen's. A fellow student who took the job became very wealthy, but died young.)

I remember little about being interviewed, in April 1955 by John McRae and his colleagues, including (I believe) Grenville Frost, Ken Russell, Bobby Moir and Walter Smith, except that I spent an interesting morning with them, and then went off to lunch, and explored Kingston during the afternoon. Later I was told that I was expected back in the afternoon, but never appeared. I wonder why I was hired just the same! After the interviews, I had a chat with Roy Dorrance, who was not on the committee, so I felt free to ask him questions that were maybe a bit peripheral. I recall two of his answers: when asked whether the University had any policy limiting growth, he said, "Certainly! The absolute limit to the enrolment is five hundred more than happen to be enrolled at any moment." And to "Is there any policy concerning raises or promotions?" he replied, "Yes, indeed, you just threaten to quit!" In view of the bad press that the concept of tenure elicits, it is worth noting that in April of each of my first three years, I received a letter reading (approximately),"Dear Dr. Stairs, You are reappointed as Assistant Professor for 195X -195Y, at a salary of $XXXX." On the fourth occasion, the letter read, "Dear Professor Stairs, Your salary for [next year] is $YYYY." That is how I learned that I was a fixture (more or less).

I found myself teaching a section of the first year course for engineers (with Roy Dorrance), and of the second year Qual (with Lloyd Munro) and Quant (Roy again). Teaching the engineers had some interesting moments.

Once Roy felt moved to comment that I wasn't keeping very good order, but that passed. When Paddy Doolan (of blessed memory!) retired, and was replaced by the very attractive (can somebody remind me of her name?), on her first appearance in the front of the lecture hall with a cart full of demonstration materials and gear, the class more or less exploded with cheers, whistles and assorted cries. I went to the board, and in my best Grade I style, printed, "See, Dick, See! See the pretty lady!" I don't recall any further disturbance. Remembering Paddy Doolan, I very much regret that the proposal that he be given an Honorary B.Sc., the degree that he helped so many young people get, was not accepted by the committee on such matters. We did not do any lobbying, but simply wrote a letter. It was not enough. It might have failed anyway, but we should have done our homework better.

The old-style lecture hall, with very steeply-raked seating, so that the lecturer could look every student in the eye, was a treasure that should not have been lost in the renovation of Gordon Hall. Paddy told of many exciting events related to demonstrations there. One I remember was the time, in a mixed class, the opaque projector was rigged so that when it was turned on, a very rude picture appeared on the high projection wall. When Paddy reached in to remove it from the platen, he found it was well stuck down, so, for the rest of the lecture, he had to turn the projector off and on again every time he changed displays. I wonder if Paddy's enormous repertoire of lecture demonstrations is still used.

Early in Grenville Frost's term as Head of Department, it was a concern that the usefulness of graduate students as laboratory demonstrators, and the value of such experience for themselves, was not appreciated by the Administration; this lack of appreciation was reflected in the Department's budget. (The success of Queen's in weathering both the Depression and the War without firing anybody, by pinching every penny, still governed the Bursar's thinking.) With our support, one day Dr. Frost decided to beard Principal MacIntosh in his lair. I happened, later that afternoon, to be at the Annex door as Dr. Frost returned. I thought he looked rather pleased, so I asked him how the interview had gone. He smiled broadly, and said, "You know, there's nothing like a few good God-damns!" He had broken the ice for all the science departments. His entree to Queen's Park via his brother Leslie was valuable for Queen's.

He once explained to me why the University of Toronto seemed to be able to obtain funds that were unavailable to the rest of the Ontario universities, saying that Toronto was in effect The Provincial University. He played a part in breaking that ice, too. Not long afterwards, I was on a train, and fell into conversation with an American academic, of what stripe I forget, who asked me about the status of Queen's and the other Canadian

universities: were they private, church, municipal, provincial? I replied that they were mixed in origin, but were now getting more and more provincial funding. He said, "Watch out, the Government will start to call the tune!" To which I replied, "No, the tradition in Canada of university autonomy is too strong." Alas! He was right!

By the time I arrived, research at Queen's was becoming important, thanks largely to NRC support. My own effort was never enormous, but under NRC's policy of the time, I was able to maintain a modest program, which continued after I moved to Trent in 1964. I should like to see statistics on the relative productivity of grants of various sizes, on a papers-per-thousand-dollars basis. I'm not convinced that the present NSERC policy of throwing heavy support to the big research groups is necessarily the most productive on that basis, though I grant that it may allow the top researchers to cut a proper figure on the world stage, as they should.

The guidelines for this project mention contacts with industrial firms. Roy Dorrance had a regular arrangement with Kingston Shipyards, whereby he was called from time to time to test spaces (holds, bunkers) in ships under repair with an "explosion meter" or "sniffer" (a device containing a heated platinum wire in a Wheatstone bridge circuit that responded to any combustible vapours in aspirated air). On one occasion, the crane operator that was lowering him into a deep hold thought it amusing to let him fall freely most of the way, and stop him delicately just clear of the bottom. I didn't hear what happened to the operator, but Roy never went there again. He handed the sniffer to me, and let me do it, which I did for some years, until the shipyard closed. My only independent industrial contact while at Queen's was brief. I was in my lab one summer day, and a young man wandered in, and let on that he needed some advice. It turned out that he worked for a plumbing firm that installed sacrificial magnesium anodes in domestic hot-water tanks. One of their tanks had developed a small leak, and when a plumber (not of his firm) tried to temporarily close the leak with welding rod, a great jet of flame came out; the tank was full of hydrogen. When the tank was opened, the relatively newly-installed anode was seen to be nearly gone. I suggested that, as the source of the water was a fairly swampy lake, they should check the pH of the water.

If it was acidic, perhaps that would take his company off the hook. He thanked me, and said that he was not authorised to offer me a consulting fee, but "the Company will not forget you at Christmas!" --- They did.

Lloyd Munro regularly spent summers in industrial settings, often in mining. This probably contributed to his ability to answer practical questions. He was one of very few academic chemists I have known who could do so. It certainly contributed to his ability to put together courses that would prepare non-chemical engineers to cope with chemical matters incidental to their work. Once I asked him what a piece of the broken

side-curtain of my beloved Morris Minor was made of. He took the sample from my hand, flexed it, bit it, and striking a match, burned a corner, smelling the smoke. He then declared that it was a vinyl acetate-butyrate copolymer, told me its trade name, and sent me to a shop on Princess St. to buy enough to make a new side-curtain.

In 1955, our views of minority groups were not always enlightened. At one of the very first Department meetings I attended, we had to consider applications for certain scholarships, to make recommendations to the appropriate committee. The Head removed one from the pile, and put it aside, saying, "We don't have to worry about that one. He's a Hebrew." None of the eight or ten of us around the table said a word. Some years later, Steve Safe (who went on to distinguish himself as a pioneer in environmental chemistry) told me he was to marry Lorna, who came from a rather strict Baptist family from a nearby village. When I congratulated him, he thanked me, but added that Lorna's family didn't approve of him, and would probably not come to the wedding, I replied that I was sorry, but that I was aware of anti-Jewish feeling in Kingston. He laughed, and said, "It's not because I'm Jewish, it's because I've become an Anglican!" In a similar vein, I was once told by a graduate student that he had met a wonderful girl who was attending the Summer School in English as a Second Language, and was thinking about asking her to marry him. Sensing a hesitation, I asked him why. He replied, "She's a Roman Catholic..", to which I replied by asking, "What does that matter to you, Cadenhead, you're an Atheist, aren't you?" He said. "Yes, but I'm a Presbyterian Atheist!" (They were married that autumn.)

Safety considerations have grown over the years. I recall reading a letter to the Editor of *Chemistry in Britain* some years ago, in which the writer cried that soon it would be impossible to teach practical chemistry. Nevertheless, it is a good thing that we have learned to take sensible precautions.

I remember being mildly astonished when a visitor from DuPont reached for safety glasses in an obviously automatic gesture as he walked into the phys-chem lab. Now we all do it. However there were some things done that worried me even back then.

I'm amazed that Harry McAdie lived to have a distinguished career. Once he came into the lab we shared, on a weekend in winter, when the heat was just on enough to stop the pipes from freezing. Harry turned a gas tap on full, and lit it, making a flame about five feet long, that warmed the lab nicely, until firemen arrived. On another occasion I saw him pour a litre of ether from one beaker into another while smoking a pipe. (It *was* in the hood). [Editor's note: In the October 1999 issue of Canadian Chemical News, there is a photograph of Harry McAdie receiving his 50 year CIC membership certificate during the 1999 CSC conference.]

At the official opening of the Frost Wing, I was commissioned to arrange a suitably chemical way of cutting the tricolour (of course!) ribbon. A small charge of rocket fuel (zinc and sulfur) was to be ignited electrically. Grenville and Leslie Frost and the Rev. Mr. Gordon together pressed the telegraph key. A brief eternity ensued, during which Grenville murmured, "Nothing is happening.", and Bobbie McIntosh said in my wife's ear, "You understand that his whole future hangs on this moment!" Then a puff of smoke went up, the ribbon parted and fell away, and the event was recorded in a photograph published in the Whig Standard.

The first computer at Queen's was a Bendix G-15. Hardly anybody now using computers has heard of it. Bendix also made domestic appliances, and the G-15 looked like a rather large refrigerator. I was the first chemist to play with it, I believe. I put together a small demonstration, simply the calculation and plotting of an acid-base titration curve, with sample volume, concentrations and constants to be supplied from the "audience". I believe I convinced my colleagues that there was something in it. The simplest programmable calculator is now both faster and more powerful that that monster.

When I left in 1964 to be the first chemist at Trent University, I found that the experience I had gained at Queen's was invaluable. Trent's style was different, but the content of the courses was at first closely modelled on the ones I had been involved with or witnessed during my years at Queen's. (I believe that I had taught at least part of every undergraduate course in the Calendar at the time, except Organic.) I tried to reproduce from memory some of Paddy's more instructive demonstrations.

Much of what I had learned by association with him and with Roy Dorrance, Grenville Frost, Lloyd Munro, Ken Russell, Walter Smith, Wally Breck, Bob Wheeler, Geoff Torrible, Bobby Moir and Ken Jones and later with Bobbie McIntosh, Don Heyding, Julian Brown, Saul Wolfe and Erwin Buncel stayed with me. I owe them all thanks!

Wallace G. Breck

1956-1983

I approached my contribution to Chemistry History with misgivings. For one matter I have had medical problems such as two hip replacements within the past eight months, ongoing Crohn's and heart disease, and I am now in the midst of a bout of pneumonia. So I am not up to your attached proforma.

Secondly, I am at odds with your chosen time span of 1950 to 1995; on the front end I precede 1950, while on the back end I retired in 1983 and so have nothing to contribute thereafter. I am puzzled why you didn't choose half-century, say 1950 to 2000, since we are almost there, and there is little we don't know from now until 2000, and that would include the 1999 celebration of the 100th year of the Engineering Chemistry Program. Or another half-century might have been 1945 to 1995, thereby including that unique period 1945 to 1950 containing the post war adjustment and the accommodation of the war veterans. What was unusual about that period was not only the application of the veterans to their studies, against all prediction, but also the Herculean efforts of the department to handle large classes, sometimes with two years telescoped into one, with such small staff.

Imagine, if you can, the incoming post war horde being taught twice a year by five staff members, two technicians, and one young lady who looked after the library, all phone calls, and secretarial work. The name of this unique lady was Iva Spears and the two legendary technicians were Paddy Doolan and Norm Hyland. This contribution by the Chemistry staff is not much made known and appreciated, and can only be ignored by an ingrate.

My undergraduate years in Engineering Chemistry were 1946-50 and I stayed for an M.Sc. under Dr. Grenville Frost. At that time undergraduate instruction was a top priority; chemical research was quite limited, being mostly confined to organic research under Dr. John McRae, research on explosives (RDX) under a special Defense Council appointee, Dr. Arthur McKay, a bit of analytical research under Dr. Lloyd Munro. Administration was at a minimum, with Chemistry accounts kept by the Head, Dr. McRae, in a black book, and communication with one Faculty office was said to be by messages pinned to the Dean's Collie dog. Individual staff did not have telephones.

I came on staff in 1956 from RMC, along with Robert Wheeler. Before that, staff had been hired in the persons of Dr. Robert Moir, Dr. Kenneth Russell and Dr. Robert Stairs. During this decade, the then Head, Dr. Frost, was busy consulting with staff and architects to plan the new large Frost Wing named partly for Grenville but also for his brother Leslie, the Premier of Ontario who arranged funding.
The provision of the new space for labs, lectures, library and offices came in time for the education explosion of the 60's.

This explosion was owing to a new education policy of the Provincial Government which attempted to assure every qualified applicant of a tertiary education. Besides physical accommodation recruitment of additional staff was top priority. This challenge was accepted by Dr. Robert McIntosh, newly appointed Head of the Department. I should leave the selection of recruits to you who were a part of it, but it seems appropriate to comment from my point of view that the assimilation of new staff went relatively smoothly and they seem to have been readily accepted by the old hands, partly because of their quality and also because they could share the load.

But a new era now pervaded the Department. The top priority was now published research (known as publish or perish), over undergraduate instruction to say nothing of administrative chores. Since the new recruits knew little of the traditions and relation among faculties and departments, these chores fell mostly to the old hands and an ill defined and rarely vocal aura of superiority was felt at times by the 'old fudds' at the hands of the 'young turks'. The above division is too drastic, obviously, as proven by Ken Russell, a top administrator who kept up viable and rewarding research.

Worthy of special mention in the 60's and 70's was the appointment of Dr. Kenneth Jones FRS to the Chair of Carbohydrate Chemistry. His research earned world attention and was housed in a newly finished top floor of Gordon Hall. His reputation brought students from the UK and other parts of the globe. Dr. Jones was very well liked because of his ability, humanity and humility. No one could be more concerned for the welfare of his graduate students. After his death, one of his graduates, Dr. Walter Szarek, took over control of carbohydrate research.

A trend which arose in the 60s was a division of the department into sub-disciplines, with some assigned priorities, including emphasizing some and creating others. Organic and Physical and possibly Analytical were the originals, so a decision was made to build Inorganic and create Theoretical, with a push towards research in Analytical. In view of the strength of these components, maximum staff strength of about 30 was accepted.

A trend of the 70's was certainly the establishment of new staff priorities, with priority given to bringing in outside funds in the form of grants and contracts.

Research publication and undergraduate instruction and administration were presumed to be maintained at adequate levels without special attention. Incentive to provide good undergraduate instruction was instead driven by conferring critical power in the students.

As the Chairman of the Engineering Chemistry Program for a considerable period I should comment on its position in the Department.

This program began in 1898 as the "B" course to serve the Applied Science on the chemical side such as Mining, Metallurgy and Geology. It has had its ups and downs over the years but generally has been able to outdo the Arts and Science undergraduate program in quality or quantity of students and certainly difficulty of program. There have been some remarkable graduates such as Alfred Bader, who became a highly successful chemical supplier and an art collector and Connoisseur. Other examples of outstanding graduates who held the top positions in chemistry in Canada were J.R. Gordon, Dr. Leo Marion and Dr. Richard Manske heading respectively International Nickel, The National Research Council and the Canadian Institute of Chemistry.

Outside the Department, the attitude to the program has generally been antagonistic with the exception of the other Engineering Sciences (Mathematics, Physics, Geology). This occurred because there was always survival competition for prestige students and student numbers (justifying budget for staff). The fact that Engineering Science Departments generally attracted the top students was not appreciated by those deprived. On the other hand the Engineering Science Departments were at a disadvantage at times of accreditation and in this respect suffered at times at the hands of the "hard-nosed" Engineers.

Within the Chemistry Department support from staff who came from elsewhere and didn't realize the prestige and value of the program to the department could most kindly be described as lackadaisical, but there were outstanding exceptions.

It is difficult for me to understand what the Chemistry Department stands to gain by the loss of the program but perhaps it is simply getting what it deserves - a loss of the most and best of students to say nothing of prestige or funding.

I retired from the department in 1983 and thereafter was shocked to realize that the Engineering Chemistry Program blossomed in my absence both as to standards and numbers. Much credit should go to the guidance of Drs McCowan and Heyding. My rationalizing is that the infusion of new blood, ideas and personalities was like changing the coach in the NHL.

But without genuine and continuing departmental support the program would have ultimately to die or move.

The latter has occurred so its survival is assured. Another advantage is that accreditation will be less painful the greater the separation from Pure Sciences.

Bob Wheeler

1956-1988

Bang!!! The forward barrier gate slammed down on the flight deck ahead of my aircraft. The deck control officer, probably anxious to return to the wardroom mess and a quiet afternoon, frantically waved his bats signaling me to open throttle and get airborne---NOW. I gated the throttle of my Spitfire, released the pneumatic brakes and rapidly picked up speed down the deck. I barely got airborne before reaching the bow of H.M.S. Ravager and was closely followed by the three other members of the flight.

We had spent the morning practicing deck landing procedures for a full flight in close sequence with the ship steaming in the narrower confines of the Firth of Clyde. This meant that the four of us played follow my leader in a very tight circuit on the port side with very little time between each aircraft approach. Why, you ask, were we imitating the Gooney Bird with its ever-decreasing spiral flight to complete disappearance? For this reason, the ship would normally steam into wind when landing-on and it would be hazardous to maintain such a steady course, engendering the possibility of a torpedo attack.

No sooner has the batsman guided one "kite" over the round-down and signaled him to land-on, crossing his bats over his knees, than he must pick up the one following who would be only a few yards off the stern. Meanwhile the leader has cut his throttle, landed, hooked an arrester wire and rolled to a short stop. The deck handling crew then leap up from the "nets" beneath the lip of the deck at either side, push the aircraft back to release and retract the hook and direct the pilot to open the throttle, taxiing the kite forward over the rear barrier which they have lowered. This has to be done with alacrity and the barrier raised again as number 2 is about to get the signal to cut throttle and land on.

So that afternoon with practice finished I led the four off in close sequence and we climbed and formed up in echelon heading west over the Mull of Kintyre. Wheeling back, now in line astern we swept low over the Ravager. Our goodbyes and thanks to the good old ship were expressed as we all performed "upward Charlies" over the deck (upward slow roll). Oh, if I had only known then that this was my last flip in the grandest aircraft of the 39-45 era, my return would have been much more eventful.

Now headed eastward over the coast of Ayrshire we landed at the aerodrome at Ayr just south of the now international terminal at Prestwick. We were scheduled to leave our good old Spitfires here and travel to Arbroath in eastern Scotland, there to reform the squadron - aah, but with new more powerful Spitfires of a later mark with 2000 hp Rolls-Royces. This being early summer 1945, the war in Europe just over, we expected to refit at Arbroath and sail to the Far East in due time. But----it wasn't to

be. The events of Aug 6th and 9th cancelled all plans and our thoughts turned to what to do when we were demobbed.

The war being over in both theatres I wondered what was in my future. Evidently, as a member of the Royal Navy and not the Royal Canadian Navy, I would be demobbed in Great Britain and left to my own devices in returning to Canada. Fortunately, I had a friend in the ship staffing office in the Admiralty in London and when I visited him seeking advice he very kindly agreed to devise a solution. The net result was he hitchhiked a ride for me on a destroyer heading from Britain to Halifax in late Oct 1945. So, early in November, after four years absence, I joyfully stepped ashore on Canadian soil again, soon to be a civilian once more.

What to do, what to do? An extended visit with the counselors at the Department of Veterans Affairs in Toronto convinced me that, as I had accumulated considerable credits from four years overseas service, I would be wise to consider further education leading to a university degree. DVA would provide the support and fees required and as I had already completed my senior matriculation I could enroll in a first year programme directly. Fine in theory, but, and there's always a but, the registrar of U of T suggested that, as my matriculation occurred at such a great distance in the murky past, I would be better served in first attending Rehab School to polish up my maths and physics. This would, of course, use up some DVA credits and I should say now that I am glad I did not follow this advice because in the end, I needed all the credits I could garner to complete not only a Bachelors degree but to continue on in a PhD programme. The short of this story is that I then applied to enter Queen's University at Kingston and was overjoyed to receive a letter from the registrar, Miss Jean Royce, to inform me that I had been accepted into the first year of Engineering at Queen's.

In September of 1946 I managed to find some digs on Earl Street, shared with a second year engineer, and began classes that month. It was the exceptionally large enrolment in the first year of Science 50, something greater than 400, which made an impression on campus. That and the fact that we looked somewhat older than the usual freshmen owing to 95% of the class being ex-servicemen with only a few students entered directly from grade 13. The University policy of accepting all veterans who were matriculated caused this disproportionation.

Our classes would run for 6 months until the end of March 1947; then we would start our summer jobs, meanwhile a new class would start lectures and course work running through the summer 'till September. Queen's had previously started a 6 months on 6 months off teaching year, hence the existence of Science 48-1/2, in order to deal with the hordes that wanted a post secondary education after the war. This meant that the poor old staff had no break for research or contemplation of any kind before they had to

start all over again at lecture number 1 with the next crowd entering. When the crush of freshmen numbers abated, the University reverted to the normal length of year with no summer course work. Whewww - said many of the Chemistry staff.

The first year of the engineering curriculum was common for all students and we seemed to spend most of the daily routine in the drafting room or the various science laboratories. I'll concentrate on Gordon Hall (chemistry) activities here. Our course was Chem 1 (App. Sc.), then ably taught by Professor Roy Dorrance, assisted by Bill Stevens. The course consisted of a study of the inorganic compounds of the first elements in the periodic table, some analytical procedures and the physical chemistry of some near ideal gases. Problem solving was an important pragmatism in this curriculum. Prof. Dorrance was well liked and we appreciated the strain he underwent as he lectured to three different sections, each with well over 100 students.

As I found out myself later, it is no easy task to repeat lectures with a second class, look after and keep a record the students. It is difficult to keep straight where one left off with which group and at what point to start this lecture. I can only imagine the difficulty Prof. Dorrance had in dealing with three different sections, three times each week. We mustn't forget that, at that time he was also teaching a quantitative analysis course to the third year. His delivery was clearly enunciated with a clipped style of speech. To give an example, the elements hydrogen, oxygen, nitrogen ... sounded like HY-GEN, OX-GEN, NI-GEN.

This of course was 1946 but it was with profound sadness we learned later that Prof. Dorrance was killed in an automobile accident in the USA in 1961, at the age of 68.

It should be noted that our lectures in Chem 1 (with of course many of the other chemistry courses) were delivered in the lecture hall in Gordon Hall labeled "210". Now the building actually had four levels. The basement (halfway below street level) held the second year qualitative analysis laboratories and a large storeroom; the "first" floor, up the front steps from the street consisted of a small library, the organic and quantitative labs, another storeroom and some staff offices; the next floor up held the first year labs and the lecture room 210; the uppermost floor was only half the area of the lower levels and was given over to an electrochemistry lab, the biochemistry office and lab and a smaller lecture room, 310. This room was narrow, actually wider than it was deep, but the board stretched completely across the front and it wasn't a bad place to take lectures. The reason for this reduced area had to do with the structure of the lecture hall 210 on the floor below.

The main room 210 was actually a large amphitheatre with steeply sloped seating reaching up from the second floor through the upper floor to the roof. No student remained far from the lectern or blackboard and hearing

the lecturer or seeing the board was never a problem. The normal seating arrangement used only the tiers rising directly from the space in front of the demonstration bench at the front of the room, although the upper rows continued around the sides of hall, right to the front wall to form loge or box seats directly overlooking the bench and lecturer. These would only be used when an important scientist visited and addressed a large University audience; Linus Pauling's visit in 1950 comes to mind.

Behind the demonstration bench at the front, the wall was covered with blackboards, the center pair rising to reveal a fume hood and an entrance to the preparation room behind. The bench itself was at laboratory height and 15 feet or so long, long enough for Paddy Doolan to set up several live chemical demonstrations. Overhead, the high ceiling reached upward to the roof of Gordon Hall and was broken by a large skylight with a retractable blind. Entrance to and egress from the hall was gained from a staircase on either side, somewhat crowded at changeover time.

The lecture hall, 210, was without a doubt one of the finest on campus and was the center of a teaching style that had disappeared in other universities. William Patrick (Paddy) Doolan acted as the lab assistant preparing the first year Arts and App. Sc. Labs surrounding 210 on that floor. But it was his preparations for live lecture room demonstrations that continued a Queen's tradition that sponsored avid student interest, so much so, that students from other courses, some not even following a science degree often came to witness them as well.

In the late 1940's, Paddy was nearing retirement but his ability to demonstrate rapid chemical or physical change to even a large audience was unmatched. He rightly believed that interest was sparked and maintained by reactions carried out at large scale. No puny 250 ml mixtures, visible only to the first few rows would do; to capture and retain attention, it had to be a reaction displayed at multi-liter levels. The oohs and aahs when the students saw, for example, his instantaneous crystallization of 20 liters of supersaturated sodium thiosulphate, the 30 liter ammonia fountain, the rapid reaction (explosion) of elemental phosphorus with potassium chlorate, the propulsion of a tube of hydrogen and chlorine mixture up to the skylight above when the production of HCl was initiated by ultra-violet light, the diffusion rates of HCl and NH_3 compared in a large diameter glass cylinder, the tenor effect on his voice when he spoke in a medium of hydrogen, his catalytic initiation of H_2 and O_2 explosions in a quart milk bottle and, of course, many others.

Paddy was remarkably calm throughout even the loudest bangs and flashes which was perhaps understandable when we learned that he had earned a Military Medal (M.M.) in WWI by taking a German machine-gun post with the capture of several prisoners.

In first year, Gordon Hall was one of our "homes" where we seemed to spend many hours during the week. There was at that time an addition started at the back of the building called the "Annex" which erased some tennis courts. In 1946 only the bottom level was complete and capped off, waiting for more funds for the three upper floors to be added in 1949. Unfortunately, this addition had been made, joining the Annex directly to the back of Gordon Hall without any space left between the two buildings. So that there was no chance of easily adding a further staircase at the back of the old building to better serve lecture room 210; and perhaps more importantly, any truck traffic to the back making deliveries to the large storerooms in Gordon Hall was eliminated.

Later, in the early '60s a further building addition was started when the Frost Wing (named for the then premier of Ontario, Leslie Frost and also his brother Prof. Grenville Frost of the chemistry staff at Queen's) was joined directly to the south end of Annex creating an elongated "L" form.

Old Gordon Hall itself was a well-designed building, the maximum floor area being given over to "work space" and the hallways kept to a minor component. This was achieved by having laboratories on each floor at the corners of the building with the hallways in the center. It served some 60 years from its construction in 1910, with only one major revision (a "fireproofing" by replacing the original wooden floors with terrazzo in the '30s) until the early '60s when a complete renovation was attempted.

The hip and gable roof was removed and the upper floor area increased to match the ones below. This meant, unfortunately, the removal of both lecture rooms. 310 wasn't missed perhaps but the loss of 210 was sorely felt. Its replacement, a lecture hall created from the removal of the wall between two labs at the west end of the basement, was a disaster owing to the necessity of using a very deep webbed I-beam to support the 3 floors above. This beam projected down from the ceiling across the center of the lecture room and prevented the speaker from being heard beyond the 4th or 5th tier of seats. This lecture hall was soon abandoned and re-converted to an instrument laboratory.

In hindsight, the loss of 210 was a major setback. As it transpired, there would be no replacement in the Annex or the Frost Wing that provided anything like the features that 210 had given us those many long years. This fact without doubt was a major reason that live lecture room demonstrations were scaled back and only a dwindling sequence of demonstrator technicians employed after Paddy finally retired. The position disappeared completely in the early 70's.

In the second year, the engineers of the A, B, C, D and M sections (Mining, Chemistry, Geology, Chemical Engineering and Metallurgy respectively) took lectures in Qualitative Analysis and spent their laboratory time in the very basement of Gordon Hall. This was under the aegis of Prof. Lloyd Munro, who had researched and developed flow chart

sequences for cations and anions. Their identification in unknown samples were readily deduced if the charts were carefully followed. Several other colleagues used the Munro charts, they were that good.

It was a busy time for Prof. Munro and his assistant, Welland Ott, for there were three large sections of engineers taking qualitative analysis in 46-7 as well as an Arts section taught by Prof. Art MacKay. This type of course was a valuable one, not only for the mystery involved in solving the nature of the sample, but mainly because the student met and handled more than 100 different compounds. No other course in the 4 years of study familiarized the student with the properties and reactions of so many elements and compounds. This type of course has now largely disappeared for two reasons. The core curriculum now must include much more structural detail in chemistry and the instrumentation to garner it. There is only so much time and space in the 4 years - something has to go. Second and perhaps more important, there is a difficulty in disposing of the residues from such a course which includes many environmental hazards, heavy metals and the like. We cannot operate in the future by simply pouring residues down the drain. One way to reduce the cost of recovery is reduction of the number of students taking this type of course.

Lloyd Munro was kept quite busy teaching qualitative analysis to the second year engineers, now divided into the "chemical" group A to M (including of course B, C, and D). In fact, all students in A to M took a common curriculum in the second year so that a final choice of discipline could be delayed until the beginning of third year. Those engineers who chose to enter the "physical" group E to H (E = Civil, F = Mechanical, G = Electrical, H = Physics) took a course in Industrial Chemistry in the second year, which was without a laboratory. Prof. Munro taught this course also and he must have been exceptionally busy as, in addition, he offered a course in Colloid and Adsorption Chemistry to 4th year science students.

Lloyd Munro was a kind and likable instructor; he offered help to every student who sought it. His early scientific work had been in forensic chemistry and he later developed a research interest in the adsorption of gases as well as acting as a consultant in the physical aspects of the medical problems incipient in silicosis. One amusing characteristic was noticeable to observant students. He was able to face the class in a lecture and write on the board behind with either hand without turning, and it was quite legible. He retired in the mid-60s and he died in 1987.

The third year, beginning in the fall of 49, saw the final sorting out of disciplines. Most of the engineers of Science 50 chose to enter the core programs, Mining, Metallurgy, Civil, Electrical, Chemical Engineering, Mechanical, etc. Rather fewer picked Engineering Chemistry or

Engineering Physics. There were 14 of us in the former, the B course, and of these, 11 were ex-service types.

In third year, specialist courses in chemistry were introduced. Organic Chemistry 1 was taught by the head of the department, Prof. John McRae and was a traditional basic organic course of nomenclature and reaction pathways. We took lectures with the Arts students of the second year and shared laboratory space with them also.

Dr. McRae was a man of somewhat generous girth; he reminded us of the British caricature of the typical citizen of the U.K. called "John Bull". Out of earshot, the students called him "Tubby". He was a very formal and moral man as one recurring incident during lectures demonstrated. There happened to be a young lady in the Arts section who was quite statuesque and was given to wearing Lana Turner sweaters, as well as always to enter the class after the second bell, while he was busy putting the next synthesis on the board. It was always too much for the rowdy engineers and they whistled raucously. Having endured this a number of times, Dr. McRae turned to the class with, "Hummph, I've never witnessed such disgusting behaviour in all my years here", delivered in his deep baritone voice.

He was said by some to be humorless but one incident in my memory of those times refuted that. One of the B course members that year was little Bud West and he wasn't the best organic student. However, he had led a charmed life to that point. Being small he had been recruited as a tail gunner in the Air Force during WWII and had survived many operations over Germany in Bomber Command. And the tail gunners position was perhaps the most hazardous and fatal in the aircraft. One procedure we followed in an organic synthesis liberated acetylene and we normally burned this off as it was generated. With the reaction in the flask proceeding apace, this was routine. However, Bud evidently lit his gas efflux before the synthetic reaction was well initiated and some air remained in the flask. Fortunately, he had bent down to get some apparatus from the cupboard below. The boom was heard over the whole floor in Gordon Hall. Dr. McRae was conversing in the hallway just outside the lab when he heard the explosion and the subsequent tinkle of glass shards falling from the wall adjacent to Bud's locker. When he had determined that no one was injured, he remarked in his familiar gruff baritone, "Harrmmph, One more explosion out of you West and out you go". Prof. McRae was head of the department from 1941 until he retired in 1956. He died in 1960.

Physical Chemistry 1 and General Chemistry 3 were two courses taught by Prof. Grenville Frost taken by the B course in third year. The former was largely a beginning in classical thermodynamics, the structure of phase changes and elementary kinetics, while Gen Chem 3 was our first

introduction to quantum mechanics and structure of compounds of the elements of the first two rows of the periodic table.

Prof. Frost was an excellent lecturer and sparked our interest in thermodynamic problem solving, so much so that two or three of us followed that as a specialty later, leading ultimately to some research in non-equilibrium thermodynamics. He had one small vice however. Although I am sure there was a university rule prohibiting smoking in class, he chained-smoked his way through the whole hour. One event that transpired brought our attention to his perspicacity and acumen.

Bob Bowley, one of our B group appeared in class one afternoon and presented Dr. Frost with a Churchillian cigar, several inches in length, (in fact it seemed more like a foot in length) with this remark, "Bet you can't finish this cigar by the end of the lecture, sir". What was he thinking? What indeed? We took this course in the timetable slot at the end of the afternoon session, 4:30 to 5:30 p.m., last effort of the day.

Dr. Frost won the bet. After a great many years of teaching thermodynamics and kinetics, his memory served him well and he had the ability to extemporize. When he had run through all of the notes he had brought to class that day he kept on going with no break. We left the building at 7:45 p.m. that night, but the cigar was finished. There was some "finishing" of Bowley subsequently by the rest of the class.

Several years later, when Dr. McRae retired in 1956, Prof. Frost, although at a retirement age of 65 years himself, took on the onerous duty of Head of Department until a younger candidate could be found. When Dr. Robert McIntosh at U of T accepted the Headship in 1961, Dr. Frost retired to pursue his love of music and particularly that composed for the organ. He was diagnosed with type II diabetes later and died in 1963.

Third year in 48-49 meant also surviving the course in quantitative analysis taught by Prof. Dorrance. The emphasis was on problem solving and on lab work, which was executed in the two labs at the west end of the first floor of Gordon Hall. We seemed to spend all of the third year in those labs. Our schedule called for 9 hours a week (three afternoons) devoted to either volumetric or gravimetric analyses of samples drawn from simple natural aqueous sources all the way to complex lithographic sites. The coverage was quite extensive. There were also a few simple instrumental procedures as the limited equipment allowed. I said we lived in this lab; we did - lunch hours, spare afternoons, any time that could be rescued from finishing another course early. I am afraid we broke the rule about unsupervised lab work being a no-no. This was mostly because practically every student carried more than the required duplicate samples through the analysis; many were working in quadruplicate or greater numbers.

I said that the lecture side of the course was devoted to problem solving. Each analytical exercise required the following sequence of steps; sort out the reaction pathway, determine the correct equation(s) for the chemical change, work out the proper stoichiometry for that reaction (balance the equations), determine the mathematical relation between the end measurement and the desired element of the original sample. Some of these steps were extremely difficult. My lasting memory of this course was of the final 3-hour exam, which was a race to solve 13 such problems.

Fourth year, 49-50, brought further specialist courses, among them electrochemistry taught by Prof. W. McF. Smith and an advanced organic course by Dr. R. Y. Moir. Walter Smith was the first professor to join the staff after WWII in 1945. He was a very enthusiastic kineticist in research and had a flair for bringing other physical chemistry subjects to the interest of students. His lectures on electrochemical thermodynamics added to the basic areas that Grenville Frost covered, rather than overlapped them. He was a somewhat reserved and shy man but we greatly appreciated his dedication that prevented him from "draining" over the border to the USA. His teaching style was superb, although some later first year students in Arts Chem 2, when he took over the instruction in this class from John McRae, kept referring to him as Machine-gun Smith. He retired in 1980 and unfortunately suffered an accident at home that proved fatal in the autumn of 1993, a few weeks before his 79th birthday.

This year we were introduced to advanced organic syntheses by Prof. Bob Moir who had just joined, replacing Art MacKay. He had been a brilliant student in chemistry at Queen's just before and during WWII and indeed he was the first staff member appointed whose undergraduate training had been here in Kingston since John McRae joined the department before WWI. Later on, in 1956, Wally Breck and myself, both together in the B course this year would come on staff and be numbers 2 and 2' in the series.

Bob Moir's interests were exceptionally broad and encompassed much more than organic chemistry. He was widely read and skilled in electronics at a time when the vacuum tube was becoming history and the transistor the new boy on the block. At that time he was interested in constructing an instrument to detect the chlorine substituent of an organic host to aid in some stereo structure determinations. Although he was younger than many of us in the 4th year B course, he was well liked and taught a tight course, well received by the class. He spent many fruitful years of teaching and research at Queen's and retired in 1986. He died ten years later in 1996.

