From Coronation Street to a Consummate Chemist

Brian Halton

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The Oxford Thesaurus defines ***consummate*** (adjective) as ultimate or best but it is used here only as the single embolden synonym implies it to be:

Consummate: able, absolute, accomplished, ***complete***, conspicuous, downright, faultless, finished, flawless, gifted, ideal, impeccable, inimitable, matchless, out-and-out, peerless, perfect, perfected, polished, positive, practiced, ripe, skilled, superb, superlative, supreme, talented, thoroughgoing, total, trained, transcendent, unmitigated, unqualified, unsurpassable, utter, virtuosic, whole.

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Clarity arisis in the spaces in between

From *The Fifth Woman* by Henning Mankell published by Harvill Press
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About the covers

The painting that adorns the front cover is the 1901 watercolour entitled *Victoria College: First Chemistry Laboratory, 1901*, painted by Sybil Johnson. It was donated to the Victoria University of Wellington Art Collection in 1949 by Lady Easterfield, wife of the first Professor of Chemistry and hangs in Laby 101, the Level 1 Office of the School of Chemical and Physical Sciences in the Laby Building. The laboratory was not on the Kelburn campus but housed in an upstairs room of the then Wellington Technical School in Victoria Street.

The photograph on the back cover is of the author at age 70 years, taken by Sean Craig in 2011.

Foreword

Brian Halton's autobiography tells the story of one of New Zealand's most distinguished organic chemists, from his birth and early life in Lancashire, his university education at Southampton, his migration to New Zealand in 1968, and his subsequent life as an international research chemist and academic from his base at Victoria University of Wellington. The book is full of contemporary detail and illustrated with interesting pictures and diagrams. Professor Halton tells of the intricate interplay of events and interactions which led him through various waypoints and junctions on his remarkable professional career. Many other characters appear, individuals who will be known to many of Professor Halton's professional colleagues. This adds to the book's interest. For those who seek to understand what makes some scientists 'tick', I commend this book as a fascinating, highly personalised account of one man's life journey.

Professor Sir Paul Callaghan GZNM FRS FRSNZ

Preface

Many biographies and autobiographies of famous chemists have been published over the years, perhaps none better known than those from the American Chemical Society. So, one may ask: 'Why one from a not so famous (infamous?) chemist?' The reason is simple enough. On his retirement from Monash University in Melbourne in 1996, Roger Brown encouraged retired and retiring chemists to provide an account of their careers. As few such documents exist in New Zealand, and as my life has been one immersed in chemistry, I can see no better way to move my career closer to its end than by recalling those things that have served me so well for so many years. In so doing, my more than 40 years at Victoria University of Wellington form the essence of what follows.

This book is for all those colleagues around the world, named or not, who have influenced my life, my research and my being, for the students who worked with me, and for those whose horizons have been expanded because of me; they have all made my life in chemistry so much the better. Whatever has been achieved has been made possible only because of the love and support of my family. This book is dedicated to them.

I am particularly grateful to my wife, Margaret, for proof reading the entire manuscript through various stages of production and Rebecca Hurrell, the managing Editor of *Chemistry in New Zealand*, who has transposed the text and its illustrations into the book form that Canterbury Educational Printing Services have so professionally produced. Dr. Joanne Harvey (Victoria University of Wellington) has proof read most of the text in her typically astute and attentive manner and I am especially grateful to her for her perceptive comments. The School of Chemical and Physical Sciences at Victoria University of Wellington has been particularly supportive during the time from the concept of the book to its ultimate appearance and the New Zealand Institute of Chemistry has assisted with the production. All remaining errors are the sole responsibility of the author.

My appreciation goes to John Spencer (Head of School) and my colleagues in Chemical and Physical Sciences at Victoria University of Wellington for their patience, tolerance and good humour over many years.

BH
Wellington, August 2011

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The Early Years

Lancashire and London

Mary (Halton) Spencer 1912-1996

John Henry Halton 1906-1965

Life for me began at about 6 am on Sunday the 9th of March in 1941 at a maternity home in the Lancastrian town of Accrington (Lancashire) in northwest England, the first and only child of Mary (May) and John Henry Halton. After what was in those war days considered an appropriate time, I was taken home to 13 Coronation Street in Great Harwood, a nearby cotton town then of some 8000 people. Coronation Street remains small and terraced, and is located close to the top left of the aerial photograph (above what became the Oxo Factory in 1938).[1] It was an unmade road, then paved with cobblestones and ultimately surfaced conventionally in the 1950s.

March 9 has few noted events save for the birth of Mickey Spillane (1918-2006), the US author who wrote the Mike Hammer detective novels, and Yuri Gagarin (1934-1968). Michael Dewar reported[2] solidifying liquid air to the Royal Society on this day in 1893, and the Barbie doll was launched on it in 1959 when I was 18!

The town of Great Harwood lies within sight of Pendle Hill and easy reach of the Ribble Valley, but it has always been just off the main road and, like many

Left: Outside 13 Coronation St, spring 1941. *Centre*: With gate in 1997. An uncle and aunt lived at No. 17 (satellite dish) from 1948 to 1995. *Right*: Gt. Harwood from the Air.

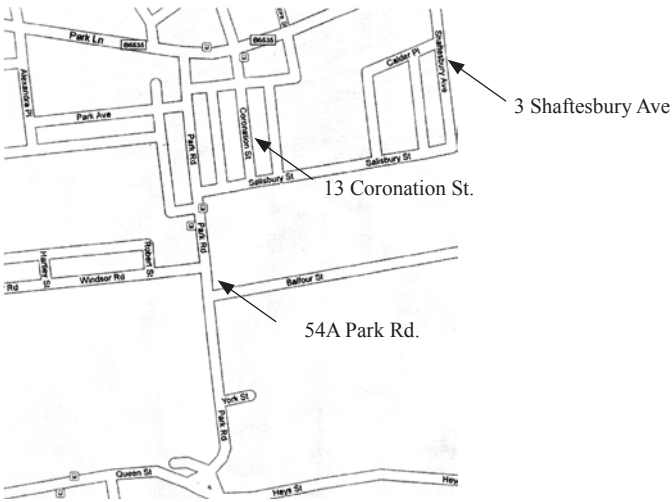
small towns or large villages, it is quite unremarkable. Indeed, it is not on a major road, and one does not pass through it on the way to somewhere else. Its only buildings of significance are the Town Hall and Mercer Memorial Clock, and the Mercer Hall (now the public recreation centre). It has had many distinguished sons and daughters though not many who are famous.[1]

Gt. Harwood (*left*): Clock Tower and Town Hall; (*right*): Mercer Leisure Centre (photos kindly provided Ian Fairclough).

The most renowned citizen was John Mercer who, in 1844, devised a process to cause cotton fibres to swell upon treatment with sodium hydroxide. In the original version of his process the fibres swelled and caused the overall size of the fabric to shrink, making it stronger and easier to dye. The process became popular after H. A. Lowe improved it in 1890 by holding the cotton during treatment to prevent it from shrinking. On doing this, the fibre gains a lustrous appearance and represents the modern form of the process. The self-taught Mercer became famous and wealthy; he excelled in the dye industry working in the Oakenshaw Calico Print Works (where he became a partner) 'dyeing cloth with certain colours and figures on a ground of a different hue'. The factory was located in what is now Clayton-le-Moors and close to the Hyndburn river. His development of chemistry and chemical processes became widely recognised and he became one of the founding members of the Chemical Society (1842), joined its Council in 1860, and remains

the only 'Harroder' to be elected to the Royal Society (1852) upon nomination by James Thomson, Thomas Graham, Walter Crum and Lord Playfair.

And so I was raised for my first years at No. 13 Coronation Street. My mother, a nurse of the mentally handicapped (the terminology of those days), remained at home as was the custom, while my father worked as a male nurse in Calderstones. This was a mental institution that had been converted into a wartime children's hospital, but the children were evacuated shortly before the troops came back from Dunkirk. It is set in lovely country near Whalley with beautifully kept grounds, and it lies in the Ribble Valley some three miles from what was our home. It still exists, but is now a UK National Trust Home providing specialist services to people with a learning disability. I do not know when Dad became a male nurse as, during the 'great depression', he had been a full-time musician (saxophone and clarinets) touring Ireland as a member of the Elite Orchestra – a dance band comprising his elder brothers (Bob and Eddie) and two friends – and they still did Saturday dances from time to time. I recall being on-stage with the band in the Mercer Hall for a post-war dance, probably during the 1945-1946 winter and I have always regretted not taking up drums, likely from the impact of that single event!



My part of Gt. Harwood. My maternal grandparents lived at 3 Shaftesbury Ave., Uncle Ted and Aunt Mary at 17 Coronation St. (from 1948) and Uncle Bob and Aunt Alice on Park Ave. (*2nd from top left*) until 1950.

Apparently I was a sickly child and caused my parents much concern as medications and food were not easy to come by with the rationing system then in place. My earliest memory of those war-time years is of my father arriving home from work at about 8 o'clock in the evening having cycled or taken a bus. Our next door neighbour at No. 11 was the local coalman, and I remember him, and the milkman

from the nearby farm, with their horse-drawn carts making deliveries in the street that was, at the time, merely an unmade road. He, with his half-leather jacket, taking hundred weight sacks off the cart onto his back to carry them to the outdoor coal-hole, and the milkman transferring gill, pint and quart measures from the churn into the jugs and pans left for them; pasteurization was yet to come. While I do not have any memory of the barrage balloons that floated above the nearby recreation ground or the last day of the war, I do remember the celebrations that ensued and straddling lower Queen Street (see bottom of map) with many other families, presumably well intoxicated, as we returned from the festivity on that auspicious night. Being on the northern edge of the Lancashire cotton towns, Gt. Harwood escaped damage as the German bombers concentrated on Manchester and Liverpool; the closest they got was with an unspent payload dropped near Whalley to speed the return home.

I do not know precisely when it was that my parents told me that Dad was to leave Calderstones and that we were moving to the fish and chip shop at 54A Park Road (less than a hundred yards down the next street over as can be seen from the map), but it was 1945. I do remember the horse and cart (from the coalman) in the back street (that not labelled between Park Rd. and Coronation St.) and all our belongings being loaded on, and then making our way down to the rear entrance of 54A. The row of houses of which this was a part was demolished in the late 1980s but in those days the back street crossed Salisbury and ran down to Balfour St. The chip shop was located opposite the recreation ground just beyond the junction with Windsor Rd., and about three houses above Balfour St. We remained there for four years.

Over the next few years (1946-48) I went through a phase of contracting all the childhood diseases (measles, German measles, whooping cough, diphtheria). These kept me away from St. Hubert's Primary School, where I had started when I was 5 years old, and then St Wulstan's to which we all transferred at age 7. The culmination of these events was a sizeable swelling on my left neck that, ultimately, proved to be a result of bovine tuberculosis contracted from contaminated milk. At that time it was a rare condition in humans and no simple remedy was available in NW England. Eventually, my parents located a specialist practising at Great Ormond Street Children's Hospital in London who was able to excise the abscess from a small incision. Surgery was performed on the 15th of November 1948, the date remembered because Prince Charles was born the evening before and the street outside my ward window was decorated and the passing trams were decked in flags.

Left ca. 1942; *Right, with father*, 1948

Recuperation was slow and I was required to spend time in the country air. I was absent from school for most of 1948 and 1949 and spent the summer months of ’49 with my father’s eldest sister, my Aunt Mary and Uncle Tom (Aspey) at their home ‘Key Hills’ at Martin Top, near the small village of Rimington, some 20 km northeast of Gt. Harwood in glorious countryside.

Left: Key Hills (1997) – largely unchanged from 1949. *Right*: Stonyhurst PO *ca*. 1953 (*Copyright: The Francis Frith Collection* – reproduced with permission).

In the autumn of 1949 we moved from Gt. Harwood to Stonyhurst whose fame lies in its Jesuit public school which, at the time, comprised three colleges for boys and young men – Hodder House, St. Mary’s Hall and Stonyhurst College – located close to each other in the delightful countryside bordering the Hodder River in the Ribble Valley. My father became the Stonyhurst sub-postmaster, the office housed within our small local store in the hamlet of Woodfields; we stayed there until the summer of 1956. Having lost almost two years of schooling through illness, I did not take to full time school easily, but the two staff of St. Joseph’s Primary School in the nearby Hurst Green, Miss Hall (junior) and Miss Burns (senior), sorted me out and I passed the all important 11^{+} exam in 1952. My time at Woodfields remains very special and I retain fond memories of the place and the people.

Passage through the 11^{+} headed me to grammar school in September 1952. However, Stonyhurst was some 25 km from Preston and the nearest grammar school, a journey of 70 minutes involving a 10 minute walk to the bus stop, 45 minutes on the bus and then 15 minutes from the bus terminal to the college, all to be repeated for the trip home. Instead, I entered St. Joseph’s College, Blackpool, in early September 1952 as one of about eight new Form II boarders. It was a school run by the Irish Christian Brothers and attended by a good number of the local grammar boys. The excessive disciplinary demands, whims and fancies of the Brothers were hard to adapt to and life was completely regimented. Being a Catholic institution run by a religious order, Mass was held daily, but we only had to attend on five weekdays as Wednesday and Saturday were designated lie-in days, and Sunday Mass was held at the late hour of 8.30 am. Typical of the grammar schools of the day, classes for 11^{+} entrants started in Form II and involved

English Language, English Literature, Maths, Science, French, Geography and History, with Religious Studies from noon to 12.30 pm daily and Music once or twice per week. Wednesday afternoon was sports (the boarders had sport every day after school from 4-5 pm) and, to make up the teaching time, school was held from 9-12 am on Saturdays.

Age 14 signified entry into the Lower Fifth form and choices had to be made as the syllabus split into arts and sciences. I had little capacity for languages even though I enjoyed my year of Latin, and so the choice was easy – it had to be the sciences. And so my journey into Chemistry, Physics and Mathematics at a more serious level began. We retained French, selected between Geography and History, and were required to take both English Language and English Literature. This gave us each seven subjects, plus the mandatory Religious Knowledge, for the General Certificate of Education Ordinary Level programme that culminated with external examinations in the Upper Fifth form. The end of the 1955-1956 year marked our departure from the Post Office to begin a life in southeast London where my parents subsequently purchased a larger (but still 'corner') grocery store in Lee SE12 with a separate family home at 31 Heather Road, some 50 metres down the street.

I returned to Blackpool in September 1956 for the Upper Fifth year and the 'O' levels, the essential school leaving examinations for grammar schools of that era. As mid-term breaks were but a Friday and Monday, there was no opportunity to return to London until the Christmas holidays. The prospect of spending these breaks at school had no appeal and I was fortunate to have my father's eldest brother and his wife (Uncle Bob and Aunt Alice) invite me to stay with them in Gt. Harwood whenever I wanted to. They owned a lady's and children's outfitters on Queen Street, near the Town Gate and Clock and I spent every short break with them until I left Blackpool. I always felt sorry for those boarders whose parents lived abroad and who had no choice but to stay at school through the mid-term recesses.

The 'O' levels came and went and I travelled south to London for the summer holiday, worked in the shop, and awaited the results. I still have the card on which I had been required to enter my name and the subjects I had taken, with the results added by hand by the then Headmaster (a Bro. Dolan) and mailed to me. I passed six of the seven subjects, missing out on English Language by a few percentage points. I elected to return to SJC despite my parents asking if I would prefer to attend a day school near home. Quite why I did this I will never know, but return I did and entered the Lower Sixth taking Chemistry, Physics and Mathematics for Advanced (A) Level GCE in the new Science Block that by then had been completed. The chemistry programme was taught by Bro. Cornelius Sreenan and I think it was his knowledge and ability to put across the great logic of organic chemistry that directed me into my career in the subject. The precision

of functional groups and their ability to provide logical derivatives in a predictive manner was enough to convince me; Physics and Maths rounded things off, but I had markedly less interest in them. The demands of boarding school life and the continual removal of privileges from my peer group during that year persuaded me that there had to be a better way to gain an education and my departure from SJC in July 1958 was permanent, although at the time no one knew it.

Despite the adverse publicity the Irish Brothers attracted in the late 20th century, the demise of the college was due solely to mismanagement. Bad decisions in construction and the wish of every headmaster to have an edifice created during his tenure led to a loss of funding and its closure.[3] An Old Boys Society exists and there is a web site containing reminiscences of former pupils.[3]

When I arrived in London for the summer holiday, my parents had arranged for me to attend St. Joseph's Academy in Blackheath, London SE3, subject to a satisfactory interview with the Headmaster, a Brother Vincent. At that time, the Academy was the most prestigious Catholic grammar school in South London and it saw a high proportion of senior pupils move on to university with good 'A' level qualifications. I was accepted and began an entirely new lifestyle in the autumn of 1958.

What my time at SJC taught and instilled into me more than anything was the discipline of study and a capacity for work. This came from the routine of set study hours, limited free time and few distractions – the boarders did have a 12 inch (25 cm) TV that the seniors could watch from time to time. I was never a star in the classroom or in the examinations for it was the day boys from the professional families that dominated, but nor was I a dunce.

Bro. Austin 2002
(courtesy Tony Hyman)

Not only was my boarding school background beneficial as I started life at the 'Academy' but it has served me well throughout life. The initial stages of Upper Sixth were far from easy. Maths in the north was a single subject comprising mainly 'pure' components, whereas the Academy had distinct 'Pure' and 'Applied' programmes to fit the London GCE courses. There, 'A' levels comprised four rather than the three subjects I had taken previously, and catching up on a year of missed tuition with the prospects of calculating the friction generated by a box sliding down a slope inclined at 20° did not impress; I had great difficulty in assimilating the missed work of 'Applied'. However, to my great delight, chemistry and physics were taught by knowledgeable enthusiasts, Brother Austin (Casson) and Mr. (Pussy) Catt, respectively.

Although the labs were old by comparison with the new SJC facility, there was the real advantage in the London County Council scheme that had expensive (by 1958-59 standards) laboratory equipment rotated through the schools on a loan basis. Life was a ball! We even had tickets to the Royal Institution Christmas Lectures. These were instigated by Sir Lawrence Bragg and seeing and hearing that Fullerian Professor of Chemistry, who at age 25 had received a Nobel Prize, remains a highlight of my life; he was a master showman as well as a brilliant scientist.

The year at the Academy went well and I achieved good 'A'-level passes in all but 'Applied' for which I passed 'O'-level. Despite a second attempt the following November, I never made it to the echelons of such (to me) mysterious mathematical manipulations. At that time, the examination periods consisted of a three-hour laboratory practical in each of Chemistry and Physics and two three-hour written papers in all subjects. Prior to the summer break and the examination period, it had been suggested to me that I should return for a 7th-form year and take the Scholarship exams. This I did. Perhaps it was this extra year at school that provided a needed catch-up for me, for I have never regretted it. Kevin Prendergast, a local lad, and I were the only two taking science scholarship, both of us with chemistry and physics. We had lots of free periods and were encouraged to set up project work and write essays on all manner of topics using not simply the school facilities but also those of the neighbourhood and city. We were both prefects (there had been a suggestion that I be Head Boy but my tenure in the school was too short for the proposal to be taken seriously) and we had to make sure that we were on time when we were on duty, as 'late comers' were subject to detention. We attended plays in the West End, concerts at the Festival Hall and, of course, a second Royal Institution Christmas lecture was not to be missed. It was some thirty years later that I chaired the first ever *live-via-satellite* science lecture from the UK to NZ by Prof. (now Sir) John Cadogan, then President of the Royal Society of Chemistry, from that same lecture theatre late on a January evening. My audience was in the Massey University Marsden lecture theatre (Palmerston North) for the post-lunch plenary lecture of the 1987 ANZASS Congress held that January. Prof. Dick Batt was Congress Chairman.[4]

Once again the year passed quickly and by the time the formal examinations came around I had secured an unconditional offer from Southampton University. Initially a college of London University, it had been granted its own charter in 1952 and was typical of the 'red brick' universities of the time. I had by then persuaded my parents that to gain the full benefit of a university education I really needed to be an integral part of it and that this could only be done by living away from home. I had applied to other institutions and (to keep faith at home) London's University (UC), Imperial (IC), and Kings Colleges; I did not consider myself Oxbridge ma-

terial. Southampton was the first to invite me to interview and it was conducted by Dr. Kenneth Webb, a delightful inorganic chemist who spent his academic career at Southampton, initially when it was a university college, through its formative years, and beyond. Before we were done he was telling me what I should and should not do when I came down; the formal and unconditional offer came soon afterwards. In the event, the outcome of the scholarship year (A-levels plus one additional three-hour written paper for each scholarship subject) was passes in both subjects and the offer of a State Scholarship to both Kevin and me (which I gather were limited to a total of 500 nationally): a first for the Academy. Although the financial reward was formally no different from the London County Council scholarship, there were advantages of kudos and in the parental income test being more lenient, with the cost of travel to and from the university also met. Several of the colleges that had not previously accepted me wrote offering a place based upon the 'State' but, given the confidence Ken Webb and Southampton showed in me, I stayed with them, a decision that I have never regretted.

The summer months were spent as a laboratory assistant in the Haematology Department of Dulwich Hospital, a part of the King's College Hospital Group. This provided additional workplace and laboratory experience beyond that of the shop, cafe and departmental store gained during other school holidays. At the end of the first week of October 1960, I went down to Southampton to enter university as a freshman.

University Years – Southampton

Undergraduate Days

October 1960 saw me depart from Lee and move to Southampton where I had secured lodgings with Mrs. Pritchard and her son David at 4 Bankside, Wessex Lane, in the suburb of Swaythling. This was a distance of some 2 km from the University. I remained with them for two years. At that time, entry into a University Hall was not common for first-year men and only one of my schoolmates, classics student Rick Collett, who had been the Academy Head Boy, gained such entry. He too was at Southampton and a resident of Connaught Hall, a little closer than Bankside to campus down Wessex Lane. I spent many happy hours there as Rick was a competent guitarist and had a good voice; it was my own 'Hall of Choice' and I spent my final undergraduate year as a resident there. Kevin Prendergast, my friend and co-scholar from the Academy elected to attend Liverpool University and subsequently moved to a career with Coates Bros. (of printing inks fame) in St Mary's Cray, Kent.

Southampton University had about 1500 students of whom the BSc (Hons) Chemistry class of 1960 was 37; one was female. Lectures were held in the only lecture theatre that the Department had and comprised the traditional Inorganic, Organic and Physical. The inorganic programme started with a course on *Structure and Bonding* given by Drs. Eddie Cartmell and Gerry Fowles and it followed their renowned textbook *Valence and Molecular Structure* initially published in 1956 and as a 2nd edition in 1961 – I still have my copy of the latter with its 32/6 (£1-12-6; $NZ 3.25) price sticker inside the front cover.[5] Organic was split between Drs. Ishbel Campbell and Eric Parker, and covered functional group chemistry in all its 1960s glory. The set texts were the two volumes of Finar and Cram & Hammond's *Organic Chemistry* book.[6] Ishbel was a delightful lady, one of the very few female academics in science and whose fame lay in being the first to isolate chiral phosphorus compounds; her research was dominantly in the organophosphorus, -arsenic, -antimony, and -tin areas in the era prior to most spectroscopies becoming commonplace. She had few research students but Martin Hocking (who completed his PhD in 1963 and was one of my laboratory demonstrators) rose to the rank of Professor at the University of Victoria in Canada. Ishbel was a chain smoker and in her first lecture she advised us that, as Southampton University forbade smoking in

all lectures, her classes would be tutorials. She then lit her next cigarette from the stub she was holding, put out the stub under the bench tap, and launched into the first lecture! Eric Parker was very much the gentleman, even changing into a suit in his office prior to delivering most lectures. He was an excellent communicator but left academia for the Registrarship of the Royal Institute of Chemistry in 1962. Physical chemistry had Drs. H. M. Frey and A. C. Riddiford as the principal exponents; the latter was assigned as my tutor.

The first term of university involved lectures, no formal study requirements and only one assignment (in physical chemistry); it seemed to provide an easy entry into university life. The needed shock to the system came with an end-of-term examination that produced an average mark well below 30%. A more structured regime of study was needed and the SJC idiom was reinstated! In those days, there were no classes on Wednesday afternoons so that sporting activities could be followed but, like SJC, to compensate, laboratory classes were held on Saturday mornings, ostensibly from 9-12. Ostensibly, because the first term programme of *Qualitative and Quantitative Analysis* had the requirement of results being presented before the end of the laboratory period. While this seemed straight forward enough – the time allocation was more than equitable – what was not pointed out to us was the level of accuracy required for the quantitative components. Volumetric analyses had to have an uncertainty to ±0.5%, whilst the gravimetric components were somewhat more lenient with a mere ±0.75% – and with any result falling outside the limits it was back to the bench to start over again. The midday close was a mere indicator as it was closer to 1 pm before everyone was finished, washed-up and out, sometimes much later depending upon the whim of the demonstrator. The rigours of that programme have meant that, even now, I can use a balance, a pipette and a burette with a precision far greater than most of the early 21st century graduates.

In the early 1960s the university BSc (Hons) programme comprised two parts, the first covering the first two years of study, and the second for year three. All chemists were required to take one subsidiary subject (a maths, physics, or biochemistry option) and a non-examinable non-science offering, as well as gain a passing grade in translating a scientific paper from one of Russian, German or French into English, before being able to graduate. The French option was discouraged on the basis that few chemistry papers then were written in the language. My choices were Physics, International Law and German, of which the law option was magical. Subsidiary physics and I engaged happily until the electricity component ran to complex circuits and electronics, while the German translation was left until 2nd year. The end of first year had no formal examinations as the Part I degree examinations applied only at the end of year two. However, this did not mean an easy ride for the freshmen. Departmental examinations were scheduled

and these decided one's fate – to remain in the Honours stream, to drop to the Ordinary Degree (a BSc plain and simple), or to be 'sent down' with one's university days ended. Thus, in late June 1961 the examinations began; for me two papers in physics and three in chemistry scheduled over three days with only the afternoon of the second day free.

Unlike today's norm where courses are split into small components with much in-course assessment and feedback, the 1960s had none of these. For us, the summer exams were the first since the disastrous pre-Christmas exercise and there was some trepidation about the outcome. Again, and contrary to the mandatory practices of the 21st century, the procedures did not comply with anything I have seen elsewhere. Two lists were posted on the first year notice board. The first, headed '*The following candidates may proceed to the second year of the Honours programme*' contained an alphabetical list of family names with given initials. It comprised thirteen entries – 13 of the original 37 had made it beyond 1st year! The second list was similarly annotated and gave the list of candidates who were allowed to proceed to the second year of the ordinary degree. This list carried fewer than the remaining 24 names. There were but two lists. If, after (frantically) searching, one had not found one's name then a visit to the tutor was necessary, after all, the tutor was expected to nurture the student's well being. I well remember one such individual reiterating his experience of being told that '*as far as this institution is concerned you no longer exist*'. For me, the news was good – I was through to year two of Honours.

Year two at Southampton saw a few changes. Dr. Ian Stevens had joined the organic staff headed by Richard Cookson (1922-2008) who had taken over the Chair in Chemistry in 1957 from N. K. Adam and, at the time was the youngest Professor of Chemistry in the UK at 35 years of age. Ishbel Campbell, Eric Parker and the 'new man' provided the organic lecture courses, while Ken Webb continued his discourse of systematic inorganic chemistry from the *green Partington*,[7] and Gerry Fowles continued with coordination chemistry. Thermodynamics formed a good part of Physical Chemistry and the entire laboratory programme moved to a higher plane. I successfully completed the German translation programme in May of that year when Eric Parker announced his impending move to London to become Secretary and Registrar of the Royal Institute of Chemistry. During 1962 Drs. John Hudec and Leslie Johnson joined the organic staff.

The first part of the degree examinations that counted to the final outcome followed in June 1962. Chemistry had three three-hour papers (one for each subdiscipline) and Physics just two. They began at 9.30 on Tuesday June 12 with Chemistry I and were completed by 12.30 on Thursday 14th with Physics II – five papers in two and a half days with a one and a half hour break between each. The organic and inorganic papers were regarded by us as essentially what was

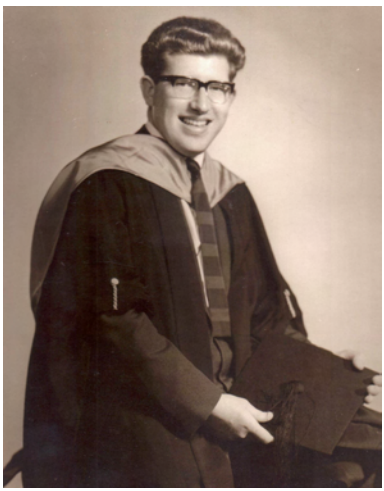
expected, but the Physical Chemistry paper caused us problems; I gathered later that only three of the class of eleven actually passed it. Nonetheless, save for my good friend Mike Gravestock, we proceeded to the final year of Honours; Mike moved on to the Ordinary degree programme only to be vindicated the following year when he was invited to return for honours in a fourth year. This he did, subsequently completing a PhD degree in Canada. Once again, the results were posted in the same indicative way.

The second year at Southampton had seen the start of a new Chemistry building to abut the existing two storey structure at its southern end creating an internal courtyard. The impact on us was the construction of a corridor inside the eastern wall of the lecture theatre reducing its size. This had minimal impact on our small numbers and the lecture programme continued. Richard Cookson, who had been involved with (Sir) Derek Barton whilst at Birkbeck College (London) on the conformational analysis work that led to the latter's 1969 Nobel Prize, gave one of the courses and our first exposure to him. Unfortunately, lecturing was not Richard's strong point. I recall one lecture in which the blackboard became covered with jottings and a series of lines joining them together in what appeared to be a completely haphazard fashion. Nonetheless, he impressed with his breadth of knowledge and willingness to discuss. John Hudec and Ian Stevens were joined by John Mellor who retired from the Department only in 2002 or thereabouts, while Leslie Johnston had little contact with the senior class. Physical had its first professor appointed and Graham Hills appeared before us for a good part of the year. By the Christmas break, the new edifice was almost ready for occupation and the spring term saw the new laboratories for 'organic' opened. The postgraduate students had already moved into the upper level labs of this spacious building and, with the expansion, came a drive to increase the postgraduate population by attracting students from elsewhere. I well remember Gerry Fowles more regularly stopping to give me a ride up the hill from the Swaything-Portswood Road to the rear entrance of the Department and I think that he was somewhat disappointed that organic was my preference to his co-ordination chemistry.

The final examinations comprised the three-hour papers in each area of chemistry followed by Paper IV that was *designed to test the candidate's breadth and depth of knowledge in the subject*. There was no syllabus, no formal lecture programme and anything (within reason) was fair game. Invariably, a question would be based on a topic presented in one of the seminars by a visiting academic and the interpretation of 60 MHz proton NMR spectra (the new technique) would be there. Physical chemistry frequently drew upon one of the thermodynamic derivations and Inorganic offered essay-style review topics. These examinations were generously spaced, being mornings only over four days from June 7. For these, Ishbel Campbell waited with us outside the examination hall checking that we

were all there and wishing us the best. If one of a class failed to appear, she would promptly rush away to bring the errant being in. I cannot remember just how long after the exams it was before the results were announced, as it always depended on when the external examiner could attend. We were given a date and told to make ourselves available should we be required to attend for interview. As it happened, none of us were so invited and we all gained degrees. There were a few thirds awarded, a good number of lower seconds and four upper second degrees, one of which was mine – but no first class honours. Much to my surprise, I was awarded the Smith Prize in Physical Chemistry while the organic equivalent went to a peer who went on to do a 'physical' PhD. My clear preference for research study was for a PhD in organic chemistry and preferably under Cookson's guidance.

The July graduation ceremony involved a procession through the centre of the city, conferment of degrees in the town hall, and an afternoon reception in the grounds of Stoneham House, adjacent to Connaught Hall on Wessex Lane (now a part of the Wessex Lane Halls Complex). My parents came down from London for the day and enjoyed the success that I gained. That final undergraduate year had been spent as a Connaught Hall resident in a room next to John L. (Jack) Frost who also gained an upper second degree and proceeded to a PhD with Gerry Fowles. Gerry left Southampton in 1966 to take up a Chair at Reading, joining Monty Frey (physical chemistry) who had made the move a year earlier. Our rooms in Connaught faced the entrance to the quadrangle and were below the accommodations occupied by Dr. Webb (who never married and became a very longstanding member of the Connaught community, even well into his retirement years I believe). I heard later that he and other academics resident in the Hall kept a 'book' of the degrees each resident was expected to gain!



Postgraduate Study

After the vacation, it was back to Southampton on a Science Research Council scholarship for PhD study. As was customary, I found I was to be supervised jointly by Cookson and Ian Stevens whose expertise lay in the chemistry of small rings – azirines and diazirines – at least until he destroyed the NMR probe of the newly installed Varian A60 during the early

azirine

diazirine

part of 1964. Chris Watts (who with Mike Gravestock remain the only contemporaries of those days with whom I have maintained contact) was likewise assigned. There were also additional recruits to organic from other places. John Henstock joined our year while Brian Drake, who had graduated via the Royal Institution of Chemistry examination process, had started his postgraduate study a year earlier; both were with Richard and had John Hudec as co-supervisor.

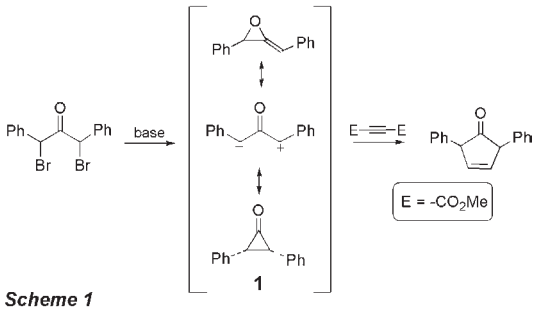
Organic chemistry was located on Level 5 of the research building with offices for Ishbel, Ian and John at the eastern end and research laboratories occupying the remainder of the floor. John Mellor and Leslie Johnson were located in the old building, while, as Head of Department, Cookson had his offices on Level 2. I was allocated to Room 514 which comprised four research benches, each with a writing space at the end, and two fume cupboards.