The end of fourth year brought graduation in May of 1950. A very large number of ex-service types finished four years of engineering at all of the Ontario universities and appeared on the job market together that year. This meant a dearth of employment opportunities and very few interviews.

With the Korean Conflict beginning some of the graduands went back into the armed forces, while others managed to land some offers.

The graduating B course managed to send nearly half their number into MSc programs in physical or organic for a fifth year of training and research. Four of the total, including Wally and myself, chose to tackle problems in thermodynamics of crystallization posed by Grenville Frost. We spent a productive 12 to 14 months and finished our stay at Queen's.

Wally Breck and I both wished to continue studies in physical chemistry and as it happened we both got scholarships that enabled us to attend Cambridge University in England in 1951. He chose to work on an electrochemical problem with Dr. John Agar and I spent three years with Sir Morris Sugden working on the thermodynamics of ionization in the hot gaseous efflux of flames. Time rolled on and we finished up our PhD theses in 1954 and we prepared to board the Empress of Australia in Sept. of 1954. A greater number of us returned to Canada than left in 1951, for Wally and Viola had produced a daughter, Jean, in June 1953 and Shirley and I were blessed with a daughter, Lesley, in May 1954.

Both Wally and I accepted positions, teaching at the Royal Military College for two years and engaging in some research. We both left in 1956 to take appointments in chemistry back at good old Queen's.

The return to Queen's after 5 years found some changes in the chemistry department. Grenville Frost was head, most ably supported by the old hands, Profs. Dorrance, Munro, Smith, Moir and with the addition of 3 recent appointments. Dr. J. K. N. Jones had accepted the Chown Research Chair in 1953 and brought with him from Bristol a large and active group working in organic carbohydrate research. Ken. Russell, a physical chemist from England had arrived in 1954 via Pennsylvania with enough experience to assist in teaching organic classes if needed. We were pleased to find that Ken had also graduated from Cambridge, although from the wrong college (Clare, the right one being Emmanuel). Bob Stairs, another physical chemist joined the staff in 1955, after a study of the forces of surface tension at Cornell University. Bob only remained here for a few years and left in 1964, accepting a position at Trent University in Peterborough when it first opened.

There were some material changes in the department that warrant comment also. Of course, the add-on building called the Annex had been completed, the three upper floors finished in 1949. Chemical Engineering occupied the basement and the first floor until such time as Dupuis Hall on Division St. was built. Later, in 1961, the further addition to the Chem. Dept. called the Frost Wing was started at the southern end of the Annex. The construction proceeded in two stages: the partial basement and three floors were completed by 1962, with the 4th and 5th made ready in the late 60's. There were three design encumbrances that made the addition not

quite ideal. The floor levels matched between Old Gordon Hall and the Annex but those of the new addition to the south did not, making for a difficulty in transporting heavy materials (gas cylinders and the like) from the loading bay on the Campus Drive end of the Frost Wing to the other sections. Secondly, the Frost, stretching from the southern end of the Annex easterly to an area behind Nicol Hall was a long skinny building. Unlike Old Gordon Hall, this new building did not use its floor area in the most efficient way. Too much was devoted to "communication" in terms of long hallways at the sacrifice of "work areas" i.e. laboratories. A more compact design like Gordon Hall reaching higher on a smaller footprint at the end of the Annex would have allowed the layout of the needed labs at the corners with less "communication" areas. Thirdly, the new lecture hall in the Frost was an abysmal replacement for 210 in Gordon Hall when it was lost in the renovation of that area. Its plan was less than ideal having a rather narrower width than required for its depth with a pitiful attempt to simulate an amphitheatre (too little slope to be effective). The preparation area accompanying this was the size of a cupboard with several steps leading down to the demonstration bench in the lecture hall proper. It was extremely difficult to show any demonstration requiring heavy equipment. Goodbye lecture demonstration. Goodbye Paddy Doolan. Goodbye Helga Pieniak.

Well, the staff increased to 11 when Wally and I joined in 56. There was plenty for all to do. Three faculties, Engineering, Arts and Medicine sent copious students for instruction and not counting graduate courses, every staff member had a lecture-load that left little time for research. J. K. N. Jones had no undergraduate teaching as his was a Research Chair, but he offered a number of graduate courses.

What was particularly onerous and time consuming was the setting up and running of a lab course. Wally and I were charged with the working up of modern physical chemistry lab, more or less from scratch. From scratch in the sense that there was very little instrumentation available at that time and the lectures had not dealt with topics like spectroscopic measurements for instance. The students would not know how to interpret the data to derive dissociation energies and the like. Couple this with the fact that there were no lab technicians (except Wally and I) and you can appreciate the effort involved. We managed to scrounge enough equipment from various sources to get it off the ground in the first year (stole enough gear said some staff).

A comment about staff "nicknames". As Wally and I had very similar career events and were often at the same university at the same time the students at one time began to refer to us as DAMON and PYTHIAS. It was odd but we both noticed that every student exhibited an identical speech impediment when they uttered the word Pythias. It was as if it was a reverse lisp. Instead of substituting a th sound for a s in the word as would have been done by a person with a normal list (i.e. lithp), they

reversed this. The consequence of this is that both Wally and I claimed to be Damon.

Life in the ensuing years and decades was much as you might expect and the years rolled on. Wally was a brilliant instructor who loved to teach and the students loved him back. He was very gifted as a teacher. His research topic continued along the lines of his Cambridge work leading to some measurements in non-equilibrium thermodynamics. Later on he developed an interest in marine chemistry and went on to lead a small group of enthusiastic scuba divers mapping the Kingston basin for heavy metal contamination, using fresh water mussels as the filter feeder. He retired in 1983 and after a long battle with Crohn's disease, he died in Feb. 2001.

One recurring event during our years at Queen's is worth mentioning, the Christmas Lectures. Prof. Jones had some support from the Faraday Society for the sponsorship of a popular lecture given near the Christmas break. Wally and I put together several of these, beginning in the late 50's. The purpose was to spark the public's interest, as Faraday did himself in London, using graphic demonstrations of stimulating experiments. In the first one I remember we called on Paddy Doolan to assist and his best bangs and flashes combined with our contribution, demonstrating the visible radiation from a shock wave in air along with some colorific flame events, produced the desired results. Every three or four years we would attempt another presentation, hopefully without too much repetition. The lecture room, 210, in Old Gordon Hall proved to be ideally suited as the audience was seated close to the display. We tried one in the new lecture hall in Dupuis Hall when it was first opened but it proved too difficult to bring all of the required apparatus to the stage. Several Christmas Lectures were presented over the years on various topics but I think the most successful one, at least in terms of audience reaction, was the one mounted in 1968 on the topic of Carbon Dioxide in the Lithosphere and Biosphere.

By this time, Paddy had long ago retired and we had in his place Helga Pieniak, who was the last trained demonstration technician at Queen's. We were fortunate and benefited greatly from her training in Germany. Each of the demonstrations went off flawlessly (not always the case in the past). The program focused on the essential features of CO_2 reacting with life forms as well as marine and geological bodies, rather than with negative effects, one might say, like "greenhouse gas". Every reaction was demonstrated life-sized, 30 liters or so. And even the most senior or sophisticated member of the audience was intrigued enough to solicit answers after the lecture. Dr. Ron Watts, later Principal of Queen's, and some senior Profs from other departments attended and seemed to enjoy the fun and games.

There happened to be a disproportionately large number of young ladies graduating in chemistry that year (68-69), so Wally and I pressed them into service. We formed groups of 2 or 3 each and had these teams do the actual presentations for each experiment. The M. C. for the whole lecture was Helen Sutcliffe (later, Cooper) who graduated that year and went on some years later to become the Mayor of Kingston.

The fact that this Christmas Lecture went off so well was in large part owing to the performance of these young ladies. The final act is worth describing here. Wally and I assembled them in two teams, suitably attired in the appropriate chapeaux (courtesy of Wally and me), one representing a naval gun crew under my command, the other an army artillery battery led by Wally. We then fought a gun battle, firing Ping-Pong balls at each other. The balls were actually propelled from the "gun" by CO_2 from fire extinguishers.

Regardless of the success of this Christmas Lecture and the plaudits we received, it was to be our final presentation. Helga left the next year and we knew that the applause at the finale was in very large part owing to her hard work in ensuring each experiment proceeded without a hitch. With no suitable replacement, the position soon disappeared from the department roster.

We, each and every one, did a lot of undergraduate teaching in the 50's. However, with the coming of the 60's, a rapid recruitment of young staff was initiated. Prof. Bob McIntosh accepted the headship and led the way to building an active staff whose research interests were supported by the granting agencies. The greater numbers eased the teaching load and research and scholarly work could be entertained in term time too. The recruitment continued at a reduced rate into the 70's until it appeared the Ontario university enrollment had peaked in numbers. One sad event of the 70's was the death of J. K. N. Jones in 1977 while still occupying the Chown Research Chair.

And so it goes. My teaching centered on physical chemistry early on with an ever-increasing segment of analytical chemistry later on. I continued research on thermodynamic issues of flame gases and in the 70's and 80's developed an interest in atomic absorption spectrometry. There was a period of the 70's when I collaborated with the marine chemistry section helping them with the analysis of the metals picked up in the Lake Ontario basin by fresh water mussels.

Well, the years rolled on and 1988 brought another adventure, retirement. Go! Go! -- said the Dean -- so I went.

My seams gape wide so I'm tossed aside
to rot on a lonely shore

While the leaves and mould like a shroud enfold,
for the last of my trails are o'er.
But I float in my dreams on Northland streams
that never again I'll see,
As I lie on the marge of the old portage
with grief for company.

Then the sunset gilds the timbered hills
that guard Temagami
And the moonbeams play on far James Bay
by the brink of the frozen sea,
In phantom guise my spirit flies
as the dream blades dip and swing,
Where the waters flow from the long ago
in the spell of the beckoning spring.

Do the cow moose call on the Montreal
when the first frost bites the air?
And the mists unfold from the red and gold
that the autumn ridges wear,
When the white falls roar as they did of yore,
on the Lady Evelyn,
Do the Squaretails leap from the black pools deep
where the pictured rocks begin?

Yea the fur fleet sing on Temiskaming
as the ashen paddles bend,
And the crews carouse at Rupert's House
at the sullen winter's end,
But my days are done where the lean wolves run
and I'll ripple no more the path,
Where the grey geese race 'cross the red moon's face
from the white wind's arctic wrath.

Though the death fraught way from the Saguenay
to storied Nipigon,
Once knew me well - now a crumbling shell
I watch the years roll on,
And in memories haze, I live the days
that forever are gone from me,
As I rot on the marge of the old portage
with grief for company.

The Old Canoe Author Unknown

Dr. Robert L. McIntosh

1961-1979

Dr. Robert Lloyd McIntosh was appointed Head of the Chemistry Department at Queen' s in 1961 and was responsible for its revitalization and its considerable expansion of faculty, students and facilities. He influenced a generation of faculty and through them of students, graduate and undergraduate, with his dedication to both teaching and research. His subsequent appointments in the Arts and Science Faculty and in the School of Graduate studies enabled many in the University to enjoy working with and take pleasure in the company of a unique person.
Dr. McIntosh, known as Bob to most of the University community but always affectionately called R.L. within the Chemistry Department, was the son of a notable Canadian chemist. He obtained his undergraduate education at Dalhousie University and his Ph.D. at McGill. The latter was under the supervision of Dr. 0. Maass, a renowned chemist and a leader in Canadian chemical contributions to the Second World War. He was soon drawn into research in this area, a period of his life which led to many close friendships with leading Canadian chemists who, like himself, were to organise the expansion of university chemistry in the sixties. His experiences also led to a fund of stories with which to regale his companions in all walks of life. He was awarded the MBE for his war work but in characteristic fashion threatened to return it personally to the Queen when he heard that the Beatles had been similarly honoured. He continued his academic career at the University of Toronto where he was Professor of Physical Chemistry, specializing in research on the adsorption of gases on solid surfaces. His appointment as Head of Chemistry at Queen' s was contained in a letter from the then Principal W .A. Mackintosh which was, as recounted with great glee many times, unsigned.

From 1961 to 1969 as Department Head, R.L. oversaw the increase in the complement of the Chemistry Department faculty from 10 to 26, the opening and subsequent expansion of the Frost Wing and the renovation of Gordon Hall. He took a keen interest not only in the scientific lives of his new (and old) faculty but also in their family lives, knowing all about wives and children and freely giving of his advice on all matters. A short period as Associate Dean of the Arts and Science Faculty was followed by appointment as Dean of the Graduate School in 1970. He increased the profile of the School both inside and outside the University and laid the groundwork for its present important place in the University structure.

He officially retired in 1979 but was called back in 1982 to organize and become the first Director of the new Department of Health and Safety. He accomplished this difficult and somewhat delicate task in his inimitable fashion, without engendering rancour among researchers who never enjoy being told what they must do, even if it is to obey government regulations. R.L. took his final leave from Queen's in 1983.

In his years at Queen' s R.L proved himself to be an extremely able administrator who left his mark in many areas. We remember him for these admirable accomplishments but we most fondly remember him as a warm-hearted leader and companion who captivated his colleagues by his gracious charm, his enthusiasm for the world around him, his joy in our successes and his comfort during our ordeals. He had a strong sense of honour and instilled this attitude in all his companions. In all his dealings there was a twinkle in his eye and an ability to see a humorous angle in almost any situation.

From Faculty of Arts and Science Minutes.

Geoffrey Torrible

1961 – 1966

The late 1950's and 1960's were turbulent and exciting times in Canadian Universities, particularly so in Ontario. This period saw the foundation of many new schools, eg. Guelph,York, Carleton, Waterloo, Brock, Trent, Laurentian, and Windsor, and the rapid expansion of existing established universities. Some of the new schools were based on existing institutions but many were 'green field' start-ups. All this was in response to a rapidly increasing demand for university education. It obviously created a tremendous demand for new university staff.

In 1961 I was completing what had been a couple of years as a post doctoral fellow at the University of Western Ontario. I had finished my Ph.D (physical inorganic chemistry) at the University of Wales (UK.) in 1958 and had gone into industry. However, I was beginning to find life in Britain somewhat 'confining'. I had been approached by my old university in regards of my interest in a faculty position, but after exploring it concluded it would not change my basic discomfort with the UK. So as many had done before me I looked across the Atlantic and took up the fellowship at UWO, funded by the United States Airforce. I worked on inorganic polymers with Prof. Don Bradley, another expatriate, who later returned to the UK. to Birkbeck (Univ. of London). While at Western I gave some part courses at both the undergraduate and graduate levels, and this lead to the inevitable question 'did I want to enter full time into academic life?'. This had not been my intent nor had it been my intent to remain in Canada. However, I had no real reason for saying no to either, and as a result found myself talking with Dr. R.L. (Bob) McIntosh about a position at Queen's. This conversation took place in Toronto, at his home!, as he was still then on the faculty of the Univ. of Toronto but had just been appointed the new head of the department at Queen's.

He talked of his goal of significantly increasing the research activity in the department and in particular in establishing a strong inorganic presence at both the undergraduate and graduate level. He told me he had someone in mind to lead this and he was offering me the junior appointment. I accepted. He later told me that the senior appointment was Dr. R.D. (Don) Heyding. It was the start of a very friendly and happy relationship with both men that lasted long after I departed academic life. They both played significant roles in career decisions which I have made and never regretted.

At the end of August 1961 my new wife and I put all our belongings in a small car and drove from London to Kingston. Essentially this was the first time I had seen either the town or the university. The new Frost wing was not yet completed and there was a shortage of offices. I was asked to share space Dr. Wally Breck who occupied a very large office in Gordon Hall. This was an excellent move as I got to know a superb teacher and I was able to observe and learn from him as he talked to the many undergraduates who found their way to his office for advice. The Frost wing was completed some months later and we moved into our new individual offices. However, I remained eternally grateful for my learning period in Wally's office.

In the correspondence prior to taking up the position I had received a letter from Prof. Roy Dorrance who informed me that I was to join him in giving the first year course to the engineering students. I would take one third of the class and he would take the other two sections; each section was about eighty students. He also sent me an outline of the course. I must admit the course content looked a little 'dated' and wrote as such to Prof. Dorrance. He responded to the effect that that was what he had been giving for many years and that he was not prepared to change in what would be his last year before retirement; after that I could change the content as I wished! Unfortunately, I never met Prof. Dorrance, that summer he and his wife were tragically killed in a car accident while on holiday in the US.

This unfortunate event caused much reorganization of the teaching schedule. Prof. Dorrance's first year teaching load was taken up by Prof. R. A. Stairs, who later joined the new faculty to get Trent University running, and, I think, Wally Breck. It also meant that Don. Heyding, who originally was to join us in September1962, now commuted from Ottawa starting in January 1962. This came about by bringing forward by one year the plan to designate one of the third year courses the 'Inorganic Course' to be given by Don Heyding and myself. The first half, (periodic relationships, bonding theory, coordination chemistry), was my responsibility. The second half, given by Don, was solid state inorganic chemistry.

These third year lectures were given on Wednesday and Friday mornings. Don used to drive down Tuesday evening and stay in Kingston until after the Friday lecture when he would return to Ottawa where he was a senior researcher at NRC. Needless to say we spent a lot of time together and he joined my wife and I for many meals in our tiny attic apartment on Clergy St. West.

With the move into offices and laboratories in the new Frost wing we were now able to begin to set up our research facilities, and look for graduate students.

My interests were essentially the synthesis and structure of transition metal complexes mainly with organic compounds containing oxygen, nitrogen or sulphur as the binding sites. Don's interest was the chemistry of materials in the solid state; specifically those identified as III-V compounds. Over the course of the next year he transferred his X-ray and magnetometer equipment from Ottawa and rebuilt them in his new laboratory in Kingston. We were both fortunate in attracting graduate students fairly quickly and so were soon able to start productive research.

About a year after Don arrived one of his colleagues from NRC, Will Henry, joined Queen's as a professor in the Metallurgy Department. A goal of both of them was to set up an interdepartmental 'institute' possibly including some faculty from Geology and Physics, for solid state research. The implication was that equipment would be shared by the members of the group, joint graduate courses would be developed, and joint research could be undertaken for which specific funding would be applied for. We talked about it a lot but they were ahead of their time and the departmental barriers were such that they could not be broached. Nothing formal was ever achieved, although Don and Will did cooperate informally on some research projects. Sadly Will Henry died of cancer some years later.

As I mentioned earlier a major goal of Bob. McIntosh was to increase significantly the research activity in the department. Up till now the main research activity in the department was led by J.K.N.Jones FRS, Chown Research Professor, who had no undergraduate teaching responsibility and who had built up a significant carbohydrate research group of graduate students and post doctoral fellows. Smaller research activities were pursued by Profs. Walter Smith, Ken Russell, and Bob Moir. All carried very heavy teaching loads, as did the other members of staff. Bob McIntosh realized that to increase the research activity and to meet the future increase in teaching demands he would have to increase the staff. At the same time that he hired me he also brought in Dr. Saul Wolfe (organic chemistry), and also, of course, Don Heyding. Over the course of the next five years he recruited amongst others Profs. Buncel, Brown, Kewley, Davis, Stone and Norris. Many others came just after I left the department.

This big increase in staff, particularly staff who had an expectation to set up their own research activity and recruit graduate students, soon put pressure on the existing laboratory space. A committee, of which I was a member, was struck to examine the issue and make recommendations as to how new space could be made available. We looked at many options, none very practical or inexpensive. At last, in desperation I asked if the foundations of Gordon Hall were such that we could add another floor.

This idea was grasped as a drowning man clutches a straw. It was then further explored by the appropriate professionals and declared feasible. And so a new floor was added.

The limestone came from a different quarry to that of the original building so the match in colour is only approximate and the addition can be clearly identified. However. it served its purpose and Ken Jones and his large group of carbohydrate chemists moved in to occupy the whole new space freeing up large areas in Gordon Hall Annex.

After a few years of teaching I began to question whether I really wanted to teach for another thirty years. In many respects I enjoyed teaching but I found the period before any lecture very stressful and even with increasing experience it did not seem to decrease. And so in 1966 I made the decision to move back into research in industry. I had graduate students at the time and a major concern for me was that I not desert them. I was fortunate that in Kingston was a large industrial research centre owned by Alcan, a major Canadian multinational company. I had done some consulting for them in the past and they were enthusiastic that I join them. They agreed that I would continue to supervise my graduate students, as long as it was in the evening or at the weekends, until they had all graduated; we were talking of a period of about two years. This is what we did.

It is fitting to say a few words about the communal life of the department in the period that I was there. Bob McIntosh understood well that to have a group of people perform well they should feel part of the overall direction of the organization. To this end he worked very hard to include all, including the most junior, in decisions affecting the academic direction of the department as well as, when appropriate, the overall management of the department. This came as a pleasant surprise to me as I had been more used to the traditions of British universities where the Head of the Department was some one whom you rarely saw, was addressed very formally and whose decisions were issued as edicts.

In the early 1960s the department was still quite small, Saul Wolfe and I were, I think, the tenth and eleventh members of staff. It was to grow to about thirty by the end of the decade. Both Bob McIntosh and existing members of staff made great efforts to integrate newcomers as they arrived into the social life of the department and the university as a whole. My wife and I recall very fondly the lively parties given by given by Bob McIntosh, Ken Jones, and Ken Russell to name a few. It was at a party at Don Heyding's home that I told Bob McIntosh that I had decided to leave. I had left it to late in the evening before telling him and he was not overjoyed, he harangued me for making, as he put it, a foolish decision that I would live to regret! However, the decision was final and I formally resigned my position in August 1966.

In conclusion I need add that I never lost touch with the department and the friendships developed in this period with members of the faculty have endured through the years. Bob McIntosh continued for many years to invite us to his parties to meet new staff; I think he thought I might still learn to regret my decision to leave!

Saul Wolfe

1961-1990

REMINISCENCES

Teaching Medical Students

When I arrived at Queen's in September 1961 I was the ninth member of faculty, and the total student population was about 3000. I was assigned two courses, Medical Organic, and Organic Chemistry V, about which more later.

At that time, the Medical faculty accepted 65 students directly from high school into a six-year programme, consisting of two years of premedicine followed by four years of medicine. These students were guaranteed admission into the first medical year if they could maintain an average of 60% over the premedical years, with no more than three supplemental examinations in any one year, i.e., repeats of examinations failed in April. In addition, the students were enrolled in the Aesculapian Society, were addressed by the faculty as "Dr. X", and in many ways given to understand that they were members of an elite group. Many of them, advised by the clinical faculty that "real" medical training did not start until the second or third medical year, did no more than the minimum required to remain in the course.

The Secretary of the Faculty was H.D. McEwan, a member of the Pharmacology Department, and his principal function seemed to be the protection of the students from the horrible fellows in the basic sciences. There were three of us in chemistry that first year, all new to teaching, the others being Jim Thompson, who taught First Year General Chemistry, and Jamie Thomson, who taught Second Year Physical Chemistry. Before the beginning of term, Dr. McEwan called the three of us in for a pep talk. He informed us that we had been given a great honour to be 'allowed to teach this group of elite students, that these students were going to be future leaders, that we should learn their names as quickly as possible', etc., etc. I finally interrupted to ask Dr. McEwan whether he remembered my name. After a long pause, he did.

In the spring of 1962, 44 of the 65 students failed my course. I wish that I had a tape recording of the discussion that ensued at the faculty meeting called to discuss the marks. My life and my academic career were saved by the intervention, support and encouragement of two people: Bob McIntosh, Head of Chemistry, who had hired me; and Jim Beveridge, Head of Biochemistry, who was delighted to discover that students entering Biochemistry would actually have to know some organic chemistry.

As time passed, many of the students and faculty grudgingly accepted that I was competent to teach organic chemistry, but I was never able to convince Dr. McEwan, and over the years I had many interesting conversations with him, two of which are reproduced below (the way I remember them):

M: "Now Dr. Wolfe, why can't you teach these students a nice simple course in organic chemistry instead of all this theoretical stuff?"

W: "Well Dr. McEwan, what did I do this time?"

M: "Did you really have to show them how to prove the structure of glucose?"

M: "Dr. Wolfe, premed students in Toronto have no trouble passing organic chemistry. Maybe your exams are too tough."

W: "Okay, why don't you get me a copy of the final exam that they wrote in Toronto?"

W: (some time later):"I'm impressed with that exam. It's more difficult than mine! Wait a minute, let's have a look at the exam they wrote last year."

W: (later again):"Did you notice that last year's exam is the same as this year's. And so is the one the year before, and the year before that!"

One year I decided to tell the students about the isoprene rule, for which Leopold Ruzicka won the Nobel Prize in chemistry. The realization that the skeleta of many terpenes are constructed from units of isoprene allows one to predict where methyl groups must be located. However, there are some noteworthy exceptions because of rearrangement during biosynthesis, and on the final examination I had the following question: "When the structure of eremophilone (shown) was established, it created a sensation. Why?" One student answered: "Organic chemistry is such a dull subject that they will make a fuss about anything." I gave him full marks.

After about six years of this, Bob McIntosh was able to get me appointed to the Faculty of Medicine Curriculum Committee. At a retreat arranged to discuss changes in curriculum, I managed to persuade the other members of the committee that both their problems and my problems would be solved if we just abolished premedicine, and accepted students into the first medical year after a minimum of two years of university. They agreed, but Harry Botterell remained skeptical. He reduced the incoming premedical class to 40. One or two years later, premedicine disappeared completely.

Some 15 years later, at the opening of the John Deutsch Centre, I was standing with Bob McIntosh when I spotted Dr. McEwan, whom I had not seen during all this time. I rushed over to greet him, and we had our final conversation:

W: "Dr. McEwan, do you remember me?"

M: (after a pause): "Sure I do. You're Greenberg from the class of '68."

Organic Chemistry V

Metallurgical engineers use the flotation process in their work, and so they have to know about detergents. At some point in time it had been decided that the way to do this was to have someone teach the students organic chemistry. The problem was that their programme was already so full that the only available time was one hour in their third year at 11:00 a.m. on Fridays, their third lecture of the day. The exhausted students didn't know why they were wasting this hour and, after a few weeks, neither did I. Somehow we all survived, and at the end of the year I managed to convince the metallurgical engineers to teach this material themselves.

A Bottle of Sodamide

In the 1960's, lecture/laboratory courses were taught by the same person. When I began teaching Chemistry 12 (the predecessor of the 28X courses), I found that the standard laboratory manual, Adams and Johnson, later Adams, Johnson and Wilcox, had a sequence in which n-butanol was convened to n-butyl bromide and the n-butyl bromide was then carried forward to methyl n-butyl ether. This sequence had probably been performed by organic chemistry students since Roger Adams was a boy, and I soon realized that many of them had well-thumbed lab manuals and also well-thumbed laboratory notebooks of former students. One year I therefore informed the students that when they had completed the sequence they would have to synthesize methyl n-butyl ether in the opposite sense, i.e., from n-butanol and a methyl halide. This was not an experiment that could be taken from a laboratory manual, and so they would have to go to the library, find a procedure, and obtain my approval to carry it out.

One student came to me with a procedure that involved addition of sodamide to excess n-butanol, to form the sodium salt, followed by addition of methyl iodide, and fractional distillation to isolate the product. This seemed reasonable, and I approved the experiment.

As I was walking around on the day of the laboratory, I noticed this student working away with a large amount of white powder on a watch glass.

The following conversation ensued:

W: "What's that powder?"

S: "It's my sodamide."

W: "Holy smoke, be careful! Sodamide is dangerous!"

S: "I don't think so, I'm adding it to the butanol and it's dissolving."

W: "Show me." Student adds the powder, and it dissolves with no evolution of gas.

W: (after some thought) "Give me a spatula and a test tube with some water." Cautiously adds the solid to the water, and it simply dissolves!

W: (after more thought) "Let's check the pH of the solution." The solution is acidic!

W: (after much more thought) "Where did you get this stuff?."

S: "I found a can in the storeroom."

W: "Let's see it." Student brings can, which reads Sodamide, Merck, Darmstadt 1922. (Opens can to display a white powder.) "Is this what you used?"

S: "Yes."

W: (after much more thought) Turns the can upside down. All of the powder comes out, followed by a sealed bottle labelled "Sodamide".

When the reaction was performed using the contents of the bottle of sodamide, the student obtained methyl n-butyl ether without incident.

A Smoke-Filled Room

When I took over the teaching of the third year course Chem 22 (later Chem 380, later Chem 38X) from Bob Moir, I inherited a group of 25-30 very good Chemistry majors, Chemistry minors, Biochemistry and B Course students. The course consisted of 2 lectures a week for 24 weeks, with one laboratory a week for the first 12 weeks and then two laboratories a week.

With the sodamide experience in mind, I decided from the beginning to turn the laboratory into a project-oriented course. The students were divided into groups of five or six, who were to work as a team throughout the year. Each experiment illustrated some specific chemical operation, e.g., oxidation of an alcohol, hydroxylation of an olefin, carbon-carbon bond formation, preparation and use of an organometallic, etc.

Each group was asked to provide me with an experimental procedure for a specific example of the operation, e.g., cis-hydroxylation by permanganate, cis-hydroxylation by osmium tetroxide, Woodward cis-hydroxylation, trans-hydroxylation, etc.

One member of the group would proceed to the library, find a possible procedure, and, after some discussion, have it approved by me. The procedure was duplicated and distributed to every member of the class, but only the members of the specific group carried out the experiment.

Experiments were open-ended, and when every student had finished, I met with each group to discuss their results. These meetings were always held in GG4, the organic seminar room, and always in an evening from about 8:00 PM until about 1:00 AM. While I chain-smoked cigars (La Palinas in those days), I took the group through the chemistry and mechanism of their reaction, then through the details of the experiment, and finally we attempted to analyze why different students performing the same experiment obtained different results. At the end, one of the students, usually the one who had designed the experiment in the first place, wrote a detailed report and presented a seminar to the whole class. I still have most of the experimental descriptions and most of the reports.

When the laboratory became six hours a week in the second term, I proposed an extended synthesis to each of the groups. They worked on these for the last two months, and at the end of the year there was a day of seminars at which the groups reported their results.

One year I was sitting at the back of GA30, smoking a cigar and listening to these final seminars when I suddenly realized that there was much more smoke in the room than could have been produced by my cigar. When I looked around I found the explanation: every student was smoking a cigar!

How Gordon Hall Got an Air Conditioner

I spent the summer of 1968 trying to write a chapter for Saul Patai's "Chemistry of the Functional Groups", and was in my office in G22 every night for several months. Anyone who has ever worked in Gordon Hall will know that the bearing walls begin to warm up at the beginning of May, and by the end of June the temperature in the building is seldom less than 30° C day and night. I was therefore working nights with the windows open, but found that the office immediately filled with little green flies that soon covered me and all of my papers. Eventually I gave up, drove over to Bad Boy and bought an air conditioner which, with Pip Nation's help, I installed in the window. Within one day I received a call from Bob McIntosh, who had been ordered by Vice Principal Conn, a former army officer, to have this air conditioner removed.

He advised Bob that air conditioning of Gordon Hall was absolutely forbidden, and in any event the presence of an air conditioner looking out on Union Street marred the view of a beautiful building named after a former Principal of Queen's, whose granddaughters were still alive. I informed Bob McIntosh that I would be willing to remove the air conditioner if Hugh Conn would exchange offices with me.

The air conditioner remained, and so far as I know it is still there, although I understand that Bob Lemieux had to remove several kilograms of tobacco tar when he inherited G22.

Hustling at Pool

At one time, and perhaps still, the Faculty Club had one of the largest snooker tables ever built. For many years, after we had entertained a visiting speaker, we ended the evening at the Club by playing the speaker for his honorarium. Eventually some of us, myself excluded, became quite good. Current members of the department may be surprised to learn that the best hustler among us was Erwin Buncel, who could always be counted on at critical moments. However, we discovered that some of our guests were also hustlers: the late Charles Wiesner claimed that he had never played pool in his life, but he wiped us out. On the other hand, Lou Allinger eventually admitted that he had once made a living playing piano and pool in jazz clubs. But the worst pool player of all, and the one who we therefore continued to invite back, was Kurt Mislow. We still claim that he is the one who ripped up the table.

Special Conditions

One day I received a phone call from Jim Morrison, President of NRC, asking if I would host a visit to Queen's by a Professor Novikov, a Soviet explosives chemist who was in Canada as a guest of George Wright of the University of Toronto. Once Jim assured me that NRC would pay the expenses of the visit to Kingston, I agreed to host the visit.

I reserved F407 for a lecture, and when I met Professor Novikov at the train station I asked him for the title of his lecture. He brusquely informed me that he was not planning to lecture, and that I was merely expected to give him a tour of Kingston. I replied that the next train to Ottawa arrived within the hour, and that I would be glad to buy him a ticket. He agreed that perhaps he could lecture after all.

The audience in F407 included several Polish postdoctoral fellows, who were working with Walter Szarek, and who understood Russian. Since Novikov had not planned a lecture, he gave a chalk talk at the blackboard.

From time to time, he mumbled under his breath, and every time he did this the Polish postdoctoral fellows laughed. They told me afterwards that Novikov was cursing me in Russian.

At some point in the presentation, I suddenly realized that he was discussing the chemistry of hexanitrobenzene, and I became very interested. Hexanitrobenzene could not be a planar molecule, and depending on how the nitro groups were twisted, there could be isomers.

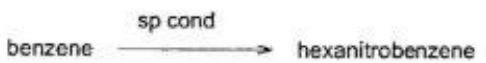
At the end of the lecture I began to question him, and the following conversation ensued:

W: "Does hexanitrobenzene have isomers?"

N: (mumbles, Polish postdoctoral fellows laugh) "I don't know".

W: "How did you make hexanitrobenzene?"

N: (mumbles, Polish postdoctoral fellows fall down laughing) eventually goes to blackboard and writes an equation:



W: "What is sp cond?"

N: "SPECIAL CONDITIONS!"

Very Short Men

When the fourth floor of the Frost Wing was completed, we discovered that the urinals in the men's room were very close to the floor. One day, some graffiti appeared on the wall: "This $g(~&* urinal is for very short men only!!" Someone then erased "short" and wrote "very well endowed". We all recognized the second author's handwriting as Bob McIntosh's.

Alfred Bader

I first met Alfred Bader in the fall of 1961. Thereafter, whenever he came to Kingston, he always stopped in for a chat. In the fall of 1962 or 1963 his visit coincided with Homecoming weekend, and we went to the football game together. He wore his yellow jacket, with a bottle in the inside pocket, and I was amused to hear his classmates call to him: "Hey Alfie, what are you doing these days?"

In the late 1960's several of our organic PhD's decided to go to Australia for postdoctoral work, including John Campbell, whose thesis contained syntheses of several dozen selectively deuterated organic compounds.

John won a NRC postdoctoral fellowship, which he took to Sev Sternhells laboratory. However, after a year, he realized that he was not going to get interviews for jobs in North America if they had to bring him from Australia, and he returned to spend the second year of his Fellowship with Laurie Hall at UBC. When the job offers still didn't come, I had an idea one day, and phoned Alfred Bader. The conversation went approximately as follows:

W: "Listen Alfred, I think that Aldrich should start a division of isotopically labelled chemicals, and I know just the person to head this division."

B: "Fine."

And that is how Aldrich entered into the business of selling isotopically labelled compounds.

Shortly after Ralph Whitney joined the department, we were standing in my outer office, smoking our pipes, when Dr. Bader walked in. I introduced him to Ralph, and we then had the following conversation:

W: "Listen Alfred, Ralph is just getting started, and he could use some money. Why don't you give him a grant?"

B: (gulp) "Would $3000 be alright?"

And that was how, at that moment in time, Alfred Bader was personally supporting the research of three famous organic chemists: Nelson Leonard at Illinois, Gilbert Stork at Columbia, and Ralph Whitney at Queen's.

R. Julian C. Brown

1962-1996

I came to Queen's in August 1962. I had just finished my Ph.D. at the University of Illinois, working on the theory of spin relaxation in NMR with H.S.Gutowsky, one of the pioneers of NMR in chemistry. My M.Sc. work at the University of Sydney, Australia, had been on nuclear quadrupole resonance (NQR), a technique to which I returned later. I was hired by the new Head at Queen's, Bob McIntosh, to begin research and instruction in theoretical chemistry. At that time there was very little theoretical work of any sort going on in Canadian chemistry departments.

My first office was just inside the front door of Gordon Hall, where Pat Mulligan now has his prep room. . Not long after I arrived, renovations to Gordon Hall began and I moved to the Frost Wing. In 1962 the Frost Wing was only partially finished internally, and I recall the first NMR spectrometer (a Varian A60) sitting on bare concrete in an unfinished laboratory.