Brian Drake and Golapudy Subrahmanyam (an Indian postdoctoral) were the first to move in and had taken the end benches with a fume hood behind them. I had a centre aisle bench facing west and the remaining vacant space behind me became the coffee centre for 'organic' for the duration of my PhD. Having had to move out of Connaught Hall, I had secured a one bedroom apartment on Hartley Avenue, no more than 50 metres from the back entrance of the Chemistry Department, and I recall the tremendous thrill of letting myself into the building after hours for the first time. The closeness to 'work', however, soon became a burden as the sight of the building from my bedsit window was too much of a pull. I found that I was spending far too much time at the bench and I had to put more space between 'home' and 'office'; a move became essential.

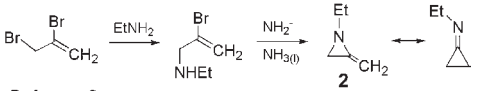
Mike Gravestock had been 'in residence' with the Eaton's at 17 Phillimore Road, Swaythling, for some years. This family was known as providers of one of the very best of home-stay accommodations for Southampton students. While I do not recall the details, a vacancy unexpectedly arose and I was fortunate enough to be accepted by them. I moved in at the end of October in 1963. Les Eaton had been a Midland Bank Manager in the Eastleigh Branch but was recently retired when I appeared on the scene. His wife Kay, formerly very active within Southampton amateur dramatics, provided us with every home comfort, meals cooked on the Aga, freshly perked coffee with breakfast and dinner, and the ability to join them to watch TV in the evenings unless they were entertaining. Les was a devout home brew enthusiast to the extent that he would only provide us (and our visiting friends) with his ale if we were not driving; the potency was legion and a pint was the maximum intake. I remained with Kay and Les for the duration of my PhD and maintained contact with them until they died. Even in 1974, when I was on leave in the UK with Margaret, my wife, and 2-year old son, Mark, Kay insisted that we come down from Reading (where I was based) to spend a weekend with them; the last time that I saw them was in 1985 when their health was clearly on the decline.

The Research

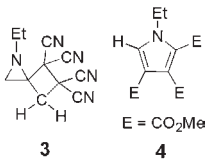
In 1963, cyclopropanones, *e.g.* **1**, had not been isolated but their presence as intermediates had been demonstrated in a transformation known as the Favorskii reaction. So also had their allene oxide resonance isomers, both from trapping studies as illustrated in Scheme 1.[8] The project that I accepted was designed to see if nitrogen analogues of allene oxides, the isolable methyleneaziridines, *e.g.* **2**, behaved similarly. The known[9] amine **2** was synthesized from 2,3-dibromopropene by amine for bromide replacement using ethylamine with subsequent dehydrobromination using amide ion in liquid ammonia (Scheme 2). The second step provided the first exposure to the use of low temperature liquids. The needed liquid ammonia was dispensed alongside the building loading bay and a fume cupboard in an unoccupied laboratory at the western end of Floor 5 was used for the reaction. The isolated yield of **2** was dire – I had not used an appropriate trap and the product had co-distilled with the solvent under vacuum, an easy mistake for the novice and a lesson well heeded. Trapping reactions with a range of dipolarophiles were largely unsuccessful as only two of nine such reactants gave products, *viz.* **3** and **4**, from tetracyanoethylene (TCNE) and dimethyl acetylenedicarboxylate (DMAD), respectively. The structure of the more interesting pyrrole triester **4** was confirmed by comparison with an authentic sample. The route from **2** to **3** was clearly by a [2 + 2] addition, but that from **2** to **4** much less obvious (there were no rules for pericyclic reactions then). Although a lengthy pathway was proposed,[10] I only returned to these studies when Victoria University honoured me with a grand retirement symposium in 2005. One of the participants, Dr. Andrew Grimsdale, represented Klaus Müllen, the Organic Chemistry Director of the Max Plank Institute for Polymer Research. I had met Andrew there and the issue of my PhD arose, the dipolar cycloadditions discussed, and a few weeks later, the mechanistic explanation of Scheme 3 came from him. As is clear, methyl 2-butynoate should be detected. That it was not may simply reflect loss during purification (bp 102 ºC 760 mm/Hg). Repetition of the sequence would seem to offer a promising honours year project.

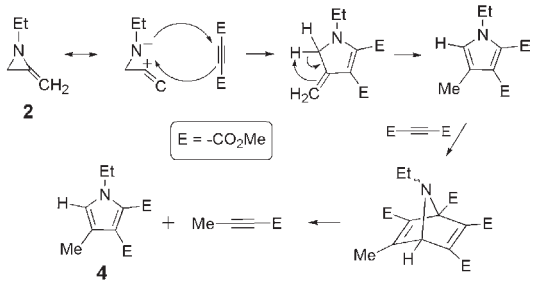


Scheme 1



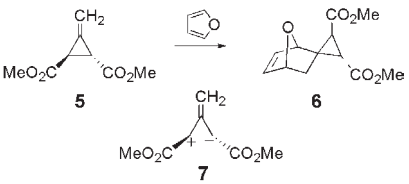
Scheme 2





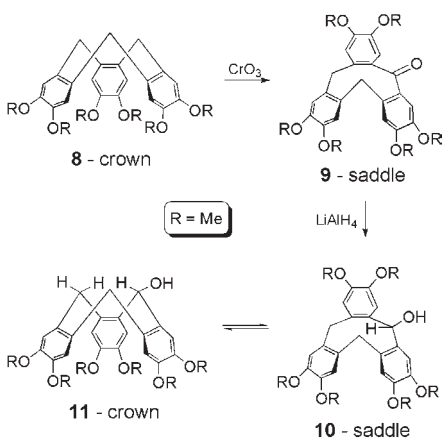
Scheme 3

While these studies took the most part of a year, they were never going to extend for the remaining two years of the doctoral degree. Chris Watts, who was awarded a Southampton Scholarship, had examined a different aspect of the Favorskii intermediate, namely the intermediate(s) in the thermal rearrangement of



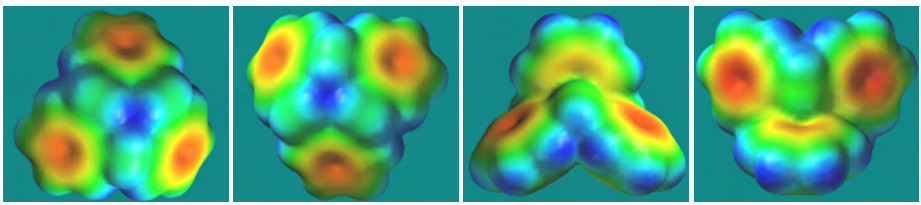
Feist's ester **5**. He showed that heating in the presence of furan (and cyclopentadiene) gave a single spirocycle, *e.g.* **6**, from [4 + 2] cycloaddition rather than interception of the ring opened zwitterion **7**; we share a common publication from our results.[11]

We both needed new topics. Chris moved to study the cycloaddition chemistry of diethyl azodicarboxylate and Richard broached with me the subject of conformational analysis and the possibility of separating and isolating distinct conformers (now termed rotamers) of one molecule. The pioneering work of Sir Derek Barton and the evolution of conformational analysis had made major inroads to organic chemistry, but in 1964 the isolation of more than one conformer of a molecule was still to be achieved; the challenge was too great to resist. The molecule that had attracted Richard was cyclotriveratrylene (CTV, **8**). It had been synthesized by Mrs. Robinson[12] in 1907 from the condensation of 1,2-dimethoxybenzene (veratrole) with formaldehyde, although it was only in 1963 that the correct trimeric structure, depicted by 'crown' **8**, appeared (Scheme 4).[13] The compound is easily oxidized to a monoketone, which has significant conjugated aryl rings (υ_{max} 1685 cm^{-1}) and, thus, cannot retain the non-conjugated crown arrangement. It had to have the 'saddle' shape **9** shown in Scheme 4. Lithium aluminium hydride reduction of **9** leads to alcohol as expected but ***two*** conformers were detected – the less stable *saddle* **10** and the thermodynamically more favoured *crown* **11.** A separation of the conformers to provide the independent rotamers was not easily



Scheme 4

found until their solubility in tetrahydrofuran (THF) was discovered to be markedly different. Saddle **10** is essentially insoluble whereas crown **11** is very soluble; simply stirring in THF followed by filtration gave almost pure **10,** stable for several days at -70 ºC. Crown **11** exhibits its methylene groups as a pair of doublets (3.48 and 4.65 ppm; *J* 14.0 Hz) with a singlet for the 18 methoxy protons (3.87 ppm) in the (60 MHz) ^{1}H NMR spectrum**.** In contrast, saddle **10** had an AB quartet centred at 3.90 ppm (*J* 15.5 Hz) for the CH_2 moieties and a split signal (3.85 and 3.92 ppm) for the OMe protons. A kinetic study concluded that saddle **10** ionizes some 6 x 10^4 times faster that crown **11**. The isolation and formal distinction of the α-**10** and β-**11** cyclotriveratrylenols formed my first scientific publication – a communication in the *Journal of the American Chemical Society* jointly with N.K. Anand who had carried out some of the preliminary studies.[14] Subsequent study of a range of transformations and kinetic measurements provided a foundation of my PhD thesis and the full paper appeared later.[15]



HF-6-31G**electron density potentials of crown (*left*) and saddle (*right*) **8**, the second of each from rotation and turning

Calixarenes and crown ethers are now of major importance in host-guest chemistry and the cyclotriveratrylenes have played their part. Nonetheless, CTV (**8**) was not revisited until recently. Out of interest, the equilibrium geometries and density potentials of the crown and saddle conformers of **8** were calculated at the HF-6-31G** level. As often happens with calculation, the pictorial outcome can have artistic appearance. For me, this is especially true for the saddle, and especially from above as depicted in the figure at the right.

In July 1965 my father, John, died. Shortly after returning from holiday in the summer of 1964, he suffered a minor stroke. He was subsequently diagnosed with arterial sclerosis for which there was no simple remedy. Having been referred to the specialist medical staff of Guy's Hospital, he chose to undergo experimental surgery to clean his carotid arteries. Prior to his hospitalization, he had visited me in Southampton and saw my work environment, met the Eaton's and, I think, was happy and satisfied with what he found. The late July surgery never saw him regain consciousness and he died on the 31st. After the funeral and attending to the essential matters, I spent some time with friends at a chateau near Lyons in France.

On returning to Southampton I met with Prof. Cookson and still remember

Richard Cookson (1922-2008) and his wife in later years

his offer of financial assistance – cheque book in hand – to help out in what he was certain had been difficult times. Richard's generosity was well known as he had lent money to many students, often the travel costs to get to an overseas postdoctoral, always done on a pay-me-back-when-you-can basis. While the Cookson family were financially independent, having owned what became British Steel, wealth was something Richard used to assist others when he perceived a need; he was a delightful individual, a first-rate chemist and a generous man.

As with my peers, the final year of study brought thoughts of the future and career prospects, and this second only to completing the degree successfully. In my pre-University days I had contemplated reading textile chemistry but my father's brother, Edward, dissuaded me from it. He had worked his life in the Lancashire mill environment rising from a weaver to become Manager of Sabden's Studdart Mill; by the late 1950s he was convinced that a future in textile chemistry was not a good idea. Nonetheless, the dyestuffs industry had appeal and I interviewed with ICI (Dyestuffs Division) in Blakely, Manchester, but I was not convinced the northern environment was what I really wanted. Indeed, even before the outcome became known, I had convinced myself that travel was important, and that a postdoctoral position would be ideal. In the mid-1960s, the US economy was strong, postdoctorals were abundant and salaries good. The choice of potential supervisors combined with locations to provide a list of six. Highly ranked was Prof. Nick Turro of Columbia University in New York City, but by February of 1966 he had already committed his funds. Others were similarly placed, but Prof. Turro proffered my name and an unsolicited offer came from Milton Orchin at the University of Cincinnati on March 30. By then, I was more attracted to small ring chemistry again and the work of Merle Battiste (1933-2009) at the University of Florida held greatest appeal. I wrote expressing my preference to work with him rather than be in Cincinnati. I received a formal offer on April 12 that I accepted for a late August/early September start on a salary of $US7200 pa – more than triple the expected UK income at that time. Without doubt the most difficult next step was to tell my mother of this decision, a mere nine months after my father's death.

Chris Watts and I were allocated the same external PhD examiner but were told that he was to leave on a one year sabbatical in late August. The choice was to have the thesis written and in final draft form in six weeks or wait until he returned the following year! Unbound copies of our theses went to him for examination ahead of the formal submission. Likely this was contrary to policy, but it happened and

Chris Watts

we had our oral examinations sequentially on the same day in August 1966. Chris was first, and I discovered that our examiner was Prof. Ken Schofield of Exeter University. When my time came, I was cheerfully greeted, and then there was a silence as he proceeded to delve into his briefcase. He produced a paper bag of broken Dreiding Model parts and assured me that my statement on the inability of the Dreiding model of crown **11** to invert to saddle conformer **10** was correct. His attempt to disprove this had cost him a comparatively new set of components! Chris and I were successful and awarded PhD degrees conferred *in abstentia* at the graduation ceremony in 1967. Ken Schofield and I next interacted in 1973 as he was then the Scientific Editor (Organic) for the Cambridge Chemistry Texts in which the Coxon and Halton book *Organic Photochemistry* appeared,[16] and subsequently on his visit to New Zealand. Chris and I then left Southampton, me for Florida and Chris for Los Angeles where he had a postdoctoral with Prof. Saul Winstein.

My departure from England for the USA was on the *Queen Mary* from Southampton to New York on Friday September 17, 1966. My mother, her twin sister Anne and her daughter (my cousin Marian to whose son Nicholas I had become godfather the previous October) came down to Southampton to see me off. In those days, the fanfare of departure had bands playing on the quay, a myriad of streamers (provided by the Cunard Line) and the decks lined with the departing passengers. For my event there was room for all of us along the rails as the boat was far from fully booked, and the smaller number of passengers provided for a most pleasant journey.

Florida in the Mid-1960s

My berth on the *Queen Mary* was in a four-man cabin in the bowels of the boat – the least expensive – and, together with the subsequent rail journey from New York to Gainesville, allowed for a further three days holiday in the city covered by the remains of my £500 legacy. The cabin, it transpired, was to be occupied by only two passengers, a British jazz musician returning to New York for work and me. We proceeded to make the most of the five day journey, using the first class golf driving range during the daytime (with 50% occupancy in steerage class and less in *First* the barriers were left open) and the bar in the evenings.

Apart from the usual tourist sites, my few days in NYC included a visit to Columbia University to meet Nick Turro who, some months later wrote asking if I was still interested in a post at Columbia for the following academic year. The offer came in a letter which stated: *My postdoc stipend will be of the order of $US 5,500. From your point of view, such a drastic pay cut would demand some other dramatic compensation. Whether living in New York would make a difference, I don't know*.

The Saturday afternoon had me board the Atlantic Coast Line Railroad train at New York's Penn Station bound for Florida, with a train 'relocation' scheduled for Jacksonville the next morning. With a single berth bedsit the journey was comfortable and enjoyable. My evening meal in the dining car necessitated joining an elderly couple as the restaurant car was full. Even after 44 years, I recall those people being most convivial and hospitable. It transpired that their son was studying at the University of Florida and dinner conversation revolved about life in Florida, the Gainesville campus and all the new things that I would see and experience; they happily befriended me and bought me dinner.

Upon arrival in Jacksonville on the Sunday morning I was told that there was ample time to take a stroll and look around the station, which I did. On return to the track (no longer a British platform) I was horrified to see a long empty space with no train – the 'relocation' had occurred. Sanity was restored when I discovered that the train had split in half with the one for Central Florida and Tampa now at a different track and the other already on its way south to Miami. The Gainesville stop

came up around noon with the external temperature close to 100 °F (37 °C). I was the only passenger to disembark. I phoned Prof. Battiste and waited what seemed like hours in the blistering heat for him to arrive. He had not accepted that a train actually stopped in Gainesville and had driven 35 minutes south to Ocala, only to learn that '*if he had gotten off then it had to be back in Gainesville as that was the only halt*'! We went directly to lunch, I rehydrated, and then we toured the town and the extensive campus, finally arriving at the Chemistry Department. Dinner was with Merle Battiste, wife Anita and son Mark. It was delightfully casual but, as the evening progressed, I became somewhat concerned about the accommodation that Merle had said would be arranged for me; he had forgotten to mention that it was to be with the family for as long as it took to find an apartment.

My first day on campus was taken with the usual signing-on paperwork, a visit to the Campus Police for issue of ID card and fingerprinting, and then downtown for issue of a Social Security Card. All of this was done with the assistance of Dr. Rolf Rehberg, Merle's first postdoctoral fellow who had been there some months and recently become married to his German fiancé, with Merle and Anita acting *in loco parentis*; they had accommodated Angelika ahead of the wedding. Some months later it became my turn to *initiate* the next postdoctoral down the line. This was a simple enough task as the individual, one Dr. John Hill, also an Englishman, was a sabbatical visitor from Hobart and William Smith College of Geneva in upstate New York where he had a permanent post; he needed no introduction to US customs, just a bit of help finding places.

Arriving in Gainesville in September (1966) meant that most of the apartments had already been rented to student groups and little was available. Anita was generous in insisting that I did not rush into any agreement and regret the decision (typically a 12 month lease with a first and last month's rent payable up front). It took only a few days to realize that a car was essential and, while I had saved what I thought would be adequate for a US start-up, a car had not featured on my list! Rolf had me sorted out within an hour; we visited the Campus Staff Credit Union of which I became a member on payment of $US2.00 and collected the application form for a loan! Being German, Rolf advocated the purchase of a VW Beetle for a single postdoc and, knowing little about US Chevys, Buicks or Fords, I took his advice, went to the local dealership and agreed to a low mileage 1964 model subject to loan approval. My loan was approved within 24 hours and I had a red VW the next afternoon. Florida law in 1966 was a little confusing. As an overseas visitor, I was able to drive on my British licence for about three months but, upon taking up residence in the State, I needed to take out a Florida one. No one would tell me for how long I could drive before I took the Florida test and so I decided to wait for some weeks until I was sufficiently settled.

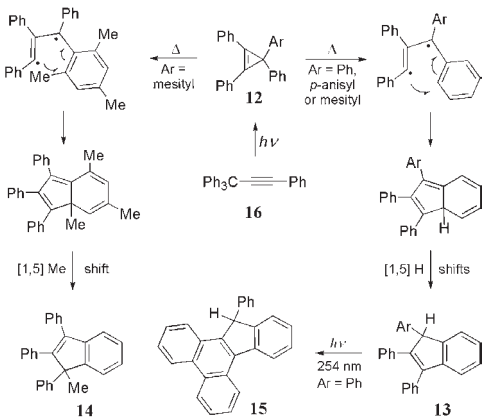
At the end of my first week in Gainesville I found a semi-furnished one bed-

room apartment on SW 14th Terrace, almost at the southern edge of the town as it was then. The complex, Cheshire Apartments, was about a mile from the Chemistry Department and set back by one block from Route 441 (13th Street), the main highway through the town. It was surrounded by woods, the trees covered in Spanish moss that provided an eerie sensation when driving in at night. My experience in gaining a Florida driver's licence was not without its own drama. With a few weeks of Florida driving behind me, I attended the Florida Highway Patrol Station. The large burly officer at the desk had me complete the paperwork, dismissed my UK licence recording *no licence* on the form and wrote *needs glasses* after the eye test. I was then asked to wait outside for the road test where I was joined by another man, who I assumed was also waiting for his test. Not so! He enquired as to how I had got myself to the testing station. I replied that I had driven my VW on my UK licence. '*How so, you don't have a licence*' I was told. Then: '*since you have been driving without a licence we should lock you up*'. Battiste's name was given as surety and only after a considerable time was I told: '*OK – if you can pass the practical we'll let it go*'. The one thing that no one had told me about driving was the difference between a UK and a US emergency stop. The former allows one to lock the wheels, while this is anathema in the US; my examiner ended up on the floor of my Beetle. I had all sorts of problems trying to convince him that this was normal where I came from and, eventually, he let me have another go doing it his way. I was given a licence and had but two other interactions with the Highway Patrol service, one when a prisoner had escaped jail in Georgia and I was subjected to visual car search, the other on my penultimate day in the country.

With my arrival, the Battiste group comprised nine students, an MSc, 6 PhD and 2 postdoctorals. My first full week in the lab was spent reading and coming to grips with the chosen project that was to involve the rearrangements of vinylcyclopropenes. It had been opened up by MSc student Bobby Grubbs who had left U of F for Columbia University (NY) and a PhD with Prof. Ron Breslow prior to my arrival. Within a few weeks the rearrangement chemistry was well underway and a series of physicochemical studies were subsequently completed and refined for publication. This provided Grubbs with his first scientific publication and me with my first from the US, a Chemical Communication.[17] For me this publication has much significance as Grubbs went on to become a 2005 Nobel Laureate in Chemistry (with Yves Chauvin and Richard R. Schrock). As fate would have it, Bob gave a lecture at Victoria University (of Wellington) at noon on October 5, 2005, some 10 hours before he was advised that he was recipient of the Prize; I have an off-print of that first communication duly signed and dated that day. I had met Bobby and his wife on a visit to Caltech (CIT) in late 1990 and was impressed by his generosity, not simply personally but also in his chemistry. At VUW he took the trouble to introduce me as someone involved with his early work and the role

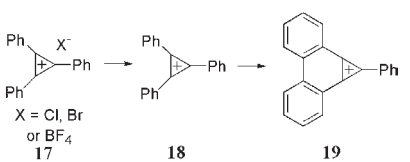
that it played in his subsequent (glowing) career.

The laboratory work went well with discussions frequent, informal and especially beneficial. The work itself involved the rearrangements of tetraarylcyclopropenes and the first phase concentrated upon the thermal process whereby triarylindenes are formed. Whereas tetraphenylcyclopropene gives 1,2,3-triphenylindene (**13**, Ar = Ph) only, the anisyl- and mesityltriphenyl derivatives provided products from ring closure on to each of the C3 substituents, *e.g.* **13** and **14**, that are now easily rationalized as resulting from [1,5] H and Me shifts as shown in Scheme 5. The studies advanced to cover the full gambit of rearrangement conditions. The photochemical work showed that **13** (Ar = Ph) is the primary product from a singlet process induced by 254 nm irradiation of **12** (Ar = Ph) and that triplet sensitization or irradiation at 350 nm returned unchanged starting material. Subsequent separate irradiation of **13** (Ar = Ph) results in cyclodehydrogenation and formation of 13-phenyl-13*H*-indeno[1,2-*l*]phenanthrene (**15**).[18] In conjunction with Prof. Gary Griffin and his students at Louisiana State University, the work was extended to show that tetraphenylpropyne (**16**) is a suitable starting material for photosynthesis of **12** (Ar = Ph). Over short irradiation periods (*ca.* 1 h), **12** (R = Ph) is the sole product, while longer times are needed to produce firstly **13** (R = Ph), and then **15**.[18]



Scheme 5

Electron impact induced fragmentation of **12** (Ar = Ph) causes loss of a phenyl radical with formation of the triphenylcyclopropenyl cation (**18**) as base peak in the mass spectrum (70 eV electron impact). Subsequent fragmentation is by way of cyclodehydrogenation with formation of the cyclopropaphenanthrenyl cation (**19**) (Scheme 6). These facts were established from the unambiguous generation of **18** in the mass spectrometer from volatilization of triphenylcyclopropenyl chloride, bromide and tetrafluoroborate (**17**) into the instrument at temperatures well below their melting points. The high melting bromide and tetrafluoroborate are ionic whereas the lower melting chloride has some covalent character. However, the tetrafluoroborate provides a molecular ion compatible with 1-fluorotriphenylcyclopropene, which fragments to both **18** and its fluorodi-



Scheme 6

phenyl analogue.[19] This aspect of the programme demonstrated, for the first time, that carbonium ion salts can be excellent sources of the free cations in the mass spectrometer.

In the mid-1960s, a major issue with a postdoctoral position was how long to stay in one place before moving on. Two years provided a great opportunity to gain several good publications, whereas moving after the first twelve months provided two independent referees. Extending postdoctoral study to a third year was unusual; indeed, employers would often wonder why you had not gained a permanent post within that time. I was fortunate in two ways. Firstly, I had had the offer from Nick Turro and, very soon after it arrived, Merle asked if I would be interested in an Assistant Professorship from September 1967. Florida was to change from a trimester to a quarter (10 week) system and junior academics were needed to smooth the transition. The opportunity to gain teaching experience without having to commit to anything more than a year had great merit and so, in short order, I applied for and was offered a post at a salary of $US 900 per month. I responded affirmatively, and then wrote to Turro to let him know what had happened.

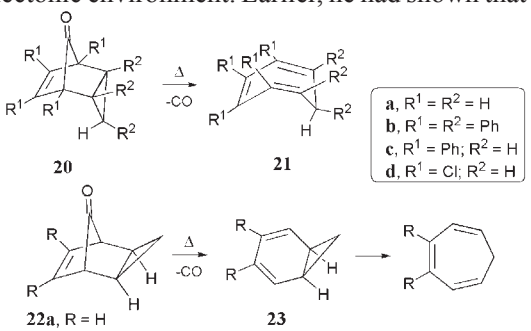
The first weeks in Gainesville provided numerous experiences in settling to life in the US South. I was ill prepared, after all, the PC and internet with their instantaneous worldwide communication had yet to be invented. In ordering my first lunch time coffee, I was asked '*How'd ya like it*', I responded with the traditional English '*white please*' – and only just managed to catch the cup as it careened across the counter from the Negro (Afro-American) assistant; '*with cream*' was immediately substituted! In similar manner, going to a colleague's place for supper at 8.15 did not auger well; the family had waited to serve dinner until I arrived!

I spent half of each Saturday tidying up the laboratory bench, writing up the results and planning the next week of work. In contrast, Sunday was my day off and for the first weeks it was spent seeing something of the local area. The east coast town of St. Augustine, with its *Fountain of Youth* and Ponce de Leon's 1513 claim of possession for the Spanish King, was one of the first places visited. Just south is Marineland of Florida and that was a must for, at that time, I had never seen a dolphin or a decent sized turtle. Florida's western Gulf coast has many fascinating places, the nearest to Gainesville being Cedar Key, a sleepy little settlement in those days that had much scenic appeal; I made several visits there. Tarpon Springs and its historic sponge industry was another. Much of central Florida is covered in State Parks with springs bubbling fresh warm water year round. Many had swimming facilities but one needed to be aware of the alligators, something I discovered only after swimming on a winter's afternoon when I found a reasonable sized specimen lazing a little further down the spring!

Merle had, at that time, just the one son, Mark, about 3 years old. His wife

Anita was a full time mother and the family hosted the research group from time to time. Nonetheless, Merle maintained long hours at the office, had lunch with some of the group most days and held group meetings in the evenings, following them with a visit to one of the local bars. The structure and formalities of England were forever lost. By early November, I was beginning to realize what *Thanksgiving* meant and just how important it is to the average American. Most of my lab mates were off home and Merle suggested that it would be a good time for me to take a trip, so I elected to go south to Miami and the Everglades. As I recall it, I drove the 340 miles taking the Sunshine State Expressway toll road to ensure an early arrival. I had made no motel booking but drove beach side to see the big (expensive) hotels and then went back a few blocks to find something more suitable. Luck was with me as the first place I pulled into was operated by a Lancastrian and his wife. They recognized my accent and after a short chat about 'home', I was given discounted accommodation and an invitation to return whenever I wanted at minimal cost. Having learned that Thanksgiving was a big thing, I decided to find a good restaurant for dinner. Many places had private functions or were open only for those with reservations, but eventually I found what seemed like a nice place, and went in. Apparently the restaurant was fully booked, but the maître d' spoke to a lady who seemed to be in charge, and a small table was set up for me. To my surprise, the meal was from a designated menu with no choices, but it was out of this world. I was graciously tended, exceptionally well fed, and somewhat concerned about the likely cost – no matter it was a real experience! When I came to pay the bill, the total was a trivial $US20.00; I questioned this but was told that it was Thanksgiving and to enjoy my stay in Florida. After returning to Gainesville I found the restaurant to be the most respected Jewish establishment in Miami with Thanksgiving Dinner offered to their regular customers at a fraction of its real cost.

One day in spring 1967, Merle came into the lab very excited about an idea he had had of how to demonstrate the dependence of cyclopropyl ring participation in solvolyses as a function of the stereoelectonic environment. Earlier, he had shown that tricyclo[3.2.1.$0^{2,4}$]octen-8-one (**20a**) underwent facile decarbonylation (Scheme 7)[20] and a report by Clarke and Johnson[21] had triggered his imagination. Thus, four of us in the group began the work to synthesize and then decarbonylate the series of derivatives **20** and **22**. As I had studied the tetraarylcyclopropene–to–indene rearrangement kinetically using



Scheme 7

^{1}H NMR methods, my role was to take the materials synthesized by the others and determine the first-order rate constants for decarbonylation. This was done over a quite short period of time. The *endo*-compounds **20** lost CO very easily, with rates dependent upon the substituents present. In contrast, the *exo*-derivatives **22** were markedly more stable, **22b** losing CO only at 400 ºC. The study led to the clear conclusion that the orientation of the three-membered ring in *endo*-**20** is ideal for the bent 'banana' bond of the bridge to overlap with the σ bonds holding the carbonyl moiety in place. Through this interaction, concerted ejection of CO yields the cycloheptatrienes **21a-d** directly in their favoured conformations. The markedly more stable *exo*-**22** provided cycloheptatriene product, but only by way of the norcaradiene **23** that undergoes six-electron electrocyclic ring opening. The work was completed in short order and subsequently published.[22]

The remainder of that 1966-67 academic year passed quickly, but with one notable visitor to the university. Professor Emanuel Vogel (1927-2011), who held the senior Organic Chemistry Chair at the University of Cologne, was a visiting professor during the late spring quarter. He gave some first-rate research seminars and a graduate course entitled *New Trends in Aromatic Chemistry – The Annulenes*. It soon became clear that his trademark was to draw impeccable structures on the blackboard, and deliver his lecture in perfect English. I was fortunate to be invited, with Merle, to join him and his wife Christiane for afternoon tea and that meeting became what was the first of many throughout my career.

Through my next door neighbour, Bill (Eaton), I widened my circle of (non-chemistry) friends and also started to play cricket for the U of F team – financed by successful college football – and played at the MCC-affiliated Savannah (Georgia) Cricket Club. We enrolled a Canadian, John Haywood-Farmer who was Merle's third postdoc. Some years later he left chemistry for commerce and spent several years at Victoria University as a faculty member in the mid-to-late 1970s where our friendship was renewed.

My summer holiday of 1967 took the form of a tour up the east coast of the US in early June covering Williamsburg, Washington DC and then Niagara Falls. Brian Odell, one of Cookson's postdoctorals and former '514' lab mate had moved to a second postdoctoral at Cornell University in Ithaca and so I stayed with him and wife Cathy for a few days prior to our both attending the 20th National Organic Chemistry Symposium of the American Chemical Society from June 18 in Burlington, Vermont. It was my first attendance at a specific organic meeting and it was a real thrill to hear von Schleyer, Pettit, Closs, J. D. Roberts and Orville Chapman presenting their work. To me it was a grand affair, made all the more memorable from the ease of mixing and socializing. The feature lecture was by Nobel Laureate and Harvard great, R. B. Woodward, whose orbital symmetry rules (with Hoffmann) had begun to have real impact. It was the only time that I heard him; it was

simply magical chemistry even though he did speak for close on two hours. Back on vacation, I headed north for Montreal and *Expo '67*, and then back to Florida via New Hampshire, into Portland (Maine) and the delightful coast (and lobsters) to Boston, where I stayed with friends for a couple of days. The move south by-passed New York and the main centres to allow time for the Shenandoah National Park, the spectacular Skyline Drive, and the Blue Ridge Parkway from just south of Front Royal and along the high Appalachian ridges into North Carolina. From there to the Great Smokey Mountains National Park in Tennessee prior to taking I-75 back to Gainesville. Shortly after returning to Gainesville, I moved from Cheshire Apartments across the woods to the brand new Williamsburg Apartments (No. 114) on SW 14th St. where Bill Eaton and I shared a place that served us well.

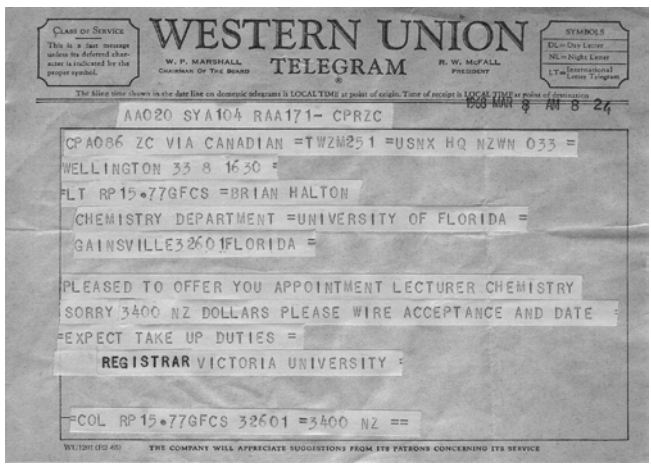
I started teaching first-year chemistry (CY201) in mid-September 2007, the junior of a three-man team that had the 900+ candidates in almost equal sized groups, each with its own lecturer. Each class had to reach the same point every Friday so that a common quiz could be sat during the ensuing week. It was an excellent way to start teaching, as Dick Dresdner and Bud (E. E.) Muchlitz were the other lecturers and willing to help the novice as needed. I tried to communicate effectively in the lecture room and, like most new to lecturing, I was keen to get questions from the class. As time passed, my southern accent increased to the extent that I was not understood when I arrived in New Zealand a year later.

My formal appointment to the faculty of the university was marked by an invitation to an official reception hosted by the University President. As an interim appointee, I had no real interest in attending, but my colleagues thought that it would be a good experience and pressured me into accepting. Indeed, it was a grand and formal affair held on the lawn of the Presidential Residence on West University Avenue. When I arrived there was already a lengthy reception line, which I passed along like everyone else until I met the President's wife, the last on the line. She introduced herself and politely asked what I would like to drink. My response was a simple: *A scotch and water please*. And that is why my colleagues had been so enthusiastic about my attendance! The President's wife, a staunch Southern Baptist, was teetotal and proceeded to give me a lecture on the evils of alcohol. The line froze for what seemed an aeon and I was ribbed about it the next day by those who had known exactly what would happen.