For four years I taught various aspects of physical and theoretical chemistry, and pursued research in molecular dynamics in gases and liquids. I also did some work on the structure of electronic wavefunctions with David Peat. David came to Queen's as a post-doctoral fellow, and stayed to teach theoretical chemistry for a year after I went back to Australia in 1966. Subsequently he became a broadcaster and author, and wrote a number of books including a biography of the quantum theorist David Bohm.

I taught physical chemistry to second year pre-medical students, and upper year courses in quantum chemistry and statistical mechanics. Because of the growing importance of NMR in chemistry at that time, I emphasized the quantum mechanics of spin systems in introducing quantum theory, and dealt explicitly with the common types of NMR spectra. In those days of proton NMR in relatively low fields, it was common to encounter spectra which could only be understood by applying quantum theory, because the spin-spin couplings were comparable to the chemical shifts. I also gave about a dozen lectures on atomic structure and orbitals at the beginning of the third year inorganic chemistry course.

It is hard to remember the simplicity of life in Kingston in the early sixties. The number of "good" restaurants was very limited, only one or two most of the time. The dining room at the Lasalle Hotel (in the Lasalle Mews building), Prokop's Steakhouse out on Bath Road were two. After a while the Townhouse Motel (now the Rest Inn on middle Princess Street) opened and in its early days had a good dining room.

The Town House also provided an opportunity to experience the swinging sixties when they introduced go-go dancers in the basement bar. The main source of coffee on lower Princess Street was the United Cigar Store. There was no University Club, but we were able to make some use of the RMC senior staff mess. Sale of liquor was very restricted in Kingston, and beer was sold only in special beer halls where you had to remain seated at all times (to avoid fights, Bob McIntosh assured me). To buy liquor at the LCBO, you had to fill out your name and your order on a piece of yellow paper. I remember one social evening in the fall of 1962 when Kaaren and I were taken by car to Bath, where there was a hotel licensed to sell liquor as long as you also bought sandwiches. We drank a small amount of whisky sitting around a large plate of sandwiches; when we were finished, the sandwiches were removed and probably were later used to legitimise some other group of drinkers.

The area in the neighbourhood of City Hall was very run down. The original portico on City Hall had been removed from the building, and the rest of the building was in poor condition. (I have heard it said that the city demolished the portico and sold the stone to be crushed up for gravel.) The present Confederation Park was an industrial railway freight yard, and the area to the south, along the waterfront, was occupied by a locomotive workshop and a shipyard. As part of the celebration of the Tricentenary of Kingston in 1973, City Hall was renovated (including rebuilding the portico) and the park was established. We are still waiting to see what is to be built on the site of the locomotive workshop, now called Block D.

In 1966 I returned to my native Australia, to a research position at the Australian Atomic Energy Commission in Sydney. I began my work with some calculations on the structure of beryllium oxide. Later I did some computer simulations on percolation theory in connection with the determination of pore size distributions in porous solids, and started some experimental work on the NMR of solids. At a time of great optimism for the nuclear power industry, there was a very active group of people working in solid state physics, nuclear chemistry and nuclear physics, in preparation for possible introduction of nuclear power in Australia. I learned a great deal from my exposure to research in a large applied research establishment, and in recent years I have returned to further study of several aspects of the work that I started at AAEC.

For a variety of reasons I returned to Queen's in 1969, where I have remained until my (early) retirement at the end of 1996. We found that the chemistry department, Queen's and Kingston had all grown and changed considerably while we were away. The theoretical chemistry group had expanded to four people (Vedene Smith, Hans Colpa, Neil Snider and Doug Hutchinson) and was well established.

On the social level, the Faculty Club had opened in its present location, and with liberalized liquor laws it was possible to have wine and cheese parties there, as well as drinks of all sorts in the bar while standing up.

It was normal penance in those days for those who spent time away from Queen's to be assigned to teach first year, particularly in Applied Science, and that is what happened to me. At that time the engineering course was almost the same as the Arts and Science course, and used the same textbook. In 1970 or 1971, my colleagues (Wally Breck and Jim McCowan) and I concluded that there was no inherent reason why the engineering students should not have a course more suitable for their interests and training. It was at that point that the idea of writing a textbook was born. Don Heyding was the new Head of the department, and was very supportive of the project. Since teaching engineers was not popular, it was easy to arrange that the team of Breck Brown and McCowan should teach three of the four sections of the course for several years in a row. This gave us the stability needed to put together the project.

Our first step was to agree that for one year we would each follow our own interests in our own sections, and have separate examinations. We each developed our own approach with our individual sections, with the understanding that the following year we would choose the most successful parts to form a common curriculum with a common examination. We then put together our own textbook, in the form of two volumes (one for each term) printed locally with the help of the bookstore. The book was used in this form for several years, until Jim McCowan negotiated with McGraw Hill Ryerson for commercial publication. Preparing the manuscript in the days before word processors was, in retrospect, a painful process. I was on sabbatical leave in England in 1979-80, and at one point Jim and Wally came over to Canterbury for several weeks so we could work together on the later stages of the book. The book was published in 1981, and was updated and expanded in a second edition published in 1987. It has always been known as "Breck, Brown and McCowan".

There were several aspects of the book that are worth recording. In every section, we presented experimental data or factual information before introducing the theory. There was an emphasis on providing a firm thermodynamic basis for understanding chemical equilibria and simple phase equilibria. The book included a large table of accurate thermodynamic data, which could be used by students and instructors to deal with a wide variety of phenomena. Those tables were published separately in small blue booklets, some of which are still in use around the department. The department purchased enough of the blue booklets that they could be issued to each student in examinations, obviating the need to add relevant data to each question on the question paper.

An important aspect of the book was the inclusion of information about the role of chemistry in Canadian industry, and the relevance of chemistry to the Canadian economy and the environment.

The 1956 book by Graham and Cragg (at McMaster) contained many photographs from Canadian industry, but all the other introductory books with Canadian content seem to have been written at Queen's. These books are "Breck, Brown and McCowan", Gus Shurvell's Canadian adaptation of an Australian book on the chemical industry, Lloyd Munro's book on industrial chemistry, and my current first year textbook, CHEMBOOK.

In the 1970s I returned to research in experimental NQR. With the assistance of Don Heyding, who generously provided some high pressure apparatus on long term loan, I built an NQR spectrometer for studying spectra up to pressures of 400 megapascals over a range of temperatures. Stan Segel, in the physics department, was also working on NQR and NMR, and I collaborated with Stan and Don on several projects over about a decade. It was as a result of the high pressure studies that we discovered the interesting properties of ammonium perrhenate, a salt which was to occupy me for a long time. For the perrhenate experiment it was necessary to extend the high pressure measurements down to low temperatures, and for this purpose I built a second high pressure system using compressed helium, which worked extremely well from liquid nitrogen temperature to above room temperature, and up to a pressure of 140 megapascals. Since I had just lost my NSERC funding, it was a low budget experiment, and it was hard to keep the work moving forward. The helium compressor was operated by hand. Eventually I regained NSERC support, probably as a result of the work on ammonium perrhenate.

One of the people that I miss is Roger Kewley, a microwave spectroscopist whose career was cut short when he died in 1981. In addition to being a good physical chemist, he had a great sense of humour, and was prominent on the local chess scene. I once saw him play 25 simultaneous games of chess at the Kingston Shopping Centre, and win most of them.

The early 1970's was the period of the review of chemistry departments across Ontario by an organization known as "ACAP". Although initial reports seemed to be favourable, the final report was critical of parts of the Queen's Ph.D. program, and restrictions were imposed. In some areas of the department, this was very damaging to morale for the best part of a decade.

After collaborations with Don Heyding on powder diffraction, with Stan Segel on NQR and NMR, with Ron Weir (at RMC) on heat capacity measurements, with Gus Shurvell on Raman spectroscopy, with Gerald Dolling, Brian Powell and Ian Swainson (at AECL) on neutron diffraction, and with Ruth Lynden-Bell (at Cambridge and Belfast) on molecular dynamics simulations, the "ammonium scheelite" problem is now reasonably well understood.

The powder neutron diffraction work at Chalk River was an opportunity to do essential crystallographic work on several ammonium salts, and to make contact with an interesting group of scientists outside the academic environment. I am continuing theoretical studies with Ruth Lynden-Bell's group in Belfast on the dynamics of ammonium ion rotation in crystals, and we are still finding interesting new phenomena.

I continued research on NQR and NMR of quadrupolar nuclei for several decades, and attended the biennial Symposium regularly. Mariusz Mackowiak from Poland spent two years with me as a post-doctoral fellow in the early 1980s, studying pressure effects. I organized the 1983 NQR Symposium at Queen's, and was Chairman of the International NQR Committee for the period 1989-93. With the advent of high field NMR, many quadrupolar nuclei are now commonly studied, whereas previously they had been ignored by most chemists. I am presently continuing collaborative NMR work with Gang Wu in this field.

The 1985 CIC conference was held in Kingston, and I chaired the scientific program committee. Having organized the NQR symposium two years earlier, I recognized that there was an urgent need to introduce computer techniques into the planning of the scientific program, which contained over 700 papers in 20 parallel sessions. With the help of a student, Tom Haslett, we planned the whole program by computer. In this way we avoided the problem that had plagued earlier conferences, namely lack of coordination between divisions resulting in some speakers being scheduled to give papers simultaneously in different places. Some of our innovations are still recognizable in the 1999 CSC conference program. We used a spreadsheet (Lotus 123) on my 5 MHz IBM PC; I had to increase the size of the memory to half a megabyte to accommodate both the program and data. We even produced and submitted part of the program in electronic form, but the printers in Ottawa were unable to use our files and in producing the program everything was retyped.

In the late 1980s, the Faculty of Applied Science, for reasons related to accreditation, invited proposals for courses that would educate students about the relationship between the engineering profession and society. In response, I proposed a course dealing with disasters, their causes and consequences. I had advocated such a course more than a decade earlier,

but could find no support as it was thought that such a course would frighten the students away from engineering as a profession.

The proposal was accepted in 1988, and I taught the course for four years beginning in 1989. The textbook for the course was Volume 1 of the Royal Commission Inquiry into the sinking of the Ocean Ranger drilling rig in 1982. The course was well received by the students, and for several years it was the largest of the four "linkage" courses, with an enrolment of about 150 students. I handed the course over to the mining engineering department when I went on sabbatical leave in 1993, and the course has continued to be popular.

As part of my preparations and study for the disasters course, I became interested in the problem of global warming, which had become topical partly as a result of the very hot summer of 1988. Chemistry (particularly physical chemistry) is important in understanding the possible causes of global warming, but a full appreciation of the problem requires study of other disciplines as well. As part of the Queen's Sesquicentennial celebration in 1992 I organized a one-day symposium on global warming, with the primary purpose of bringing to students the best contemporary thinking on the subject. There were four speakers, one each from environmental chemistry, climatology, economics, and politics. The speakers were in general agreement that evidence for global warming would continue to accumulate, and that reducing man-made contributions would be very difficult at the economic and political level. These views have become more widely accepted in the period since the symposium. The global warming symposium was one of the few times when the chemistry department has contributed to public discussion of a current issue.

During the period of international student revolts in the late 1960s, Queen's students began to take an increasing role in university affairs. Student membership in committees, faculty boards and the Senate became established practice, and the rights of students as members of the academic community were gradually improved over the next two decades. On several occasions I took an active role in issues in which university was not respecting student rights.

Engineering chemistry has been a centre of discussion in the department during several periods over the past two decades. The program is one of four in the Faculty of Applied Science which are based upon sciences rather than engineering disciplines, and has had some very good students. Prior to the formation of the Faculty of Arts and Science in the late 1950's it was the only undergraduate program offered by the department.

Engineering chemistry has made several important contributions to academic development in the chemistry department. The interdisciplinary environmental program initiated by Wally Breck was begun nearly two

decades before other environmental programs began at Queen's; unfortunately this program was terminated deliberately for reasons related to accreditation.

The engineering chemistry students have been consistently ahead of the Honours chemistry students in the use of calculators and computers, due in part to the work of Brian Hunter in several committees. Starting with the "Breck, Brown and McCowan" textbook project in the mid-1970's, the Engineering Chemistry program developed in ways which differentiated it more and more strongly from the Arts and Science honours program; these developments enriched the department's offerings and attracted many excellent students.

The engineering accreditation review in 1984 (or thereabouts) was a major turning point for the program. Faced with loss of accreditation, the program was radically altered. In several core subject areas, such as thermodynamics and kinetics, students began to take instruction in chemical engineering courses rather than chemistry courses, and in other areas, such as organic chemistry, special chemistry courses were set up with separate instructors. The result was that the Engineering Chemistry students were largely removed from contact with the Arts and Science chemistry students, and the two streams of students diverged in their training and interests, as well as social contacts. Subsequently, some of these changes were reversed, but the two streams of students have remained largely apart. The department supported these changes almost unanimously, and willingly accepted the increased teaching load imposed by the additional engineering courses. The program grew in size as a result of these changes in the program, and graduates continue to do well in subsequent careers. However the changes in the program were not sufficient to avoid continued problems over accreditation.

One consequence of the reorganization in the mid-1980's has been that very few engineering chemistry students have taken senior physical or theoretical chemistry courses over the last decade. It is a pity that these students leave Queen's without adequate training in the recent developments in quantum chemistry, molecular dynamics and statistical mechanics. The usual excuse offered is that the curriculum is overcrowded.

At the end of 1996 I took early retirement. This has allowed me time to carry on several academic activities without the pressure of full employment, and to spend more time travelling. I am continuing my collaborative research with Ruth Lynden-Bell on computer simulations of ionic motion in crystalline ammonium salts. For several years I have taught second year thermodynamics on a contract basis. The main project

that has occupied me is the writing of a first year textbook, CHEMBOOK, which could not have been done without being free of other pressures.

In retirement I have been able to focus my attention for extended periods, which is almost impossible during the ordinary life of an academic.

In this new mode of living, I seem to be busier than ever, and this is turning out to be a very productive time of my life.

Erwin Buncel

1962-2002

Reminiscences of a Career at Queen's in Teaching and Research

THE INTERVIEW

My career at Queen's began with an interview in March 1962. I was at that time in Stamford, Connecticut, where I was employed as a research chemist at the Central Research Laboratories of the American Cyanamid Company. After only 1 ½ years there, and even though I was engaged in fundamental research, I had come to the realization that the academic environment was strongly beckoning me. Canada was the obvious choice, having spent 2 years as a NRC postdoctoral fellow at McMaster prior to my stint at Cyanamid. Even so, I had good memories of a US university from my one year (1957-58) as a Research Associate in Chapel Hill at the University of North Carolina. On balance, however, Penny, and I believed that the Canadian cultural environment would suit us and our two daughters, Irene and Jacquie, better than in the US – more like Britain where we both received our education.

So, on a Sunday in early March 1962, having flown from New York to Montreal, taken the train to Kingston and a taxi to the LaSalle Hotel (the only respectable hotel in downtown Kingston at the time), I had my first impression of the small lakeshore town that was to be our home in the years to come. Bright and early on Monday morning I was met at the hotel (by Saul Wolfe, if I recall correctly) and taken to the Department to meet the Head, Professor McIntosh, whom we came to call R.L., or simply Bob. I was given my itinerary for the day – interviews, lunch with R.L., more interviews, research seminar and wind-up with staff. The day passed in a haze but some memories remain such as the interviews with the organic staff (Professor J.K.N. Jones, Robert Moir and Saul Wolfe), the informal lunch at the LaSalle where R.L. told me about his sailing hobby and enquired whether we would be able to manage on the starting salary of $7,000 compared with what I was receiving ($11,000 U.S.). Finally I was driven to the rail station by Professor Jones, in whom I immediately recognized a true British gentleman.

The interview appeared to have gone well since a few days later I received a telegram from R.L. with the offer of the position as Assistant Professor. That spring and summer was spent completing my project at Cyanamid and preparing mentally for teaching at Queen's. So it was, that at the beginning of September 1962, after camping on the way in the Adirondacks, we arrived in Kingston to take up the challenge of an academic life.

TEACHING GENERAL CHEMISTRY TO PREMEDS

I was to teach general chemistry to First Year Premeds, the Class of '68. This was a group of 65 students who would continue in Medicine provided they received a passing grade in the first 2 years of their programme. Failures were quite rare. The course consisted of 3 1-hour lectures per week (including Saturday at 8 a.m.) and a 3-hour lab. I enjoyed giving it and, although I still had my own undergraduate lecture notes from the University of London, I prepared a complete set of new lecture notes using texts such as Mahan and Glasstone for reference. With a class of 65 and being fully in charge of the lab, I quickly got to know the names of the students and a friendly relationship was established. The class responded well and I was greatly pleased when their Aesculapian Society selected me as Professor of the Year. The only friction came with some members of the Faculty of Medicine during their monthly meetings in Richardson Hall (which all Premed lecturers were required to attend) when term exam results were discussed. Dean Bottrell of the Faculty of Medicine usually took a supportive attitude toward us lecturers and any failures were given a second chance to write a supplemental exam in August.

Overall, I look upon my teaching of Premeds as a challenging but deeply satisfying period. Preparing lectures the first year, as any beginning teacher knows, is a grind, but this was made easier with my background in physical organic chemistry. Most satisfying was the response from a group of students towards an inexperienced, but enthusiastic, young professor. On a number of occasions since then, while receiving treatment in hospital, there has come a loud and friendly greeting from the attending physician: "Hi, Professor Buncel, you remember me in your class of 19.....?"

As is well known, the institution of two premedical years was abolished some time ago, with entry into Medicine being the most competitive among different university faculties. More of that later.

THE CHALLENGE OF TEACHING ORGANIC CHEMISTRY

Over the years I have taught organic chemistry at all levels – 2nd year (currently Chem. 280, previously Chem. 12, 281), 3rd year (381), 4th year (480, 481) and graduate (981, 982, 983, 984) – and different subdisciplines – general organic, synthetic organic and physical organic. The class would rarely consist exclusively of honours students, more often of life science and engineering students (including Mining and Metallurgical Engineering in the early years). The challenge was how to give a "service course" in a meaningful and interesting way. Often we erred on giving the course in a more demanding way than was warranted.

Why is organic chemistry apparently more demanding than, say, 1st year general chemistry? The upper years of the high school curriculum cover a goodly portion of the material of the 1st year general chemistry course, while very little, if any, organic material is covered there. As a result, in the introductory organic course the students are expected to learn the basic vocabulary, grasp the grammar and, finally, how to write an essay in a new language. The very good students have no trouble in meeting the challenge that organic chemistry offers, and even come to love the subject, but those who are unable to make the mental commitment become increasingly discontented. In the life sciences/biology stream, the majority (over 50%) intend to apply to medical school and that's where the crux of the problem lies - organic chemistry is a pre-requisite for them. They are expected to do well in their most difficult subject, and many medical faculties use organic chemistry as a weeding process. This leads to a scrambling for marks on the part of student and even to some bad practices.

To return to my personal experiences, I say without hesitation that I derived enormous satisfaction from the teaching of undergraduate students at all levels. In line with my own training in the Ingold-Hughes School at University College, London, I have always taught organic chemistry from the viewpoint of structure and mechanism, delving deeper and deeper on going from 2nd to 3rd to 4th year courses. Even the 3rd year course in organic synthesis (Chem. 381) could be so approached, as for example the synthon method being used to analyze Sir Robert Robinson's "one-pot synthesis" of tropinone for which the classical Willstatter synthesis consisted of 17 consecutive steps. Another example that I used was Nicolou's synthesis of endiandric acid, which is based on molecular orbital theory, specifically electrocyclic processes governed by Woodward-Hoffmann rules. So the student was introduced to the 'beauty' of organic synthesis.

All is not rosy in the garden and I would like to recount a point of dissatisfaction, namely the increasingly large classes which are becoming the norm. During the 1960's, 70's and even early 80's, our 2nd year organic classes did not exceed 100 students. In conjunction with lab supervision, one could learn the names and get to know most of the students. In the late 80's, class sizes crept up to 200 and over, reaching 400 by the late 90's. Now, we have some academics leading very large classes which look to me more like performances than traditional lectures, where the professor could make eye contact with the students and look for recognition in their faces as to whether the ideas have come across or not. I have always tried to engage the students in discussion during class but that is almost impossible in a class of 400 students.

I realize that the computer brings with it opportunities in teaching methods but I cannot agree with the stance (e.g. of the Queen's School of Business) which requires all students to bring lap tops into class, "to ensure that all students would understand all the material presented in class". I believe that some ideas may not be immediately understood but require further thought, in private, before light pierces through and one can exclaim "Eureka"!

RESEARCH – ITS BEGINNING AND MATURING

When I began writing these reminiscences I thought that the section on Research would be easy since, after all, my love of research has been the dominant guiding force of my academic career. Now, I see otherwise, for how can one summarize, in a couple of pages, the work of 35 years involving over 50 graduate students and an equal number of postdoctoral fellows, which has led to the publication of over 250 research papers in refereed journals, 20 major reviews/book chapters, 3 texts and 15 edited monographs? So I will not attempt an overall summary but instead will recount some highlights of my early period at Queen's, namely 1962-1970, and then the most recent period through the 1990's, with some overlaps as needed to tell the story. This telling will reveal also how the great diversity of my research came about.

From chloro sugars to nucleophilic substitution at carbon, phorphorus and sulfur centres, to metal ion-biomolecule interactions and environmental studies

My first few years at Queen's were greatly influenced by my association with Professor J.K.N. Jones, whom we called simply J.K. A world-renowned carbohydrate chemist, he was a marvelous man, a gentleman in all ways and generous almost to a fault. Several eulogies have been written about him after his death in 1976 just prior to his 65th birthday, and this is not the place to recount them. After intensive discussions with J.K.'s group I decided that I could make a contribution in the area of sugar chlorosulfates which were being exploited by several of J.K.'s students for the synthesis of chlorosugars. I reasoned that the processes were occurring by the sequence ROH $\rightarrow$ $ROSO_2Cl$ (retention) $\rightarrow$ RCl (inversion) with reactivity being governed by steric and electronic factors in ground state and transition state. The great variety of structures widely available to the carbohydrate chemist proved fertile ground for my hypothesis and several joint papers followed with J.K.'s group (notably Harry Jennings, Inge Thiel, Tim Mepham).

At the same time, I was fortunate that a graduate student of J.K.'s, Peter Millington, who was just completing his M.Sc., decided to continue on to the Ph.D. with me, and I proposed to Peter a classical structure-reactivity study involving a series of alkyl chlorosulfates designed to undergo a change in mechanism from SN2 to SN1. The results verified my ideas but, unexpectedly, while analyzing the thermodynamic activation parameters for solvolysis I observed that the entropies of activation were abnormally high in comparison with usual values in solvolysis of RX derivatives. I interpreted this on the basis of multiple bond scission in the transition state, i.e. Nu----R----OSO_2----Cl and R----OSO_2---Cl for the SN2 and SN1 processes, respectively, and proposed $\Delta S^{\#}$ as a criterion of such fragmentation. This was published as a communication in 1964 and two full papers in 1965 – a good beginning. Next I suggested to an incoming student, Peter Bradley, for his M.Sc. project that through structural variation one might induce a change in mechanism for the classical glycoside hydrolysis, from A-1 to A-2. The substrate I chose for this, methyl-2-chloroglucopyranoside did, indeed, show expected trends in $\Delta S^{\#}$, and in the log (rate) versus H_o plot (where H_o is the Hammett acidity function), marking another successful foray into carbohydrate mechanisms. Yet another was the interpretation that ascorbic acid anion acted as a bidentate nucleophile (Keith Jackson).

By 1970 we had completed our studies of model chlorosulfates with an article in Chemical Reviews, having extended reactivity patterns to different bond scission processes under the series heading "Bond scission in sulfur compounds" . Later, the studies were further generalized to nucleophilic substitution processes at carbon, phosphorus and sulfur centres, exploiting an unexpected observation of alkali metal ion catalysis in these processes. This was discovered by Ted (E.J.) Dunn in 1984 and we have since then been busy delineating different systems as to the roles played by metal ions in bioorganic processes. Apart from Peter Millington and Ted Dunn, the following graduate students have been involved on these related projects: La Im Choong (Lancaster), Alain Raoult, Jim Wiltshire, Claudio Chuaqui, Ken Albright, Marko Pregel, William Park, Rick Tarkka, Ruby Nagelkercke, Ping Liu, Vim Balakrishnan, John Omakor and Mike Annandale. Postdoctoral fellows on this project included Paul Forsythe, Parsanathy Krishnan, Nguyen Truong, Anurag Kumar and Hai-Qi Xie.

The role of metal ions in biological systems had intrigued us for a number of years and already in 1979 together with Al Norris and Bill Racz (Pharmacology) and with the support of NRC's Strategic Grants Programme, we launched a series of investigations entitled Metal Ion-Biomolecule Interactions.

Initially this was concerned mainly with the interaction of heavy metals, especially methylmercury (CH_3Hg^+), with DNA bases but was later expanded to other metals of interest, e.g. Co^{3+}, Cr^{3+} and Pt^{2+}, with the participation of a number of students: Charlie Boone, Diane Yee, Jim McKenzie, Laurie Vanderwater, Helen Joly, Fan Yang, Xinhua Jin, Omoshile Clement and postdoctoral fellows Spencer Taylor, Hiroyuki Abe, Raj Kumar. Currently this project is being pursued jointly with Donal Macartney. These studies have led us to the environmental sciences, initially as environmental toxicology, and currently, together with Gary vanLoon, on the fate of agrochemicals in the environment. It turns out that many agrochemicals, such as pesticides, are phosphorus ester derivatives, often attached to an aromatic moiety, so the same principles of nucleophilic reactivity, catalysis, etc., apply.

From the Wallach rearrangement to molecular electronics

Molecular rearrangements have always fascinated me and in reading the chemical literature prior to coming to Queen's I found that no kinetic study had yet been reported on the rearrangement of azoxybenzene to p-hydroxyazobenzene in sulfuric acid media, known as the Wallach rearrangment after its discoverer (1881). Incidentally, Wallach was later awarded the Nobel Prize for his various discoveries in organic chemistry. What was amazing to me was that different workers – Russian, Japanese Australian – had speculated on the mechanism and proposed various reaction intermediates, without having undertaken a kinetic study! Any plausible mechanism would have to account for the role of acid but this was not attempted. Following my experience as a postdoctoral fellow with Professor Bunnett in Chapel Hill and previously under the influence of the Hughes-Ingold School in London, it was clear to me that a kinetic study would provide these answers, and so with a starting M.Sc. student, Brian Lawton, we embarked on this project in the Fall of 1962. By the Spring of 1963 we had the answer and our communication appeared later in the year with a full paper in 1965 and a book chapter in a monograph on molecular rearrangments in 1968.

How this initial study became metamorphosed into our present investigations in the general area of materials science – photogenic and photoconducing dye/pigment molecules, thermo- and photochromic compounds, molecular switches, information storage, xerography and molecular electronics – is a long story, albeit exciting, and will have to await another account. However, the names of individuals who took part in this project must be mentioned.

Graduate students: Brian Lawton, Kandiah Mahendran, Bill Strachan, Allan Dolenko, Sam-Rok Keum, Srin Rajagopal, Andrew McKerrow, Kap-Soo Cheon, James Wojtyk, Nabil Mailloux; postdoctoral fellows: Robin Cox, Ikenna Onyido, Yuehui Zhou, and a long lasting collaboration with Peter Kazmaier of Xerox. An account of this research was given in my Award Lecture for the R.U. Lemieux Award in Organic Chemistry, presented by the Canadian Society for Chemistry in 1999.

Electron deficient aromatics-base interactions; sigma complexes, super-electrophiles

Another area of research was initiated quite early on with an undergraduate student, Allen Zabel, in the summer of 1963. In the early 1960's, dipolar aprotic solvents such as DMF and DMSO came into the limelight following the seminal studies of Donald Cram at UCLA and Alan Parker in Australia which revealed the dramatic effect of these solvents on a variety of reactions including up to 10^9 rate acceleration of SN2 type and proton transfer processes, while greatly influencing also their stereochemical outcome. Being interested in the reactivity of aromatic systems. I decided to investigate, on a hunch, how solvents such as DMF and DMSO influenced hydrogen–deuterium exchange in compounds such as nitro-activated aromatics where some literature reports appeared to me as contradictory. H/D exchange was a key feature of my project at American Cyanamid so with Allen Zabel and Alan Symons, a student in our Applied Science stream, first as summer students and later as graduate students (both earned their Ph.D. degrees in 1970), we delved into different fascinating aspects of the interaction of electron-deficient aromatics with bases. This research proved to be remarkably varied, broaching on topics such as charge-transfer or π-complexes, covalent anionic sigma complexes, electron-transfer, proton-transfer, as well as nucleophilic aromatic substitution. It has been very productive with a number of reviews, the first in Quarterly Review of the Chemical Society (1968) co-authored by Albert Norris and Ken Russell, followed by a chapter in the Patai series (1981), a monograph (1985) jointly with Mike Crampton (U.K.), Mike Strauss (U.S.) and Francois Terrier (France), and yet another article in Chemical Reviews (1995), co-authored by Francois Terrier and Julian Dust (currently at Memorial University).

My most able co-workers on this project have been – graduate students: Allen Zabel, Alan Symons, John Webb, Suresh Murarka, Dave Ferguson, Rick Renfrow, Julian Dust, Richard Manderville; postdoctoral fellows: Bill Proudlock, Bob Tucker, Peter Sheridan, Harry Wilson, H.W. (Ed) Leung, Andrzej Jonczyk, Walter Eggimann, Masachi Hamaguchi, Christobal Innis, Alain Chattrousse, Ikenna Onyido, Kuk-Tae Park.

This general topic, featuring the interaction of electron-deficient nitro-aromatic electrophiles and super-electrophiles with electron donors, formed the basis of my Award Lecture for the Syntex Award in Physical Organic Chemistry by the Canadian Society for Chemistry in 1985.

From deuterium exchange to carbanions, to group 14 anions

Implicit in the hydrogen-deuterium studies was the generation of carbanions, with attendant questions on their stability, reactivity and so on. I became interested in this area and over a 3-year period wrote the text "Carbanions. Mechanistic and Isotopic Aspects" (1975). The "isotopic" angle came from my experience at McMaster as a postdoctoral fellow with Arthur Bourns, who later became President of McMaster . Currently, together with Julian Dust, we are revising this text under the heading "Carbanion Chemistry. Structure and Mechanism", to be published by ACS in 2000. Moreover, our work has encompassed not only carbon centred anions, but also other group 14 centred anions, namely silicon, germanium, tin, and lead anions, in collaboration with Professor Ulf Edlund of Umea University who is a renowned NMR expert. In this way I have come full circle, since my Ph.D. topic under Proefssor Alwyn Davies in London was concerned with organosilicon chemistry (though not anions). Graduate students whose thesis topics focused on isotopic exchange/carbanion studies include: Alan Symons, Allen Zabel, Kevin Walkin, John Davey, Omoshile Clement and Krishnan (Venku) Venkatachalam; postdoctoral fellows: Shi-Ming Wu, Balachandran Menon.

MY LIFE AS AN EDITOR

Complementing the research activity, I became extensively involved with editing over the years. While working on the "Carbanions" book in the 1970's and delving into different aspects of isotopic studies it occurred to me that the great power of this method could be documented through a series of monographs containing contributed articles from authors expert in different areas. Together with Chuck Lee of the University of Saskatchewan we founded the series of monographs "Isotopes in Organic Chemistry" of which 8 volumes were eventually published. (I co-authored a chapter featuring isotopes in aromatic rearrangements.) This series was later expanded under the title "Isotopes in the Physical and Biomedical Sciences" co-edited by my friend John Jones (University of Surrey) with 3 volumes published on diverse topics including NMR studies, labellings, etc. Yet another series, "Comprehensive Carbanion Chemistry", in 3 volumes, was co-edited by Tony Durst at the University of Ottawa with one chapter authored by Balachandran Menon and myself.

I was also asked to edit the Proceedings of an IUPAC Conference on Physical Organic Chemistry (1981), and in 1991 I co-edited the proceedings of the International Isotope Society Conference of which I had been co-chair. To round off the editing experience, I was an editor for the Canadian Journal of Chemistry for 10 years until I gave that up in 1992, and from 1995 I have been an editor for the Journal of Labeled Compounds and Pharmaceuticals. As well, I have been appointed to the International Advisory Board of a number of journals: the U.K. Royal Society of Chemistry Perkin Transactions, the Journal of Physical Organic Chemistry, and Chemistry International.

MY LIFE WITH ISOTOPES (cont'd)

My experience with isotope chemistry, which dates back to my postdoctoral fellow years at McMaster, has continued to influence events for me. In 1982 the first conference of the International Isotope Society (IIS) took place in Kansas City where I was asked to present a plenary lecture and to join the Executive Committee (as Trustee, etc.). The IIS continued to grow in scope with local national chapters being established and I became the founder and first President of the Canadian Chapter of the IIS. In 1991 the tri-annual conference of the IIS took place in Toronto with myself as Co-Chair. I continue to be actively involved in the IIS, participating in all conferences and also as Chair of several Canadian Chapter meetings.

WHERE CREDIT IS DUE - IT TAKES A VILLAGE

Undergrads, grads, postdocs

An old African proverb says that it takes a village to raise a child. In the same way I can never forget that research has been a collective effort. I am fully aware of my indebtedness to the many co-workers with whom I have had the privilege of being associated over the years – undergraduates, graduate students, postdoctoral fellows and many colleagues at Queen's and in different countries. Proper acknowledgement of credit is of great importance in academia, and rightly so seeing that a graduate student spends 3, 4 or even 5 of his/her prime years in close collaboration with her/his research supervisor in what is essentially team work. Undergraduate students are even more impressionable, so our responsibility towards them as role models is even greater. (Undergraduate students have not been specifically named in this account, there are simply too many for that, but I receive letters [or e-mail] from many and I like to meet with them on Homecoming Weekends. They always recall their organic chemistry courses and we laugh together over

them.) Postdoctoral fellows are looking to expand their experience so as to promote their careers in industry or academia.

I feel that I have been most fortunate in that these young people placed their trust in me and I am greatly indebted to all of them for their contributions. It has been especially meaningful that a number of these former co-workers have followed in my footsteps and have become independent researchers on the faculties of different universities in Canada, the U.S.A. and around the world. Several contributed articles to the Special Issue of the Canadian Journal of Chemistry (June, 1998) honouring my contributions to chemistry and were most generous in their attributions. (This special Issue contained 46 articles by scientists from 11 countries in 5 continents.)

Queen's colleagues

Formal acknowledgement is also due to a number of colleagues at Queen's. The very important role that Ken Jones played in my early years at Queen's has already been highlighted. Bob Moir was another colleague who shared with me his deep knowledge of organic chemistry and who played a role model to us as a devoted teacher with a remarkably original mind, often ahead of his time. I had deep respect for him. Other colleagues with whom I have had extensive collaborations are Albert Norris, Ken Russell, Gary vanLoon and Saul Wolfe, until he left for Simon Fraser, while somewhat shorter projects were shared with Brian Hunter, Hans Colpa, Bob Gordon and Gus Shurvell. I currently collaborate and share students with several of the younger faculty, namely Greg Thatcher, Bob Lemieux, Steven Brown and Donal Macartney. Less formal but nevertheless friendly, supportive relationships have existed as well with other colleagues, especially my organic colleagues Ralph Whitney and Walt Szarek, and, since his appointment as Head of the Department, Stan Brown. Special thanks go to Bob Lemieux and Greg Thatcher for organizing on my 65th birthday the Ontario-Quebec Physical Organic Minisymposium (POMS) at Queen's. It was truly a memorable occasion for me as well as Penny, Irene and Jacquie.

Collaborations, national/international

Long lasting collaborations with colleagues outside Queen's must also be mentioned. While an interesting story could be written about the origin and the development of these collaborations, space limitations only allow a listing of their names, institutions and countries of origin:

Bob Bannard (DREO), Andre Beauchamp (University of Montreal), Michael Crampton (Durham University, U.K.), Brian Cox (Zeneca, U.K.), Robin Cox (University of Toronto), Tony Durst (University of Ottawa), Ulf Edlund (Umea University, Sweden), Jean-Francois Gal and Pierre Maria (Nice University, France), Shmaryahu Hoz (Bar Ilan University, Israel), John Jones (Surrey University, U.K.), Peter Kazmaier (Xerox), Sam-Rok Keum (Korea University), Chuck Lee (University of Saskatchewan), Ikenna Onyido (Makurdi University, Nigeria), Kuk-Tae Park (Korea National Univeristy of Education), Garfield Purdon (DRES), Sason Shaik (Ben Gurion University, Israel), Brian Smith (London University, U.K.), Bob Stairs (Trent University), Michael Strauss (University of Vermont, U.S.A.), Francois Terrier (Versailles University), Lynn Williams (Durham University, U.K.). I am indebted to these colleagues for the friendship and trust they have offered me.

These international collaborations have had a major influence on our research over the years. A direct result has been exchange visits with these colleagues, myself visiting their institutions and the reverse, for exchange of ideas, developing new research and writing joint papers. Equally important, graduate students from my group were able to enjoy extensive study visits abroad: the University of Surrey (U.K.), University of Versailles and Nice University (France), and Umea University (Sweden), thereby greatly enhancing their experiences.

Funding

No researcher can operate nowadays without financial support of his/her research from granting agencies and I have been very fortunate over the years that a number of these have generously supported our work. The National Research Council of Canada (NRC), later the Natural Sciences and Engineering Research Council (NSERC), have been the primary grantors of our research under the title of "Dynamic Aspects of Organic Chemistry", while other Canadian grantors have included the Ontario Research Foundation, Atomic Energy of Canada, Defense Research Board, Defense Research Establishment Ottawa and Suffield, AUCC, URIF, OCMR, MMO, ESTAC. Granting agencies outside Canada have included the American Chemical Society Petroleum Research Fund, NATO and the U.K. Royal Society of Chemistry. I would like to give special mention and thanks to the Xerox Research Centre of Canada (XRCC) for 10 years of support and the friendship of Peter Kazmaier. The Canadian International Development Agency (CIDA) is thanked for major funding and enabling our association with the University of Ibadan and the University of Agriculture, Makurdi, Nigeria, and the friendship with Jack Hirst and Ikenna Onyido.

Finally my thanks to Penny for her unrelenting support and friendship, and to Irene and Jacquie, for their understanding and for forgiving me the time spent away from home.

AND NOW.....

On August 31, 1996, having reached the age of 65 a couple of months earlier, I reached mandatory retirement and was named Professor Emeritus. (By contrast at York University and at McGill retirement age is at 70, while at the University of Manitoba there is no retirement age at all, just as in the U.S. where mandatory retirement has been outlawed as unconstitutional.) I have long known that I would want to continue teaching and research after 65, if at all possible. This has become a way of life for me and I felt that I had still much to contribute. Fortunately, the granting agencies felt the same way and in 1997 NSERC renewed my research grant for 4 years, and I have been able to obtain other grants (ESTAC, OCMR, MMO) since then. (Possibly NSERC's view is influenced in part by the fact that there is no mandatory retirement for scientists working at NRC labs, with Gerhard Herzberg fully active past 80, and neither is there mandatory retirement for scientists in the federal civil service.) Again, fortunately, the administration at Queen's currently has an enlightened attitude towards retirees. I have been allowed to stay in my office (which is now choked full with journals, texts, research notes, lab note books, reprints, reports, journal correspondence, past exams, student records, etc.) I have been allowed to retain most of my lab space in the present building and space is allotted to me in the planned new chemistry building. The retirees are encouraged to collaborate with regular faculty which suits me well as it allows me to continue the various collaborations with colleagues that I have had for years. My working week and schedule is the same as before, but I do miss teaching undergraduates – I miss the stimulation/inspiration of the class environment. But I feel happy doing research with undergrads, grads, postdocs and continue working with my international collaborators, as well as the editing activities.

So, I look forward to the challenges that unfold in science, while doing my best to maintain connections with developing countries and contributing my expertise on that front. While our work with Nigerian colleagues, which became greatly extended in scope through CIDA support, enabled us to make an inroad with developing countries, greater opportunities were opened up following my appointment in 1993 as the Canadian representative on CHEMRAWN (Chemical Research Applied to World Needs), a Committee under the auspices of IUPAC with the mission:

(1) to identify human needs amenable to solution through chemistry with particular attention to those areas of global or multinational interest;

(2) to serve as an international body and forum for the gathering, discussion, advancement and dissemination of chemical knowledge deemed useful for the improvement of humankind and the environment.

During my association with CHEMRAWN I have attended meetings and conferences in Lisbon, Washington, Berlin and Seoul, Korea. Currently, we are planning a world conference on "Chemistry in the Service of Sustainable Agriculture and a Healthy Environment in Africa and the Developing World" with myself as co-chair. I have derived great satisfaction from being able to participate in this international effort with like-minded scientists and hope to continue contributing in a tangible way.

George Hay

1963-1994

The relatively small, relatively young, but absolutely talented and amicable community of the Chemistry faculty into which my wife and I were welcomed in the late summer of 1963 made us certain we had made the right decision. From the very outset the idiosyncratic character of the Department was impressed on my consciousness. While waiting to see the Head of the Department, Bob McIntosh, that first day, Mrs. Jardine, his terrorizing secretary, sloughed me off on Don Heyding. As we sat in his office, I, somewhat overwhelmed by all the changes taking place in my life and he, probably wondering why this bland person had been inflicted upon him, suddenly asked, in his laconic style, "Any tads?" Saucer-eyed in bewilderment, and non-plussed by this question pertaining to frogs, I stammered some stupid, forgettable response. With pained restraint he tried again, "Any children?" But the relationship now was cast in concrete; I never really recovered from it. Many years later, when Don was Head I still retained that apprehensive feeling, whenever we had a meeting, that he was going to ask something utterly incomprehensible to me in the context and sigh at my vacuous response.

There was a sense of camaraderie then which in many cases bordered on something deeper. The frequent invitations to share a meal at one another's home, the plenary gatherings at Bob McIntosh's or Don Heyding's home, and the excitement of being a part of a Department that was on the verge of burgeoning, not only in size but in research accomplishments and teaching vigour, bred a contagious enthusiasm.

The real "Head" of the Department, from my perspective as an organic chemist, was Bobby Moir. It was to him I turned for advice about teaching and marvelled at the comprehensive acquaintance he acquired with his students---and not just his graduate students but *all* of them. He knew where they came from, the names of their parents and siblings, and often their deepest secrets. He provided me an example of how to relate to students at every level. His wry humour often unsettled me and challenged my assumptions about life, but I never knew him to exhibit malice in any area of our friendship. His reminiscences about alkaloid chemistry were unending and ceaselessly of interest, and his insight into conformational analysis and the (for me) mysteries of theoretical organic chemistry were far-reaching and novel. If Bob Moir was the "Head" the renowned and ever gentlemanly J.K.N. Jones was the counsellor for me.

He was as generous in affording me time and advice on research and grant applications and opportunities to publish with him, as he was in giving me access to his important and expensive lab equipment such as gas chromatographs and gel and paper electrophoresis apparata as well as exotic chemicals and reference samples. Famous as he deservedly was, there was never a hint of superiority in dealing with a junior such as I, and this courteous openness extended to all who had contact with him

However, when it comes to those who nurtured the Department during the past thirty-something years I would point to the cadre of so frequently overlooked technicians who backed up and made feasible so much of the teaching. The ones I knew best were Alf Bialek, Art Hutchinson, Pat Mulligan, David Ward, and Ted Ison, and Greg Richmond. Pat Mulligan, with his seemingly endless patience and electronic skill surely instructed and rescued numberless students faced with the idiosyncracies of the T-60 NMR spectrometer, various gas chromatographs and IR spectrometers. He had a cheerfulness and sense of perspective that was invaluable. Art, David, Ted and Greg possessed a huge store of practical knowledge that they were happy to share.

For many years Art was the sole possessor of the recipe for British Museum preservative--a messy mixture of lanolin and various solvents--and supplied literally gallons to the library on request. During the long hours of supervising labs in the old "bull pen" he amused any who would listen with his wryly humorous tales of operating a sugar plantation in Central America. In a day when computerized inventories and bar coding were unknown, Art had an encyclopedic knowledge of the hundreds of chemicals stored in such noxious proximity to his desk in Gordon Hall. He seemed to know not only what was there but also approximately how much was there--perhaps reflecting his Scottish frugality. But he was extravagant in his willingness to assist any who came for assistance.

Ted and David were different in that, having PhD's in chemistry, they did formal teaching as laboratory supervisors. Both loved to interact with students. Both had a unique sense of humour that endeared them, and laboratory skills which they demonstrated with immense excellence and delight to floundering students. When seemingly insurmountable problems with an experiment arose it was to one of these experts the student almost invariably turned (as did I in many instances).

One year, during the clean-up-and-check-out period, two students having the surnames Martin and Remy, presented David with a fine bottle of Remy Martin in appreciation of his help over the year. David also taught some sections of students in the engineering program.

He loved to teach and his abilities were almost instantly recognized by the students who saw to it he received a Golden Apple award.

Greg, too, delighted in interacting with students and couldn't be kept out of the lab. He ruled the first-year labs with a firm but velvet glove, always on the move, always helpful and cheery, always available. When one thinks of the thousands of students that moved through the undergraduate labs and the extent to which so many interacted with these persons, I suspect that for many past students they were the most memorable contacts with the Department. Their significant, and ongoing contribution to the development of the Department is something that deserves recognition

Roger Kewley

1964-1981

Roger Kewley was born in the Wirral region in the west of England. After school, he served two years of national service in the Air Force, and then studied at Cambridge both as an undergraduate and graduate student. He took his Ph.D. in physical chemistry under the supervision of T.M. Sugden, and then crossed the Atlantic to a post-doctoral fellowship with Walter Gordy in the physics department at Duke University. Following that, he spent a year working with Harry Gunning at the University of Alberta. He was appointed to Queen's as assistant professor in 1964.

Roger's field of research was microwave spectroscopy. As was the tradition, he built his own spectrometer at Queen's, and among his graduate students were Jim Sloan and Richard Lowe. During several sabbatical leaves he visited the laboratories of Bob Curl (subsequently Nobel prize winner) at Rice University in Houston in 1971-72, and Ian Mills at University of Reading in 1978-79. He attended the spring Ohio State conference on spectroscopy regularly with a group of spectroscopists from Queen's, including Dave McLay from physics.

Roger once remarked that, in the United States of those days, research in microwave spectroscopy was largely the preserve of southern gentlemen. Coming as he did from England, he couldn't be a southern gentleman himself, but he did the next best thing and married a southern lady, Jane, who grew up in Tennessee.

During his sabbatical leave in Reading, he was diagnosed with a brain tumour, and underwent extensive treatment in England. He and Jane returned to Kingston in 1979 and he carried on with teaching and research. His lecture notes and problem sets for the fourth year kinetics course for the fall of 1980 are still extant. His last paper on microwave spectroscopy (on which he was the sole author) dealt with ring-puckering and ring-twisting in -valerolactone; the paper was received by the Journal of Molecular Spectroscopy on 5 August 1980, and was published in 1981. But eventually he had to withdraw from his work in the department. During his illness he retained his characteristic courage and humour, and with the support of Jane, the parishioners of St. James' Church, and his friends in the department, he maintained an active social life. He passed away in 1981.

Roger taught physical chemistry at all levels, and the first year course for students who had not taken Grade 13 chemistry. He liked conundrums in physical chemistry and had a great sense of humour. On one April Fool's day, he lectured the second year class on "How to Make Ethel Palpitate".

Roger and Jane entertained his classes at home regularly. According to his lecture notes, they invited the fourth year class of 1980-81 to dinner on 26 October. Several times the entire CHEM 240 class came to dinner, about 40 people at a time. They also entertained Gael groups at the end of Orientation Week each September.

Roger had many interests outside chemistry. He enjoyed music, and played the piano and the cello. He collected limericks and rhymes, and comedy records. He took courses in art while on sabbatical leave in Texas. He was a keen and knowledgeable birdwatcher. He played golf (one of his partners was Mike Baird), and duplicate bridge, particularly with Bob Curl in Texas.

He was a very good chess player, and won the Kingston Whig-Standard Chess Tournament several times. I once saw him playing 25 simultaneous games of chess in public at the Kingston Shopping Centre; as I remember, he won most of them.

He is sadly missed by those who knew him.

RJCB

John A. Stone

1965-2002

I arrived in Canada in late 1958 as the first NRC Postdoctoral Fellow at Atomic Energy of Canada in Chalk River and after two years I joined the permanent staff. In 1964 I attended the annual CIC Conference which was held in Kingston and there I met Dr. R.L. (Bob or RL) McIntosh, the Head of the Queen's Chemistry Department. He seemed to me to be a very human and certainly amusing individual. At the time, Queen's was rapidly increasing in size and he had the mission of expanding his department. Since I did not intend to spend all my working life at Chalk River, I later wrote to him asking about the possibility of a faculty position. His return letter was so very badly typed that my wife suggested that Queen's could probably not afford secretaries and "the poor man has to type his own letters." I was invited down for interviews, which consisted of meeting individual faculty members in their offices or laboratories. I was never introduced to a dean or put through any of the procedures to which prospective faculty are now subjected. We had lunch at RL's house, prepared by his sister Kitty, where he presided and I obtained my first introduction to his wealth of anecdotes. The result of the visit was that I received another badly typed letter offering me a position of Assistant Professor in the field of analytical chemistry. This offer I declined, saying that although I had had experience in and was very interested in gas/liquid chromatography and mass spectrometry, I was a physical chemist and wished to remain as such. RL's reply was that I would be welcome as a physical chemist – I soon found myself teaching analytical chemistry however. He later told me that the analytical chemist he really wished to hire was someone who was at the University of Toronto and of whom I had never heard – John Page.

An interesting event occurred soon after I arrived, when new faculty members were invited to a Principal's evening reception in the Agnes Etherington Art Gallery. Alcoholic beverages were served, which I was told was a very rare, if not unique, event on the Queen's campus. Indeed, at that time Kingston was a dry city and we had to travel across the Little Cataraqui River, the City boundary to find a restaurant with a liquor licence.

There were no research start up funds but a small NRC research grant (the forerunner of the NSERC grant) could be applied for in the Fall and the money came through promptly early in the new year. I was a member of the fraternity of gas passers, as RL described researchers who worked with vacuum lines.

These, of necessity, required large quantities of mercury for McLeod gauges, mercury diffusion pumps etc. 15 years later, when my research had turned wholly from radiation chemistry to mass spectrometry, metallic mercury had become a deadly poison and I had much difficulty getting rid of the the large quantities that I had on hand. My "office" during my first year was a small narrow lab on the top floor of the Annex, very cold in winter and very hot in the summer. However, to compensate for this discomfort, the office across the hall was occupied by Lloyd Munro, who to my way of thinking, knew everything that there was to know about practical, everyday chemistry. Lloyd taught very practical, applied chemistry to engineering students and wrote a book on the subject that was translated into five or six different languages. One very senior faculty member from an engineering department complained to me once that Dr. Munro had no business telling engineering students what thickness of boiler plate should be used in power stations, that was the job of real engineers.

In my first year Bob Wheeler showed me drawings of the layout for the imminent renovation of Gordon Annex. He showed me where my lab and adjacent office were to be on the third floor and helped me plan lab layout, even down to where the many electrical outlets etc were to be sited. I was especially pleased with the many bookshelves that would be in my office. In my second year I therefore moved to the new Dupuis Hall and for a year sojourned in an excellent office while the Annex renovation occurred. We also moved the second year physical chemistry lab to Dupuis where Roger Kewley and I erected plywood tables in a bare laboratory to hold the equipment. We even conscripted Roger's wife Jane to help in painting the wood. One year later we moved everything back to the Annex. Very little had been done in that building except to put a floor in the two storey open pilot plant area recently vacated by the Chemical Engineering Department. Two other changes that have had a lasting impact on our department were the provision of the two worst lecture rooms on campus, GA20 and GA30.

The one year away from the department was not entirely lost since I moved back to an office, this time a real one, on the third floor of the Frost Wing. However, this office was directly on the path taken by Bob McIntosh when on his way from his office one floor below to the coffee room on the third floor. Since he was very gregarious, he never drank coffee alone and so would collect people along the way to make up the group that he would regale with stories of his wartime experiences and of the characters that he had known. We were living in the time when chemistry in Canada was almost synonymous with physical chemistry since many department heads were physical chemists with backgrounds at both the National Research Council and also with wartime projects. RL knew them all. Regular habitues of the coffee club were Wally Breck and

Bob Wheeler, who had both been in the services during the war and had great funds of stories – some of which sometimes stretched our credulity.

Class sizes were small in the 60's. A class of 100 students was very large. I taught analytical chemistry, CHEM270, for many years and each year knew each of the 60 or so Honours and Engineering chemists in the class. This was relatively easy since each student was in the lab for two afternoons each week. Much of the laboratory work in the course was of the quant/qual type with heavy emphasis on wet chemical analysis for cations and anions. However things were changing, and with some money available for instrumentation we bought the first simple gas chromatographs and polarographs (mercury was not then a dangerous material) and we had access to the newly-installed atomic absorption spectrometer. Classes were small enough that in the second half of the winter term the students did small research projects, sometimes in cooperation with faculty outside the department. Some examples: the lead content of the dirt at the side of the Kingston roads (large quantity found); the fluoride content of teeth (carefully saved by a parent from a student's early years); the mercury content of the fish served in residence (none found).

When I first arrived at Queen's there was a scientific society, the Baconian Society, with members from all faculties, even the Medical Faculty. The meetings were held on Friday afternoon when a lecture of general interest would be given by one of the members. The meetings were lively, with much discussion at the end of the lectures. However, the University was growing rapidly and more and more competing specialized scientific meetings were being held in the individual faculties and departments. In 1969, when I was President of the Society, no one was willing to assume the organizational duties and the Society died. To me, this signaled the end of Queen's as a small university, and we lost something of value that would never be replaced.

R.L. McIntosh retired as Head of Department in 1969 to take up the position of Associate Dean in the Arts and Science Office. The University advertised externally for a replacement and we had a number of potential candidates who came to look us over and to give us an opportunity to look them over. The highlight of each visit was a reception in the faculty lounge when sherry was served. Things had really changed in the few short years since I arrived. The only detail that I remember clearly of those events was one of the visitors suggesting to me that the Department should invest its own small plane. He of course had a pilot's licence.

Don Heyding succeeded R.L. McIntosh as Head and during his tenure RL moved from the faculty office to become Dean of Graduate Studies. There he was influential in setting up the provincial ACAP program, which was supposed to investigate the quality of the various graduate disciplines in Ontario Universities. Chemistry was the first discipline to be examined.

Although our visiting examiners gave us a good rating, the committee overseeing the process ignored their report and found imagined faults in our Honours Program, none of which the visitors had reported.

It was really a preview of what was to later transpire in our troubles with the accreditation of our Engineering Chemistry Program. Although the ACAP procedure turned out to be a province wide fiasco its repercussion in the Department was to bring graduate enrolment down to a very low level from which it would take many years to recover. At this time I became Chairman (note not Chair) of the University Safety Council, a body whose edicts and suggestions most researchers were very reluctant to heed and a body given few powers and not much support by the University. Later in my tenure on the Council, when Vedene Smith had taken over as Head of Department I was also involved as Safety Officer in the Department. One of the problems that we had always faced in the Frost Wing was that the fumehoods were very inefficient. Many modifications were tried on the fumehoods themselves, but with little success. We did find that on wind-free days the exhausted fumes would drop down into the building air intakes. This was a particular problem when acid digestions were being carried out in the undergraduate analytical laboratories on the second floor. Eventually we solved the problem of the inefficiency when we examined the Herman-Nelson units on the outside walls of the laboratories. These supposedly opened to the exterior when the fumehoods were operating to supply the requisite air to make up for that exhausted by the hoods. We found that the air filters in the units had never been cleaned since the building was constructed and that the layer of accumulated dirt on each filter was far thicker than the filter itself. Vedene carefully bagged the filters and delivered a filter and note to each of the Principal, the Vice-Principal and the Dean and never has there been such an instantaneous reaction to a complaint from the Chemistry Department as that action elicited. The units were all cleaned and all hoods were immediately placed on a regular maintenance schedule.

Also in Vedene Smith's tenure we became embroiled with the City of Kingston over sewage. Vedene, John Page and I were called over to Vice Principal Jim Bennett's office where we were informed that the city was suing the University for half a million dollars because we were putting mercury down the drains. For many years, Kingston had been allowed by the Ministry of the Environment to spread digested sewage on agricultural land but had recently been ordered to discontinue the practice. The reason was that the nitrogen/heavy metal ratio was now below the acceptable and allowed level. On being informed of this ruling, the city had surreptitiously taken samples from drains around the campus and had found spikes in the background mercury levels in some of the samples taken from the Chemistry drain. John Page and I were asked to investigate, meet with city officials and report back with details of how the University

should proceed. We obtained analytical data on sewage from the very cooperative officials in the provincial Ministry of the Environment offices. We found that the per capita input of heavy metals into the Kingston effluent was in fact lower than that of many of the surrounding cities and towns.

We presented this finding to a meeting of the Mayor, City and University lawyers, councilors and city engineers. In discussions with the engineers we found that the City had found that it could substantially cut down on the cost of trucking digested sewage to the local areas if they retained the material for a substantially longer time in the digester and so reduced its volume. This of course removed more of the organic material and in so doing reduced the nitrogen/heavy metal ratio. It was a very interesting exercise to explain this to the politicians and lawyers and to try to educate them as to the difference between concentration and amount. I do not think that we succeeded. There were two consequences: 1) The city solicitor asked if we would appear as expert witnesses for the City when it appealed the Ministry ruling; 2) For saving the University $500,000 dollars plus court and lawyers' fees, Jim Bennett graciously gave John and me $500 each for our research.

One interesting sidelight to all of this resulted from our asking Physical Plant to supply the Department with drawings showing the routing of all drains. It did not surprise us to be told that no such information was available and no one had the faintest idea how our liquid wastes found their way to the main outflow. The Administrative Assistant, Pip Nation, and I spent an amusing afternoon putting gentian violet solutions down various sinks to map the water flows.

I was the Head of the Department from 1989 to 1994. During that time the funds available for running the department reached their lowest level. The budget for operating the undergraduate laboratories had remained static in actual dollars for at least ten years and did not increase during my tenure. There was no budget for such things as seminars or building renovations and the contributions from the University and Faculty for start-up funds for new faculty were miniscule. The Department was saved from abject poverty only because two faculty members joined the administration, Jim McCowan as Associate Dean in Applied Science and Suzanne Fortier as Associate Dean in the School of Graduate Studies and Research. In addition John Poland's Analytical Services Unit could be relied on to bring in a steady income from overheads. It was also during this period that Dr. Alfred Bader generously provided the funds for the Bader Chair in Synthetic Organic Chemistry.

During this period, on numerous occasions I had the pleasure of escorting Principals, Deans and Associate Deans around the departmental complex. They all expressed their horror at the dilapidated state of the fabric and I

was always reassured that we were high on the priority list for something, maybe complete renovation, maybe a new building. Sympathy for the conditions in which we have to work has still, as of January 2000, not produced anything tangible, although things do look a little brighter since we have some, but not full, architectural drawings for a new building.

Robert Gordon

1966-1996

Personal Background.

Born in Toronto 15 December, 1936, the son of a Baptist clergyman, I moved a great deal, attending school in Montreal, Stanstead, Que., Buffalo, N.Y., and Toronto. I attended McMaster (B.Sc., Hon. Physics and Chemistry, 1959), winning the Chancellor's Gold Medal and learning (I list what seems now to have been of lasting importance): the beauty and excitement of the concepts and methods of calculus and physics; the fascinating nature of structural chemistry; the tiresome, to me, though necessary, details of synthetic chemistry and gravimetric analysis; an appreciation of the difficulty of making reliable experimental measurements and the importance of assessing them critically; enough literature, philosophy, and history to whet my appetite; a taste of living independently and getting along with my peers; and (rather later than would happen in the 1990's) an introduction to beer and sex. I thoroughly enjoyed undergraduate life, was successful in it, and aimed to pursue an academic career.

I stayed at McMaster for an M.Sc. in electronic spectroscopy with Prof. Gerry King. I pumped various freons (forbidden starting compounds nowadays!) through discharge tubes in an unsuccessful search for the emission spectrum of the CF_2 radical, but found instead that of CCl whose rotationally-resolved uv spectrum I photographed and analyzed in detail. I had a strong desire to sample life abroad, and in December 1960 sailed to the U.K. for Ph.D. studies with Prof. David Craig at University College London (Gerry's alma mater). There, I studied the polarized absorption spectrum of thin, oriented single crystals of phenanthrene at liquid-He temperatures.

Shortly before leaving McMaster, I married Diane Merkley, and in our first home in Hamilton we had as neighbours and role models a young postdoc, Erwin Buncel, and his pregnant wife Penny. Our two children, Christopher and Sian, were born in England during my Ph.D. studies. Given our enthusiastic immersion into British life, the pressures of marriage and parenthood, and part-time teaching in a grammar school to relieve the financial pressures thereof, I was unable to spend as much time on chemistry as I had previously. Since then my practice has been not to work all the time, but all the time that there is not some other worthwhile and important activity to attend to. However, I got a good grounding in the spectroscopy and crystallography of aromatic molecules, and, in 1964, my Ph.D.

Anxious to do something out of the ordinary before settling down to the career grind, I obtained a post at the University of Ibadan, Nigeria.

There, I did a bit of teaching and attempted, with little success, to set up a crystallography lab, despite frequently interrupted power and water and very primitive computing facilities. I admired the graceful rhythmic movement, entrepreneurial spirit, and ancient culture of the Nigerians, but not their tribal animosities and corruption in high places. I learned how easily one can become accustomed to a comfortable life in the midst of extreme poverty. As a Canadian, I chuckled at the differing behaviour of my British and American fellow expatriates. After two years I returned to Canada because Bob McIntosh offered me a job at Queen's. This was fortunate since, just before I left, Nigeria suffered its first of many military coups, and shortly afterwards the bloody Biafran civil war erupted.

Expectations upon Arriving at Queen's.

In looking forward to life as a professor, I thought mainly about the teaching. I wanted to immerse myself in sophisticated physical chemistry, develop my own thorough understanding of it in terms of fundamental principles, explain it in logical and lucid lectures, and devise challenging problems through which my students could develop and solidify their own understanding. I looked forward to discussing science with individual students and helping them with their difficulties. I was willing to do my share of academic counselling and administration. I also hoped to find a wider intellectual and cultural life with my university colleagues.

I saw research as a challenging and enjoyable hobby in which I would be free to indulge once my important work was done. As a graduate student I had demonstrated that, given a problem and the appropriate equipment, I could make careful measurements and interpret the results in a sound manner. I had also learned that facile and innovative experimental technique, and the design and construction of apparatus, were not my strong points. I completely lacked both a taste and a talent for grantsmanship and scientific entrepreneurism, and had no appreciation of the importance of these in an academic career. I saw research as fun, and a way to keep scientifically alive, but not as a central focus of my career.

In the light of today's emphasis on high-profile and well-funded research, my expectations would be seen as hopelessly naïve. I would probably not have been hired, and certainly not granted tenure. However, there was frantic university expansion in the 1960's. No fewer than five new faculty were hired in the fall of 1966 (Gord Erskine, Bob Gordon, David Peat, Neil Snider, and Jeff Wan). Bob McIntosh eagerly hired me on the basis of a good academic record at McMaster, without spelling out what would be expected of me or inquiring deeply into my (quite undeveloped) plans for research.

It was not until two years later, on an occasion when we were both drunk, that he bluntly expressed his disappointment with my research progress.

On the other hand, my expectations were compatible with important parts of the University's stated mission. Over the years I believe that I have made a valuable contribution to teaching and administration, and, after a slow start, my research, while never prolific, has matured. It is to the credit of Queen's and the Chemistry Department (and fortunate for me!) that I have been able to tailor a thirty-year career to my own tastes, strengths, and weaknesses, rather than forcing it into a more conventional pattern that would not really have fit.

Teaching.

Most of my teaching has been in general chemistry, second year physical chemistry, and molecular structure and spectroscopy at the third/fourth year or graduate level. In the early version of CHEM 420, which featured set experiments in physical chemistry, I also supervised projects in crystallography and high-resolution spectroscopy. I took my teaching responsibilities seriously, and they occupied most of my time and effort during term.

I quickly realized how fragmentary and disorganized my own understanding of the material was, and it is to the laborious preparation of lectures and problem assignments that I owe much of my present knowledge of chemistry. There is nothing like teaching (and writing) to develop and test one's understanding of a subject (and to expose the gaps in one's knowledge). I did not follow the textbook closely, but varied the emphasis, examples, and order of presentation. I argued that this allowed students to choose between the textbook author's approach and my own, but cannot deny that my own ego was a factor. A set of lecture notes could be used a second time, but I found that further repetition made me feel (and no doubt appear) stale. Thus I attempted to rework old notes and welcomed changes in teaching assignments.

Organizing and delivering single-section courses was a satisfying challenge. In multisection courses, however, there was always a frustrating conflict between allowing different instructors to emphasize the material they thought to be most important (and would therefore teach most enthusiastically and effectively), and trying to maintain consistency between sections. For example, I tried to emphasize basic principles and to make students feel comfortable and confident with mathematical analyses. Other colleagues preferred to keep the math to a minimum, feeling it would turn off the students, and favoured a more qualitative and descriptive approach.

Perhaps my greatest weakness as a teacher is impossible handwriting, especially on the blackboard. This led me to increasing use of handouts for summaries and background material (first handwritten or typed on my portable machine onto ditto masters, later photocopied, and after 1996 posted on the web).

In the early 1980's I began to write out lecture material on overhead projector sheets, often filing photocopies of the lectures in the library. This made the presentation more legible and lessened the task of notetaking for the students. However, it tempted me to lecture too quickly, since I always found it a struggle to cover the subject matter in the time available. The preparation of teaching material was a major chore, but seemed to be appreciated by the students. However, when, in my last year, I went to the effort of posting detailed lecture notes on the web, it was counter-productive. Attendance at my first year engineering classes fell off, and those students who came complained of boredom.

Although I scheduled student presentations in a few senior courses, most of my classes were too large for student participation other than occasional questions. I did, however, encourage students to come to me with their problems, and in larger classes I scheduled frequent optional evening tutorials. About 5 – 10% of the students (rarely either the best or the worst ones) came regularly, many more as exams approached. I very much enjoyed these individualized sessions, received valuable feedback, and believe that the students benefited.

General chemistry labs had been planned and organized by others, and I left it to the demonstrators to supervise the experiments. In an effort to insert a bit of personal contact into large classes, I would regularly wander through the labs, but I found the students more interested in getting help from the demonstrators, who actually marked the reports, than from me. I devoted much more of my time and effort to physical chemistry labs. There I would often have played a major part in organizing the lab, designing the experiments (especially those involving spectroscopic techniques), and preparing the manual. The demonstrators alone could not introduce the students to the many complex experiments scheduled simultaneously, and the active help of the professors was essential. It was not unusual for me and my teaching partners to each spend 2 hours in the lab on each of 2 or 3 lab afternoons. Not only did our presence aid the operation of the lab, but I believe that it also made large classes less impersonal and contributed towards a good ambience.

Perhaps a dozen out of many thousand students told me that I, together with other faculty, had influenced them to pursue a career in physical chemistry.

More typical of any compliments received was that I had worked hard to make an inherently dull subject less tedious, helped them to get through a difficult course, or perhaps even convinced them of the importance of boring physical concepts and measurements in the wider scientific arena. However, the greatest number of compliments I ever received were prompted by my rendition of a comic, self-mocking song at the last class. Might it be that entertainment is what students most look for in their classes?

The instructional climate in the Queen's Chemistry Department has changed, especially in the 1990's. Less faculty time is committed to teaching an increasing number of students. Section sizes in first and second year courses have increased from 100 to 200 or 300 students, and labs, tutorials, and marking are increasingly delegated to teaching assistants (some of whose fluency in English is unacceptable). This is due in part to government cutbacks, but also to increasing pressure on faculty to devote more of their effort to grant-winning research. It does not contribute to a good educational experience. The University boasts of its high-quality faculty, and the undergraduates deserve more of their time.

Administration.

In my first month at Queen's I was made secretary of a committee planning the addition of the top two floors of Frost wing. I had no special expertise or interest in buildings, but I impressed Bob McIntosh by producing lucid minutes in a timely manner. Twenty nine years later I was appointed Chair of the Space and Renovations Committee for no better reason than that I was thought capable of producing a report in the time allotted.

To justify the Frost project we estimated that first year enrollment would soon reach 1000; extrapolated the resultant number of upper-year students; calculated how many faculty would be needed to teach them with the existing, or an even smaller, student/faculty ratio; and declared that each of these faculty members would have a well-appointed lab housing 4 graduate students and 0.5 postdocs. Associate Dean (and later Principal) Watts accepted our proposal, questioning only whether our space needs would materialize in one year or three, and the funds were forthcoming.

Planning and justifying a new building in the 1990's was a very different exercise. Projected needs had to be justified as part of a detailed Academic Development Plan. An increased enrollment was predicted, but there was no provision for additional faculty to teach them. The emphasis was not on providing additional space, but on replacing aged facilities that were

decaying, inefficient, unsightly, and seriously deficient in terms of modern health and safety standards.

I served a total of 18 year-long terms on various Faculty Board committees, including the Curriculum and Nominating committees and the Board of Studies (which deals with students' appeals of academic sanctions). For 8 of these terms I acted as Chair. Administrative chores are rarely exciting, but (almost) always are necessary, and they tend to be assigned to the minority of academics which is willing and able to complete them efficiently.

For 12 of the years between 1980 and 1996 I was Chair of Undergraduate Studies for Arts and Science students in Chemistry. Following the pattern established during the long and distinguished tenure of Walter MacF. Smith, I assumed responsibility for organizing registration and orientation activities, assessing transfer credits, advising individual students, approving their programs, dealing with their problems and complaints, checking their degree eligibility, and nominating scholarship winners. I also played a major role in the departmental Curriculum Committee, was the local expert on what the Academic Regulations said and which ones could be judiciously bent, advised the Head on teaching assignments, and completed interminable paperwork. Being Undergraduate Chair was a heavy responsibility but a very satisfying one. I thoroughly enjoyed the contact with students, and realized that this was the sort of task in which I could best make a significant contribution to the Department.

The role of Undergraduate Chair has evolved in the 1990's. Many of the administrative tasks have been computerized or delegated to support staff. Preregistration makes September less frantic, but shifts administrative work into what used to be research time in the early summer. But it is still essential that sympathetic and knowledgeable faculty should be available to counsel students and monitor their progress.

Research.

On arriving at Queen's I found a 1.5 m spectrometer, purchased in 1965 for an undergraduate experiment, and capable of photographing uv spectra at 0.03 nm resolution. As I never attracted substantial grants, most of my experimental work done at Queen's used this instrument, supplemented by a cryostat for low-temperature single-crystal studies and a long-path multiple-reflection cell for gas-phase work. Fortunately, I had access to more modern and powerful equipment during three sabbatical leaves.

I first studied electronic transitions in aromatic molecules and cyclic carbonyls, using polarized crystal spectra to determine excited state symmetries and the vibronic mechanisms by which forbidden transitions

gain intensity, and the Franck-Condon principle to infer structural changes upon excitation from the observed vibrational structure.

Studies with M.Sc. students Don Newman (on multiple $n\pi^*$ excited states in crystals of tetramethyl-1,3-cyclobutanedione), and Doug Orr (on the effects of conjugation on $n\pi^*$ states of cyclic ketones) were the most noteworthy.

Progress was often slow, and in 1975 I lost my NSERC grant. Ironically, I was then writing up what is probably my most important paper: we studied the visible absorption spectrum of the blue gas CF_3NO and came to the, then quite novel, conclusion that its torsional conformation changes from eclipsed in the ground state to staggered in the excited state.

Our conclusion for CF_3NO was confirmed by later studies in Chicago, using more sophisticated supersonic-jet cooling and laser-induced fluorescence techniques, and since then similar conformational changes have been found in many other molecules. Much of my subsequent work has involved hindered internal rotation and other large-amplitude molecular motions such as inversion and out-of-plane ring bending. During two sabbaticals I was able to carry out higher-resolution studies. Working with Keith Innes in Binghamton, N.Y. in 1975/76, I made interferometric measurements of linewidths in the rotationally-resolved spectrum of ICl, and used them to infer sub-nanosecond lifetimes in several predissociating states. A few years later fast processes such as this were being studied directly with picosecond lasers. I spent 1982/83 with John Brown in Southampton, using a ring-dye laser to study highly perturbed rotational structure in the electronic spectrum of CrO_2F_2.