Being the gentleman that he was, Merle had asked if I wanted to start my own research work or continue on with him. I had decided that the small ring chemistry we had underway was going too well to simply drop as at least two more publications would come were I to continue with it; this I did. That fall quarter passed very quickly and all too soon the winter quarter of 1968 arrived and was notable in that I had a class of my own. The content was inorganic structure and bonding for a group of students who needed a one-off course to take them through the

transition to the quarter system. It was a great time as I had the freedom to do as I wished and it cemented my desire for a career in academia. The northern spring of 1968 had my joint PhD supervisor, Ian Stevens (who had looked after the day-to-day running of the project), in the US and visit Florida to lecture. Whilst that was enjoyable, the visit proved unforgettable. Bill and I had invited Ian and his wife, Jeanine, to dinner and it was a nice occasion but turned out a night to remember. Just after starting the meal the phone rang. It was Merle. The date was April 4 and Martin Luther King had just been assassinated. Shot on the balcony of his Memphis motel at 6.01, he was pronounced dead about an hour later after emergency surgery. Riots erupted in many US cities, Gainesville being no exception with the city streets taken over. The local TV and radio station advised everyone to stay indoors; the dinner party lasted longer than planned – until about 11.30 – by which time we were able to get our guests back to their hotel.

I had started to look for a permanent post and, with the exception of an offer of a teaching fellowship at Bristol University (with the possibility of a permanent position later), my approaches to UK institutions led only to offers of further post-doctorals. As I was in the USA as an exchange visitor, my stay was limited and so I began to explore possibilities in Canada and further afield. Roger Brown, who had spent a sabbatical with Cookson at Southampton during the latter part of my PhD tenure, wrote to let me know he was moving from the Australian National University to a more senior position at Monash in Melbourne. His job was to be advertised and he encouraged me to apply. This I did, but I also sought out other Australasian possibilities as a few years in the antipodes had much appeal. Of the institutions I wrote to, Victoria University of Wellington in New Zealand, responded advising that a lectureship was about to be advertised and would I care to apply and have referee reports sent. I did, and some months later enquired of both universities about the status of my applications. Each told me that I was on their short list but ANU advised that no decision would be made for some months. VUW responded with the news that a decision was imminent and, on April 8, I received a cable offering me the lectureship in chemistry. Knowing that nothing



WESTERN UNION

TELEGRAM

W. P. MARSHALL, Chairman Of The Board — R. W. McFALL, President

CLASS OF SERVICE: This is a fast message unless its deferred character is indicated by the proper symbol.

SYMBOLS: DL = Day Letter; NL = Night Letter; LT = International Letter Telegram

The filing time shown in the date line on domestic telegrams is LOCAL TIME at point of origin. Time of receipt is LOCAL TIME at point of destination

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CPA086 ZC VIA CANADIAN =TWZM251 =USNX HQ NZWN 033 =

WELLINGTON 33 8 1630 =

FLT RP15•77GFCS =BRIAN HALTON

CHEMISTRY DEPARTMENT =UNIVERSITY OF FLORIDA =

GAINSVILLE32601FLORIDA =

PLEASED TO OFFER YOU APPOINTMENT LECTURER CHEMISTRY

SORRY 3400 NZ DOLLARS PLEASE WIRE ACCEPTANCE AND DATE =

=EXPECT TAKE UP DUTIES =

REGISTRAR VICTORIA UNIVERSITY =

=COL RP15•77GFCS 32601 =3400 NZ ==

WU.1201 (12-65) THE COMPANY WILL APPRECIATE SUGGESTIONS FROM ITS PATRONS CONCERNING ITS SERVICE

would come from ANU for some while, the decision was simple – I accepted the Wellington position. Once again, fate played its role as, that day – April 9 in the US but the 10th in NZ – the main item on the US TV news was of a terrible storm in Cook Strait and the sinking of a passenger ferry, the T.E.V *Wahine***.** The storm that exploded upon Wellington was one of the worst ever recorded in New Zealand. Its ferocity was due to two fronts that by chance merged directly over Wellington at the same time as the overnight Lyttelton-Wellington ferry approached the harbour entrance. When I reached the office the next day I was greeted by: '*Is that where you want to go to*' and (by the less well educated): '*Just where is that – somewhere in the Baring Strait?*'; world geography was not well studied in the US then and the thought that New Zealand could be located between the easternmost point of the Asian continent and the westernmost point of North America was considered likely by more than a few!

With a permanent position secured, the opportunity to pursue research was greater and the work with small-ring organics took on a new pace to get as much completed as possible in the time remaining. The mass spectral study referred to already was completed[19] and, because of the belief that what happened when light interacted with an organic molecule was mirrored in the mass spectrometer, a photochemical study of the arylcyclopropenes was initiated. This was my final contribution in Gainesville but it needed to be brought to a publishable state and this was done subsequently by Marty Kulig, one of Merle's newer PhD students.[18] The time in Florida had continued my excursion into non-natural (synthetic) products and the sheer excitement of laboratory work aimed at providing something that had not been done before and the exhilaration of success had me hooked. A career involving unnatural products, compounds made because they did not otherwise exist, that could then be explored in all facets of chemistry and predictions confirmed or denied, was the direction I was determined to take.

The Spring Quarter ran from about the end of March till early June. My teaching then was a repeat of CY201, the introductory chemistry programme, but with a small class that I handled alone. Outside of teaching, the research moved along and my thoughts were more and more occupied by the move to the antipodes. I had written to the New Zealand embassy in Washington DC, enquiring of requirements for immigration and, as a British subject, I was advised to '*catch a plane or a boat and go'*, as British subjects had open access in those days. Victoria University's Registrar had written with the standard set of conditions and advised on travel arrangements. The University was to meet the cost of economy air travel from Gainesville and put me on the salary from the day I left the US. However, I was not bound to air travel if I had a preference for surface, although the allowance would be no more than the airfare; salary would still be paid from the day of departure from the US. In 1968, international air travel was not common and so I

took a berth on the Pacific and Orient (P&O) liner *Oriana* from Port Everglades on Sunday, August 25th to take in the Bahamas, the Panama Canal, Acapulco, Los Angeles, San Francisco, Vancouver, Honolulu and Suva before arriving in Auckland on September 21st. I advised my future employer that I was not accustomed to air travel and that a voyage was more appropriate. Nonetheless, I made a short visit (by air) to England after the teaching was over and arranged for my few possessions to be shipped to New Zealand on the same sailing of the *Oriana*.

The return to Florida was into Jacksonville on a new airline to service the southeast. When I reconfirmed in New York I was asked what I would like for dinner and given a choice of six mains. That has never happened since, despite all the flying I have done. I was met at Jacksonville and whisked back to Gainesville and the incessant humidity of Florida in the summer. The few weeks remaining were spent bringing the research to a suitable point for handing over and writing the essential reports. All too soon my final week arrived and all those myriad of minor details that take so long were attended to. I got the essential tax clearance for departure from the US that was never checked, closed my accounts at the Bank and Credit Union, and I rented a large Ford convertible for the drive to Port Everglades with my luggage.

My ticket for the Oriana arrived only on the morning I was to leave and, after collecting it, it was back to the apartment for the final time, sad goodbyes, and then south to Fort Lauderdale and Port Everglades. The large car accommodated the trunk that I had bought for all the things that I now owned as well as my cabin baggage. After some time cruising along the expressway, I noticed a flashing light closing up on me through the rear view mirror. It turned out to be a Florida Highway Patrolman who pulled me over and asked why I was going so fast. Bearing in mind that the maximum speed of a VW was 72 mph, I had no idea how fast I was travelling. Apparently it was 85 with the limit at 65 mph. This officer was far nicer than the ones I had dealt with when I got my licence. Seeing the trunk on the rear seat, he asked where I was going. Discovering that it was New Zealand he said '*And if I give you a ticket I suppose you'll toss it off the rear of the boat as soon as you sail'. 'No officer'* I said, *'I would never dream of polluting US waters; I would wait until we were outside the US limit.'* I was asked to slow down but given no ticket! On arriving in Ft. Lauderdale, I found a motel for the night and then had dinner, which was a lonely affair. My two years in the US had been the best of my life to date. I had been successful in my job, made advances in my career that I had never expected, and had made some really good friends. I was financially sound and had I been able to stay in the US then I would have done everything I could to have secured a permanent post there, but that was not the case. I had to leave for a two year minimum period and, when I looked over the stern of the *Oriana* late the next afternoon at the disappearing Florida coastline, this is what I vowed to do.

A New Life, a New Place, and an Emerging Career

Voyage to the Antipodes

The *Oriana* left port late on the afternoon of Sunday July 25, 1968 and, as I have said before, it took in the Bahamas, the Panama Canal, Acapulco, Los Angeles, San Francisco, Vancouver, Honolulu and Suva before arriving in Auckland on September 21st. Once again, my cabin on the liner was one of the least expensive and on the lowest passenger deck. I had come to like this on the *Queen Mary* as the night before arrival in New York there had been a storm and there was much less roll than higher up. The berth was for four but I had the place to myself. The voyage was very pleasant, especially for the first two weeks as these involved traversing the Caribbean and then north up the Pacific Ocean taking in the North American ports. Grand Bahama was pleasant enough with its markets, restaurants and large sailing club, but I was in no mood for sightseeing so soon after leaving Gainesville. From there, it was a three-day sail to Central America and the Panama Canal and enough time to take stock and begin to enjoy what the next weeks could offer. Our evening arrival in Central America meant that we spent the night at Balboa (Panama City) before traversing the Panama Canal, a fascinating experience. The *Oriana* was one of the largest vessels to pass through the waterway with the clearance to the side of the locks frighteningly small in places. Progress was slow though constant, but it increased once we were in the upper reaches and in Gatun Lake. After crossing it, the 26 metre descent through more locks to sea level at Cristobal was easy and we moved into the Pacific Ocean.

Our journey took us north as far as Vancouver before heading southwards. The first Pacific port was Acapulco (Mexico), where we moored off-shore and were ferried to land by a series of small tenders. I mention this stopover for the notoriety that I gained from it. After viewing the *La Quebrada* cliff divers and looking around the city I made my way back towards the harbour and, having time, entered a café and settled for a Mexican beer. One of the other customers was a crew member that I had interacted with a few times and so we got together, chatted and had another beer. All at once we realised the time – somewhat beyond that of the last passenger tender back to the *Oriana*. We raced the short distance to the quay and

were lucky to find the shore crew still there but packing up. My new found friend was in trouble because his shore leave had expired much earlier and he blamed himself for my being late. The duty crew, far from impressed, made much messaging to the ship by walkie-talkie. By the time the shore gear was stowed, the *Oriana* had raised anchor and was underway. It proved to be a fascinating experience to ride the small craft to the liner, come alongside the large moving vessel and then be hoisted aboard. Once safely secured in the hellions, a ladder appeared at the side of the craft and I was asked to disembark first. What a sight! The available crew and most of the passengers had been gathered on deck. I was given a formal ceremonial welcome aboard, being then passed down a line of officers who increased in rank until I met the Captain. He then introduced me to the Commodore of the P&O Fleet who was on an inspection tour for our voyage. After him was a solitary more junior individual who, quite drolly and almost in an undertone, said: *We know you will never do that again*. How right he was! Of course, the embarrassment remained and gave me more than an element of notoriety.

The voyage continued via the scheduled ports with San Francisco providing two full days ashore as it housed the main US P&O Offices and allowed time for the liner to be restocked at its mid-point on the Southampton–Sydney run. The leg from Vancouver denoted a precise repeat of the on-board activities and, whilst the weather was great, the vistas of continuous ocean instilled a boredom that only landfall would break. The Hawaiian Islands came up and everyone had made plans for their Honolulu stopover given the week in which to do it. Holding a US driver's licence made it simple to hire a car and a 'round Oahu island' tour was made with the car full. In the evening we visited one of the beach bars common before Waikiki gave way to the high rise developments, drank Piña Coladas, then returned to the ship for a midnight departure and days of sailing south to Fiji. There was a full day ashore there and then a comparatively short time to Auckland and the start of a new life.

The *Oriana* docked in Auckland mid-morning on Saturday, September 21, 1968, and the business of clearing customs in one of the staterooms began. I left the collection of my research samples until that morning. On embarking in Florida, I had decided to have the chemicals put into safe storage to avoid possible loss and/or injury to a fellow passenger. No questions were asked when the box of synthesized compounds was deposited or retrieved and I can but wonder what would happen if the same thing was tried in 2011, given all the regulations now in force. I was astonished that my entry to New Zealand was complete within seconds with a single stamp affixed into my passport. Dockside, the trunk that had been on-loaded in Southampton was there as was a 'not needed on the voyage' suitcase, but there was no sign of the large trunk that had disappeared into the hold of the *Oriana* in Port Everglades. It had been incorrectly stowed and it took almost two months

before it appeared in Wellington without any apology or compensation for the inconvenience P&O had caused. What I did have was forwarded to Wellington on the overnight train that I took that evening.

By then, it was afternoon and I took a taxi to the railway station to uplift my sleeper ticket for the Auckland-Wellington night express. Though short, the taxi ride was odd; everything was from an era past – the cars, the streets and the dress of the people. Clearly, the US of A was well behind me and I had the impression that things were even years behind England. On paying the taxi fare, I tipped the driver as one would in the US. This provided me with my first lesson in NZ etiquette, for the driver took the time and trouble to point out that such things did not happen in his country as everyone was paid a living wage. He recommended that I didn't try to do it again, and returned the additional money I had proffered. After dinner it was back to the station and on to the train. This was lesson two for life in NZ. The train was like something out of the ark and bore no resemblance to the one that had whisked me from New York to Gainesville two years earlier, or even the Blackpool–London expresses that I had taken while at boarding school. The seats were uncomfortable, there was no refreshment car, and the sleepers were what I had known as *couchettes* on the European expresses to and from Austria some ten years earlier. There were a few stops and then one, at about 10.30 (my guess is that it was Tauramanui), where everyone in the carriage rushed to the platform. I got a '*come on*' from my fellow passengers. This was the last stop where refreshments could be bought and it seemed that pies and tea were the order of the day. The pie was fine but the hot NZR tea, the first one non-iced in a couple of years, proved undrinkable. We reboarded, the train moved on and, after a fitful night, the ancient train pulled alongside Platform 9 at Wellington Railway station a little after 8.30 am.

Despite this being Sunday and early, Professor Stan Slater, the then Head of Chemistry was there to meet me. After the pleasantries, he took me to his car that had to be at least 15 years old (I gathered subsequently that it was a 1952 model), put my cases in the boot and drove me to my hotel, the Columbia, in lower Cuba St., central Wellington. My room was reserved but not yet available as it was only a little after 9 am. My enquiry about breakfast was met with a: *But it's after 9 and weekend service ends at 8.30*. After much intercession by Slater, it was agreed that a pot of coffee and some buttered toast could be brought for me. At this Stan left, I waited for my breakfast, and really began to wonder where I was. By about 10.30 I was able to get to my room and clean up. After lunch at the hotel, Slater reappeared and showed me around the city. Although I do not remember where we went, the initial view of Victoria University sitting on the ridge of a hill higher than anything in Florida has remained with me. As we approached the steep street upwards I was convinced that the car would never make it, yet with much crunching of gears we

advanced slowly, onwards and upwards, to the Easterfield Building on Kelburn Parade that housed Chemistry. I was given a tour of the Department, told that there was no office available but that something would be worked out, and then deposited back at the hotel.

The First Months

On my first day at Victoria University of Wellington (VUW), I was collected by Prof. Slater and driven up the Dixon Street hill again, and it did not seem quite so bad the second time. I was taken to his office on the third floor of the Chemistry Department, and introduced to the then Secretary, Unity Jones. After the usual pleasantries, we moved into the inner sanctum where we discussed what I was going to do. I was then taken to meet the staff. The first was Dr. Ted Harvey (1925-2008), externally a quite loud and forceful individual who, I was told *will look after you*. Ted was the senior organic chemist after Slater and he attended to all the chemical orders, supervised the stores, and scheduled training for the technical staff. He took me around the Department, introduced me to staff and arranged for completion of the necessary paperwork *in the castle*, the affectionate name then given to the central administration on the assumption that what went in rarely, if ever, came out! I do not recall all that had to be done except that there was little by way of formality. The major concern was over my lack of removal expenses and how it was that I could have arrived without shipping household furniture and kitchenware separately. That I did not have any of these things by virtue of my US furnished accommodations made little impression. I was told that I could stay at the Columbia for a maximum of three weeks, but that most people were out of the place within one. I was given information on the various city suburbs and advised to consult the *Evening Post* 'To Let' section for available flats (not the US apartment). When asked if there was a map of the city that I could borrow, there was a sigh and a look of disbelief.