My third sabbatical, in 1990/91 with Mike Hollas at Reading University, was very productive. I used a high-resolution spectrograph, supersonic-jet apparatus, and a very temperamental laser on loan from a central facility, to study out-of-plane ring vibrations in the ground and electronically excited states of 1,4-benzodioxan. This was also the time at which a fallow decade of pondering the consequences of internal rotation came to fruition, and I was able to make important contributions towards the interpretation of torsional structure in spectra recorded by collaborators in Illinois, Reading, Portugal, and Toronto. These included the interaction between internal and overall rotation in methylglyoxal, $-CF_3$ and $-CH_3$ torsional structure in several aminobenzotrifluorides and toluenes, and torsion-inversion coupling in excited acetaldehyde.

In summary, my research was never prolific or well-funded, and it had no potential for commercial exploitation. Only 6 graduate students chose to work in my lab. However, I believe that most of the work was sound and a few interpretations were innovative. It certainly kept me scientifically alive to the benefit of my teaching. My best work involved interpreting experiments performed in better-equipped labs elsewhere. When my funding improved in the late 1980's I planned hoped to develop more

modern facilities, relying heavily on cast-off equipment scrounged from colleagues.

In 1995 M.Sc. student Peter Brodersen began construction of supersonic jet apparatus, but my early retirement in 1996 brought an end to this project. Ironically, having relied heavily on collaboration with others, and having survived several periods without external funding, I found myself spending leftover funds on collaborative work with younger colleagues.

Departmental Issues and Personalities.

Bob McIntosh was politically incorrect and, even for his time, had old-fashioned ideas. But he fought hard to build a strong department at the time of its fastest growth, supported his staff, and was the sort of person one couldn't help liking. For better or worse, the colleagues he hired set the tone of the department for three decades. I fondly remember the days when, at obligatory morning coffee, one would listen patiently to his yachting stories and then suddenly be told that the Principal had given him some money to support graduate studies, and how much of it could I use.

In my early days I saw Walter Smith, Ken Russell, Don Heyding, Roger Kewley, and the Breck/Wheeler duo as worthy role models to emulate. In my Undergraduate Chair role I had valuable support from Gary van Loon and Doug Hutchinson. Gus Shurvell was always good for a laugh, and was a helpful collaborator in spectroscopic projects. Jeff Wan offered generous research support at times when funds were short. Julian Brown became a good friend and teaching partner, and taught me the importance of questioning conventional wisdom.

Not all of my colleagues contributed to the Department in a collegial way. A few went to great lengths to avoid their teaching responsibilities, especially at the more junior level. One organic chemist, winner of many research honours, assigned his third year students such a heavy workload that they had little choice but to neglect their other courses. His course was excellent and central to the program, but he was too arrogant to admit that material in other courses was also important. He refused to teach courses that did not suit him. Unwilling to offer a course he had been assigned, he advised students that it would not be offered, but did not deign to inform the Head of his decision. As Undergraduate Chair I was given the task of attending the first scheduled meeting of the class and reporting to the Head that a handful of students, but no instructor, was in attendance. I then had to scrounge around to find alternative courses for the disappointed students.

At one time Engineering Chemists and their Arts and Science fellow students shared classes and were almost indistinguishable. Largely because of accreditation pressures, however, they increasingly went their

separate way, and I mildly resented being excluded from their instruction (although I occasionally taught first year engineering).

I had never liked the purple-dye culture, herd behaviour and superior attitude of engineers in bulk, and from my Arts and Science perspective the procedures and rules of the Faculty of Applied Science seemed bizarre. Thus, throughout the various crises of the Engineering Chemistry program I wished them well, but no more enthusiastically than I would another "foreign" program such as Physics or Sociology.

I have a healthy respect for all the Heads under whom I served. I did not agree with all their policies and decisions, but recognized that they all worked hard for the good of the Department as they saw it, often in difficult circumstances with severely limited resources.

More Personal Comments.

I did not shine in my first decade at Queen's, and my career probably reached its lowest point in 1976 as I returned from sabbatical and approached my 40th birthday. I had lost my research funding. Diane and I realized that we were not really compatible and agreed to end our marriage. Fortunately we were able to continue jointly with the active care of our children who were then entering their teens.

Things then began to improve. As mentioned above, I achieved some breakthroughs in research which gave me momentum and enhanced my self esteem. I started a relationship with Juliet Milsome which remains loving, supportive, and exciting today. Shortly afterwards I began as Undergraduate Chair, a position which suited me and in which I felt a more valued member of the Department. My publication record improved after my 1990/91 sabbatical and I was promoted to full professor.

When the University was forced to downsize in 1996 I was about to turn 60, and at this age the early retirement offer was too attractive to refuse. Since then I have found a life free of deadlines and pressures to be very attractive. The University and the Department are changing rapidly, and some of these changes---particularly a lessening of the resources devoted to teaching and personal interaction between faculty and undergraduates--I would find it difficult to endorse. Fortunately, I do not have to. Nevertheless, I am very impressed with the expertise, energy, and accomplishments of the new generation of my colleagues.

I now have time for a bit of volunteer work: peeling vegetables for a dinner program and helping low-income clients to fill out tax returns. When the weather is good, I cycle, hike, canoe, swim, or ski. When it is bad, I stay indoors, write up unfinished research, and try to keep up with the journals. I continue my longstanding interest in choral music.

H.F. (Gus) Shurvell

1966-1996

"Those were the days". When I was nearing the end of my post-doctoral year in Marseille, France in the summer of 1965, my wife Irene and I were considering where we would like to live in Canada. We had lived in both Toronto and Vancouver and thought a smaller city might be a nice place to work and raise our family. Kingston seemed about the right size and being on Lake Ontario, we would be able to do some sailing. After having worked in the chemical industry for various periods, before, during and after university, I wanted to try an academic career. I sent a letter to The Department of Chemistry, Queen's University to ask about the possibility of a teaching position. Within a few days a telegram arrived from Department Head Bob MacIntosh asking for a CV and names of referees. Very shortly after another telegram arrived from Bob offering me a position as an assistant professor. I started at Queen's in September 1965. Indeed "those were the days".

It was a bit of luck that Bob MacIntosh was a keen sailor and rumor has it that the fact that I was interested in sailing helped me get the job. I was also fortunate in my first two summers at Queen's in having the opportunity to work at NRC in Ottawa in Gerhard Herzberg's and Harold Bernstein's spectroscopy laboratories. This experience and the contacts that I made at NRC may well have helped me obtain operating and equipment grants. It was at NRC that I first met a former Queen's professor, R. Norman Jones. Harold Bernstein and Norman Jones were pioneers in the applications of Raman and infrared spectroscopy and both gave me help and encouragement at the start of my career.

Queen's was much smaller in the 60's and 70's than it is now and there was a pleasant collegial environment in both the chemistry department and at Queen's in general. Members of staff would discuss chemistry departmental and university affairs over coffee with Bob MacIntosh. The department was run under what I consider to be the best form of government namely a "benign dictatorship". There were very few senior administrators in those days and they were chosen from among "Us". There was no "Union" and there was none of the "Them and Us" nonsense that prevails today.

I was born in London, England in 1934 and had an interesting childhood in London during World War II. After leaving school at the age of 16, I worked for five years in the coal gas manufacturing and chemical by-products industry in the UK. Qualification for university entrance was gained through evening classes and I attended Exeter University from1956 until 1959.

It was during this period that I first came to Canada, spending the summer of 1958 as an inspector on gas pipeline installations in several towns in Southern Ontario. On graduation from Exeter in 1959 I became a "landed" immigrant to Canada. (I have never understood why the word "landed" was attached to my status as an "immigrant"). After another summer working on pipeline installations in Owen Sound and Amherstburg, I worked for a year in Toronto as a chemist at a company that made food-packaging materials. During this time I met and married my wife Irene. We moved to Vancouver in the fall of 1960 for my graduate studies at UBC. All this experience gave me a useful perspective on the "real world" outside the university and it was a source of anecdotes and examples for my lectures at Queen's.

One of my jobs in the Chemistry Department was to write publicity articles for local publications such as the Campus Gazette and the Kingston Whig Standard. At this time Bob McIntosh decided to establish a Theoretical Chemistry Group at Queen's and I duly wrote an article for the "Whig-Standard". When published the headline read "Theatrical Chemistry Group Established at Queen's". Perhaps the Editor of the Whig had attended one of the "Christmas Lectures" staged by Wally Breck and Bob Wheeler.

In the early summer of 1969 Walter Szarek had an accident that resulted in a broken leg. This was most unfortunate for Walter, but presented an unusual opportunity for me. J.K.N. (Ken) Jones had arranged with a colleague at the University of Cape Town for Walter to lecture there for six weeks. A substitute was needed and Ken asked me if I would be interested in filling in for Walter. Of course I jumped at the chance and had a most interesting visit to Southern Africa. On the way to Cape Town I took the opportunity to visit Denis Diaper in Lusaka, Zambia. Denis was an RMC professor who had gone to Zambia on assignment to organize the Department of Chemistry at the new University of Zambia.

My first lecturing assignment at Queen's was a Physical Chemistry course for pre-medical students. Saul Wolfe lectured in Organic Chemistry to the same group of fifty or so students. These students were handpicked and guaranteed a place in the medicine program provided they passed all their courses, so we were expected to pass them. Any failing grades had to be justified at a meeting of a committee chaired by the Dean. Saul and I only taught pre-meds for two years, because the program was discontinued.

My next major assignment was to teach the second year Physical Chemistry course CHEM 240 with Bob Gordon and Roger Kewley. This course had a substantial laboratory component. Each year Frank Wild would take photos of the students and the Profs in the lab and make a montage. These photographic montages were hung on the walls in the third floor of Gordon Hall Annex.

In 1972-73 I was on Sabbatical leave in Australia and the montage that year contained a photo of me upside-down in a map of Australia. Bob, Roger and I also gave fourth year and graduate courses in spectroscopy.

At some point in the seventies I started teaching First Year Chemistry. Over the years I taught CHEM 112, CHEM 128 and 138 and later a course for mature students or students who had not had Grade 13 chemistry in high school (CHEM 111). The latter was a challenging but very rewarding job. Several of these "remedial" students ended up in law, medicine and the art conservation programs at Queen's. One student (Jesse Ng) obtained a first class honours degree in Chemistry and did an M.Sc. with me before going on to UBC for his Ph.D.

For several years I gave a one-term course on Colloid and Surface Chemistry for Chemical Engineering students (CHEM 248). This course was given previously by John Stone, who passed along some useful demonstrations to me. Simon Hesp took over from me and taught the course until it was discontinued a few years ago

In 1990 I organized a course on Vibrational Spectroscopy for RCMP forensic scientists. The Participants were housed in the newly renovated Donald Gordon Centre, where the lectures were given. I was able to persuade some well-known spectroscopists to be instructors and several spectrometer manufacturers to demonstrate their state-of-the-art instruments.

In all my first and second year courses I always tried to illustrate my lectures with demonstrations, films and other visual aids. As first year classes grew we had to move to larger classrooms in various parts of the campus. I used to carry the ingredients and equipment for my demonstrations in a cardboard box inside a plastic bag on which I wrote "have lab, will travel". I once showed this container to the Principal to emphasize the problems of teaching chemistry outside the chemistry department.

The late 60s and early 70s saw a lot of "musical offices ". My first office on the third floor of Gordon Hall Annex had been occupied previously by the former head of the department Grenville B. Frost the brother of Leslie Frost the Premier of Ontario. The lower floors of the Annex were occupied by Chemical Engineering at the time. When Dupuis Hall, the new Chemical Engineering building, was completed the Gordon Hall Annex underwent extensive renovations and all of us who had offices in the Annex moved temporarily to Dupuis hall. The Electronics workshop was located in GA39. When it was moved to the basement of the renovated Annex I moved into GA39. After I took the "early retirement" package in 1996, I moved to the basement of the Annex into GB19 an office previously occupied by my graduate students. In the summer of 2000 I was "downsized" again. With a few days notice and with no consultation I was asked to vacate my office for a new faculty member.

I returned from a visit to the UK to find the contents of my office piled on benches in the adjacent laboratory. Eventually I expect to have office space in GA19 again. Meanwhile, I have moved a desk and my computer into my spectroscopy laboratory (GB17).

My first research space was in a laboratory on the second floor of the Frost Wing. After the renovation of the Annex I was assigned space with Bob Gordon and Roger Kewley in a large laboratory on the top floor of the Annex. I also had a small room for a laser Raman spectrometer, which had been given to me by Harold Bernstein of NRC in Ottawa. In 1967 I obtained a substantial grant (with Bob Gordon) from NRC to purchase a high-resolution infrared spectrometer and a smaller grant from Queen's to purchase a He/Ne laser to use with the Raman spectrometer.

In 1981 I returned from sabbatical leave to find the contents of my laboratory piled in the corridor. My lab had been reassigned while I was in Australia. However, things worked out well in the end, because I acquired a large fully serviced lab in the basement of Gordon Hall Annex GA17, with an office for my students (GA19) and space in an adjacent lab. for sample preparation and chemical storage.

In my early days at Queen's, computing was done using boxes of cards with holes punched in them. A Fortran program to calculate the force constants and vibrational frequencies of a small molecule could be written on about a thousand cards. The box of cards was taken to the computing centre and the cards were read into the computer. A single calculation was done overnight and the next morning the results were picked up and analysed. A complete normal coordinate analysis, which can now be carried out on a PC in a few hours, could take several weeks or even months.

A professor's time is divided among three main activities; teaching, research and administration and committee work. During my time at Queen's I served on many committees including the University Computing Committee in the early days of computing at Queen's and more recently on the University Radiation Safety Committee. My most important administrative position was Chair of Graduate Studies in the Department of Chemistry, during John Stone's tenure as Department Head.

In the mid 70's I was invited to contribute a section on Vibrational Spectroscopy to the book *Organic Structural Analysis*, by J.B. Lambert, H.F. Shurvell, L. Verbit, R.G. Cooks and G.H. Stout. The book was published by Macmillan Publishing Co, New York, in 1976. The text has since gone through two revisions. The first in 1987 was titled *Introduction to Organic Spectroscopy*, by J.B. Lambert, H.F. Shurvell, D. Lightner and R.G. Cooks and was again published by Macmillan. The second revision, entitled *Organic Structural Spectroscopy*, by the same authors, was published by Prentice-Hall Inc., Upper Saddle River New Jersey in 1998.

In 1980 I prepared a descriptive chemistry text suitable for Grade XIII high school, or first year university students. The book entitled *Chemical properties and Reactions*, was a Canadian Edition of a book written by three Australian colleagues: A.R.H. Cole, D.W. Watts and R.B. Bucat. I was unable to find a Canadian publishing house interested in publishing the book, so I set up a company (H.F. Shurvell Publishing) and published it myself. Unfortunately, in spite of very favorable reviews in *Chemistry in Canada*, the book was not approved by the Ontario Department of Education for use in the Grade XII chemistry curriculum. However, I was able to sell single copies to teachers in several hundred high schools in every province of Canada. The book was also very useful to my CHEM111 students. I still have several boxes of unsold books in my office at Queen's. My latest literary contribution is a chapter on Infrared Spectra-Structure Correlations in a *Handbook of Vibrational Spectroscopy*, to be published by John Wiley & Sons in 2001.

Over the years I have had the good fortune to be the supervisor of some outstanding graduate students and post-doctoral fellows. My first Ph.D. student in 1966 was Joshua Faniran from the University of Ibadan in Nigeria where Bob Gordon had taught in the early 60's. Joshua did some excellent work on gas phase vibration-rotation band contours of symmetric top molecules. He also studied the low temperature infrared and Raman spectra of polycrystalline trifluoroacetonitrile. After graduating from Queen's, Joshua returned to the University of Ibadan, where he obtained a lectureship. He went on to be Head of Department, and then Dean of the Faculty of Science.

Joshua Faniran's studies lead to further important work by a later student Steve Daunt on the low temperature properties and the gas phase infrared and Raman spectra of s-triazine, cyclopropane and symmetrically substituted benzene molecules. Another excellent student, Tim Bulmer, studied hydrogen bonding in non-aqueous solutions and adapted the mathematical techniques of factor analysis and band contour resolution to the analysis of the infrared spectra of carboxylic acids. Tim's son John is at present an undergraduate at Queen's. A Polish Ph.D. student Barbara Petelenz followed up on Tim's work by studying the temperature dependence of hydrogen bonding in phenol. During Barbara's studies with me, her husband Piotr worked with Vedene Smith. A Finnish post-doc. Jouko Korppi-Tommola and later graduate students extended the work to several other systems using both infrared and Raman spectroscopy. Jouko's family came with him to Canada and his wife Liisa studied for her Ph.D. with Walter Szarek. Jouko also worked at NRC in Ottawa and he I collaborated with Julian Brown on studies of the motion of the ammonium ion in various ammonium salts.

University professors are fortunate in the opportunities for travel that the job offers. Conferences and sabbatical leaves take us to exotic locations and opportunities to lecture overseas often occur. My first sabbatical leave was taken at the University of Queensland, Australia, where I carried out some gas phase and low temperature Raman spectroscopy. My wife Irene and I made several lifelong friends in Brisbane and we enjoyed living there so much that we have since made six more working visits to Australia. Four of these visits were to the University of Queensland and the two most recent were to the Queensland University of Technology (QUT). In 1999, in spite of being retired, I was offered a QUT Visiting Research Fellowship for six months. (I must have done something right during my previous visit in 1995). The spectroscopy laboratory where I worked is very well equipped and I was able to use state-of-the-art infrared and Raman instruments to study stresses in raw diamonds, identify jade and ivory artifacts examine sediments from tidal creeks and help to solve many interesting practical problems.

I have already mentioned my visit to Southern Africa in 1969. In 1975 I was invited to be the external examiner for the Ph.D. thesis of Joshua Faniran's first graduate student at the University of Ibadan. It was a very interesting trip, during which I gave lectures at several new universities in South Western Nigeria. Unfortunately I had to reject the thesis at the first examination. However, the student subsequently rewrote the thesis and obtained his Ph.D.

In 1976 J.K.N. Jones had a visitor, Dr. A. Barsan, from the Universidade Federal Rural do Rio de Janeiro (UFRRJ), a small university in the State of Rio de Janeiro, Brazil. Dr Barsan arranged for me to give a short course during June and July 1976 on spectroscopy at the UFRRJ. While there I was able to revitalize an old infrared spectrometer and make a start on the identification of some natural products that Dr. Barsan had extracted from local plants. Of course I also had some excursions into Rio and other places, with students in my course as guides. I returned to the UFRRJ the following year for a shorter visit, laden with equipment and spare parts. During this trip I gave lectures at the Universities of Campinas and Curitiba, where my host was a former graduate student of Ken Jones'. In December 1979, I spent three weeks recording some low temperature Raman spectra in the laboratory of Oswaldo Sala at the University of Sao Paulo.

In 1980-81 I spent half of a sabbatical year at the University of York, U.K. working with Ron Hester and Reuben Girling. Reuben has since made several visits to Queen's. In 1987-88, I was a Visiting Research Fellow at the Thornton Research Centre, Shell Research Ltd. in Chester U.K. During this visit I assisted in setting up a system to record Raman spectra using a new technique known as Fourier transform Raman spectroscopy. This was the first industrial FT-Raman set up in Europe.

During the next six years I made regular visits to Thornton to record FT-Raman spectra on the instrument for Shell research people and for collaborations with colleagues at Queen's and at Health Canada. In 1996 I spent part of my last sabbatical at the Thornton Research Centre.

For over 30 years I have been a member of the Spectroscopy Society of Canada. During this time I served in various capacities on the National Executive Committee of the Society including a term as President of the Society. For the past 20years I have been Editor of the Society's Newsletter *Canadian Spectroscopic News*. In 1991 I was made an Honorary Member of the Society in recognition of my many years of service. In 1998 I was Chairman of the 44th International Conference on Analytical Sciences and Spectroscopy (44th ICASS) held at Queen's university.

At early retirement in 1997, I was made an Emeritus Professor of Chemistry. This allowed me to retain my office and laboratory in the basement of Gordon Hall Annex until the new chemistry building is ready in 2002. Since retiring, I have started a "new career" as an Adjunct Professor of Art Conservation. This entails giving lectures and advice on vibrational spectroscopy to students in the Master of Art Conservation program and helping with student research projects. I am also the longest serving member of the University Radiation Safety Committee and at present I am preparing a Laser Safety Manual. Other current activities include writing papers and organizing the vibrational spectroscopy sessions for the 46th International Conference on Analytical Sciences and Spectroscopy held at the University of Winnipeg in August 2000.

Neil Snider

1966-1995

I was a professor in the department of chemistry at Queen's University from 1966 to 1995. I had received my B. Sc. in chemical engineering from Purdue University in 1959 and my Ph.D. from Princeton in 1964. From 1964 to 1966 I was a postdoctoral fellow, first at Cornell and then at Yale.

My teaching assignments at Queen's were courses in general, physical and theoretical chemistry. They ranged from first year undergraduate courses to advanced graduate courses. At every level I found something interesting in the science, and I got positive response, however small in quantity, from students at every level.

My research interests were in the area of theory, particularly theories of gas phase reaction rates, of molecular collision dynamics and of the thermodynamic properties of dense fluids. I investigated relationships between gas phase phenomenological rate constants and cross sections for inelastic and reactive collisions between molecules in the gas. At a more fundamental level I sought quantitative relationships between these cross sections and the masses and the force fields characteristic of the colliding molecules. The search for relationships between molecular force fields and thermodynamic properties was at the heart of my work on the theory of dense fluids.

During my time at Queen's remarkable advances were made in computer technology. Most theorists took advantage of the opportunities offered by these advances. I continued to be drawn to more traditional theoretical approaches and made minimal use of the computer. My opportunities for collaboration were limited thereby. Apart from a brief period of collaboration with David Wardlaw's group and an even briefer period with the group of R. L. McIntosh, I worked independently.

In addition to teaching and research I have been much drawn to music and to literature. In my spare hours I worked at learning to play the piano. I also wrote drama, short fiction and poetry. A few of my poems have been published.Sometime around 1990 Queen's extended the offer of a substantial early retirement package to faculty between the ages of 55 and 65. It was made clear to those of us within or approaching those age limits that the university would greatly appreciate our accepting that offer. I saw early retirement as a way of bringing about a better balance of my life's activities. Hence, in September of 1994, nine years before my official retirement date, I retired from my position at Queen's. I then continued for some months as an adjunct professor.

During the 1991-2 academic year I had been on sabbatical leave in the chemistry department at Northwestern University. I was favorably impressed by the high level of congeniality among the faculty members there. After deciding to take early retirement from Queen's, I secured appointments at Northwestern as a senior research associate and as an intermittent adjunct professor. In the spring of 1995 I left Queen's.

Someone once made the following comment regarding teaching and research, which to me rings true: "Rare and valuable are the people who can make a creative union of these two activities which so often give rise to destructive tensions." Rare and valuable people are just that. The rest of us do what we can. I only hope that, while at Queen's, I did manage to add something positive to the education of at least a few of my students and that I did make at least a few worthwhile contributions to science.

Jeffrey K. S. Wan

1966-1999

Where have all the good old times gone?

When I was a graduate student back in the 1960's I asked my supervisor, a very wise man, what would I be in the 1990's. Will I be rich? Will I be famous? Will I still have a job? Here is what he said. "To be a Canadian academic, you will not be rich. You probably will not be famous. You will have a job and a very satisfying life."

After 33 years as a professor of chemistry at Queen's University, I am afraid that he is right on all counts. Now when I was asked if I had any second thoughts about my life and career, I would not say that I would not have changed a single thing. Rather, if I could go back 33 years, I would at least have tried to live my life in a few different ways. Here is what I say.

ONE. I would not take advantage of my colleagues. 12 years ago I had a heart attack, heart surgery and heart failure all in rapid succession within a period of 6 months. It took me another whole year to recover and learn how to live with my limitation. During these 18 long months, my colleagues took over all my teaching and other chores without worrying about the increased weight on their own career or asking for anything in return. Just like a family. It made it all possible for me to continue my career in research with some success beyond my limited dreams. Indeed, in recollection I would say my greatest single achievement in my career is the survival of my heart problems and the satisfying knowledge that I do have so many dependable friends. So, if I had my life to live all over again, I would make sure I would not suffer a heart attack and not take advantage of the generosity and kindness of my colleagues.

TWO. I would make my fortune first before I start an academic career. 33 years ago in July when I showed up in the department for work, I was told that I would be paid only from September and that I would have to make ends meet on my own for the summer months. Professor R.L McIntosh, the head of department then, humorously apologized for the fact that there would neither be an office available, nor a lab for me. He wondered aloud why any sensible persons would want to come to Queen's. We stayed in graduate student cubicles over in the Chemical Engineering for over a year. In retrospect, that was the most wonderful year I had at Queen's as I made my vow to succeed against all odds. So, if I had my life to live all over again, I would make it easier on myself and start at Queen's only after I made my fortune. THREE. In Canada, particularly in the province of Ontario, the traditional role universities and colleges play will substantially change in the new millennium. Times have changed and times will change.

Any sensible persons will adjust to the changes. But then, in Professor R.L. McIntosh's own word, we ARE NOT sensible persons. Amen.

The problem with making recollection from my own personal perspective is analogous to sitting at the bottom of a deep well and trying to visualize what the sky looks like. After my heart attack, my memory banks have been partially erased. On the other hand, I remember the old fable of 9 blind men trying to describe an elephant. If they can do it, perhaps a few of my friends and colleagues with elephant-like memories, also can.

Michael C. Baird

1967-

The Early University Years

I attended McMaster 1958 – 1962, during which time the undergraduate population there soared to about 1800 students. Queen's was regarded at the time as being one of the "big" universities, along with Toronto, McGill and Western, although I suspect that the student population at Queen's amounted then only to three or four thousand. Most of my class of five or six went on the graduate studies – it seemed the natural thing to do at a time of major expansion of the university system and ever increasing numbers of faculty positions – and I enrolled at the University of Toronto. There was a fellow there, A.D. Allen, whose work looked interesting, but more importantly I could best continue my athletic career there. Even then I did not always have my priorities right.

Bert Allen was in retrospect not an ideal research supervisor, as he moved into administration at an early age and was also very easy going; his graduate students really could have worked harder and played less golf and bridge. However, I shall always be grateful to him because he saw me through to my doctorate in just over three years (see below), and induced me to write Geoffrey Wilkinson, at Imperial College, London about a postdoctoral position. I, in my ignorance, knew little of Wilkinson except that he had co-authored a new and seemingly very good inorganic text book. However, the move to London was undoubtedly a wonderful opportunity as Geoff was arguably one of the most influential chemists of the century and subsequently became Sir Geoff as he won the Nobel Prize in Chemistry. While his prize was not awarded for the work which I did, some of the prestige rubbed off and expedited my academic career to some extent at least.

I thoroughly enjoyed my eighteen months in London. Having been brought up in an anglophilic family, I much appreciated the history and culture while enjoying the benefits of being a "colonial" rather than an American; my Maple Leaf pin was indeed a passport to smiles in pubs and similar places. I also got a lot of research done on cutting edge stuff, resulting in nine publications, and finished it all off with a three month camping tour of Yugoslavia, Greece, Italy and France. A good post doctoral position is truly a time of opportunity!

While in London, I of course applied to several universities for positions and was absolutely tickled when an offer came through from Bob McIntosh, the Chemistry Head at Queen's.

(I was actually out in Kent doing brass rubbings on the day the telegram came, and I received it from a friend whom I had arranged to meet in the George Inn on the Old Kent Road.

It's a wonderful old stage coach inn, if you are ever in that part of the world.) Although I had never visited Queen's, even to interview, a quick look at a slightly out-of-date calendar in the I.C. library suggested (incorrectly) the presence of relatively few inorganic chemists. It seemed, to this naïf, that I would have considerable freedom in planning lecture material and little competition for graduate students, large numbers of whom would undoubtedly be yearning to do my kind of research. Bob McIntosh even advanced me some travel monies so that I could blow my bundle traveling, and things were definitely looking up.

Queen's University -- First Impressions

At any rate, I arrived in Kingston in July, 1967, raring to go. After three years at Toronto in a modern lab filled with graduate students, followed by about half that period in a (slightly dingy) world class lab in London, followed in turn by the longest and best vacation I had ever taken, I was really quite anxious to get started. Unfortunately, it seemed that the office painters at Queen's were a little behind schedule, while my lab would not even be built for another couple of years, as part of a planned fourth and fifth floor addition to the then three story Frost Wing. In the meantime, I could have a bench in a lab shared with three other faculty, and there were no grad students for me that year. All in all, a bit of a downer except for the fact that I soon learned that those of my classmates who had remained at McMaster for grad work were still there! I, smugly, found this amusing initially, but quickly came to appreciate how lucky I was. The era of university expansion came to an end by about 1970 as the baby boom expansion ended, and the good jobs essentially dried up. However, I at least had one, no matter how questionable the situation seemed on occasion.

I was also told that I was lucky in that university salaries had recently started to rise, and I indeed received a starting salary of $9,200, equivalent to about $46,000 in current dollars and so perhaps not too bad. Bob McIntosh turned out to be a very supportive Head, and a man to whom I took an instant liking. It turned out that he was a very good friend of Bert Allen, and I realized why it was that neither an interview nor a letter from Geoff Wilkinson had been necessary to get the job at Queen's. Bob had probably just picked up the telephone and called Toronto to get the dope on me. I guess this was an 'old boys network' in play, something which does not go down well today.

A bit of background history might be appropriate here. Queen's, hesitatingly, and in fact the entire North American university system had entered a new era at just about the time I joined the burgeoning numbers of undergraduate students in 1958.

Prior to World War II, most North American universities were essentially teaching institutions, research being something a few professors did almost as a hobby during the summer months.

Where research was done, the classical von Humboldt model of the universities held sway. One carried out pure research, inevitably in close conjunction with teaching, without any consideration of possible practical applications.

Regular term teaching loads were high, salaries were low and there was very little funding for research. In contrast to the modern era, governments played only a token role in supporting research, and scientific research was supported by private and largely local resources. Thus there was a relatively undeveloped research culture at Queen's and elsewhere. In Canada, McGill and the University of Toronto were the first to make serious efforts to develop doctoral programs which might compete on the international stage, and the rest of the universities either were largely teaching institutions (*e.g.* Queen's, Western, McMaster in Ontario), or did not yet exist.

During WW II, however, academic scientists contributed quite significantly to the war effort, an achievement which fostered an appreciation among governments for university research. The idea that the funding of university research might be beneficial persisted during the immediate postwar years, and the crash program undertaken by the American government immediately after the awakening that was Sputnik, reinforced by the imminent arrival of the baby boomers, resulted in greatly enhanced funding for all manner of endeavours in Canadian universities. In particular, the federal government in Ottawa began during the 1960s to focus on establishing a national policy with respect to scientific research, and this decade saw the creation of programs designed to fund university research and to improve salaries and working conditions of faculty.

There began a golden age of university research, in which government funding provided the stimulus for enormous growth in all aspects of the university systems in North America and Europe, at least. From the perspective of the highly competitive world of 2000, it seems that almost anyone could obtain federal and/or provincial research funding, albeit at much lower levels than the competition to the South was getting or what is considered essential now. Whatever the true situation, there certainly developed a notion that research funding was a right.

The nature of research also began a gradual transformation during this period. The concept of "pure" research slowly gave way, to the continuing discomfort of many, to one of "fundamental" research, intended to advance knowledge, on occasion motivated by and funded with specific ends in view.

This trend continues to the present day, of course, and provides much intellectual fare to the social scientists, historians of science and would-be critics who analyze us and sometimes even judge us as being morally lacking for proceeding down this path. More on this below.

At any rate, I began my academic career in September, 1967, with an all male (!!) class of first year engineers. I determined that I would always have a few lectures prepared well ahead of when I would need them, but the weather on my first day of lectures was absolutely superb. Mindful of a playful suggestion by Bob McIntosh that we shouldn't really begin until October anyway, I asked the class on the day of my first lecture if they really wanted to stay and listen to me carry on for the period. Not surprisingly they said "no", and so we adjourned. Some years later, I met one of those students in a restaurant while on a trip, and he mentioned that class, telling me that it was generally agreed that I just didn't have a lecture prepared. How could they be so mistrustful?

Anyway, I taught and thereby learned something about thermodynamics for a large part of the year, discovering something about myself during the process. Thermo was not my strong suit by a wide margin, in part because it was poorly taught to me as an undergraduate and thus had just not seemed very interesting, in part because of my failure to appreciate its beauty, exquisiteness and magnificence. (Some say that sarcasm *is* my strong suit.) However, I found (a) that I could figure out enough of the thermo in order to teach it, (b) that I could successfully bluff sometimes when I wasn't sure what I was doing, and (c) most importantly, I could admit my ignorance to the students on occasion. I gather from students over the years that not all faculty can do this. Anyway, there was one bright student in particular who occasionally asked very good questions. He was not trying to trip me up, but I would sometimes just have to offer him a rain check on the matter, ask the question of a senior colleague, Walter Smith, and report back to the class in the next lecture. I quickly found that teaching was not difficult for me, and that I enjoyed this part of the job. Although in subsequent years I think I have been considered by some of my colleagues to be a hard core researcher, disdaining those who would teach well, in fact the former assumption is correct but not the latter.

I have mentioned Bob McIntosh and Walter Smith, but there were over twenty faculty in the department at the time. The balance between the traditional subdisciplines was quite good – there were actually three other inorganic chemists by the time I arrived – and there was a wealth of expertise to tap if and when one needed help on problems relating to teaching or research. As an impatient, often overly anxious youngster, I know I occasionally irritated my senior colleagues, but like sometimes proud parents they alternatively humoured me or put me in my place.

I occasionally thought that I could do better elsewhere, of course, and looked quietly for a while for opportunities to jump ship. However, as indicated above, the era of growth had ended and I did not find a situation better than the niche I occupied. Just as well, as I ultimately realized that I had in fact better opportunities at Queen's than I would have had *anywhere* else in Canada.

Queen's University – the Early Years

After a somewhat discouraging first year with no graduate students, I attracted four in my second and two more the next. I also began to have opportunities to teach more interesting courses, my new lab was built, and the future looked good. Our research interests were and are in the interdisciplinary area of organometallic chemistry, which as the name suggests is involved with the interface between the traditional subdisciplines of organic and inorganic chemistry. I spent much time at the bench myself, and continued to do so for over a decade until pressures of the job coupled with the space needs of a growing research group forced me to stop. Looking back at our publications of the time, it is clear that I subscribed subconsciously to the above-mentioned ideals of "pure" and "fundamental" research, as most of what we did was certainly of no use to anyone except for its possible intellectual value.

I deviated from this straight and narrow path occasionally, of course, submitting a proposal to Imperial Oil and getting funding to do something vaguely related to the problems of SO_2 pollution. We also dabbled in bioinorganic chemistry, doing one study related to mercury toxicity, another on the use of some ruthenium compounds as anti-cancer agents and a third on interactions of amino acids with metal ions. The last was a collaboration with Walt Szarek, a fellow Mac graduate who came to Queen's about the same time that I did. I also justified many grant applications on the basis that the work could have great impact on industrial catalytic processes. We never fooled anyone, of course, but this was a hot area at the time and we made hay while we could.

A morally unjustified approach to research? I don't think so. We were having fun.

After about a dozen years at Queen's, I decided to begin a new line of catalytic research, one that might save western civilization. Anyone who drove an automobile during the late 1970s and early 1980s will recall the burgeoning gasoline prices which had resulted from the Arab oil embargo, the Iranian revolution and the Iran/Iraq war.

There followed considerable interest in chemistry which might give rise to new technologies for the manufacture of synthetic fuels, and many people harkened back to the synthetic fuels technology developed in Germany in the 1930s. This Fischer-Tropsch technology had provided Germany with essentially all the fuels used by that country during WW II, but had not been developed after the war because of the discovery and subsequent exploitation of the Middle East oil fields.

Since the feed stock for Fischer-Tropsch chemistry was carbon monoxide, a favourite molecule of organometallic chemists, this kind of chemistry seemed a natural for organometallic chemists and many of us took up the challenge of converting carbon monoxide to liquid hydrocarbons. Well, we didn't enjoy much success at Queen's with the stated objectives of our various research proposals, but we did obtain some interesting results, including the finding that we could convert carbon *di*oxide to methane, the major component of natural gas.

Our first publication on this work brought a telephone one day call from an awards officer with the US Office of Naval Research. He was running a program to fund university research to find ways to enhance the chemical reactivity of carbon dioxide, and while he had received many pie-in-the-sky proposals, he had little from researchers who actually had successful research in hand, like we did. He was vague on just what the impetus for the Navy program was, but he did wonder if I had sufficient funding and seemed pleased that I thought I could find ways to spend his cash. The funding we subsequently received dwarfed all previous grants which I had had from Canadian sources, and made possible a very large expansion of our research.

One of my graduate students at the time questioned the morality of accepting money from the US military, but I assured him that it would all turn out well. Whatever the aims of their program, we undoubtedly would fail to produce anything of value and so we would ultimately end up weakening the American ability to wage war! He laughed and pretended to be mollified by this response which, if genuine, would have been truly Canadian.

However, as it happens my joke was right on the money (no pun intended). The Navy wanted to find ways to recycle carbon dioxide, the major product of human respiration, in submarines. Not even close to the chemistry we had been doing and continued to do.

Something really important did happen during this part of my life. I fell in love with Shirley Stevens, one of my undergraduate students; and she with me. Too late I found out that her father was one of the more influential chemists of post-war Canada, and a good friend of both Bob McIntosh and Art Bourns, one of my profs at McMaster. This development could have been disastrous for my career at Queen's, but again I was extremely lucky

as although student-faculty affairs were frowned on, they were not formally banned until some years later.

Anyway, although there were almost no outwardly obvious negative responses from my colleagues, we walked on eggs for some time. Perhaps most just did not know? However, some offered quiet support, and we are forever indebted to the McCowans, Barb and Jim, and the Buncels, Penny and Erwin, for inviting us into their homes. I'm sure they did not approve, but they showed that they accepted, no matter how irresponsible I must have seemed.

So, after about a dozen years as a young guy at Queen's, my personal life was settling down, and I had entered a very long period of good funding and large, very high quality research groups, which still continues. I was also about to be promoted to the rank of full prof and to be invited to serve on the NSERC committee which provides funding for most of the chemistry research carried on in Canadian universities, and I realized that a rather peculiar thing had happened.

I was now regarded, internally and externally, as a part of the establishment! A strange feeling.

Whither Now?

I won't try to bore the reader with details of more modern history. However, our research in organometallic chemistry over the years has involved an intricate mix of the "pure", the "fundamental" and even the "applied". There has been and continues to be a healthy synergism at play here, as our applied research has invariably evolved from the pure and the fundamental while, conversely, the problems of industry and society frequently turn out to be intellectually quite challenging and interesting.

Close academic ties to industry are becoming much commoner in the universities these days, and are oft criticized on the basis that the apparent "commercialization" moves us away from our traditional missions of providing academic training and carrying out pure, unfettered basic research, *i.e.* it gets us away from our roots. As a result, the quest for knowledge becomes limited and students are prepared for pre-ordained futures. It is also claimed that basic academic research is corrupted because of the desire for proprietary rights, and thus that traditional openness suffers. In addition, the universities are being pushed down specific intellectual tracks, with the result that collegial and governance structures are undermined.

All of this can happen, of course, but one can take issue with the notion that closer ties with industry means betrayal of our "roots". As I have indicated above, the pre-WW II university system was a relatively tiny

entity which did very little research, and that almost completely with funds from *private* sources, *not from governments*.

The notion that we should function entirely separately from the needs of society and that government funding should generally be available in quantities sufficient to permit at least a large proportion of university faculty to carry out pure research actually derives from the funding deluge that occurred so briefly during the Cold War/baby boomer era, what I refer to above as a golden age. We look back on this as the norm, although it was in fact but an brief moment in time, an anomaly.

In any case, is there no upside to funding from private sources? Sometimes the problems of industry are extremely intellectually interesting, and it should be well recognized that, by persuading private individuals or companies to offer funding, we can cope better with the capriciousness of government funding. The equipment thus purchased can be used for "pure" research, and the resulting industrial contacts can help students to obtain jobs without in fact forcing them into straightjackets. Indeed, I find that companies often want new people to do something quite different from their doctoral research. Helps to evaluate them, I suppose.

And must research of interest to a company be of necessity *undesirable* to the public? Without going into details, we are currently working on a project, with funding from a Canadian company which is comparable in magnitude to that obtained from the US Navy all those years ago, to develop new technology for a process which is one of the keys to our way of life. If successful, and our research is actually looking quite promising, we will provide that company with the basis for technology which will be much more energy efficient than that currently used anywhere in the world, and also environmentally much more benign than the technology of the main competitor, a huge American company. All in all, if success does come our way, a pretty good outcome for Canada.

The idea for the project clearly evolved from curiosity driven work in our lab, aided by a bit of serendipity and information about the industry's needs. However, as the project research continues we are also making discoveries of genuine academic interest. In addition, of course, we have obtained equipment worth about a quarter of a million dollars which the entire research group is using.

In another, totally unrelated project, we are collaborating with colleagues in the Cancer Research Laboratory at Queen's. We have ideas concerning a possible new line of metal-based, anti-cancer drugs, and have synthesized one which has tested very positively against at least one cell line. It's early days on this project as yet, but it is also looking very promising.

Would it be truly better for us to return to the ivory towers? I think not. We can not.

The tremendous advances made over the past half century have completely changed the relationship between science and society. We previously enjoyed a social contract based on the traditional understanding that universities would provide teaching and fundamental, research-based knowledge in return for public funding and a high degree of institutional autonomy. Now, however, scientific research has been so successful at altering society that we can no longer claim that our work is culturally neutral, that we are somehow not responsible for the results of our collective labours. There is now developing a new social contract in which the public, through government, insists on a measure of control and accountability. Thus the "right" of access to research funding for university faculty has become a "privilege", which perhaps is what it always was.

Is this the end of exciting careers in scientific research? Definitely not!!

Note that we shall still be better off than almost all of our predecessors over the past century!

Acknowledgements

I must thank Erwin Buncel and Julian Brown for putting together this history of the department and then bugging me to set aside everything else for a day or so to get my contribution written.

It's up to them and the readers to decide whether or not my efforts are worthy or appropriate, but it's been of benefit to me. As one of my heroes, Winston Churchill, has said, "The farther backward you can look, the farther forward you can see". There remains much to see and do. I have intimated above that I have had a more successful career at Queen's than I could have elsewhere in Canada, and while this may seem strange to those enamoured of some of the larger universities, I can certainly justify my claim to anyone who is curious. Indeed, this department is now stronger than it has ever been, and the opportunities to enjoy oneself remain.

I would also like to thank all of the Queen's faculty, in several departments, with whom I have interacted over the years, the graduate students and postdoctoral fellows who have both worked on and inspired my best ideas, and the secretarial and support staff. There are far too many in these groups to name, but you know who you are. And if you have read this far, thanks to you also.

Finally, I wish to thank my wife Shirley, who did the most of all to make the past twenty-seven years so enjoyable.

James D. McCowan

1967-

Erwin Buncel has asked that I put down a few notes about my career prior to coming to Queen's and some of my memories of my time at Queen's. I doubt that my memories will add much to those of the others serving in the same period, but in deference to Erwin and to Julian Brown, I have done what was asked.

I was born in Toronto and went to the University of Toronto because it was closest. I never considered doing otherwise. Money would probably have precluded my doing so anyway, but it is interesting to me now that I never contemplated it. My first degree was in Physics and Chemistry, and was followed by a Ph.D. in surface chemistry at Toronto and a Ph.D. in solid state physics from Cambridge. My supervisor at Toronto was Bob McIntosh, who later came to Queen's, and my supervisor at Cambridge was Philip Bowden.

Following Cambridge, I joined the Du Pont Research Centre in Kingston. At Du Pont, I spent five years working in titanium catalysis, an area for which I had no preparation and no great enthusiasm. The choice of research topic was theirs, and I could never see that the work I did had much hope of contributing to the corporate goals. A decade later, Gerry Dyer took charge of the Research Centre and transformed it into a lively place where teams worked on projects of well understood relevance and enjoyed great satisfaction. Had I entered that atmosphere, I might be at Du Pont still, but in the day that I was there, I found the work frustrating and pointless, and I should have fled much earlier.

Not being fully occupied at Du Pont, and concerned about environmental issues, I organized (with Gerry Dyer) a Committee and a campaign which led to the formation of the Cataraqui Region Conservation Authority. In February 1965, I became the first Chairman of the newly-formed C.R.C.A. and said at the time that I hoped to remain in that role (subject to the annual vote of the members!) for five years. The Authority acquired in those five years the largest part of the lands it now owns, including the upper and lower Little Cataraqui holdings, the Mac Johnson area at Brockville, the Gould Lake area, and an area at Blue Mountain that would later be turned over to the Province to form the basis of Charleston Lake Provincial Park.

This was a very satisfying time in my life. The satisfactions in the Authority offset the dissatisfactions at Du Pont, and delayed my departure from Du Pont by at least a year.

When I received unsolicited offers in late 1966 from both McGill and the University of Toronto (something totally unimagineable now), I was

forced to review my position and realized that a move was inevitable and overdue.

I consulted my old supervisor, Bob McIntosh, who was by then Head of Chemistry at Queen's. And I ended up going to Queen's.

My first four years at Queen's were hectic, to put it mildly. I was C.R.C.A. Chair and probably gave that position twenty or thirty hours a week. I held responsibilities too at the Provincial level. I felt an obligation, and a strong desire, to make the Authority a success. I started at Queen's on September 1 with lectures beginning a couple of weeks later. There was little time to organize either lectures or research.

Because time was so scarce, and because I was up-to-date on the organotitanium literature and seven years out-of-date in surface chemistry, I decided to develop research based on my Du Pont work. This decision was reinforced by my desire to avoid any sense of dependency, since Bob McIntosh was still actively involved in the kind of studies of porous solids that interested me so much. This decision was understandable, but it was an error. Had I taken the time to re-establish myself in the area that truly interested me, and in which I had proven to have some skill, I probably would have enjoyed a research career of at least average success. In fact, I got off to a very slow start (really beginning only when I left the Chair of the C.R.C.A. in 1970) in a field that was always interesting but never interesting enough. Of all the decisions that I have made, none affected me so adversely in my career as my decision not to return to surface chemistry. Years later, in the early eighties, I tried a late start, investigating the role of the substrate in heterogeneous catalysis as well as a look at some of the curing properties of concrete, but N.S.E.R.C. is unforgiving, and no funding was available. I finished up some organotitanium work using my own money, and withdrew from research in the late eighties. My teaching at Queen's, in contrast, was a much more pleasant and, I hope, successful venture. The courses taught were mostly in physical chemistry, primarily in thermodynamics and kinetics, as well as in first year.

Because of my time in industry, Bob McIntosh asked me to give Wally Breck a hand with the Engineering Chemistry program and therein began as association with "Eng Chem" that continues to this day. Wally is a great person and a joy to work with - practical, sensible, hardworking, and full of innovative surprises. He was a pioneer of environmental chemistry, whereby he cleverly combined his love of analytical chemistry with his love of scuba diving.

Wally, Julian Brown and I wrote a textbook designed for first year engineers, a book which was enormously popular with students for a few years and then fell from favour. Writing the book was a memorable exercise, and most of the memories are pleasant. A very intense week in

which Wally and I visited Julian in England to finish up the last details has particularly fond memories for me.

The book had a Second Edition, and an International and a Spanish edition, but it never sold enough copies to become a hit. It is regarded as too rigorous, a fault that has made it a very popular textbook with instructors, but not with students.

Bob McIntosh remained in the Department for only a year or two after my arrival, heading first for Arts and Science as an Associate Dean and then for Graduate Studies as Dean. Bob was a very strong department head, who held court in the coffee room daily dispensing news and, sometimes, money. The department had a cohesion and direction which is now gone, and most of that was due to Bob's wit, integrity, hard work, and constant communication.

Bob was followed by Walter Smith, as Acting Head for two years while the department grappled with finding a successor. The eventual choice was Don Heyding, a happy choice in my view. Don's self-deprecating, no-nonsense style was very appealing, but he presided in an era when Ontario tipped over from the over-expansive, over-expensive years that Bob McIntosh had enjoyed to an era of year-by-year attrition that continued for over twenty years. Neither Don, nor his successors, Vedene Smith and John Stone, had much opportunity to do anything but slow the rate of decline, but all performed admirably in a very unrewarding period.

Throughout this period, I worked first with Wally and then with Don in trying to improve the Engineering Chemistry program. Curriculum was modernized and new courses introduced. The energetic, enthusiastic and industrially savvy Warren Baker entered the Department as a NSERC Chair. Wally, Warren and Don were all engineers by education, but I was not, and after some years, I entered the profession through the licensing examination process of P.E.O.

On the alumni side, I built up a list of every graduate, with as much information about each as I could obtain, and began writing an annual letter to all of them. The program struggled to meet the criteria of accreditation, which came to prominence in Canada in the early seventies. (Prior to that, every graduate had to be evaluated independently for licensure.) Not everyone would agree, but I think that the changes forced by accreditation strengthened the program. It emerged, along with Engineering Physics and Mathematics and Engineering, as the elite programs in the Faculty.

If one looks each year to see which programs have the largest percentage of Dean's scholars, those three are always the top three. They may be in any order, but they collectively hold their place. Engineering Chemistry frequently attracts the person topping year one, and frequently has one third or more of its graduates on the Dean's Scholars list. Graduates in the

nineties have won numerous prestigious medals and scholarships, including the Sterling Medal three times, a Rhodes Scholarship, and numerous NSERC and other major awards.

It was therefore surprising to me that support for this century-old program was reduced in the late nineties, and administrative control handed over to Chemical Engineering. Fortunately it continues to thrive in Chemical Engineering, with the support both of members of that Department and of the band of enthusiasts in the Chemistry Department who had nurtured it before. If that support in Chemistry ever diminishes, the program may find it difficult to survive.

In 1990, I was offered the opportunity to become the Associate Dean in Applied Science and took it. I set off learning about national and international recruiting, about admissions, about the theory of teaching and learning, about the possibilities of enriching the learning environment for engineering students, about distance and continuing education and about university financing.

My contacts with Chemistry after 1990 were limited but not zero. My major role was teaching CHEM 244, most recently in combination with CHEE 210. When there was a rumour that a decision had been taken to build a new Chemistry Building rather than refurbish the old one, I advocated designing a building that emphasized "chemistry" rather than "Chemistry", the discipline rather than the Department. I wanted an emphasis on interdisciplinarity in the design. Much of what is most interesting in Chemistry goes on at its borders and I thought a building that served interdisciplinarity would be distinctive and important. It would also attract a much broader range of donors. Regrettably, we were not asked to explore the possibilities once a new building was adopted, nor even allowed to discuss the issue at a departmental meeting. I believed then and I believe now that no one should invest forty million dollars without a long, open, thoughtful discussion of all the alternatives that might exist, and the potential advantages that each option might bring.

The decade in Applied Science has been a very satisfying period for me. The Faculty has grown in size and in reputation, and attracts increasing numbers of outstanding applicants from across Canada and beyond. There is greatly increased participation in international exchanges, in internships, in taking concurrent degrees in Arts, and in a host of student activities including QPID, Science Quest and Solar Car. The learning environment has never been richer.

The latest and potentially most important initiative to emerge has been the concept of Integrated Learning and the development of the Integrated Learning Centre. It has been a great pleasure to play a role in these developments.

Walter A. Szarek

1967-

Someone once said that one of the keys to happiness on earth is to "Find out what you like doing best and get someone to pay you for doing it." These words may not be the most noblest to adopt as a philosophy of life, but, when I reflect on my almost four decades at Queen's, there does appear indeed to be an element of truth in them. Although there have been times of frustration, I consider myself extremely fortunate to be able to pursue a vocation at Queen's.

My association with the Department of Chemistry at Queen's began in 1962, when I started my studies towards the Ph.D. degree under the tutelage of Professor J.K.N. Jones. I was an undergraduate at McMaster University in Hamilton, and was well advanced towards the completion of my Ph.D. program when a certain restlessness had set in and I began contemplating a change of venue and a change of research interests. Thus, with the M.Sc. degree, I "boldly" left the jewel that McMaster was, in the Golden Horseshoe of Lake Ontario, and came to Queen's at the other end of Lake Ontario. I should mention that, when I was a graduate student at McMaster, across the hallway in the laboratory of Professor Arthur Bourns (later President Bourns) there was a postdoc called Erwin Buncel. Erwin also left McMaster to come to Queen's, but not directly; he came by way of a position at American Cyanamid in the United States. At this point I must confess that my coming to Queen's was not motivated by selecting Queen's as a university for graduate studies, but by selecting Professor Jones as a person with whom I wished to study. At McMaster I completed my M.Sc. degree with Professor David MacLean working in alkaloid chemistry. Professor Jones' research area was carbohydrate chemistry. Having worked with both alkaloids and carbohydrates, one might say that my career has been "bitter-sweet."

My graduate career at Queen's was like a trip on a magic carpet. Everything went right. Even being a Demonstrator for Saul Wolfe was fun. Professor Jones (J.K.) was a fantastic supervisor. I was impressed with the man literally seconds after we had met. Here was a man called Jones who pronounced my name correctly! The fact that for most of my time we (i.e., the Jones boys) did not have an identifiable laboratory did not appear to matter. Venerable Gordon Hall was being renovated, an operation that included the construction of the "modern" top floor. Most of our equipment, which appeared to have been passed down from Emil Fischer, was in the corridor of the second floor of the Annex.

Some of the rooms on this floor, which now serve as offices, also constituted our laboratory space, and we shared a laboratory on the third floor of the recently constructed Frost Wing with the physical chemists working with the Head, Professor R. L. McIntosh. I finished my Ph.D. studies with J.K. in the minimum time, namely two years.

Upon completion of my studies in 1964, I went on to do postdoctoral work with Professor M. L. Wolfrom at the Ohio State University. Although I was profoundly sad to leave Professor Jones, I really did not like Kingston and upon my departure the words used by Martin Luther King, Jr., went through my mind: "Free at Last!" Little did I know what the future would hold. Ohio State was one of the shrines of carbohydrate chemistry; the legendary Ray Lemieux also had been a postdoc with Professor Wolfrom and Steve Hanessian had only recently completed his Ph.D. studies. I did very little laboratory work at Ohio State. Professor Wolfrom had a very large research group and I served as his Chief-of-Staff and as a co-supervisor. This experience was truly excellent training for my own future career in academia. Within a year I was offered the position of Assistant Professor of Biochemistry in the Department of Physiology and Biochemistry at Rutgers University in New Jersey. In those days (1965) a widespread view among American organic chemists was that anyone who called himself/herself a carbohydrate chemist knew only two reactions—methylation and hydrolysis. Hence, I accepted the position at Rutgers and taught Biochemistry without ever having taken a course in Biochemistry. The people at Rutgers were really excellent, and I was developing my own research program with three graduate students. However, within two years I really felt that I wanted to be back in a Chemistry Department, and so I let the word go out that I was searching for a new position. One day, while I was pondering on how to make a lecture on the Krebs Cycle interesting (it really is Organic Chemistry and not simply a series of abbreviations to be memorized), I received a telephone call from Bob McIntosh, the Head of Chemistry at Queen's, a call which eventually led to my abandoning the stimulation of living in the New York area and returning to Hicksville (i.e., Kingston). Although I was "coming home" to Chemistry, the two years spent in the Department of Biochemistry and Physiology at Rutgers had a profound impact on my future research activities. Since that time I have always had an interest in biological aspects.

My faculty appointment at Queen's officially was effective September 1, 1967. However, "I couldn't wait," and so I arrived to visit the Department early in July. Within a few hours I was part a meeting of the Organic Caucus, the topic of which was one of the eternal quests of the Queen's Organic Chemists, namely, "how do we obtain better NMR facilities?" Finally, during the tenure of our current Head, Stan Brown, we have reached an important and very gratifying milestone in this quest. Before the first day had ended, I became involved in another activity.

J.K. was chairman and chief organizer of the Fourth International Conference on Carbohydrate Chemistry, which was to be held in Kingston later that summer. He asked me to help him with the myriad of aspects involved in the organization of such an event, and I consented. I essentially became a full-time Faculty Member during July and August without any salary.

In the Fall of 1967 J.K. went away on sabbatical leave, and I was asked to assume the responsibility of supervising his entire research group—we continued our association for the next ten years until his death in 1977. J.K. was an experienced and eager traveler, and took great pride and joy in cultivating and displaying his flowers not only at his beautiful home on Treasure Island on the St. Lawrence River but also in his office. Indeed, when he was away on his travels, one of my additional chores was watering his plants. I was tempted frequently to include this activity in my Dean's Report.

The decade with J.K. was really one of the most memorable and fulfilling periods in my life and academic career, and I owe him a great debt of gratitude. A copy of a photograph that I treasure is shown in this article. The expression "a really decent human being" truly described the man. In the obituary that I wrote for the Proceedings of the Royal Society of Canada (Series IV, Volume XVI, 1978) I stated the following.

"Professor Jones was, at all times, an educator of the highest rank and an inspiration to a large number of graduate students, from whom he evoked, as a result of his enthusiasm, sincerity, and gentle character, tremendous respect and affection. All of his students, former research associates, university colleagues, and friends will long remember this truly fine and outstanding gentleman."

In all of the years that I knew J.K., I had never heard someone speak ill of him, and similarly I had seen him to be only kind and generous in his opinions of others. At J.K.'s Memorial Service, the University Chaplain, the beloved "Padre" Laverty (who deserves the title "Mr. Queen's" better than anyone that I have known) referred to J.K., quoting from the Book of Job, as "One Among a Thousand."

In 1975, when J.K. was 63, I thought that an appropriate tribute for his accomplishments would be a Symposium in 1977 in celebration of his 65th birthday. With J.K.'s blessing, I started the organization of this Symposium which I entitled "Perspectives in Carbohydrate Chemistry."

I had assembled an all-star line-up of speakers and discussion leaders, which included Ray Lemieux, Claude Bishop, Bob Marchessault, Steve Hanessian, Arthur Perlin, Harry Jennings, and Gerald Aspinall from Canada, Steve Angyal from Australia, Allan Foster and Les Hough from the United Kingdom, Hans Paulsen from Germany, John Moffatt and Derek Horton from the United States, Steve Gero from France, and several others. However, in 1976 J.K. was beginning to show symptoms of illness, and in the summer he had to undergo surgery for cancer. He still wanted me to proceed with the Symposium, which I had scheduled for May 25—27, 1997. On April 13, 1977, J.K. died after a valiant struggle with his illness. His beloved wife, Marjorie, encouraged me to continue with the Symposium. I quickly sent telegrams and made telephone calls informing the world's carbohydrate community that the Symposium was being dedicated now to the memory of Professor J.K.N. Jones. Over 200 delegates came from as far away as Japan, Australia, and South America. We celebrated J.K.'s life in a truly family atmosphere. I should mention that I was assisted in the task of organizing the symposium by two of my graduate students, Ted Ison and Mario Pinto.

In 1976, George Hay and I started to collaborate in research with the aim of fostering some multidisciplinary projects. George Hay was also a carbohydrate chemist with a specialty in polysaccharide chemistry. He had received his Ph.D. with the distinguished Professor Fred Smith at the University of Minnesota. I specifically mention Fred Smith because he and J.K. were great friends and scientific colleagues. They used to play cricket together in England and, when J.K. married Marjorie in 1937, Fred Smith was the best man. The year 1937 was important to both J.K. and Fred Smith professionally as well. In that year the rights to a patent (involving W.N. Haworth, E.L. Hirst, F. Smith, and J.K.N. Jones) on the nitric acid oxidation of L-sorbose to L-ascorbic acid were sold for a return of £100 sterling to each co-author. J.K. liked to quip that this money "allowed" him to get married.

One of the major projects that George Hay and I became involved in together was concerned with the establishment of a facility for metabolic imaging at Queen's University. The technology to be developed was Positron Emission Tomography (PET). This initiative had its genesis in the Division of Neurology, in particular with Drs. H.B. (Henry) Dinsdale and D.C.N. (David) Howse. The project was a grand one and truly multidisciplinary.

In addition to Drs. Dinsdale and Howse, and George Hay and myself, the team included Dr. Rick Riopelle of the Division of Neurology, Drs. A.T. (Alec) Stewart, Bill McLatchie, B.T. (Barry) McKee, and Rob Douglas of the Department of Physics, Dr. Jeff Kulick of the Department of Computing and Information Science, and Dr. Peter Shragge, a medical biophysicist who was the Senior Physicist at the Kingston Cancer Clinic.

George Hay and I were successful in obtaining a number of grants from the Medical Research Council, a special achievement at that time for chemists. Among our successes was a synthesis of 2-deoxy-2-{^{18}F}fluoro-D-glucose (2-{^{18}F}DFG) using accelerator–produced ^{18}F-fluoride ion generated in a water target. The accelerator employed was the Queen's University 4-MeV Van de Graaff accelerator. The use of 2-{^{18}F}DFG for the measurement of regional cerebral glucose metabolism by PET has become well established and has generated a widespread interest in the applications of this technique. In order to formalize our program we formed an official group at Queen's which became known as the Metabolic Imaging Research Group (MIRG). Initially the members of the individual disciplines obtained their own funding. However, in 1980 MIRG submitted a large proposal which resulted in the formation of a special joint committee of MRC and NSERC. We had a site-visit by a committee of ten scientists (Drs. Bernard Bellcau and Ian Spenser were the chemists). We were making history in the Canadian milieu, but, unfortunately, the project apparently was too big to be funded. Eventually, the "well ran dry" and the project, to my great regret, met its demise.

During the 1980s George Hay and I were successful in obtaining a number of very significant contracts from Industry, for example, the Crown Zellerbach Corporation, Schering Corporation, and Abbott Laboratories. It is noteworthy that all of this Industrial money was from the United States. We were never successful in obtaining any funds from a Canadian company. There is a message here which I leave to the reader to ponder. In the case of the project with Abbott Laboratories, George and I enlisted the collaboration of Dr. W.T. (Bill) Depew, a gastroenterologist in the Department of Medicine. As an aside, Bill was one of our undergraduate chemistry students; he received his B.Sc. from Queen's in 1969. Also, Bill's wife, Cathy, was a Ph.D. student in my group; she obtained her Ph.D. in 1976. The project with Abbott Laboratories concerned the development of a system for total parenteral nutrition. We obtained close to $200,000 for one year for this research. George Hay, Bill Depew, and I then began to develop a program concerned with the targeting of drugs to the liver. We were successful in obtaining very significant funding and jointly published several papers. I am still pursuing this project.

In this article I have discussed at length my special relationship with Professor Jones. I want also to acknowledge the association and friendship with George Hay. We worked together on chemistry, but, whenever I experienced some frustration, a discussion with George always brought clarity to a situation. As an aside, Saul Wolfe had a relevant saying, which I like very much, namely, "A problem is like a kidney stone; this too will pass." George and I attended many scientific meetings together, especially meetings of the American Chemical Society. These were times not only to discuss chemistry but also to share interests and life experiences.

For example, we always took advantage of an opportunity to go to a major league baseball game in the United States (in my opinion, the Kansas City Royals have the best hot dogs and beer).

Over the years at Queen's, I have been involved also in a number of other scientific collaborations. There were, for example, successful projects (as regards funding and publication) with Dr. Don Walton of the Department of Biochemistry (who also received his Ph.D. with Professor Jones) and Dr. Andrew Kropinski of the Department of Microbiology and Immunology. I mentioned earlier that I had studied towards the M.Sc. degree at McMaster University with Professor David MacLean working on alkaloid chemistry. I continued to visit frequently McMaster and Dr. MacLean, and, once in the early 1980s, during a chemistry discussion, the two of us started contemplating some reactions involving alkaloids and carbohydrates, and another beautiful and productive collaboration was born. Dave saw the compounds that we synthesized as alkaloids having sugar appendages; I saw them as nucleosides. This work eventually led to a highly successful program on the enantioselective synthesis of alkaloids.

The last collaboration that I will mention is that with Dr. Bob Kisilevsky of the Department of Pathology, a collaboration which has changed my life. Bob had developed an interest in amyloid as a result of an association with Dr. Michael Axelrad of the Department of Pathology. Dr. Axelrad began the work on amyloid at Queen's because he was suffering from and eventually succumbed to a form of amyloid disease himself. In the mid-1980s Bob approached me about a possible collaboration. There was growing evidence that a carbohydrate polymer, known generally as a glycosaminoglycan, was implicated in amyloid deposition. Bob and I started working together on the problem, and we approached the University's Invention Committee with an Invention Disclosure and a request for a little "seed money." We were turned down. Again an American company solved our funding problem and in a really significant manner. The company was The Upjohn Company of Kalamazoo, Michigan. Starting in 1988, for four years, we jointly received hundreds of thousands of dollars for research. However, Upjohn began to experience financial problems, and they disbanded not only our program but also their own internal program on Alzheimer's Disease.

We approached John Molloy at Parteq and John initiated a search for a company to replace Upjohn. Eventually, John started thinking that perhaps we could form a company at Queen's. In late 1993 Neurochem Inc. was born. The company now has its headquarters in Ville Saint-Laurent, Québec and employs about 50 people. In June, 2000 Neurochem completed a $32-million initial public offering.

Since becoming a Faculty Member of the Department of Chemistry in 1967, I have served under nine different Heads or Acting Heads, and I am

grateful to all of them. I would like to mention specifically Don Heyding who was Head during the 1970s.

Don, as Head, was truly a dedicated "Queen's man" who clearly recognized that leadership implied service more than being served. It was during the 1970s that the entire climate of funding and employment was shifting. It was also during the 1970s that our Department had to suffer the slings and arrows of the outrageous ACAP Report. For example, during a visit to Birkbeck College of the University of London, at a luncheon with the Master and some of his colleagues, he said to me, "I hear that Queen's is losing its Ph.D. program in Chemistry." I immediately assumed the role of a spin-doctor and corrected this fallacy. Don successfully shepherded us through this period and we have survived. In my opinion one of the major lessons to be drawn from this experience is that the success of each of us is the success of all of us.

With the limitation of space, I clearly have stressed my research activities at Queen's. However, I really have always viewed teaching as my main responsibility. I have never subscribed to the argument of teaching *versus* research; I think that the proper role of a university professor is teaching *and* research. I have always truly enjoyed the teaching function. However, in the last few years there have been some frustrations occasioned by the inadequacy (the polite word) of the University's lecture rooms and the very significant increase in class sizes. I also am not enamored with a legalistic distribution of teaching loads and assignments for the sake of equity. Some people have a special ability to teach and inspire first- and second-year classes and, in my opinion, it is this aspect that is most relevant.

I thank Erwin Buncel and Julian Brown for the invitation to contribute to this Millenium Project and I thank them for their patience.

And now, the torch is passing to a new generation in the Department of Chemistry. That's the way it has to be and should be.

John A. Page
1968-91

I joined the Department in 1968 in the last years of the McIntosh era. I came to Queen's from the University of Toronto where for some years I had an enjoyable career in teaching and research in analytical chemistry. I was encouraged to come to Queen's by Bob McIntosh; I remember with some fondness his direct speaking and political incorrectness and often wonder how he could have survived in the current era. I also remember the courtesy and kindness of Pip Nation who as departmental administrator did much to smooth the difficulties of settling in to Gordon Annex. With me, from Toronto, came a group of exceptional graduate students who I remember as close friends; we remember with some chagrin the difficulties in establishing the group at Queens; we were disappointed to find that the establishment of communication channels to the higher echelons was more difficult than at the supposedly impersonal University of Toronto.

At Queen's, I arrived to find the teaching of undergraduate analytical chemistry in the good hands of Wally Breck and Bob Wheeler. This duo put an enormous effort into their teaching and sustained the analytical lectures and laboratory for a long and continuous period. The undergraduate laboratory did suffer from lack of equipment and supplies and Scott Meskis deserves a good deal of credit for the work he did in keeping the laboratory operating smoothly and efficiently. Wally was a gifted physical chemist and ahead of his time in combining chemistry with environmental concerns; he received little credit for establishing an environmental course in the Department well before such courses became universally popular. On retirement, his program disappeared only to reappear with a flourish at a later date in other ways. Bob was also a gifted physical chemist with a background in spectroscopy and a solid knowledge of spectroscopic techniques for trace elemental analysis. It was an exciting time in analytical chemistry and Wally and Bob were able to bring to the students the flavor and sense of the change during this period. Their analytical courses are remembered for the personal touch and the closeness and achievement of the undergraduate groups.

Since my arrival at Queen's, analytical chemistry and the teaching of the subject has continually evolved and this required support from the Department. Gary vanLoon joined the staff at an early stage in my career and we worked closely together, sharing students and equipment; his friendship and help were important at every stage. Gary's interests in both Soil Chemistry and the Environment led him to develop new programs, but analytical chemistry has remained central to his interests.

Diane Beauchemin joined the Department from the National Research Council in 1988 and proceeded to establish her own strong research group in analytical spectroscopy. Stephen Brown joined the Department at a later date with a cross appointment in Environmental Studies; his interests also have a strong analytical component. In recent years, these people have taken over the guidance of the analytical program and under their direction it has continued to move forward. In 1999, in the most recent step, Igor Korzin was instrumental in introducing a component of computer based analysis into the program.

At Queen's, Gary and I were both concerned with teaching analytical chemistry at the graduate level. Together we developed an Analytical Chemistry M.Sc. program based on lecture and laboratory material, but the success of the program was in large measure due to Gary's vision and enthusiasm. Over the years the program graduated a very large number of students to fill a demand for environmental and analytical chemists. The course was demanding in terms of contact with the students and both Gary and I devoted considerable energy to the program. Gradually the demand and support for these students diminished, and the program is no more. In later years, Gary has brought his energy and enthusiasm to the problem of teaching analytical chemistry to the undergraduate engineering students. The result was the establishment of a 'Summer Camp' where the whole range of analytical techniques is taught outside the regular term.

Another important aspect of analytical chemistry at Queen's was the formation of the Analytical Service Unit (ASU) within the Department in 1978. The original aim was to help the chemists and others within the University with their analytical problems. The Unit was founded with support from Dean Sinclair and John Poland was recruited as director. Under his leadership, the Unit has developed analytical and environmental expertise on a major scale and has assembled sophisticated instrumentation for analysis purposes. Over the years, the Unit has contributed immensely to the teaching of analytical chemistry at both the undergraduate and graduate level in the Department. The Unit has also developed contacts with the Royal Military College and is a significant contributor to their reactor program. The appointment of Phil Beely through Queen's Chemistry Department provided the College with the necessary expertise in nuclear chemistry and activation analysis. Sadly, space and other considerations have forced the Unit from the Department, but it's growth and success under the leadership of John Poland continue unabated. It continues to function as a major support for teaching and research in analytical chemistry within the University and the Department.

Finally, I thoroughly enjoyed my many years of teaching and research within the Department. I usually had a major commitment in the teaching of introductory courses where both the students and my colleagues were always a challenge. The teaching of electrochemistry and analytical chemistry in advanced undergraduate and graduate courses was always enjoyable. In research, electrochemistry was always an exciting field and I had the opportunity to work with many graduate students. Every one of these students was an exceptional person and it is they who have left me with my fondest memories of Queen's.

J. P. Colpa

1969-1991

I was born in Arnhem, Holland in 1926 but grew up in Amsterdam. After attending the special academic (university preparation) high school, I was ready for university in 1943, but chose not to go because of political requirements imposed on students during the German occupation. Instead I was trained privately by professors until after the war was over. I enrolled in the University of Amsterdam in August 1945 to study chemistry and physics, and graduated in 1948. I then started graduate work and took the equivalent of the M.Sc. degree in 1953, and the Ph.D. in 1957. My Ph.D. research was on the high pressure rotational and vibrational spectrum of hydrogen (both pure and in mixtures). Transitions which are forbidden in the free moloecules become allowed as a result of molecular collisions in the gas at high pressure. From 1950-57 I taught part time at an art school, teaching a course on the chemistry of paint and related topics.

After my Ph.D. degree I worked at Shell Research in Amsterdam starting in 1957. During the summers of 1958 and 1959 I attended courses in Oxford and Cambridge where I came into contact with Charles Coulson (Oxford) and Christopher Longuet-Higgins (Cambridge). I spent a year in Cambridge in 1961. In 1963 I became part-time professor teaching theoretical chemistry in Amsterdam while I was still working part time at Shell. I published several papers in "Molecular Physics", which began publication around 1957. Later I became involved as a co-editor of that same journal for many years. I came to Queen's as Professor of Chemistry in 1969. I taught theoretical chemistry, particularly quantum mechanics and group theory to upper year and graduate students. I also taught first year general chemistry and second year physical chemistry.

My research was in the quantum theory of triplet states in aromatic molecules. I studied the spectroscopy and photochemistry of both free molecules and the crystalline state. I maintained my contacts in Europe through summer visits and several sabbatical years, in 1975-76 (Heidelberg), 1982-83 (Heidelberg and Berlin) and 1989-90 (Berlin). During these visits I collorated with Karl Hausser in Heidelberg and Dietmar Stehlik in Berlin.