W.E. (Ted) Harvey *ca.* 1960

Back up Kelburn Parade to Chemistry and Ted took me to lunch in the Rankin Brown Staff Club, arranged for membership papers to be sent to me, and introduced me to various people. Upon return, a decision was reached that the best place to put me was in Bob Hay's office. Bob, another organic chemist, had left on sabbatical leave and was to be away until mid-1969. So I moved into his Level 1 laboratory–cum-office with its window looking across to the Library in the Rankin Brown building and opposite the smaller of the first-year laboratories. The first days were a struggle as I had little idea what was expected of me and I became fully occupied doing things non-academic staff did in Florida. The Columbia Ho-

tel was not particularly comfortable and I needed to find a place of my own. Flat hunters waited outside the Evening Post Building on Willis St for the first edition of the evening paper to appear at about 1 pm. My problems were in working out where the various suburbs were and, with no car, how to get to them. I saw numerous dark, dingy and damp places that convinced me that a car was essential; a new Austin Mini was chosen as my funds were in US currency that gave eligibility to purchase a new vehicle outright. The impact of this was that the car was worth more when it came out of the British Motor Corporation showroom a week later than it was when it was inside. It also accounted for the aged (vintage!) vehicle fleet and the number of old bombs on the streets.

On the Thursday of that first week I was summoned to appear at the monthly Science Faculty Board meeting. Prof. Slater formally introduced me to the August gathering (as it was in those days) and espoused the great things that I was (apparently) going to do for VUW. In those days each Department had as Head, its Professor or, if there was more than one the longest serving of them. Chemistry had three Chairs, Slater (Organic) as Professor of Chemistry, James Duncan (Inorganic and Theoretical Chemistry) and John Tomlinson (Physical Chemistry). The Faculty Meeting lasted for a long time with much formality and a strict code of practice that I needed to become familiar with. Once complete, it was to lunch in the staff club with new found colleagues. On the Friday I was told that there would be little by way of 'To Let' in the paper that afternoon as Saturday was the big day, and that I should get that paper as soon as possible. Indeed, the Saturday evening paper did carry most advertisements for flats and I made appointments to view several. One of these was on Sefton Street in Wadestown for which, after interview, I was accepted by the elderly couple who lived in the upstairs home. In 1968, setting up electricity and telephone accounts was simple. The Municipal Electricity Department had its showroom and office almost next door to the Columbia Hotel and the paperwork was straightforward with the power turned on later that day – no security deposit, merely a small connection fee. The telephone connection was just as easy and made the following day; the account was billed monthly without need for a deposit.

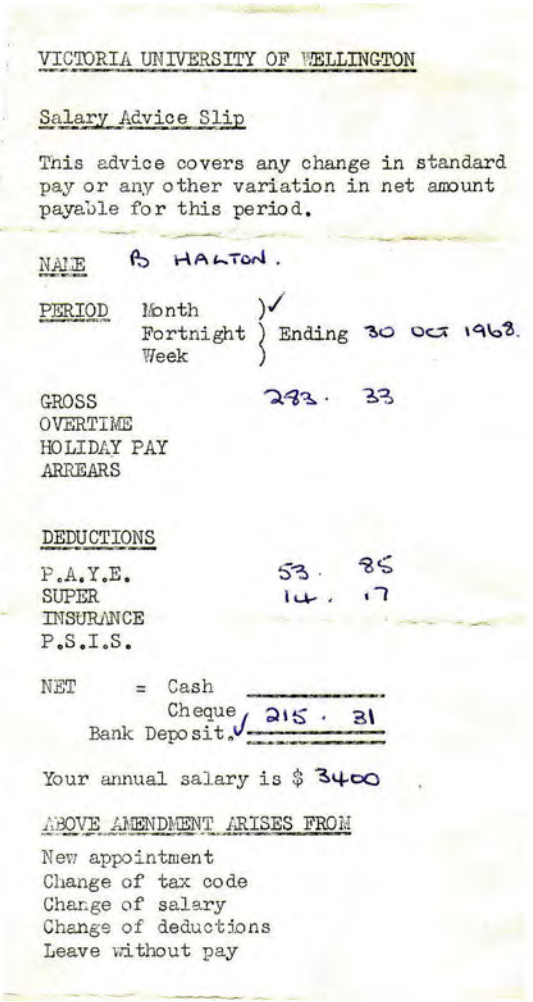
Because I had arrived in September, I had no allocated duties in Chemistry other than to start my research and prepare for the 1969 academic year. However, I was asked to mark some undergraduate and Honours answers to examination questions set by Bob Hay. This I did, subsequently to discover that an additional 5% was added to the Honours papers by the external examiners. Inevitably, having not taught the courses and with no model answers or guidelines to go by, the standard was one that I devised and not that common to VUW at that time; to the present day I have never enjoyed marking what others have set. During those early days, I found that John Craig, another organic colleague and a Scotsman, had been

at VUW for some six years and researched aromatic systems. His work was not too far removed from my own interests in strained molecules and novel aromatics. He was a delightful individual and always helpful; I have fond memories of the years we worked together and our lunch time sojourns to the Staff Club. Ted was the better bench chemist, but he had few students and only rarely published his natural products studies; his last paper appeared in 1979, the year that he moved to become VUW's Registrar. He more than encouraged me to apply for a VUW research grant for a student assistant over the summer months. In those days, there was no application form. Rather, one had to spell out what one wanted and why, and then wait for a call to interview; the panel consisted of the Geology Professor, Bob Clark, his Philosophy counterpart, George Hughes and (I believe) Prof. Gordon of Botany. In due course, I was called for an interview with Clark. This was the first of its kind for me and I was far from comfortable attempting to tell this senior non-chemist academic what I needed money for. Some years later I learned that Bob would give financial support to every new science staff member, but that any subsequent monies depended upon what was achieved. I got the grant and employed Rosemary Coveny, a married third year student, for the summer months as the funding then was sufficient for that. She made a range of starting materials and I prepared several more-specialized small-ring compounds for subsequent use, including the notorious skin sensitizer, diphenylcyclopropenone. As Christmas approached, Prof. Slater advised the Department that he had accepted the position of Assistant Vice-Chancellor and would formally leave Chemistry on January 1, but retain his chair in the subject. What I had not understood prior to this was the need for me as a 6th organic chemist. His move made sense of it all and he never returned to the Department.

Ted Harvey obtained $NZ 550 for me to purchase equipment of my choice. It was the equivalent of a modern set-up grant and it was spent on the purchase of a photochemical reactor as I hoped to pursue further research in that area. A Rayonet unit was ordered from the US, duly arrived six months later and was put to use. It became the longest serving piece of equipment in the Department for it was still emitting radiation in 2007; it blew remarkably few of its four horseshoe interchangeable light tubes during the many years of service. The month of October also saw the Department's first nuclear magnetic resonance (NMR) spectrometer arrive. It was a 60 MHz Japanese Hitachi state-of-the-art instrument that Ted took charge of. Once appropriately installed and commissioned, he invited me to become an operator with one of the Technical Staff, knowing that I had operated both an NMR and mass spectrometer during my time in Florida.

I began to get accustomed to VUW and it to me. I worked long hours at the desk preparing course notes for the following year and I spent my other time at the bench to get my research underway. The pay was low by US standards but I was

able to get by. I assumed that a salary of $NZ 3400 was appropriate for a staff member with a year of teaching experience, but subsequently found that it was only just sufficient, largely because there had been no adjustment to academic salaries for some time; the next review was due only in 1970. My salary increased on the anniversary of my appointment and I was given an accelerated increment to $NZ 3900.



VICTORIA UNIVERSITY OF WELLINGTON

Salary Advice Slip

This advice covers any change in standard pay or any other variation in net amount payable for this period.

NAME B HALTON.

PERIOD Month ✓ / Fortnight / Week Ending 30 OCT 1968.

GROSS 283. 33
OVERTIME
HOLIDAY PAY
ARREARS

DEDUCTIONS

P.A.Y.E. 53. 85
SUPER 14. 17
INSURANCE
P.S.I.S.

NET = Cash
Cheque ✓ 215. 31
Bank Deposit

Your annual salary is $ 3400

ABOVE AMENDMENT ARISES FROM

New appointment
Change of tax code
Change of salary
Change of deductions
Leave without pay

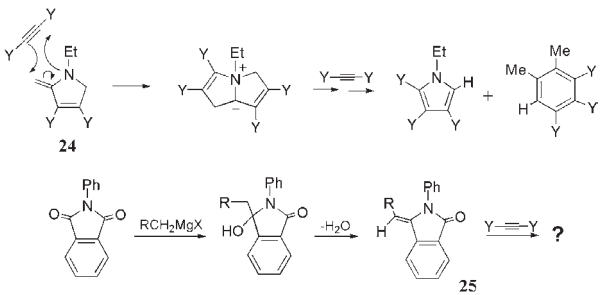
I found living alone depressing and I felt isolated. As a staff member, there was a limit to the socializing that could be done with students and I was the only one single. Unlike Florida, where there had been a lot of social interaction, Wellington provided almost none with Gary Burns being the only colleague to invite me to his home in the first few months. Christmas came and went and I was back in the lab on Dec. 27 having not been told of VUW's two week close down period. I was discovered there by Ted Harvey who took me to his home (where his widow, Helen, still lives) for dinner. During the course of the evening, he explained what Christmas and New Year meant in terms of NZ, the university and academic staff holidays, and so I toured the South Island being back before my colleagues began to reappear. The travel gave me a good impression of the South Island with a return via Christchurch where I stayed with Jim and Fay Coxon for the second time. I had first met Jim (and Fay) when he joined Cookson's Southampton group as a postdoctoral for the 1965-66 year and worked on the bench facing me. Although a year ahead, Jim and I are the same age and had maintained contact while I was in the US. It is not surprising, then, that I accepted their invitation to visit for a weekend shortly after arriving in Wellington. This second

visit was more relaxed and I learned more about his work at Canterbury University where he held a lectureship. When I returned to the office it was as though I had never left – there was still no one about and few ever knew that I had had almost three weeks in the South Island that summer.

Time to Settle In

After the holiday in the South, I found a flat in a large house on Pembroke Road in the suburb of Northland that had a vacancy for a flatmate. I saw the place, met the two men already there (Warwick Jenness, a salesman in boating equipment and Jon Blomfield, who worked for the telecommunications industry as a major installation engineer) and, being accepted, moved into the shared environment a week later. Warwick was a delightful flatmate but often out and Jon, though less outgoing, was very reliable. I got on with them both and enjoyed having people around again.

As the weeks and months passed, I began slowly to appreciate Wellington. After an initial impression that winds blew continuously, I became able to distinguish between a gentle Wellington breeze and a decent storm, and even walk up the hill from the city to Victoria. Teaching was going well (4th-year Honours courses on non-benzenoid aromatics and physical organic chemistry, and more routine 3rd-year undergraduate work) and the research programme began to pick up pace. The allocation of Honours students entering their 4th-year in 1969 was made at about the same time as the arrival of the NMR instrument the previous spring, and I was allocated Ang Weng Soon, a Chinese Malay. He was more than happy to be with the new lecturer and settled on a project involving three-membered ring nitrogen chemistry; I gave him reading material for the summer months. Ang started laboratory work for his MSc (Hons.) degree, with its 12 month maximum completion period, before formal registration early in February, and by the end of that month I had written my first reference. He applied to the Singapore-based Lee Foundation and was one of few in NZ to have success in gaining a grant. Of course, as I had no experience of the NZ science scene and the laboratory courses involved in the undergraduate programme, I did not know what I could (or should) expect of him and how much, if any, of the work I would need to do myself. As it happened, Ang was a keen learner and we got on well together. He made a foray into the heterocyclic chemistry associated with my own aziridine PhD studies. The intent was to see if methylenepyrroles **24** would add dimethyl acetylenedicarboxylate (DMAD) and, if so, what products would ensue (Scheme 8). At that time, methylenepyrroles of type **24** were unknown and so we set out to synthesize various *N*-phenylphthalimidines **25** and find if they would react with DMAD; Ang got no further than to make a series of compounds and establish the stereochemistry about the exo-



Scheme 8

cyclic double bond during his MSc degree, for which he was awarded an upper second class. The work was written up during 1970 and formed my first publication from NZ.[23] Ang returned home to Penang (Malaysia) where, for some years, he was employed in the rubber industry. However, he decided to move to a business venture and set up his own firm in Singapore. He married, has two children, and now works only for two hours each day – long before me. Although he has remained in contact since those days, we have only once met, and that was on my first visit to Hong Kong in late 1988. Ang was there on business but we had a delightful afternoon together and followed it with dinner at one of the Floating Restaurants in Aberdeen.

Those early times in Wellington signified a further break from my origins as several of my relatives died. The first was my mother's twin sister, Anne, who suffered a fatal heart attack in October 1968 aged only 56 years, and she was followed by John Parker, the husband of my cousin Julia, who died in a diving accident, and then my maternal grandmother.

My 1969 allocation of laboratory supervision included one afternoon session a week with each of the second- and third-year students in the second floor organic laboratory, and a first-year lab on Level 1. The academic in charge of the organic laboratory was Ted Harvey and I can still picture him telling the students that I could do everything they had to do in half the time using bench skills that were on a par with his own; high praise indeed from one whose skills exceeded mine quite considerably. The first year lab covered the General Chemistry programme of the day, which at that time took the full teaching year with the lab and lecture programmes integrated. I think that there were about 350 candidates enrolled. Most of these were medical and engineering intermediate students who spent their first year of tertiary education at the local university and then, if selected, moved to the specialist schools elsewhere in NZ. Many non-chemistry science students were required to have a chemistry input of first and (for some) second year study. Dr. Malcolm Carr was in charge of the 1st-year studies, a position designated as a Senior Lectureship, and his noon lecture was repeated at 4 pm. Malcolm was the remaining organic chemist, but did little by way of research, preferring instead to concentrate his efforts in the teaching environment. He was a delightful individual and great to work with but he moved to the new Waikato University in late 1970.

There he taught the first science courses in 1971, taking charge of the introductory programmes and subsequently doing much for New Zealand science education.

The introductory chemistry practical course involved analytical chemistry for the first twelve weeks (the first-half year) and general and organic in the second period. My first half-year afternoon class comprised some 70 students and I was allocated demonstrators at the rate of one to twelve students (a ratio that has worked well for more years that I care to remember), so there were five demonstrators with me. By the time the mid-year came, Bob Hay had returned and was none too happy to find his office-lab occupied. As a Senior Lecturer he got his office back and I moved to the second floor and took over E208, Alan Freeman's small office-lab as he had, in turn, gone on leave. In reality, this office was the preparation room for the Easterfield 206 lecture room and located at the southern end of the Easterfield Building, behind the E206 lecture room. Entry to the office was by way of the E209 research laboratory. Other than that door, there was one that led into the lecture theatre but access was prevented by the desk; there was also a double-sided fume cupboard that served both the prep room and the lecture theatre. Even now, after 40 years and major building renovations, the fume cupboard is still evident, though sealed up with some, now historic, molecular models remaining in it. The office itself was completely hidden from those not in the know and meant few extraneous visitors. On moving in, I made the point that although I was the last one to arrive, I did need a permanent home and, as this was on the organic chemistry floor with my colleagues, I should be left there. I remained in that office until the 1980s when I moved to Room 210 overlooking Kelburn Parade.

The annual NZ Institute of Chemistry (NZIC) conference was always held in the common university break during August and in 1969 the venue was Dunedin. I needed to attend and meet colleagues from the other universities as well as present some of the US-based research work to show the sort of chemistry I was involved with. Ted Harvey was the Hon. General Secretary of the NZIC, a role that he held for some 20 years. He took the trouble to introduce me to all of those organic chemists I had not met and many of the industrialists as well. In fact, throughout my first years at VUW, Ted invited me to socialise with the NZIC Councillors after their deliberations, which in those early days spanned a two-day meeting three times a year. Each of the annual conferences included a Student Papers Competition with one entrant representing each university; it was taken seriously. Margaret Leach, a PhD student with James Duncan and a demonstrator in my first half-year laboratory, was selected as the VUW candidate.

Jim Coxon had come down from Christchurch by car with a couple of his research students that included Dick Garland who subsequently joined NZ Pharmaceuticals at its inception and rose to Managing (now Executive) Director of the company. That vehicle, decidedly old, became the mode of transport for es-

capes to the local hostelries and was always well loaded with a group of us that included Margaret. After our return, we saw more of each other and on November 4, we drove to Havelock North (where her parents lived in retirement) to seek their approval to marry; we became formally engaged on November 5th. By then, my mother had set her plans to spend some six weeks in NZ over the 1969/70 summer – as much time as her job would allow; she met her future daughter-in-law on arrival.

By that time in November, the teaching year had ended and the formalities of settling final grades had passed. It provided me with an experience very different from the US scene. At VUW, there was a much greater effort to ensure that candidates close to the borderlines were fairly assessed. Answers from those on the borderlines were revisited and marks adjusted if appropriate. With that done, meetings to settle the 1970 teaching duties were held and I began to appreciate how easily the long summer *research period* could vapourize, more so as Margaret and I started to think about a future life together and where we could live after our May wedding. I had arrived in NZ with some significant savings and there was enough left for us to make a deposit on a house, provided we could get a mortgage. After much searching, we located a property at 52 Croydon Street in the suburb of Karori, some 4 km from the university, had our offer accepted and secured the needed two mortgages. Settlement was set for late February 1970 and Margaret moved in while I remained in the Northland flat until after our marriage. As Margaret's parents lived in Hawkes Bay in retirement, our wedding was held in Wellington on May 9, 1970, at St. Teresa's Catholic Church in Karori with both Catholic and Anglican priests officiating.

A Career Develops

Once home from honeymoon, Margaret returned to her PhD research and I was back into teaching. Ang had completed his thesis, been examined, and left. I had gained no new researchers for 1970 and had no one active in the laboratory. However, I was fortunate in gaining a new grant from the Faculty Research Grants Committee and I employed Paul Milsom as my research assistant – but more of that later. I began to feel the need to make known my presence as an academic. After some deliberation, I came to the conclusion that the best way to do this would be by way of a textbook. The chosen topic was *Organic Photochemistry* as there were few books on the subject suitable for senior undergraduates at that time,

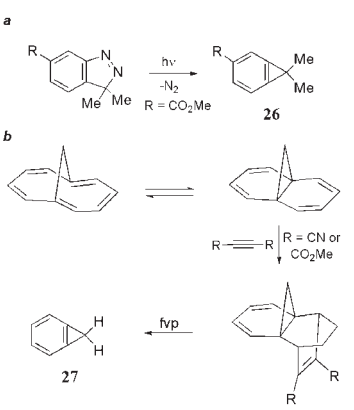
and my interest had been kindled by the Florida work; it also gave me a job that could be done privately at my own convenience. However, as this was my first such venture, I sought a like-minded co-author and a contract from a publishing house. Jim Coxon joined me as co-author and Prof. Michael Grundon in Northern Ireland accepted our proposal for the book (subject to acceptability) into the *Oldbourne Chemistry Series* for which he was Editor. A contract was executed at the end of May 1970 with delivery of the text set for June 1971. Jim and I decided on content, divided the sections between us and scoured the literature reports prior to writing. The task of putting words on paper did not come easily but, eventually, I found that jotting down ideas in the evening and then rising early the next morning to commit pen to paper before going to the office, worked best for me.

By autumn, 1971, the draft text was written, the writing checked and many amendments made. Nevertheless, we needed to unify the style and settle remaining discrepancies. This was done over a long weekend at a beach house in Akaroa, near Christchurch. Thereafter, the final manuscript was typed in Wellington by former chemistry secretary, Rhyl Singleton, who subsequently returned to the Department after her children attained school age; she remained with us until about 2001, shortly after she was diagnosed with cancer. The completed manuscript was dispatched by air to Grundon, who liked what he saw, edited lightly and accepted the tome into his series. The manuscript was passed to Oldbourne Press where it sat for almost two years with no progress to print despite efforts to elicit explanations and a publication date. Excuses were plentiful and apologies prolific until, finally, we were told that the series was to be terminated. A breach of contract payment had to be made by Oldbourne and they were generous enough to pass our manuscript to the same Ken Schofield at Exeter who had been my external PhD examiner; by then he was the Organic Chemistry Editor for the newer Cambridge University Press series, *Cambridge Chemistry Texts*. In short order, Ken accepted the book into the series, allowed us to update the material, made a few amendments himself and the book appeared[16] in both paper- and hard back versions in

mid-1974, and as a second edition in 1986. Surprisingly, as this is being written, CUP has advised it is interested in releasing the same 2nd edition as an 'e-book' although it would be hopelessly out-of-date.

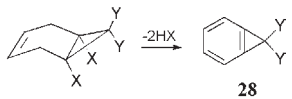
The most notable event in Chemistry at VUW during 1970 was the creation of the first (and only) Chair in Organic Chemistry. The position was advertised in March and two candidates were short-listed, with interviews taking place in June. The first, Bill Crow from the Australian National University, was congenial but did not impress the selection committee (whose composition excluded the organic chemists) as much as the more austere Robin Ferrier, a Scotsman from London's Birkbeck College, who was appointed. He arrived some months later, in mid-spring, and ahead of his family, intent on settling into the university and finding a home. Margaret and I decided that he should not be left alone as I had and we entertained and assisted in whatever way we could. In fact, the day his wife, Carolyn and the two children arrived in December, we were taken to their temporary accommodation in Kelburn to meet them and have a meal. During the following twelve months we saw Robin and Carolyn socially at both their home and ours. However, a year after arriving, at an end-of-year post-Christmas barbeque at his home, Robin told me that he thought it no longer appropriate for a Professor to socialise with junior staff – and that was the last time that Margaret was invited to his home. He became a difficult colleague to work with, probably because he felt that NZ should treat professors as in the UK, and it led to several strained relationships. For my part, I got along well enough, even to the extent of agreeing to collaborate in some of Robin's work with unsaturated carbohydrates. I say agreeing as it never actually eventuated. As best I recall, Robin had a sabbatical visitor who was to provide some unsaturated compounds that were to be cyclopropanated. My involvement ended when I was asked not to waste the visitor's time and only enter discussion when he got to the cyclopropanation part I was to be involved in.

Paul Milsom had been in the laboratory since March 1970 as my second research assistant. He made excellent progress in the *benzocyclopropene* field by starting what was to become my life's work. The now used 'cyclopropabenzene' was undefined in 1970 and both terms appear in what follows. My interest with such molecules came from my Florida studies that had required the cyclopropanated phenanthrenyl cation **19** (Scheme 6) as a fragmentation product in the mass spectrum of triphenylcyclopropenyl halides. Little was known about the series of compounds from which such ions could be derived. By 1964, Anet and Anet[24]



Scheme 9

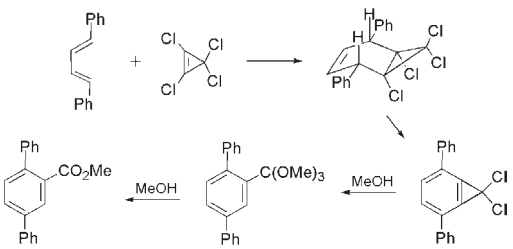
had synthesised the first benzocyclopropene **26**, and Vogel and his group reported parent **27** a year later in 1965 (Scheme 9).[25] As our 1968 US study was concluding, the Vogel group reported a simple synthesis of the *gem*-difluoro derivative **28** by way of a double dehydrohalogenation.[26]



28

When Paul started his study, evidence had been gleaned to suggest the intermediacy of benzocyclopropenones,[27] and Dürr's group in Saarbrucken had reported[28] a new photochemical synthesis of the ring system. Yet, despite these reports, little had been done to study the ring system systematically, explore the family of derived compounds, or seek evidence for the derived cations, anions or radicals. This we hoped to rectify.

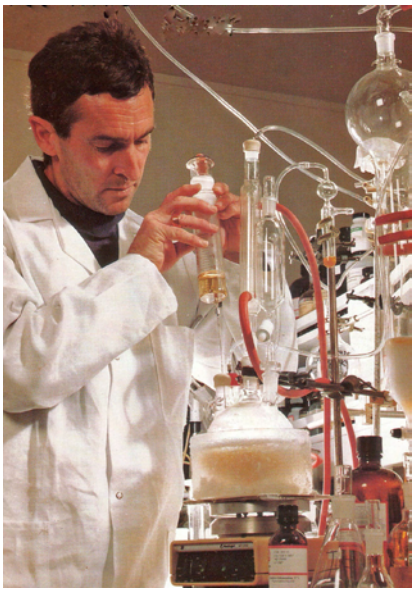
Paul's task was simple: to provide a straightforward, high yielding synthesis of an ionizable cyclopropabenzene in an amount that would allow for systematic chemical and spectroscopic study. By the NZ spring of 1970, Paul had evidence for the first of the Wellington cycloproparenes, **29**. He was competent at the bench, got results quickly and returned for an MSc Honours degree in February 1971, by which time he had learnt how to obtain the dichloro derivative **29** pure and in high (93%) yield (Scheme 10). His results were published in communication form that year.[29] Paul was as enthusiastic a tramper as he was a chemist and, even with the stimulation of his publication, he did not get his thesis submitted until 1975. His preference had been to explore the mountains of South America and parts of the USA, but subsequently, he rued not returning for PhD training; the idea of a return to academic research stayed with him for more than 30 years.



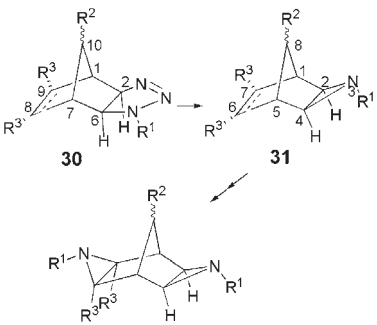
Scheme 10

29

In February 1971 I started my first PhD student, Tony Woolhouse, with whom I have a continuing friendship. Tony had completed his MSc degree with John Craig, gained 1st Class Honours in late 1969 and worked for the Wellington Hospital Board in the metabolic unit laboratory for a year. However, he decided that research work was more what he wanted and returned to VUW for PhD study under my direction; he made the lab buzz. He was, and still is, a remarkable enthusiast, thoroughly enjoying the challenge of bench chemistry. Even today at Industrial Research Limited (formerly the DSIR that he joined in early 1976) he continues as a model bench chemist. Although he had a wife and two small daughters, he worked long hours in the lab, ran marathons at the weekend for relaxation, and had gained more than enough results for a PhD thesis by mid-1973; he left for postdoctoral work in Liverpool with Prof. Charles Rees in October of that year. The eight



Tony Woolhouse at DSIR *ca*. 1980



Scheme 11

papers that came from his thirty months laboratory work cemented my position at VUW and internationally, and his completion time, within the three year norm, was a record in the Department.

Tony started working on the synthesis of aziridines, with a view to preparing tricyclic derivatives from bicyclo[2.2.1]octanes and -octadienes (Scheme 11) for thermal and photochemical ring-opening of the three-membered aziridine ring. In essence, azide addition to a bicycloheptene or -heptadiene led either to a single isomer or a mixture of *endo/exo* stereoisomeric tricycles **30**, with the orientation a function of the C10 and the C8/C9 substituents. Products in the diene series were sequentially transformed into bisaziridines. Photochemically induced loss of dinitrogen was found best and a series of all the possible aziridine isomers, *e.g.* **31**, was characterized and subsequent ring opening followed. The ultimate ring opening reactions were soon settled and the study was completed, but with insufficient data and scope to lead to a thesis.[30] Tony then moved into the more profitable cycloproparene area.

By this time, it had become apparent that the early historical studies laying claim to cycloproparene synthesis were most likely incorrect and needed to be repeated. Tony's initial foray was a repeat of a 1953 study reported by the Egyptian workers Mustafa and Kamel,[31] which we gathered had been done by Harold Shechter and his group earlier, but not published. After writing and gaining no reply, we proceeded to the laboratory and showed that the claim to *gem*-diphenylcycloproparenes from diphenyldiazomethane addition to, and dinitrogen loss from, *N*,*N*-dibenzoylimines was incorrect. While the additions and losses of molecular nitrogen did occur, the products were simple cyclopropanes and not their aromatized analogues. Very shortly after this work was completed, a delightful letter from Shechter arrived telling us that my enquiry had fallen behind a filing cabinet and that should we have repeated the work then we should publish jointly, which we did.[32]

During that year of 1971, it became necessary to make some serious decisions regarding research in New Zealand, and specifically at Victoria University. Research groups were small by international standards, frequently comprising only one or two 4th-year (Honours) students. Instrumentation was barely adequate despite laboratories that were more than appropriate. Monies, which were never great, were made available to each university by the (national) Universities Grants Committee, with funding agencies evolving only in the late 1980s. Each institution allocated its monies, not to individuals but to Departments, based, to a point, on budgets submitted. In chemistry this led to separate allocations for organic, inorganic and physical, from which the duly appointed professor allocated teaching monies, but no individual amounts; each of us in organic chemistry had the ability to draw on the whole for our area on the assumption that if one individual had an expensive year it would not be repeated in the following one. Bob Hay, who returned to Scotland (Stirling University) that year, had wanted this split into individual accounts but it never happened. Chemicals were not easy to obtain and impossible to get if needed in a hurry – unless one of the researchers elsewhere had the compound and was willing to lend some, a regular occurrence. Despite this, Ted Harvey had maintained a tradition of annually adding to the range of chemicals held in the store and the list of compounds was extensive.

But this was neither the US nor the UK. A decision had to be made whether to follow modern international research trends and try to keep up without being scooped by others overseas, or to select an area of work that one could research and become recognised for. The last appealed as benzocyclopropene chemistry fitted well to the philosophy. Thus, 1971 saw my work move almost exclusively to the cycloproparenes, a field that I had never anticipated staying with for the rest of my career. The year was notable because Margaret completed her PhD in May and then, later, I was elevated from Lecturer to Senior Lecturer. I had let a select few know of the textbook and its acceptance into a known chemistry series, and this must have helped. In those days, and until the late 1980s, promotions could not be applied for rather than one's nomination being made by the Head (subsequently Chairperson) of the Department. I saw James Duncan and Robin Ferrier to offer my thanks for their efforts on my behalf. James was as gracious as he always was. However, Robin was surprised and said that he had had nothing to do with the promotion. A few days after the formal announcement, Stan Slater expressed his delight and invited Margaret and me to a musical evening at his home, his way of offering congratulations. We arrived to find that Alan Clark, a PhD chemist but lecturer in biochemistry, had also been promoted and he too had been invited. Stan was a classical music buff and had a room specially set up for listening with 1970s state-of-the-art reproduction equipment. After being asked what we would like to drink, Alan and I were given bottles of spirits, mine a scotch and Alan's a brandy,

both with a jug of water. To this day I have never been able to decide if this was out and out generosity or a method of measuring intake, but I err on the side of generosity.

The same year saw me invited to spend six months at the University of New South Wales in Sydney as a UNESCO lecturer, a title more grandiose than the job itself. In essence, UNESCO provided funding for a lecturer for Asian students enrolled at NSW, but the School of Chemistry used the money to solicit visiting teaching staff for six month periods. Gary Burns, my inorganic chemistry colleague, held the post for the second half of 1971 and VUW gave me unpaid leave so that I was able to take over for the Jan-Jun period in 1972. This provided Margaret with postdoctoral work with the late Ray Golding (1935-2009), a New Zealander who had worked in DSIR at Petone prior to moving to Sydney.[33] I left Tony and my second PhD student Alan Browne, who started up after we left, to fend for themselves – but with detailed reports air-mailed to Sydney every two weeks.

The Australian Experience

The move to Sydney for six months meant that our car had to be sold and the house rented to pay the mortgage. The latter took more time (and trouble) than we had imagined, but both were achieved before our Christmas at home with Margaret's parents and a December 28 evening flight to Sydney. We had accommodation in a Randwick Motel for a few nights, with our initiation and settling to the city eased by the presence of my cousin Bob Robinson (the son of my mother's brother) and his wife. They had emigrated from the UK a year or so earlier, liked the Sydney lifestyle and were generous with their time to us. After viewing several apartments, we found a fully furnished, one bedroom place in a high rise block at 56 Anzac Parade in Randwick. It was a fifteen minute walk to the university, just down the road from the Sydney Cricket Ground and various parks, and but a short bus ride into the city. The block of flats is still there as is the small garage opposite, though it is no longer the VW specialist repair place that it was.

Monday January 3 signified sign-on day at UNSW. I was assigned an office within the 1st-Year Teaching Unit where I was to be based and met its Director, June Griffith (1925-1978), a delightful outgoing lady who made our stay in Sydney so enjoyable. Margaret linked up with Ray Golding and we were quickly into work. For my part, I had lectures in General and Organic Chemistry to prepare but there was nothing dramatically different from my U of F and VUW work (I had taken over the 1st-year organic course in 1971 as Malcolm Carr had moved to the then new science faculty at Waikato University).

We bought a VW Beetle, older than the one I had had in Florida, but it let us see something of the Australian countryside and its wildlife at the weekends, as I had

seen nothing and Margaret little of Australia's indigenous species. We began to explore further afield, quickly adapting to the Australian traveller's Sunday 'sign-in'. This was when a *bona fide* traveller, who had journeyed a minimum distance was required to register upon entering any hotel or pub before gaining service; it always proved interesting to see just where those locals thought they had come from! We spent a weekend in the Blue Mountains at Katoomba that included the Scenic Railway with its steep descent into the valley near the Three Sisters Peaks, and we spent Easter in Canberra. Our time in the Randwick flat saw us visit the excellent Elizabethan Theatre, seeing Leo McKern in *Mutiny on the Bounty***.** The shopping area nearest to us had a range of food establishments of which *Tiffany's* was big in precooked foods and specialized in main meals. That and *Mr. Chips***,** a fish and chip place closer to the campus that sold boxed meals more akin to the fillet dinners now common, provided us with numerous evening meals at little cost. We absorbed our temporary environment to the full.

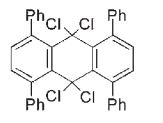
My work at UNSW went well but I caused a few rumblings when I let it be known that I intended using my research time to write a review on the cycloproparenes, as research was not common among the 1st-year unit staff. I had provided a proposal to Harold Hart, then editor of *Chemical Reviews*, seeking his approval for the first on this topic. It came in mid-January. Subsequently, I set to work, with a schedule for completion by the end of May, and the manuscript was submitted before we left. It appeared in the April 1973 issue of the journal.[34] I also expected to use some of my time to meet with colleagues in other Chemistry Departments. Setting up visits to the Sydney universities was simple enough and I enjoyed those. An overnight visit to ANU in Canberra, with Ron Warrener as host was also a part of the schedule. It proved to be an enjoyable and memorable visit as Ron had done a lot of photochemical work in an area that had much appeal, and I got along well with him and his then postdoctoral, Michael Paddon-Row; contact with both was maintained for many years and I spent two months in Ron's laboratory in Central Queensland in 1999.