Music has always been an important part of my life, and I still play the piano every day. During my time at Queen's I served on several committees, such as the Committee on Fine Arts and Public Lectures, which are concerned with the musical life of the campus and the city.

I retired from Queen's July 1991.

Gary W. vanLoon

1969-

One of the relatively new initiatives within the Chemistry Department has been the development, beginning in the 1980's, of a formal program in Environmental Chemistry. Of course, this did not evolve in a vacuum, as a number of faculty members have had longstanding interests in environmental research. Mike Baird, for example, has carried out considerable work on catalysts, some of which have environmental applications and Jeff Wan has been interested in the chemistry of air pollution control. However, there had been no undergraduate course in environmental chemistry, and no appointments of persons to work specifically within this sub-discipline. In 1987 the Curriculum Committee approved the establishment of an upper level undergraduate course called Survey of Environmental Chemistry (CHEM 425, later to become CHEM 326). The course was to build on prerequisites in each of the traditional sub-disciplines, and present an overview of chemistry of the atmosphere, hydrosphere and terrestrial environment. I taught the course for 10 years, and in the past three years it has been taken over by our new appointment, Stephen Brown. This course generally attracts about 30 to 60 students, from the chemistry department as well as other science and applied science students. In 1995, two fourth year courses, Topics in Environmental Chemistry (CHEM 426) and Environmental Analysis (CHEM 478) were added as environmental offerings.

While these courses were our first formal step into the area of environmental science, the Faculty of Arts and Science was also working to establish a broader program involving several departments. Faculty persons from Biology, Chemistry, Geography and Geology had been meeting to discuss the possibility of setting up a program that could involve students from a variety of departments. These discussions led to the development of a program supported by four science departments. Combined curricula were laid out for Environmental Biology, Environmental Chemistry, Earth Systems Science (Geography) and Environmental Geology. On the recommendation of outside consultants, the curricula were designed to emphasize disciplinary strength, while also including a core of cross- and inter-disciplinary courses so that students would be exposed to the complexity of environmental problems and the need for interdisciplinary solutions. Students were accepted into these programs beginning in 1992. The first class of 52 students completed their undergraduate studies in 1995 and since then a class of between 40 and 55 students has graduated each year. On average, nine students in each graduating class have been in Environmental Chemistry.

After the program had been in operation for one year, the heads of the participating departments (John Stone, in our case) submitted a proposal to establish a School of Environmental Studies - a proposal that was approved by Senate on May 26, 1994. Peter Hodson, a biologist, was selected as the first director of the School and has served in that office up to the present time. One of the recommendations in this plan was that the new School would be provided with four additional appointments – each appointment being shared jointly between the School of Environmental Studies and a participating department. In the case of Chemistry, this allowed us to appoint Dr. Stephen Brown to one of these positions. Stephen came to us in 1996 and was provided with office and research space in the new Biosciences Complex. He has established a very active group in the area of fibre-optics sensors with fluorescence detection for small organic molecules in environmental samples. He is also involved in teaching courses in Environmental and Analytical Chemistry.

The Environmental Chemistry program is well integrated within the department and typically about one third of the Chemistry (Arts and Science) graduating class is made up of students from this program. In keeping with the emphasis on disciplinary strength, it is worth noting that these students are required to take almost as many chemistry courses (only one half fewer) as do our students who follow the traditional Major program. A number of our graduates have gone on to post-graduate programs in chemistry, but also in other areas such as Civil and Chemical Engineering. Employment in Environmental Consulting firms seems to be a common option.

Simultaneous with the development of an undergraduate program, there has been growing research activity in areas of Environmental Chemistry. One project involves joint research in the labs of Erwin Buncel, Stephen Brown, Greg Thatcher and Gary vanLoon, as well as Bernie Kueper in Civil Engineering. The project, sponsored by ESTAC, investigates the synthesis and application of cyclodextrins (CDs) in enhancing the removal and remediation of hydrophobic organic compounds such as trichloroethylene and perchloroethylene from contaminated soil and groundwater sites. Other work involving the Buncel and vanLoon groups is focussed on the environmental behaviour of organophosphorus pesticides in soil / water systems. These investigations combine studies of the abiotic degradation of these important insecticides with studies on the partitioning of the parent material and the products within the total soil environment.

Stephen Brown's research program includes the development of a fibre-optic sensor for aromatic hydrocarbons and a fibre-optic oxygen sensor; the monitoring polycyclic aromatic hydrocarbons in solid samples by simple extraction and fluorescence detection; photo-chemical pretreatment followed by fluorescence for detection of polychlorinated biphenyls; the analysis of aromatic hydrocarbons and metabolites to study impacts on exposed fish; and the development of partition phases for use in aquatic toxicology tests; the use of molecular fluorescence in new methods for detection of organic contaminants; the development of fibre-optic environmental sensors using films for extraction of analytes; the partition of organic compounds between phases. These themes are central in developing new analytical methods and for understanding chemical behaviour in the environment.

An unrelated vignette

I was hired during the headship of Bob McIntosh. Soon after my arrival, however, he became a Dean, and I did not have a great deal of direct association with him in the new office. Nevertheless, I encountered him frequently enough to learn something about the Queen's culture, and also about diplomacy in the University setting. In this connection, I well remember one contentious meeting where department members lined up in approximately equal numbers on two sides of an issue – each side presenting views which suggested absolutely no possibility of compromise. Dr. McIntosh listened intently to the irreconcilable opinions and then began his evaluation of the proceedings by saying:

"I agree with *everything* that has been presented here. However..."

Needless to say, he then went on to present yet another viewpoint, and it was his opinion that prevailed.

Brian K. Hunter
1971-2002

Nuclear Magnetic Resonance in the Department of Chemistry at Queen's

In many ways, the development of research in the Department can be followed in the Nuclear Magnetic Resonance spectrometers in use.

1960's

The first NMR spectrometer at Queen's was an A60 from Varian Associates. It was a continuous-wave, proton only, instrument based on a water-cooled electromagnet. The main driving force behind the purchase of this instrument was Dr. R.Y. Moir. The A60 continued in use into the mid-1970's and, when it cease to give useful spectra, the magnet was given to the Physics Department where it was used in an undergraduate Electron Spin Resonance experiment. Coincidentally, my first use of NMR was on an A60 in the Chemistry Department at UBC in 1964 as an undergraduate working with Dr. Len Reeves, an early pioneer of Canadian NMR.

When I joined Queen's in 1971 a Bruker HX 60 had been installed. This was, initially, a continuous-wave multinuclear system capable of running ^{1}H, ^{13}C, ^{19}F and ^{31}P spectra. This instrument marked the beginning of a long association with the Bruker organization that continues to this day. In 1975, we upgraded it to the Pulsed Fourier Transform mode that characterizes modern NMR instrumentation. The upgrades and long-term success of the HX 60 owe much to the care provided by Dave Groves over many years. The instrument was finally shut down in the early 1990's when more modern instruments took over.

1970's

In the seventies, two Varian CW instruments were purchased. A T 60, permanent magnet machine, for routine proton use was added and a used HA 100 was installed to carry some of the proton load from the HX 60 as Carbon spectra became more important. My first association with the HA 100 again goes back to UBC where, as an M.Sc. student, I used one as a "high field" instrument to go with our home-built 40 MHz machine on which I ran some of the first ^{29}Si spectra. I used a modified Heathkit amateur radio transmitter as the proton decoupler.

1980's

In 1980, as the first of a series of major instruments, a CXP 200 was purchased from Bruker. This was the first of our machines to feature a liquid Helium cooled superconducting magnet. With a proton frequency of 200 MHz, the instrument is fully multinuclear and capable of running both liquid and solid samples. A key element in the purchase of the CXP was the financial support from Dupont Canada for the purchase and continued operation of the spectrometer. It also marked the beginning of our association with Dave Axelson that continues today. It also depended on the hiring of Stan Woodman to allow us to exploit the capabilities of the machine. When Stan returned to Bruker, Robin Roberts took over many of the service duties and Robin remains our major in-house support person.

It is interesting to note that the magnet from the CXP 200 remains at its 4.7 Tesla field and the power supply was last connected to the magnet on August 8, 1980! Over the last two decades, this instrument has served me very well. We have done an almost unbelievable variety of experiments on it. The samples have ranged from rats to rocks. I have used it to provide undergraduates with a laboratory experience in NMR where in an afternoon we run ^{1}H and ^{13}C spectra in several different modes including two dimensions and look at the measurement of relaxation behaviour. In terms of research, I have use chlorine, nitrogen, and titanium NMR to look at several solid state relaxation problems with Julian Brown. Ken Russell and I have studied many polymer problems, usually by ^{13}C NMR in solutions or solids. With my own students, Andy Crawford, Allison Rutter, and Fernando Commodari, I have used ^{13}C NMR to monitor metabolism in bacterial cultures that were actively dividing in the sample tube over periods of many hours. With Jon Foglein, I have used deuterium NMR to monitor the mobility of intracellular polyhydroxybutyrate that remains fluid even when more that one hundred degrees below its freezing point. We have also attempted to monitor the changes in phosphorus metabolism that occur in rats during seizures by running ^{31}P spectra of live rats. With Will Groten, I have used ^{29}Si NMR to study the degradation of zeolite catalysts. With John Anderson, I looked at heavily deuterated ethylene-copolymers to probe motions of those molecules in the solid state. Finally, with Robbie Flemming, I have used ^{17}O NMR in synthetic minerals in an attempt to understand some of the disorder phenomena that occur in many natural minerals. Others in the Department have added to this variety.

In 1984, a Bruker AM 400 was purchased to meet the expanding research needs of the Department. The spectrometer is multinuclear and provided a higher field complement to the CXP.

Sue Blake was responsible from much of the success of the AM 400 as a service instrument. Even though the AM 400 was designed as a solution spectrometer, we have used it for Magic Angle Spinning experiments on minerals and zeolites. In 1989, an AC 200 was purchased to provide a $^{1}H/^{13}C$ routine service machine on which most graduate students could run their own spectra. It is still running in its original configuration.

1990's

The nineties have seen both new instruments and major upgrades to old ones. In 1995, Dr. Almeria Natansohn purchased an ASX 200 for her own polymer studies. In many ways, the ASX is a next-generation CXP and has been used exclusively for solid state polymer NMR studies by her group.

In 1998, three additional spectrometers were purchased and three others were upgraded. The old CXP console was replaced by a Tecmag Console and new radio frequency electronics. This instrument is now used exclusively by Dr. Gang Wu for his research group. Mike Baird obtained an AC 200 to provide routine $^{1}H/^{13}C$ service for his group. The AM console on the 400 MHz instrument was replaced with Bruker AVANCE console as part of a major equipment purchase. At the same time, the Department purchased an AVANCE 300 to provide additional routine $^{1}H/^{13}C$ service and AVANCE 500 as the major research machine. The AVANCE 500 includes full solid state capability in addition to all of the liquid state experiments that have become so important today. The management and exploitation of much of this new equipment is in the capable hands of Dr. Françoise Sauriol who has joined us from McGill University.

It is interesting to draw some comparisons. In the 1960's, we could obtain a good proton spectrum in about ten minutes on samples that were essentially neat liquids. Today, we can obtain a spectrum with almost ten times the chemical shift dispersion on a few milligrams in ten minutes or a few micrograms overnight. Or putting it another way, If I still had all the samples, I could repeat six months worth of data collection from my M.Sc. thesis in an afternoon. Perhaps more impressive, is the range of experiments we can do. Most of the standard two-dimensional experiments we now use on a routine basis have been invented since we installed the CXP 200 in 1980.

Donal H. Macartney

1983-

I started my academic career in the Department of Chemistry at Queen's University in September 1983. I had arrived in Kingston in late August from Long Island, New York where I had finished two years as an NSERC Postdoctoral Fellow at the Brookhaven National Laboratory with Dr. Norman Sutin, after completing a Ph.D. with Professor Alex McAuley at the University of Victoria. I had started looking for academic positions the previous summer after hearing about the new NSERC University Research Fellowship (URF) program, which was established to encourage Canadian universities to hire young faculty. After studying the ACS Directory of Graduate Research, I decided to write to three universities; Dalhousie University, the University of British Columbia, and Queen's University, to inquire as to their situation regarding nominating persons for NSERC URFs. I didn't know much about Queen's or its Chemistry Department, but my sister was completing her final two years of a B.A. in Art History at Queen's so I thought I would give them a try.

I then headed off on a three-week bicycle trip around New England, hoping to hear back from the universities on my return. In southern Maine I gave my parents a call and they informed me that someone from the Queen's Chemistry Department was trying to reach me and gave me a phone number. The next morning I called and spoke with Betty McIntosh. They were interested in nominating me for an NSERC URF and wanted me to come up to Kingston to present a seminar and speak with some of the faculty. After settling on a date which would give me enough time to complete my bicycle trip, I pedaled back to Brookhaven to prepare my presentation. As it turned out, neither Dalhousie nor UBC were particularly encouraging about prospects for a nomination for a URF.

When NSERC announced the results of the URF competition, I initially found myself on the reversion list, but soon thereafter was informed that I had been awarded a Fellowship and would be starting at Queen's in September. When I arrived at Queen's I was given Jim McCowan's office, as he was on a sabbatical leave, and a laboratory shared with Brian Hunter. I got a lucky break in terms of obtaining a graduate student in my first year. John Herbert had been accepted both as a medical student and a chemistry graduate student at Queen's. John was a year older than I was and was married with four young children. As a result, he couldn't afford to go to medical school in 1983, but had been awarded an NSERC Postgraduate scholarship and decided to postpone medicine in favour of a Master's in Chemistry. John seemed quite interested in the work I was doing and became my first graduate student.

One of the advantages of the URF program was that you were guaranteed an NSERC Operating Grant for your first few years.

I had also applied for an NSERC Equipment grant to acquire a stopped-flow spectrophotometer for my kinetic experiments, but was unsuccessful. The Department did provide me with a $4,000 "start-up" grant (how times have changed!), which together with part of my NSERC Operating Grant allowed me to buy the stopped-flow apparatus. A small grant from the Advisory Research Committee provided me with a pH meter and a balance and John and I were all set to begin research. While he didn't complete his M.Sc. degree before embarking on medical studies, his research work resulted in my first two papers from Queen's, on the kinetics of electron transfer reactions of rhodium(II) dimer complexes. John entered Medicine the following year, feeding his family and funding his medical training by joining the Canadian Armed Forces and purchasing, renovating, and renting out a duplex.

When Vedene Smith and I had discussed teaching during my first visit to Queen's we came to a decision that I would create a new fourth-year course on Bioinorganic Chemistry and teach it in the Winter term in alternating years. Because the course was created after the timetabling had been completed for the 1983-84 academic year, the course ended up being offered once a week in the evenings. It was an interesting group of students, including a farmer, who was completing a medial degree in Chemistry and Religion, and Don Weaver, who was auditing the course as a Ph.D. student. I cringe when looking back at some of my overhead transparencies from that first year of the course. The lettering was too small, there was too much information on each slide, and I am sure I didn't leave each slide up for enough time. The final exam was also held in the evening, but it happened to coincide with a Montreal Canadiens playoff game, and at least one of the graduate students was not happy at the prospect of missing the game. I circumvented this problem by placing a 12" black and white TV (the "computer monitor" for the stopped-flow apparatus) under the table at the front of the classroom (with the sound off) and posting the score periodically on the blackboard. The following year I taught Chemistry 112 in the Winter term with David Wardlaw and Suzanne Fortier. I probably learned the most about teaching with this course. The first-year class sizes were much smaller (about 150 students) than they are now, but you had to be on your toes or you had a sea of hands being raised.

The first summer in Kingston, I started playing soccer with the Chemistry team in the Graduate Student league. The team has reached the finals on several occasions and won the Championship in 1990. I am still playing on the team, but for fewer minutes per game. My problem is that I am getting older and the students are staying the same age!

Mark Hoddenbagh joined my group in 1984 after completing a medial degree in Chemistry and Spanish at Queen's and became my first Ph.D. student. Mark did some excellent research into ruthenium analogs of some iron complexes and reactions that I had studied as a Ph.D. student. After spending a year as a Visiting Fellow at the National Research Council in Ottawa, Mark moved to Meadow Lake in Northern Saskatchewan, where he is currently the manager of the research lab at the Millar Western zero-effluent pulp mill.

In 1987 there was a significant turnover of graduate students in my laboratory. Manuel Aquino and David Thompson (who joined my group in 1985) finished their M.Sc. theses in the summer and went on to Ph.D. programs at Carleton and York, respectively. The manners in which they undertook their thesis writing were entirely consistent with their personalities. Manuel was extremely organized, never seemed to need to work evenings or weekends, and produced a near-perfect first draft. Dave, on the other hand, had frenetic energy and it was difficult at times to reign him in. Dave and I spent all night in the Department (with a case of beer) completing the final draft of his thesis, finishing at 6 a.m. and going to McDonald's for breakfast.

Dan Foucher, Sam Mak, and Steve Wylie joined my group as graduate students in 1987 and for a brief period that summer, the lab was very crowded with students. I had applied to a Government Student Employment program for funds to partially support a summer research assistant, but did not find out that I would receive the money until near the end of April when most of the students had finished exams and had left for the summer. Dan Foucher, who had just finished his 4th year, came by my office just after I had received the funds to see if I was hiring any summer students. During the summer Dan decided he would stay on for a Master's degree. He was extremely productive in his research right from the outset and was also a driving force in organizing social activities for the graduate students in the Department. Dan's M.Sc. thesis is still the largest thesis, including Ph.D. theses, to have come out of my laboratory. After completing his M.Sc. degree, Dan joined his wife Sandra in Toronto and became Ian Manners' first doctoral student at the University of Toronto. After an exceptionally productive period as a Ph.D. student, Dan is now working for Xerox in Syracuse, New York.

Sam Mak joined my group in May 1987 as a M.Sc. student after spending eight months in Jeff Wan's group. Sam was quite a character and always had an opinion on whatever topic was being discussed in the lab. After completing his Master's in 1990, Sam went to work for Mark Hoddenbagh in Meadow Lake, Saskatchewan before returning to Toronto to get married. Sam now works for Warner-Lambert in Toronto. Steve Wylie had returned to Queen's to complete his Honours B.Sc. after teaching for two years at a private girls school in Whitby.

Steve took my fourth year Bioinorganic course and decided to stay at Queen's for graduate studies in my lab. Steve's research work took the lab into a new direction. He began our research into supramolecular chemistry with cyclodextrins.

It wasn't very promising at first, but he persevered and set the groundwork for the next decade's work in the lab, with his discovery of self-assembly cyclodextrin rotaxanes. Steve's excellent graduate record and thesis was rewarded with the Governor General's Academic Gold Medal in 1993. Steve went to Cambridge University as an NSERC Postdoctoral Fellow and is now a faculty member at Ryerson Polytechnic University in Toronto.

The lab was a whirlwind of research and social activities in the late 1980's and early 1990's with Steve and Dan leading the way. They started the Celebrity Coffee Corner, featuring "day-old" donuts and featured guests such as Scottie Meskis. The student office area off the lab would be crammed with graduate students who could be heard at both ends of the third floor of the Frost Wing. With the arrival of Simon Hesp and Warren Baker in the early 90's, we moved our lab to its current location next door to the lab now occupied by the polymer chemists. In addition to playing soccer in the summers, weekly games of squash (most notably doubles squash with John Stone against Ralph Whitney and Warren Baker for one year) and more recently badminton have helped to keep me reasonably fit through the years. I still play badminton with Pat Mulligan, Ray Bowers, Tom Hunter, and some of the current crop of graduates and summer students that we can convince to come out and hit the bird around. In 1989 I applied for and was granted tenure and promotion to Associate Professor.

The first female graduate student joined my group in 1989. Trish Huber came from the University of Toronto to do a joint Ph.D. project with Mike Baird and myself, on the kinetics and mechanisms of reactions of chromium dimers. Trish is now a mother living in the sunny Okanagan valley of British Columbia. Jerome Imonigie joined my group as a Ph.D. student in the fall of 1990, arriving from Nigeria just as the weather started to turn cold. He suffered through his first Canadian winter and had to wait until May for his wife to join him from Nigeria. He faced a steep learning curve when it came to instrumentation and computers, but soon had them figured out and was very productive in the lab. Jerome is now a NRC fellow at the Environmental Protection Agency in Las Vegas.

Chris Waddling joined my group as a M.Sc. student in 1991, after obtaining his B.Sc. from the University of Toronto, and also worked on cyclodextrin rotaxane complexes. After completing his M.Sc. at Queen's, Chris went to the University of Hawaii for his Ph.D. and a great suntan. Chris has recently started a job as staff crystallographer in the Department of Biophysics at the University of California in San Francisco.

Angela Lyon worked as a summer research assistant in my lab in 1991 after completing her B.Sc. degree in Chemistry at Queen's. In 1992 I joined with six other chemists and pharmacologists on a joint project funded by Ciba-Geigy Canada to investigate novel vasodilator drugs.

After working at the Royal Military College for a year, Angela joined the drug project in September 1992 and when the project concluded at the end of 1994 started a M.Sc. program in my group working on cyclodextrin rotaxanes. It was great to have Angela in the lab. She kept the place running very smoothly, especially during my sabbatical leave in 1992-93 when she looked after both the lab and my house while working on the drug project. Angela is currently a research associate with Neurochem in Don Weaver's laboratory.

Catherine Smith joined my group as a Master's Student in 1995, after teaching high school for a couple of years in Toronto. Catherine's husband was going to be a medical resident based in Kingston for two years and she decided to complete her M.Sc. at Queen's during the same period. She had intended on working on a project in organic chemistry and became interested in our work on cyclodextrin chemistry. Fortunately her knowledge of synthetic organic chemistry was better than my own and she very methodically made several series of novel organic compounds and did some very nice work in measuring their guest-host interactions with cyclodextrins. Catherine and her husband now live and work in Fort Frances with their two children. Andrew Baer also started graduate studies in 1995 after completing a 4th year honours research project in my group. Andrew is currently writing up his doctoral thesis.

The year after returning from my sabbatical I started a five-year stint (1994-1999) as the Graduate Coordinator in the Department and the following year was promoted to Full Professor. As you can see, my recollections of events at Queen's between 1983 and 1995 largely revolve around the graduate and summer students who passed through my research laboratory. The friendships that were formed between the graduate students and myself, which continue to this day, are what I most fondly remember of the first fifteen years at Queen's. After attending the weddings of half a dozen or so of the students, I was very pleased to have a large number of them attend my wedding, especially Angela Lyon, who orchestrated the first meeting between myself and my wife a couple of years ago.

Axel D. Becke

1984-

ADVENTURES OF A PHYSICIST IN THE QUEEN'S CHEMISTRY DEPARTMENT

I began my appointment as Assistant Professor of Chemistry at Queen's in 1984 with a mixture of both exhilaration and trepidation. After all, the only chemistry course I'd ever taken was freshman chemistry with the engineers, in 1971. How had this come to be?

QUEEN'S: STIRLING HALL

My association with Queen's began in the years 1971-1975, as an Engineering Physics student in the Faculty of Applied Science. I was much more familiar with Stirling and Jeffery Halls than with any of the buildings in the chemistry complex. Applied Science freshman lectures were taken in a curious room in the bowels of Gordon Hall (or was it the Annex?), and labs on the first floor of Frost, as they are today. To be honest, chemistry was not among my favourite courses. I was determined to be an engineer some day. The lecturer's name has long faded from memory, but my present colleagues may rest assured that it wasn't one of them!

I was the studious type, but managed to find time to train and travel with the track team. Long jump and sprints were my specialty, and through the track team I first met Mike Baird. Never could I have imagined that, eventually, I'd be one of his young colleagues in the chemistry department!

The rest of my spare time was spent in the basement of Dupuis, writing FORTRAN code, punching card decks, ripping printouts off the Dupuis printer. I was a computer nerd, writing programs for anything that seemed amusing, from solving linear systems to integrating the trajectories of artillery shells. No CRT terminals, no graphics, just standard FORTRAN programming. How times have changed! I honestly don't know if today's pictorial, point-and-click computer environment is progressive or retrogressive. Can anyone really absorb all that colour, action, and information? Is anyone really able to abstract the essence of a subject from so much noise? Are creativity, patience, and persistence things of the past? More comments in this vein later.

I'd enrolled in the Engineering Physics program just before CEAB accreditation changed its character, and found myself by third year taking a greater interest in fundamentals rather than applications or design.

As many theoretically oriented courses as possible were squeezed into my program and, by the end of year four, graduate studies in theoretical physics beckoned. Nuclear physics was an exciting frontier at the time, and Boris Castel suggested that I contact Donald Sprung at McMaster as a potential supervisor. His advice was taken, and with an NSERC 1967 Science Scholarship in hand, I headed for McMaster in the fall of 1975.

McMASTER: GRADUATE STUDIES

Donald Sprung offered a variety of research projects in nuclear theory, from analysis of scattering data, to the study of uniform nuclear matter, to Hartree-Fock computations on nuclei and nuclear reactions. I wrote a Master's thesis on the subject of nuclear density determination from electron scattering data, and then searched for another topic to pursue as a doctoral thesis.

Professor Sprung was Dean of Science during my years at McMaster, and administrative responsibilities found him out of the physics department often. I read voraciously, and was especially impressed by the books of J.C. Slater on the quantum theory of matter, particularly electronic matter: atoms, molecules, and solids. I also got a thorough grounding in advanced quantum theory from graduate lectures of Profs. Bhaduri, Carbotte, Nogami, and Volkov of the theoretical physics group.

As an NSERC 1967 Science Scholar, my supervisor granted me considerable leeway to set my own course and explore my own options. Slater's books on electronic structure held ever increasing appeal. In the 1970s, he and K.H. Johnson enthusiastically promoted a new computational approach to molecular electronic structure called the "Xalpha" method. An antecedent of what is today known as Density-Functional Theory (DFT), Xalpha offered tremendous economization over the Hartree-Fock LCAO technology that was the mainstay of computational chemistry. Evolved from the so-called "muffin-tin" approximation of the solid-state physicists, the approach of Slater and Johnson was elegant and numerically beautiful to my mind. I was hooked on the problem of molecular structure methodology.

As the literature on the Xalpha method grew, it became apparent that the muffin-tin aspects of the Slater and Johnson approach were far too crude for molecules. The numerical standards of the quantum chemist on spectroscopic properties such as bond lengths, bond energies, and vibrational frequencies, simply could not be met by "muffin-tinning" the molecular potential. The underlying theory, however, which had been proposed by Slater in the early 1950s, remained of interest. Improved implementations of the Xalpha theory by others, using LCAO instead of muffin-tin strategies, began to appear in the late 1970s.

They delivered tantalizing molecular properties, superior to those of Hartree-Fock theory and at a much reduced cost.

Major numerical discrepancies existed, however, between these early LCAO Xalpha implementations. In 1980, definitive Xalpha values of the spectroscopic properties of even simple diatomic molecules were not known. As a theoretical physics student naive in the conventional ways of computational chemistry, I wondered if a high precision, grid based, non-LCAO approach to the electronic structure of diatomic molecules might be useful. I had written a 2D finite difference program in prolate spheroidal coordinates to investigate Thomas-Fermi-type theories in diatomics, but those investigations had produced little of value. Beef it up, I thought, and turn it into an accurate SCF Xalpha program, and a doctoral thesis might ensue.

The program was written. Testing and debugging began. Spectroscopic properties of various diatomic molecules containing H through Ne were computed. But something was wrong. Bond energies were unreasonably large, the worst case being N2. After weeks of tedious analysis and testing, the possibility of a programming error was eliminated. But my computed bond energies were still too large. Inexperienced with the subtleties of computational chemistry, my future hung in the balance...

At McMaster, the physics and chemistry departments share the Senior Sciences building, and I had come to know two inhabitants of the chemistry half. Richard Bader served on my advisory committee, and during my years at McMaster was an important source of encouragement. It is impossible not to be inspired by Richard Bader's infectious enthusiasm and vision for his subject.

Tom Ziegler, a computational inorganic chemist, paused at McMaster as an NSERC Postdoctoral Fellow on his way to the University of Calgary (where he is today). I happened to meet Tom Ziegler at an informal seminar in the Senior Sciences building given by his good friend, Evert Jan Baerends, from Amsterdam. Both were prominent researchers in the field of Xalpha computational chemistry. Baerends had created the first of the LCAO Xalpha programs, now known as the Amsterdam Density-Functional program (ADF), and Ziegler was a major user and collaborator. I chatted with them about my diatomic Xalpha project, and had periodic conversations with Tom Ziegler thereafter. Several months later, when my new diatomic program refused to generate reasonable bond energies, it was Tom Ziegler who asked the key question; exactly how did you handle the spin polarization of the free atoms? In an instant, I learned from Tom the difference between density-functional theory and SPIN density-functional theory, and everything fell into place.

Almost. My Ph.D. defense was suspended by the external examiner for many months, due to insufficient referencing in the thesis and too few

applications of my new benchmarking tool. Though the setback was a disappointment, the external examiner was right.

The thesis was improved significantly and, for the first time, accurate and reliable Xalpha spectroscopic properties of diatomic molecules were available and published (J. Chem. Phys. 76, 6037 (1982)).

My six years of essentially self-supervised exploration at McMaster were tremendously beneficial. The flavour of what was happening in the nuclear theory community (local density approximations, density matrix expansions, grid-based finite-difference computations on the structure of nuclei) stimulated a point of view and a cross fertilization into chemistry that I continue to exploit today.

DALHOUSIE: POSTDOCTORAL WORK

As the doctoral research finally came to completion, I contacted most of the major physics and chemistry departments in Canada seeking postdoctoral employment. Xalpha calculations on diatomic molecules drew little interest from the physicists, but fortunately several theoretical chemists did respond. I accepted the invitation of Russ Boyd to join his group at Dalhousie. Never having been in Nova Scotia, two (or three as it turned out) years in Halifax was an attractive prospect. Armed with an NSERC Postdoctoral Fellowship, I drove to Halifax in the fall of 1981 with all my worldly possessions in the trunk of my Toyota.

That summer, while still at McMaster revising the thesis, two events occurred which shaped my future. Working in the department late one Sunday evening (graduate students in the era of university mainframe computers developed unconventional work habits!) the telephone rang in the group office and a fellow student answered. It was for me. Who on earth could be calling at such an ungodly hour? And why? The caller identified himself as Vedene Smith at Queen's, one of the theoretical chemists to which I'd sent a postdoctoral employment query. He apologized for his late reply, claiming that my letter had gotten lost in clutter. This is an excuse I now firmly believe, having actually seen Vedene's desk these many years, and having seen how easily clutter can take over my own! In any case, the commitment to Dalhousie had already been made. After a brief conversation, Vedene closed with an invitation to "keep Queen's in mind in the future". His words were not forgotten, and I would.

Also that summer, I attended my first conference, the 1981 CIC at St. Mary's University in Halifax. I gave a contributed talk in a session opened by John Pople, who delivered a classic plenary lecture on the state of the art in Gaussian-based quantum chemistry. It was my first introduction to the man who, more than any other, had shaped the methodologies of computational chemistry. My talk on grid-based, non-LCAO, Xalpha

computations could not have been further from the themes of the plenary lecture.

John Pople, as he usually does, sat in the front row during the presentation of my results, and his was the first hand to rise during question period. I don't recall the question, but I do recall that it was relatively friendly, and so concluded my public speaking initiation.

My years at Dalhousie were very enjoyable and fruitful. There, I began experimenting with corrections to the Xalpha theory involving dynamical correlations and gradients of the electron density. I was struck by the fact that molecular bond energies, of such fundamental importance in chemistry, were so enormously difficult and costly to compute using standard (Hartree-Fock based) quantum chemical methods. Xalpha bond energies, on the other hand, were surprisingly reasonable and calculable at minimal cost. They were not, however, accurate enough for general use. Nor did they exhibit any systematic under or overbinding trend. Equipped with the accurate benchmarking tool I'd created for my doctoral thesis, a systematic assessment of corrections to the Xalpha theory was begun.

Addition of dynamical correlation corrections (what is now known as the "local spin density approximation", or LSDA) nicely systematized the bond energy errors with respect to experiment. The LSDA is, with very rare exceptions, overbinding (Phys. Rev. A33, 2786 (1986)). Though the LSDA overbinding error is enormous (100 percent in the worst cases), it is at least systematic. This offered hope that another kind of correction might be found to counteract its overbinding tendency.

Corrections depending on the gradient of the electron density had the desired effect! Tuning of the functional form of the gradient corrections was necessary, however, to bring bond energies into line with experiment. The necessary trials and modifications were carried out at Dalhousie, though not published until after my three years there had ended (J. Chem. Phys. 84, 4524 (1986)). As sometimes happens, the most important papers are the most difficult to get published.

My early findings were presented on a poster at the 1984 CIC in Montreal. Bond energies dramatically superior to Hartree-Fock or Xalpha could be obtained from an Xalpha-like theory with dynamical correlation and density gradient corrections. Tom Ziegler, who had just joined the department of chemistry at the University of Calgary, liked what he saw and requested a preprint.

For the second time, he was about to have a major effect on my career, as would unfold a few years later.

During the second and final year of my NSERC postdoctoral fellowship, it was time to seek a faculty appointment. The NSERC University Research Fellowship program (URF) was in full swing, and an application to return to the physics department at McMaster was submitted. It failed.

Thankfully, however, an E.B. Eastburn Fellowship from the Hamilton Foundation (for which I was eligible as a McMaster alumnus) provided support for a third year at Dalhousie.

My second attempt at a URF was directed to chemistry departments, having now learned twice that molecular electronic structure is not a "hot" subject in physics. I recalled the invitation of Vedene Smith three years earlier to consider Queen's, and we submitted an application. I applied also for an appointment in the chemistry department at Dalhousie. Both applications succeeded, but the encouragement I received during a brief visit to Queen's in the early spring of 1984 made the final decision an easy one. I joined the department later that summer.

QUEEN'S: RESEARCH TO 1991

Shortly after my arrival at Queen's, the investigations of gradient corrections to Xalpha theory began to pay off. Tom Ziegler quickly implemented my (yet unpublished) gradient-corrected theory into the Amsterdam Density-Functional Program (ADF) and commenced wide testing on molecules not amenable to my diatomic program. The results were very encouraging. His primary research interest was organometallic chemistry, for which traditional Hartree-Fock-based computational methods were out of reach. He applied the new density functionals to challenging and practical problems in organometallic chemistry, and scored impressive successes.

For several years, through the late 1980s, Tom Ziegler was the only practitioner of this gradient-corrected density-functional theory (DFT). As such, he was uniquely able to compute the energetics of organic, inorganic, and organometallic systems with remarkable accuracy and at very reasonable cost.

I derived tremendous satisfaction and encouragement from Ziegler's work, and I resolved to refine and improve gradient corrections as far as possible. The end result was a gradient-corrected exchange functional published in 1988 (Phys. Rev. A38, 3098 (1988)) which reproduces exact exchange energies of atomic reference systems better than any other I know, even now, twelve years later. This functional is known as "B88" in the many DFT software packages that have adopted it in recent years.

I also attempted, in the late 1980s, to come to grips with a much more rigorous approach to DFT than Slater's Xalpha model; the Hohenberg-Kohn-Sham theory (1964-65), for which Walter Kohn would receive a share of the 1998 Nobel Prize in chemistry. Slater's Xalpha philosophy was beautiful and intuitive, but had formal limitations which I was slow to understand. A memorable NATO workshop in Alcabideche, Portugal, in 1983, opened my mind to these subtleties which I had not appreciated before. There, I met Walter Kohn for the first time, and many other DFT

researchers who were to have significant influence on my career: Mel Levy, Bob Parr, John Perdew, Andreas Savin. My philosophical leap from Xalpha to rigorous Hohenberg-Kohn-Sham DFT was marked by a paper on dynamical correlation in 1988 (J. Chem. Phys. 88, 1053 (1988)).

In 1986 a prospective graduate student, Ross Dickson, walked through my door and asked if I had any programming projects to which he might contribute. He had a background in chemistry and computer science, and he could not have arrived at a better moment. It was my dream to construct a grid-based non-LCAO DFT program for polyatomic molecules to supersede the diatomic program that had served me so well. When Ross Dickson signed on as a graduate student I had vague notions of how to proceed, but no definite strategy.

The work was done in three steps. The result of each step was an algorithm which, by itself, would be generally useful to the computational chemistry community even should our ultimate objective not be reached. First, a grid-based numerical integration scheme was designed for highly accurate multicentre (i.e. molecular) integrations (J. Chem. Phys. 88, 2547 (1988)). This scheme has been widely adopted by others. Second, a grid-based scheme for evaluating Coulomb potentials and two-electron Coulomb integrals was designed (J. Chem. Phys. 89, 2993 (1988)). This scheme has also been adopted by others, but only to a small extent.

The third step was the most difficult, finding an associated grid-based procedure for computing Kohn-Sham molecular orbitals. Since a finite LCAO basis would not be used, arbitrary numerical precision would be achievable. All (other) computational chemistry programs are LCAO based, and the objective was therefore novel and unique. It would be the ideal DFT benchmarking tool.

I concocted an iterative perturbation-variation procedure for computing multicentre orbitals in 1988 and, much to our delight, it worked! A fully numerical, basis-set-free molecular structure program called NUMOL (NUmerical MOLecules) was created (Int. J. Quantum Chem. Symp. 23, 599 (1989); J. Chem. Phys. 92, 3610 (1990)). NUMOL was the first, and is still the only, basis-set-free program of its kind.

QUEEN'S: RESEARCH AFTER 1991

1991 marked a turning point. Until that time, all my DFT work was restricted to diatomic molecules and I was unable to explore more interesting realms of chemistry. Gradient corrections had caught the attention of the DFT community, but not of the chemistry community in general. Organic chemists, especially, had little interest in density-functional theory.

With the creation of the NUMOL program, my DFT benchmarking efforts could be expanded to much more extensive molecular tests. The Gaussian-1 (G1) and Gaussian-2 (G2) thermochemical test sets of John Pople, Larry Curtiss, and Krishnan Raghavachari were of particular interest.

Originally comprised of some 100 accurate experimental data (atomization and ionization energies, electron and proton affinities) and now comprising almost 300 data, the G2 thermochemical test set is a tremendously valuable calibration and standardization resource. I decided in 1991 to commence testing of density-functional theories on the G2 set of molecules (J. Chem. Phys. 96, 2155 (1992); 97, 9173 (1992)), so that direct and unequivocal comparisons could be made between DFT and other (more conventional) computational methods.

At the 1991 International Congress on Quantum Chemistry (ICQC) in Menton, France, I presented DFT G2 benchmarks for the first time. The results didn't match the precision of G2 theory itself (average errors about twice as large), but were nevertheless impressive given the tremendous simplicity and economy of DFT relative to G2. This direct comparison between DFT and conventional methods was persuasive. Less than a year later, Gill, Johnson, and Pople had implemented DFT in the enormously popular GAUSSIAN molecular structure program, with the release of GAUSSIAN 92/DFT. For the first time, density-functional methods were available to the entire chemistry community and were no longer the exclusive realm of DFT specialists.

The 1991 Menton ICQC was personally memorable for other reasons as well. My first real conversation with John Pople had taken place that week. It was undoubtedly the most important lunch of my career! Also, it was announced at the closing of the Congress that I had been awarded the 1991 Medal of the International Academy of Quantum Molecular Sciences, the body which overseas the triennial ICQC meetings.

The release of GAUSSIAN 92/DFT ushered in a new era of DFT calibration and development, and widely expanded the DFT research base. After 1991, density-functional theory entered the standard vocabulary of the quantum chemist. In 1993, I proposed an enhancement of gradient-corrected DFT which improved its accuracy and reliability somewhat further (J. Chem. Phys. 98, 5648 (1993)).

A variation of this enhancement known as B3LYP (LYP standing for Lee, Yang, Parr) has become a very popular computational tool, especially among organic chemists. The growing popularity of DFT has spawned related computational developments in linear scaling methodology, electromagnetic property calculations, and time dependence and excited states.

The impact of DFT in chemistry since 1991 was recognized by the Nobel Prize committee in 1998. The Nobel Prize in chemistry was awarded to Walter Kohn for the birth of the DFT formalism, and to John Pople for general methodological advances in computational chemistry, including the integration of DFT into Gaussian Hartree-Fock technology. Walter Kohn is a physicist, and it is significant that he was awarded a Nobel Prize by the chemistry community!

I am grateful to many people who have helped and inspired me in my research path; my graduate research supervisor, Donald Sprung, for granting me the freedom to find my own way, and Tom Ziegler for pulling my doctoral thesis out of the fire and for his foresight and encouragement in being the first to make practical applications of gradient-corrected DFT in chemistry. It is fitting that we were both awarded CSC Lecture Awards in the same year, 1994: Tom Ziegler the Alcan Award for inorganic chemistry, and I the Noranda Award for physical chemistry. I am very grateful to Russ Boyd and to Vedene Smith for the opportunities to carry out this work. I am thankful also to Ross Dickson for his assistance in creating the NUMOL program, and to research associates Ken Edgecombe, Jose Perez-Jorda, and Hartmut Schmider for assistance with various aspects of DFT over the years. In particular, Hartmut Schmider has been tireless in managing our growing thermochemical benchmarking efforts which now encompass hundreds of calculations.

Finally, grateful acknowledgments are due to NSERC, for continuous financial support since 1975 in the form of scholarship, fellowship, and grant funding. Small scale, independent research can survive only under the protective umbrella of broad and democratic funding such as NSERC has provided. It is a mistake to believe, as many do, that scientific progress can best be made by strategic funding of target-driven collaborative networks. Discoveries and innovations are serendipitous and unforeseen. I sincerely hope that NSERC will continue to nurture the explorations of our young and our independent Canadian university researchers.

QUEEN'S: TEACHING EXPERIENCES

In 1984 when I entered the department, the teaching environment differed considerably from that of today. This is particularly true for our first-year service courses. All of our lectures were delivered in our own chemistry lecture rooms. All first-year courses were divided into lecture sections of less than 120 students, so that FG15 could accommodate them. Lab sections were identical to the lecture sections and, in principle, each lecturer was individually responsible for conduct in their labs. Some would argue that this was an ideal situation, presuming that section sizes of 100 students allow a relatively personal and one-on-one touch. In

practice, however, I have found that connecting with a class of 100 students is no less difficult than connecting with a class of 300.

The teaching load implications were obviously enormous. Arts and Science and Applied Science lecture sections (not to forget "J Section" and the old CHEM 111 as well) typically totalled twelve or thirteen. In each term! Proliferation of upper-year and applied science courses added to the strain.

The only way to handle this large number of first-year sections was to employ postdoctoral fellows, research associates, or even external associates as teaching adjuncts. Also, many first-year instructors served "double duty"; lecturing in both fall and winter terms, or assuming responsibility for two sections in a given term, or additionally teaching J Section in the spring term. I have done all of these things in my years in the department and it was at times frustrating and demoralizing. Coordination of course content and creation of common exams across all sections was also problematic.

It could not, of course, continue through the ensuing years of budget and faculty cuts. Today, our first-year section sizes are considerably larger (except for CHEM 116) and the teaching load requirements significantly reduced. Unfortunately, our department's severely inadequate lecture facilities can no longer house our first-year students, but we had little choice. Lecture sections of 200 to 350 are typical. The lab component is now the responsibility of ONE faculty member, the Undergraduate Laboratory Coordinator (Michael Mombourquette). Tutorials conducted by TAs now alternate with the labs. Examinations are necessarily marked by TAs, or consist of multiple choice questions largely or even entirely. These are the simple realities of servicing some 1300 first-year students every year. And the numbers will continue to grow!

Many bemoan the changes that have taken place, but the first-year operation is without a doubt much more streamlined and better coordinated than ever before. Do I, however, long for the old days of blackboard lectures in FG15? In some respects, I do.

It is my opinion that the spontaneity and pace of an old-fashioned, low-tech blackboard lecture is preferable to the high-tech approach which we all feel pressure to adopt. I deliver ALL my lectures on blackboards whenever possible, liberally covering myself in chalkdust in the process, or with hand-drawn overhead transparencies if absolutely necessary.

Strong pressures to utilize more colourful, glamorous, computer delivery techniques abound, both from university colleagues and administrators and from society in general. I will resist. Students appreciate blackboard lectures and simple hand-written overheads: easier to follow, to take notes, and less distracting than overly high-tech approaches. Students today are bombarded with "information": thousand-page, multi-coloured, multi-

annotated textbooks, with ancillary materials such as study guides, CD ROMS, publishers web sites. etc. It is our job to simplify, to abstract the essentials, to find threads of logic and to "cut through the crap" (as a student once phrased it to me). Chemistry is a diverse and difficult subject, and we need to convince our students that it is beautiful and logical and unified, and not a collection of seemingly arbitrary definitions, rules, and recipes. Keep it simple.

There is also increasing pressure from educators to replace our customary "stand before the class and lecture" style by something more catalytic. We are told that our role should be to "facilitate" self learning, peer teaching, and interaction in the classroom. Nonsense. If we have no wisdom to impart to our students, no coherent stories to tell, then of what value are we? In my own days as a student, self learning, peer teaching, and interaction were called "homework"!

THE ENGINEERS

Administration of the Engineering Chemistry program has now shifted to Dupuis Hall. For me, the dichotomous nature of the department has always been a problem. The famous "school spirit" of our engineering students is, quite frankly, misplaced in the first-year classroom. It would be preferable not to separate Applied Science from Arts and Science students at all in first year (in my opinion), as is the case in some provinces.

The politics of teaching in the Applied Science Faculty is draining: constant course renumbering and repackaging at the first-year level, and duplication of courses to meet ever-changing CEAB requirements in upper years. Upper year course duplication has made heavy demands on teaching load. Even hiring policy has felt the pressures of the CEAB.

As a graduate of the Applied Science Faculty myself (pre CEAB), I find all of this to be unfortunate. Applied Science students are of excellent quality, and the loss of these students from courses such as Quantum Theory is regrettable. Quantum Theory is a subject ripe with logical and philosophical context, and applications of differential equations, linear algebra, and other applied mathematical tools. Applied Science students are well suited to study this kind of material. Yet, the Engineering Chemistry program now excludes such courses. Eng Chem students graduate from this university with no exposure to the fundamental laws that govern all electronic matter!

The role of a university is to EXPAND the minds of its students, not simply to train them. We are the discoverers, the keepers, and the teachers of fundamental and freely accessible knowledge. No other institution has this historical and vital mandate. As we enter the new millennium, let not

our economic and political leaders, nor our accreditors, nor we ourselves, forget this.

Greg R.J.Thatcher

1988-2002

It was May, the start of the heat-wave of 1988, that I arrived at Queen's as a new tenure-track appointment in Bioorganic Chemistry. It was gratifying to discover that there were more faculty and students than the handful that I had met at my interview the previous year. Indeed, there was even an unanticipated addition to faculty in the form of an MD/PhD Medicinal Chemist, with whom I shared a 1st year teaching lab during the important germinal stage of my research program. I seem to remember a senior Inorganic Chemist also kindly loaning a bench in his lab. Probably at the end of 1988 my research group, Stewart Campbell, Jean Davis, Ed Krol and I, moved into the newly renovated 3rd floor Gordon Hall research lab. Possibly due to Heritage Canada guidelines, the Victorian charm had been retained, nevertheless, the lab had been thoroughly cleaned – especially of glassware, shelves, anything not screwed down and, indeed, much that must have been screwed down if not riveted to the floor. The renovations to fumehood venting did require researchers to wear ear plugs in the lab, but fumes were indeed vented from the lab – and in summer were vented and revented, since all windows were kept wide open to provide air conditioning. Ironically, the annual migration of my research group about the many labs of Gordon Hall over the following decade has ended back at these same 3rd floor labs – now sound-proofed and air conditioned.

Idiosyncrasies of the labs aside, the Department at that time was lively. Strong research groups in the Baird, Buncel and Szarek labs contributed both intellectually and socially to departmental life, in particular the Szarek group, a cosmopolitan collection largely of Europeans, and the Baird group straight out of a Molson Canadian commercial. Some things change, but others .....

I was fortunate to have Stewart Campbell as my first graduate student. Stewart, now Director of Research at InsMed Pharmaceuticals in the US, introduced himself wearing Bermuda shorts and a Hawaian shirt. His colour blindness did not affect his outstanding research. In 1989, Dale Cameron and Dave Palmer, both Queen's grads, joined the group, followed by Ruby Nagelkerke, and then Felicity Buckell. Dave is now an Assistant Professor at the University of Saskatchewan, Ruby, Assistant Professor at Centralia College in Washington, USA and Dale, Director of Computational Chemistry at Boehringer Ingelheim, Laval. This group carried out excellent research and outstanding teaching, aided by very good summer students and undergraduate researchers, including Jenn Schmidtke, Julia Kubanek, Daphne Wahnon, Austin Chen, and Jodi Locke.

This first group of individuals created a marvelous atmosphere for science and invention. Perhaps the most memorable creations were the skits at the Chemistry Department Banquets. The high point of these, to many, was the demonstration of safety in chemistry labs led by Ruby, performed in the manner of airline hostesses demonstrating pre-flight safety and culminating in a senior professor proffering his middle finger to the players. There were also interactions with several distinguished Jones and McRae Lecturers, including giving Ron Breslow the finger – well, actually the whole forearm, a false one to allow him to demonstrate chirality more efficiently in future lectures. The frequent Chem Smokers, in-house Christmas Party, Chemistry Soccer team and Saturday 8am hockey maintained a tight sense of community, certainly with the students and postdocs. I personally contributed to these ventures, in particular, the championship winning soccer team, by getting sent off every other game, thereby increasing the team's chances of winning. Interprofessorial relationships were just as peaceful.

In 1988, on arrival at Queen's, it was revealed to me that Organic Chemistry was Evil and its practitioners were making black magic, if not actually in league with Lucifer. This pathological dislike was a revelation, possibly because at Oxford, from whence I had recently come, organic chemists had their own building and department, Dyson Perrins. Clearly, this had been not to aid research but isolate the bad apples. The reasons for this antipathy at Queen's seemed to be historical, including infamous blood feuds between senior faculty. Fortunately, only few of these archaic attitudes linger, although still for reasons that continue to escape me.

This first epoch at Queen's ended in 1994 when my first group of grad. students moved on. There were several key events in this period. The loss of Saul Wolfe to Simon Fraser University in 1991 and the arrival of two young, keen organic faculty in Bob Lemieux and Judy Bolton in 1994. Despite the short period of overlap, there are few colleagues in the department with whom I have had more provocative, scientific discussions than Saul. Although not welcoming Saul's departure, my research group attempted to benefit by quickly acquiring his lab and his battered leather sofa (affectionately termed the Chown Chair of Organic Chemistry). Unfortunately, (i) I was caught red-handed by the Headmaster whilst purloining the very valuable Chair, and (ii) Health & Safety spent 6 months in full chemical warfare gear clearing the left-over chemicals from Saul's cordoned-off lab.

The arrival of Bob and Judy began to change the dynamic in the department to one that was more open to change, or at least open to setting up a sub-committee to consider the concept of moving forward in a careful manner to a well-prepared and adequately discussed position document or recommendation pursuant to the concept of modification of the status quo.

In particular, Judy's presence began to create a critical mass in Biorganic/Pharmaceutical Chemistry. Progress in this direction lasted a brief few years, since when it was decided that Judy must put in a job application for the position as Queen's Assistant Professor, that she already held, she sensibly put in other job applications and moved to the University of Illinois. Some have compared this with a previous management decision, when a salary freeze was imposed by Queen's administration the year before a province-imposed salary freeze. Under new management, morale has improved considerably since those times.

Before moving to the second half of the nineties, some words should be said on teaching. My first experience of teaching at Queen's was to share delivery of an upper year course with Saul Wolfe. Remarkably, Saul made time to sit in and gently ask numerous questions at each of my lectures. At this time, courses at Queen's such as Saul's Chem 382 and Walter's 381 were highly personalized and challenging. Similarly, in Introductory Organic Chemistry, Chem 280, instructors could teach pretty much what took their fancy. I was given 2 pages of notes from George Hay for this full year course. A dozen students frequently visited me to ask questions, and I tailored the final exam to the ability of these students, using questions taken from 1st and 2nd yr exams at the University of Manchester. I subsequently discovered that most of these visitors were on the Dean's List: the final exam class average was below 30%. I was later made aware that the students who held me in their fondest thoughts used the term Thrasher Thatcher. This reputation has not diminished.

The second epoch is centred around a group of graduate students: Jenn Artz, Colin Ferguson, Dragos Vizitiu, Paul McCracken, Angela Kriste, Alison Borrajo, Yu Wang, Daphne Wahnon, Suzanne Iverson and Cristina Sanchez. This group also includes some other very influential researchers, including Dr Boris Gorin, Dr Kexin Yang and Caroline Walkinshaw. Dragos was the first of the Romanian dynasty, since followed by Dr Dan Scutaru, Dr Adina Dumitrascu, Elena Galen, Adrian Nicolescu, Vlad the Impaler and Jill Murray (a wannabe Romanian). Although their soccer skills were meagre, their creative contributions to research have been enormous. Boris's Russian dynasty has as yet only begat Dr Dergei Zavorin, but Sergei has been a right-hand man now for over 2 years. The last members of this second cohort are completing their theses. Colin and Jenn have outstanding postdoctoral research positions in the US. Boris and Dragos obtained excellent positions in industry, whilst Alison and Paul have successfully transitioned to business and management Perhaps the interests of these researchers have influenced the direction of research in the group, which is now firmly in Medicinal Chemistry and linked to industry, in particular GoBang Therapeutics Ltd, which was born from the efforts of many of these researchers.

In 1996, it was my pleasure to organize both the visit of Tony Kirby as the Jones Lecturer and the coincident Ontario-Quebec Minisymposium in Physical Organic Chemistry at Queen's. This occasion was made memorable, by honouring Erwin Buncel on the occasion of his 66th year (but not retirement), at a banquet dinner with much wine, merriment, Ossie Tee, and the sight of Erwin puckering up for a full minute, at head table, anticipating a congratulatory kiss from his wife Penny.

The Department is now under new management (or at least refurbished), in Stan Brown, with the support of the administration, who have belatedly realised the importance of Chemistry. New instrumentation, new infrastructure, and new faculty are early fulfilment of the promise that Queen's will not present such a conservative and immovable object to imagination and change as it has often proven in the past.

Donald F. Weaver

1989-2001

CHEM 280 AND THE PRACTICE OF MEDICINE: SOME PERSONAL REFLECTIONS

My perspectives concerning chemistry, in general, and the Department of Chemistry, Queen's University, in particular, are shaped by a variety of differing and definitely unique experiences. First, I am, to the best of my knowledge, the only person who has ever taken Chemistry 280 as a student and then (decades later) taught it as an instructor. I have seen this course with all of its strengths and blemishes from both sides of the lecture podium. Second, I am, to the best of my knowledge, the only practicing physician in Canada who is appointed to a Chemistry Department and who teaches undergraduate chemistry. This latter perspective provides insights which are exhilarating, stimulating, frightening and demoralizing. This unique combination of experiences permits the observation of certain common issues which occur both in the smaller world of Chemistry 280 and in the larger context of the interface between organic chemistry and clinical medicine.

Chemistry 280: Then and Now

Chemistry 280, or more succinctly Chem 280, was officially described as "An Introduction to Basic Organic Chemistry" in the Course Calendar which I used as an undergraduate chemistry major. I found Chemistry 280 to be interesting and rather exciting. It was my first exposure to actually synthesizing molecules with a purpose. It was my first insights into how biology worked at a molecular level and into how useful things like drug molecules could be made. Back in 1975, when I was taking Chem 280 from Dr. George Hay in Stirling Hall Auditorium C, we undergraduates referred to this course simply as "taking Organic"; now, 25 years later, I am teaching Chem 280 in Ellis Hall Auditorium A, and the undergraduates have shortened it even further to "doing Orgo". Over the intervening quarter century, I am sure that this course (and its instructors!) have been called many other names as well. Nevertheless, regardless of what it is called, Chem 280 is perhaps the most famous and infamous course on Queen's campus. For students majoring in chemistry, it is an essential part of their core education and training. For life science majors and biology majors however, it seems more like a rite of passage.

And for the hordes of people who wish to apply to medical school, it is a mandatory prerequisite. Few courses have provoked as many tears and invoked as many oaths as Chem 280.

Chem 280 is a fairly standard introductory organic chemistry course, similar to analogous courses offered at almost every other university in North America. In the autumn term, foundation concepts such as aromaticity, organic acids and bases, stereochemistry, and nucleophilicity, as well as the dreaded details of organic nomenclature, are taught. In the winter term, more advanced processes such as nucleophilic substitutions, elimination reactions, addition reactions, carbonyl-based chemistry and enolate-based processes are taught. That's the way is was in 1975; that's the way it is in 2000. As a student I studied from the Streitwieser and Heathcock textbook; as an instructor I teach from the Solomons and Fryhle textbook. Streitwieser and Heathcock was a rather plain black and white book, without colour pictures, 3D glasses, CD's or the other paraphernalia of a modern textbook. However, when you distill off the window dressings, both books are really much the same. They even use essentially identical examples for SN1, SN2, E1 and E2 reaction mechanisms. However, Solomons and Fryhle is 14% greater is weight and does cost 300% more than its simplistic predecessors.

These observations concerning the lack of change in Chem 280 over a quarter century are not a damning criticism - far from it. Addition and subtraction have not changed over the past 25 years, neither has the valency of the carbon atom - the fundamentals seldom change. Basic organic chemistry is basic organic chemistry, and it must be understood if one wishes to meaningfully understand biochemistry, pharmacology or indeed anything made from molecules (i.e. almost everything!). The importance of organic chemistry as a discipline with immense applicability remains unchanged over the course of time.

Although its content has remained essentially unchanged, the world of Chem 280 has changed over the past two and one half decades. Sure, there are still many extremely talented young students who are a challenge and delight to teach. However, what has changed is the attitude towards organic chemistry as exhibited by many of the students. The student of 2000 is much more likely to decry "Orgo" as useless or irrelevant. This is unfortunate; it is also wrong and misguided. The notion of "multidisciplinary" in the context of scientific research is a much more trumpeted and idealized concept than it ever was 25 years ago. Despite this, students focus earlier and with ever narrowing zones of interest. The shortcomings of this trend will become increasingly apparent in the medical sciences (as discussed below), especially with regard to the rational development of new therapies. People forget (or never knew) whom it is who discovers drugs. It is not the physician; it is not the pharmacologist; it is the chemist who discovers drugs. That is why Chem 280 was important 25 years ago, and is still important today.

Medicine and Chemistry: The Neglected Interface

In mid-January 2000 I was on-call for the general neurology service. In the emergency department I had a patient who had presented with out of control seizures. He had been seizuring continuously for 20 minutes and was in a state referred to as *status epilepticus*. I treated him intravenously with 2 mg of 7-chloro-5-(2-chlorophenyl)-1,3-dihydro-3-hydroxy-2H-1,4-benzodiazepin-2-one and 1300 mg of 5,5-diphenyl-2,4-imidazolidinedione. His seizures promptly stopped and within the hour he had regained consciousness and started to improve. Seventy-five years ago, he would have died. This man's life had been saved by the products of synthetic organic chemistry. I then left the emergency department and went to Ellis Hall Auditorium A to give a Chem 280 lecture. After the lecture, a student came up to the front of the lecture hall to speak with me. She claimed that all she wanted to be was a physician and that clearly "Orgo" was an irrelevant subject with no real value to a practicing physician. I tried (in vain) to convince her that organic chemistry was fundamental to the design of medical therapeutics.

Frequently, perhaps too frequently, the physician-patient interaction is terminated by the exchange of a prescription. A prescription is a document, a small piece of paper whereby the physician is telling you "I am giving you a molecule to interfere with your molecules and in doing so it will hopefully make you feel better". Medical based therapy is based on molecular level arguments, not on gross anatomical considerations. The drugs which have revolutionized human healthcare and the practice of medicine over the past century are the product of chemistry.

Queen's University has been offering courses in chemistry since the 1800's. Over the past 100 years, chemistry has progressed significantly. The introduction of quantum theory has immensely facilitated the theoretical understanding of atomic structure. Spectroscopic techniques such as nuclear magnetic resonance have advanced structural determination. Analytical techniques, like mass spectrometry, permit analyte identification at incredible detection limits. Over the same time period, medicine has likewise advanced tremendously. Leeches and therapeutic bleedings have mercifully been replaced with antibiotics and an entire armamentarium of receptor specific drugs. These parallel advances in chemistry and medicine have resulted in new drug therapies.

If medicine is to confront the continuing health challenges facing humankind, chemistry must be applied. The old surgical dictum that one should "heal through cold steel", although quaint, is definitely antiquated. The future of medical therapy lies at the level of the molecule, not at the rather crude level of a scalpel.

The major health issues facing us at this the dawn of a new Millennium are many and include AIDS, drug resistant malaria, prion diseases (e.g. Bovine Spongiform Encephalopathy), Alzheimer's Disease and killer viruses. These problems have the capacity to inflict human suffering on an epic scale.

They are not going to be solved solely at the biological level; molecules must be designed and synthesized - the attack must be at the molecular level. More than ever in the past century, the role of chemistry as a central science is crucial. Is organic chemistry useful - yes, it is! Is Chem 280 relevant to the biological sciences - damn right it is!

Support staff

No account of the history of the chemistry department, not even an informal one like this one, would be adequate without mentioning the support staff. In this collection of reminiscences and recollections, the faculty have given personal reminiscences of the department, but the technicians and secretaries (who probably know us better than we know ourselves) will probably recognize gaps in the record, and little distortions in some of the stories.

What is certain is that the support staff have played a prominent role in the activities of the department since its earliest days. Some of the technicians have spent most of their effort in dealing with undergraduate teaching, while others have made their contributions in the research area by building equipment and performing experiments. Whether dealing with graduate students or undergraduates, they have generated many memories for the students who worked with them or in their laboratories. The practical skills needed to bring an experiment to completion are often learned by watching and emulating trained and experienced workers.

To list all the people who have worked as technicians is not possible at this stage, but some of them should be mentioned because they are so well known. Older graduates and colleagues will remember Paddy Doolan and Norm Hyland. Greg Richmond saw more than a generation of first year students pass though his first year laboratory. Greg was preceded by David Groves, who later worked in the NMR laboratory. Upper year students met Scott Meskis, Art Hutchinson, Len Rose, Ken Sayers, Tom Hunter, Ted Ison and Pat Mulligan. Recently Tom, Ted, Scott, Pat and Len were awarded a Queen's Staff Appreciation Award in recognition of their outstanding work.

Anyone who had equipment made in the workshops came to appreciate the work of Fred Everding, Wilf Coombes, Fred Sziladi, Terry Blake and Dennis Hannah. Glass blowers Ron Collins, Karl Levay and Bob Campbell have been indispensible to many research groups. Frank Wilde provided graphics services that almost everybody needed in the period before we all acquired the computers and printers used nowadays to produce high quality drawings. Sue Blake worked in administering and operating undergraduate laboratories, and in the NMR laboratory. In the electronic shop, Bill Green, Larry Walker, Rick Boswell, and Robin Roberts helped with building and maintaining electronics, and with the introduction of computing. Alf Bialek and David Dunton kept the stores and accounts over a very long period.

In the late 1960's, Pip Nation joined the department as Administrative Assistant, and was succeeded by Neil Crowe and then Rick Boswell, who was appointed as Department Manager. These people played the key role in maintaining the day to day operations of the department.

Among the head secretaries we recall Mrs Jardine, Bonnie Lawrie, Betty Macintosh, and Laurie Rushworth. Others who worked in or around the office were Dorothy Sudds, Marilyn Oosten, and Sharon Lummis, but there are many others.

The successes of the department would not have been possible without the contributions of all these people.

The Chemistry Department
October 2002

Support Staff

Bandy-Dafoe, Pamela
Department Coordinator

Boswell, Richard
Department Manager

Campbell, Robert
Glassblower

Hansen, Eric
Network Administrator

Hunter, Thomas (Tom)
Undergraduate Laboratory Technician

Ison, Theodore (Ted)
Undergraduate Laboratory Technician

Keyes, Annette
Graduate Secretary

Maracle, Edward (Ed)
Electronics Technologist

Roberts, Robin
Electronics Shop Lead Hand

Rose, Lenard (Len)
Undergraduate Laboratory Technician

Rushworth, Laurie
Administrative Secretary

Sauriol, Francoise
Instrumentation Manager

Sullivan, Diane
Secretary

Umanetz, Kathleen
Undergraduate Assistant

Wang, Ruiyao
Departmental Post Doctoral Fellow

Academic Staff

Baird, Michael.C - Professor
Initially appointed in 1967
B.Sc. (McMaster) M.A., PhD. (Toronto)

Beauchemin, Diane - Associate Professor, Chair Undergraduate Studies
Initially appointed in 1988
B.Sc., PhD. (Montreal)

Becke, A. - Professor
Initially appointed in 1984
B.Sc. (Queen's) M.Sc., Ph.D. (McMaster)

Brown, R. Stanley - Professor
Initially appointed in 1995
B.Sc. (Alberta) M.Sc., Ph.D. (California at San Diego)

Brown, R. Stephen - Assistant Professor
Initially appointed in 1996
B.Sc. (Dalhousie) M.Sc., PhD. (Toronto)

Cann, Natalie M - Assistant Professor
Initially appointed in 1997
B.Sc. (New Brunswick) Ph.D. (Dalhousie)

Crudden, Cathleen - Associate Professor
Initially appointed in 2002
B.Sc., M.Sc. (Toronto) Ph.D. (Ottawa)

Fortier, Suzanne - Professor
Initially appointed in 1982
B.Sc., Ph.D. (McGill)

Hesp, Simon A.M. - Associate Professor
Initially appointed in 1991
B.Sc. (Amsterdam) M.Sc., Ph.D. (Toronto) P.Eng.

Horton, J. Hugh - Assistant Professor
Initially appointed in 1997
B.Sc. (York) Ph.D. (Cambridge)

Jerkiewicz, Gregory - Associate Professor
Initially appointed in 2002
M.Sc., M.Eng. (Poland) Ph.D. (Ottawa)

Kozin, Igor - Assistant Professor
Initially appointed in 2002
M.Sc., Ph.D. (Moscow)

Lemieux, Robert P. - Associate Professor, Chair Graduate Studies
Initially appointed in 1992
BA (Colgate U.) Ph.D. (Univ. of Illinois)

Loock, Hans-Peter - Assistant Professor
Initially appointed in 1999
Dipl.-Ing (Technical University of Darmstadt) Ph.D. (Victoria)

Macartney, Donal H. - Professor
Initially appointed in 1983
B.Sc., Ph.D. (Victoria)

Mombourquette, Michael - Assistant Professor
(Undergraduate Laboratory Co-ordinator)
Initially appointed in 1996
B.Sc. M.Sc. Ph.D. (Saskatchewan)

Oleschuk, Richard - Assistant Professor
Initially appointed in 2000
B.Sc., Ph.D. (Manitoba)

Slebocka-Tilk, Henryka - Assistant Professor
(Organic Laboratory Co-ordinator)
Initially appointed in 2002
M. Sc. (Poland) Ph.D. (Poland)

Snieckus, Victor A. - Professor
Initially appointed in 1998
B.Sc. (Alberta) M.Sc. (Berkeley, California) Ph.D. (Oregon)

Szarek, Walter A. - Professor
Initially appointed in 1967
B.Sc., M.Sc. (McMaster) Ph.D. (Queen's)

Thatcher, Gregory R.J. - Associate Professor
Initially appointed in 1988
B.Sc. (Manchester) Ph.D. (Toronto)

vanLoon, Gary W. - Professor
Initially appointed in 1969
B.Sc. (McMaster) Ph.D. (Toronto)

Wang, Suning - Professor
Initially appointed in 1996
B.Sc. (Jilin, PRC) Ph.D. (Yale)

Wardlaw, David M. - Professor (Head of Dept.)
Initially appointed in 1984
B.Sc., Ph.D. (Toronto)

Whitney, Ralph A. - Associate Professor
Initially appointed in 1978
B.Sc. (UBC) Ph.D. (Cantab.) C.Chem

Wu, Gang - Assistant Professor
Initially appointed in 1997
B.Sc. (Peking University) Ph.D. (Dalhousie)

Adjuncts

Cunningham, Michael - Cross Appointed Assistant Professor,
Chemical Engineering
Initially appointed in 2000
B.Sc., M.Sc. (Queen's) Ph.D. (Waterloo)

Hintelmann, Holger - Adjunct Assistant Professor, Trent University
Initially appointed in 1998
M.Sc., Ph.D. (University of Hamburg, Germany)

Hu, N-X - Adjunct Associate Professor
Initially appointed in 1999
B.S. (East China Institute of Chemical Technology), M.S., Ph.D.
(Hiroshima University)

Kazmaier, P.M. - Adjunct Professor, Xerox
Initially appointed in 1994
B.Sc. (Calgary) Ph.D. (Queen's)

Lewars, Errol - Adjunct Professor, Trent University
Initially appointed in 1998
B.Sc. (London) Ph.D. (Toronto)

Maly, Ken - Assistant Professor
Initially appointed in 2002
B.Sc., Ph.D. (Queen's)

Newstead, Bill - Lecturer
Initially appointed 2000
B.Sc., M.Sc. (Queen's)

Parent, Scott - Cross Appointed Assistant Professor,
Chemical Engineering
Initially appointed in 2000
B.Sc. (Queen's), M.Sc. (Calgary), Ph.D. (Waterloo), P.Eng

Parnis, J. Mark - Adjunct Associate Professor, Trent University
Initially appointed in 1991
B.Sc., Ph.D. (Toronto)

Rafferty, Steven - Adjunct Assistant Professor, Trent University
Initially appointed in 1999
B.Sc. (Waterloo), Ph.D. (UBC)

Rayner, David - Adjunct Assistant Professor, NRC
Initially appointed in 1999
B.Sc. (Waterloo), Ph.D. (UBC)

Reimer, Kenneth - Professor
Initially appointed 2001
M.Sc. (Calgary), Ph.D. (Western)

Rochon, Paul - Adjunct Professor, RMC
Initially appointed in 1992
B.Sc., Ph.D. (Ottawa)

Seideman, T - Cross Appointed Assistant Professor, NRC
Initially appointed in 1996
B.Sc. (Tel-Aviv), M.Sc., Ph.D. (Weizmann Institute of Science)

Stolow, Albert - Cross Appointed Professor, NRC
Initially appointed in 1996
B.Sc. (Queen's) Ph.D. University of Toronto

Svishchev, Igor - Adjunct Assistant Professor, Trent University
Initially appointed in 1998
M.Sc. (Moscow), Ph.D. (USSR Academy of Sciences)

van Gelder, Nico - Professor
Initially appointed 2002
B.Sc. (McGill) Ph.D. (McGill)

Emeritus

Baker, Warren E. - Emeritus Professor
Initially appointed in 1985
B.A. Sc. (Toronto) S.M., Sc.D. (MIT) P.Eng.

Brown, R. Julian C. - Emeritus Professor
Initially appointed in 1962
B.Sc., M.Sc. (Sydney) Ph.D. (Illinois)

Buncel, Erwin - Emeritus Professor
Initially appointed in 1962
B.Sc., Ph.D. D.Sc. (London)

Colpa, Johannes P. - Emeritus Professor
Initially appointed in 1969
Ph.D. (Amsterdam)

Gordon, Robert D. - Emeritus Professor
Initially appointed in 1966
B.Sc., M.Sc. (McMaster) PhD. (University College)

Hunter, Brian K - Emeritus Professor
Initially appointed in 1973
B.Sc., M.Sc. (UBC) Ph.D. (Western)

McCowan, James D. - Professor
Initially appointed in 1967
B.A., Ph.D. (Toronto) Ph.D. (Cantab.) P.Eng.

Norris, Albert R. - Emeritus Professor
Initially appointed in 1964
B.E., M.Sc. (Saskatchewan) Ph.D. (Chicago)

Page, John A. - Emeritus Professor
Initially appointed in 1968
B.Sc. M.Sc. (McMaster) Ph.D. (Harvard)

Russell, Kenneth - Emeritus Professor
Initially appointed in 1954
M.A. Ph.D. (Cantab)

Shurvell, H.F. (Gus) - Emeritus Professor
Initially appointed in 1965
B.Sc. (Exeter) M.Sc., Ph.D. (UBC) D. Sc. Exeter)

Smith, Vedene H., Jr. - Emeritus Professor
Initially appointed in 1967
B.A. (Emory) M.S., Ph.D. (Georgia Tech.)

Stone, John A. - Emeritus Professor
Initially appointed in 1965
B.Sc., Ph.D. (London)

Wan, Jeffrey K.S. - Emeritus Professor
Initially appointed in 1966
B.Sc. (McGill) Ph.D. (Alberta)

Wheeler, Robert C. - Emeritus Professor
Initially appointed in 1956
B.Sc., M.Sc. (Queen's), Ph.D. (Cambridge)