During the period after Easter, Margaret found that she was pregnant and the emergence of a soup craving came on our journey to Melbourne. I had convinced my colleagues that I would be able to fit the time needed into the main teaching break and arranged to lecture at Melbourne and Monash universities, the latter housing Roger Brown as a Reader. We drove down to Melbourne on the inland Hume Highway (a two-way road for much of its length then) and spent the rest of the weekend exploring the city and its environs. Monday was my day at Melbourne University. I was made very welcome by Prof. Don Cameron who, for whatever reason, became very supportive of me and my subsequent collaboration with Dr. David Kelly, an organic chemist who ran the NMR facilities. The instrumentation there far exceeded that at VUW and we soon agreed upon a collaboration that gave

my small group in Wellington access to facilities unavailable in NZ. Although I met Don on a number of occasions, it was during the period when he was Chairman of the Editorial Board of *The Australian Journal of Chemistry* and I was a member (1994-99) that I really got to know and respect him and the then Editor, Dr. John Zdysiewicz (1943-2010), who became a true friend. Tuesday was spent at Monash University in Clayton but, sadly, Roger Brown was away and David Black (whose successes since have been especially notable) hosted me. The day of meetings was good and I have retained contact with many of the people although no collaborations followed; we hold fond memories of a very pleasant dinner with David and some of his colleagues with his wife in their home. Wednesday morning saw us heading back towards Sydney but via the coastal route. The remaining time in Sydney went well with June Griffith great to work with; sadly she died an untimely death from cancer in 1978.

Our morning Sydney-Wellington flight was diverted to Auckland due to high winds. However, whilst still at cruising altitude, we flew over Wellington from the south in clear skies and with a view of the north island from its southern tip beyond the Wairarapa to the right and Mt. Egmont (as it then was) and New Plymouth to the left. It was a sight that I have never forgotten. By the time we had landed in Auckland, Margaret was feeling none the better for the flight and I remember getting her into a sick room as a pregnant lady. Whether this impacted or not, I do not know, but we got back to our Croydon Street home late in the afternoon, with minimal delay. The second half of 1972 saw Margaret continue her research at VUW, with her pregnancy becoming ever more obvious, while I had to contend with a full year's teaching; we were both busy. Our first son, Mark was born on Dec 22.

My new PhD man, Alan Browne (1948-2001), had settled in as Assistant Radiation Safety Officer as he had no scholarship. The minimum time for a part-time PhD submission was three years, but in reality it was always closer to a four-year year period. Both he and Tony Woolhouse coped well during my absence and with the little time I was in the office because of the heavy teaching load after returning. I had a dozen lectures each week, tutorials, labs, and meetings of the committees to which I had been appointed during my absence. I vowed never to get into the same position again. During that period, Tony had covered the formation of dimers from cycloproparene **29**. These were not the simple ring-opened head-to-tail or head-to-head dimers **30** and **31** that intuitively one might expect but the more complex isomeric *E/Z* bicycloheptatrienylidenes **32** and **33**. The structures of these products were unable to be settled by simple mass



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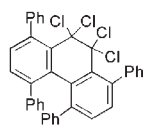

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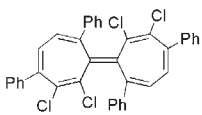

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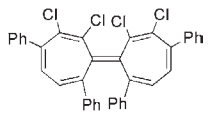

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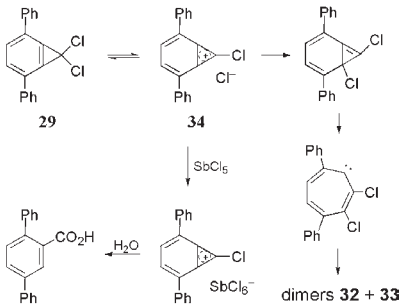
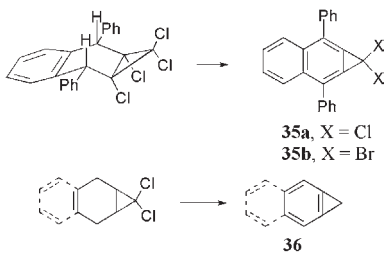


Scheme 12

spectrometry and ^{1}H NMR spectroscopy, and so the offer of assistance from David Kelly was taken up. Despite the ^{13}C spectra,[35] it was some eight years before the structures were finally established by X-ray crystallographic analyses, with the mode of formation likely involving ionization and recombination to a bicyclo[4.1.0]hepta-1(7),2,4-triene (Scheme 12).[36] Tony also settled issues regarding the generality of the double dehydrochlorination route of Scheme 10, the replacement of the *gem*-dihalo substituents of the cycloproparene, and the stability of the various derivatives.

Subsequently, Tony showed that the instability of *gem*-dichlorocycloproparene **29** to protic media was due to ionization to cation **34** and he devised an appropriate procedure that allowed for its isolation as a highly moisture-sensitive antimony salt (Scheme 12). As there was no facility to carry out low temperature NMR studies in Wellington, the procedure was forwarded to David Kelly after Tony had left, and the work was repeated in Melbourne by Helmut Hügel; the vital spectroscopic data that characterized the first cation in the cycloproparene series was gained.[37] In fact, David collaborated with me for many years before becoming Dean of Science at La Trobe University, while Helmut is at the Royal Melbourne Institute of Technology and was my equivalent as editor for *Chemistry in Australia* until 2008.

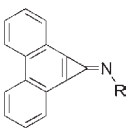
In comparison with Tony, the 1972 new man, Alan Browne, began our foray into cyclopropenes fused to other simple aromatics. During his first year, he synthesized the first cyclopropa[*b*]naphthalene **35a** (86%) (the benzo analogue of **29**) using precisely the same chemistry[38] and showed that its properties were analogous but with a somewhat more stable ring system. He also obtained dibromo **35b** unexpectedly from a halogen exchange. Subsequent work led to our only case of being scooped to publication. Ed Billups at Rice University in Houston had published[39] a straight-forward synthesis of parent cyclopropabenzene (**36**) in 1971 and an extension of this to the naphthalene analogue **36** (benzo fusion) seemed simple. Alan had this done with the compound fully characterized, but he isolated a series of side products whose proportions were dependent on his reaction conditions. While this was being sorted out, the Billups paper appeared and Alan continued on, providing a simple semi-kinetic analysis of the reactions that led to the benzene and



naphthalene derivatives before beginning our attempts to generate an angular naphthalene analogue, the cyclopropa[*a*] derivative.

Sabbatical Leave and New Colleagues

The year of 1973 saw me hosting a sabbatical visitor, Charles Spangler with his wife Brenda and their children. They came from De Kalb, Illinois (Northern Illinois University), for some six months and we had accommodation arranged for their arrival; we entertained them and they us during a happy time. Charlie was keen to do bench work and he started to re-examine one of the earliest recorded syntheses of cycloproparenes, namely the formation of iminocyclopropa[*l*]phenanthrenes **37,** as reported in 1930 by the Indian workers De and Dutt.[40] The study was completed by Honours student, Silvana Harrison the following year by showing that the original study could not be replicated because entirely different products were obtained.[41] During Spangler's stay I began to think of my own sabbatical leave, which accumulated to a maximum eligibility of 270 days in August 1974. With a widowed mother in England, leave had to be in the UK and that would also give Margaret the chance to see my homeland for the first time. In order to avoid three generations living in the same house, and to fit with my chemical interests in photochemistry, leave at Reading University with Derek Bryce-Smith and Andrew Gilbert fitted best. Arrangements were made easily and VUW approved the leave in August 1973 for the following year.



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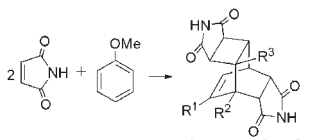
Despite Margaret being hospitalized in December 1973, I was able to attend my first international conference outside NZ, namely the Royal Australian Chemical Institute meeting on Philip Island, near Melbourne, late in February. It provided an opportunity to revisit David Kelly and settle more work and, more importantly, to present some of our research under the heading: *Developments in the Chemistry of Arocyclopropenes*. More notable was the presence of J. E. (Jack) Baldwin, then at MIT, who spoke on *2,3-Sigmatropic Reactions* and subsequently almost drowned while swimming. During the year, Alan continued as my sole PhD student and Silvana Harrison was joined by Teng See (later Christine) Chuah in what was to be the final year of Victoria offering an MSc (Hons) degree over a 12 month period. The move to a BSc (Hons) structure saw a gradual and continuous reduction of the research component, to the extent that publications from the honours projects diminished. In that final 12-month MSc year, both of my students were involved with our early approaches to the cyclopropa[*l*]phenanthrene ring system that were published.

The British Council offered Fellowships to a limited number of NZ resident academics and I sought funding for the sabbatical travel scheduled for late No-

vember. The outcome was not to be decided before we left NZ and I was advised that there were many applicants. It was in my second week in Reading that I was told I was to receive the grant. Our initial accommodation was with my mother in the same Lee SE 12 home I had lived in and we had a week there before I moved to Reading. Margaret rapidly warmed to nearby friends Sally and Peter Civil, who had been looking after my mother more and more since she was on her own.

My time in Bryce-Smith's Reading laboratory began at the beginning of our second week in the UK and I stayed in Whiteknights Hall, one of the student/staff residences, for the weekdays. I was made very welcome there and in Chemistry, especially by the former Southamptoners Gerry Fowles and Monty Frey, who were still active. By 1974, Derek Bryce-Smith had become renowned for his environmental work on roadside lead pollution from exhaust emissions and he spent most of his time on that, leaving Andrew Gilbert to run the photochemical programme. Andrew was a delightful colleague who, in my view, never gained the recognition that he truly deserved, as it was he who drove the photochemical studies. I settled to photoaddition reactions of aromatic ethers, specifically the photoaddition of maleimide to anisole, gained much valuable experience and a publication that appeared in 1977 describing the isolation and characterization of **38-40** (Scheme 13).[42] The Chemistry building was relatively new, stood alone, and had the distinction of research laboratories with windows above head height. While this provided more wall space for cupboards and shelving, the reason given to me was that earlier, the somewhat notorious Professor Guggenheim had not wanted students distracted by pleasant views. Daylight was there though, thus making it very different from the central, totally enclosed laboratories of Utah that I experienced later. The first few days provided an opportunity to visit *The Old Crown* in Gallowstree Common that we had agreed to rent from Dr. Neil Isaacs after Christmas. Neil, a physical organic chemist, was to spend a leave in Auckland with Prof. Peter de la Mare and depart for NZ close to Christmas, returning shortly after our planned departure at the end of July 1975. The property and the rental were too good to turn down. The converted old pub in the small village was more than suitable as it had four bedrooms, three for our use, and all the modern conveniences of the day. In our time there, we experienced only one problem and that was with the heating system, and not the resident ghost who we never met. I returned to Lee on Friday evenings and so began some weeks of commuting until after Christmas.



38, R^1 = OMe, R^2 = R^3 = H
39, R^1 = R^2 = H, R^3 = OMe
40, R^1 = R^3 = H, R^2 = OMe

Scheme 13

Whiteknights Hall was some 15 minutes walk from Chemistry and I recall feeling that the air in Reading never seemed to move. On one particular morning, the stroll across campus was much more enjoyable as there was a gentle breeze. When

I got to the office, I was greeted by complaints about the wind and asked what it was like in Wellington! My time in the Hall covered the pre-Christmas festivities of which there were several. I was invited to the formal Christmas Dinner, a quite grand event, and then, afterwards, to the Warden's residence for port. The port was excellent and it was only the next day when I learned that, traditionally, each warden laid down port for a Christmas function many years in the future. Christmas was spent at 31 Heather Road and provided a further opportunity to visit relatives and friends from the past, notably Chris Watts, who by then was working for Plessey Company and not using his chemical background any longer. Margaret met several of my relatives and, as fate would have it, a cousin and her husband lived in Reading, he the grounds-man of the Gallowstree Common playing fields just down the road from *The Old Crown*.

The Old Crown, Gallowstree Common, 1975

After New Year, we moved to Gallowstree Common to find that the Isaacs had left their home in excellent order; all that we needed do was to turn on the heating and get the place warm. The ensuing months there were happy. The university was easily accessible by car but not by bus. There was a service but it took over an hour, needing a change at Reading Station. We met the neighbours, including ones that the Isaacs had advised of our arrival and who had small children, and Margaret got to know some of them well. She took Mark for walks through the neighbouring woods, enjoying the openness of the English countryside in comparison to the bush of NZ. Our nearest neighbours were a delightful older couple (George and Mary Holmes) who offered to look after Mark should we wish to have an evening out. Mary, especially, was very friendly and we remained in contact with her until 2008 when she died. Our weekly shopping was done in Henley-on-Thames as it was no further than central Reading, yet easier to access by car and park.

In late January, I left for a week in Switzerland and Germany. I had visited a couple of the London Colleges to give seminars and meet staff before Christmas. But a visit to continental Europe was vital because of the small-ring chemistry being studied there. Paul Müller at the University of Geneva had collaborated with us on cycloproparenyl cation issues after the publication of our landmark communication and I wanted to meet with him. In fact, he was responsible for Bryce-Smith offering me more respect than to many of his staff. In late 1974, the English universities were in financial difficulty, manifest in Reading by only Senior Lecturers (or above) having direct dial for local telephone calls while Professors

were allowed long distance access. That I received calls from a Swiss Professor caused confusion as no one knew where I should speak to him except by using *the Professor's office*, which I had to do. My visit to Geneva (and other places) was arranged by Heinz Dürr of the University des Saarlandes and not Paul, as we had corresponded first on our mutual interest in the cycloproparenes. So it was that I flew to Geneva where I met and began a twenty year friendship with Paul Müller. My lecture was well received. I also reaffirmed that souvenir collecting should never cause packing problems and settled on a pair of cufflinks from each overseas lecture tour. A set from Geneva was added to the pair from Sydney.

From Geneva, I went by train to Freiburg in Germany to meet Horst Prinzbach, then a major force in small ring chemistry and photochemistry. My first evening was hosted by one of his junior colleagues, Dr. Gerd Kaup, who took to task the level of a new textbook on *Organic Photochemistry* that had quite recently appeared.[16] After some discussion, he looked at me then said: '*Ah, I think it was by someone from your country. Do you know him?*' I responded that I did since I was one of the authors – and we got on well from that day. Prinzbach was a great host and the next evening, after my seminar, we went to his favourite restaurant in a small village on the outskirts of the Black Forest. It was there that the wine was served in Prinzbachian chalices – the pentagonal cup joined to a triangular stand depicting the molecule named by him as *calicene*. That visit saw the beginning of a long friendship that continues even today, cemented when he visited NZ just a few years later. Saturday saw me travel to Saarbrücken to be collected from the railway station by Heinz Dürr. He was, and is, a striking individual, tall with small beard, yet he remained silent until we were sat in his car. He then turned and said: *It is now Saturday afternoon and it is my custom not to talk chemistry until Monday morning. Will this be alright?* It was, and I had a marvellous weekend with him and his wife Helga when, amongst other things, I was taken to the Roman Ruins at Trier. Monday saw my university visit, a lecture on the small-ring chemistry and much discussion. It was then a race to the station to get the evening train to Cologne. The train had no restaurant car and, as it began to pull out of the station, Heinz and his student waved to lower the window whereupon savouries, sandwiches and cakes were passed in.


calicene

In Cologne, I saw Emanuel Vogel who, with Prinzbach, Hafner and Huisgen was among the most noted of the mainstream chemists in Europe. I had met him in Florida during his visiting professorship and he had corresponded since then on cycloproparene chemistry. As in Freiburg, it was a great visit and we remained friends for close on 40 years until his death on 31 March 2011. I was fortunate as, unlike his junior German colleagues, I was an international collaborator and benefitted from gifts of chemicals from his on-campus pilot plant, mailed at no cost to NZ. I purchased another pair of cuff links to mark the end of a very successful tour.

My return to Reading was uneventful and we settled to the cold of winter with delightful weekend outings to the local sights and periodic visits to Lee. Once spring arrived, my mother and friends came more often for weekend visits. For Easter, we headed northwest to my relatives in Lancashire. The first stop was in Lytham–St. Annes where my oldest cousin, Margaret, lived; my father had stayed with her and husband Bernard Lancaster prior to his surgery. We were made most welcome but, in the short time available, we had to move on to Gt. Harwood, where we stayed with Cousin Julia and her two children in their home at 6 Coronation Street, across the street from my first home. To fit the accommodation, a typical terraced property, modernized, but with only three bedrooms the smallest of which was very small, her children stayed with their maternal grandparents at No. 17. Margaret was then exposed to the gaggle of northern relatives through a gathering that Julia organized on Easter Sunday. We saw something of the local countryside including the Woodfields Village and Stonyhurst, Chatburn where Margaret Lancaster's sister Teresa and her family lived, and Sabden, the small mill village at the foot of Pendle Hill where my Uncle Ted had been Mill Manager. We returned there together only in September 2009.

My mother, Mark and friend, Reading, 1975

Back in Reading, I continued to work and give university lectures. Robin Ferrier had been more than keen for me to gain exposure in the UK and set up several desirable visits. In total, I must have given a dozen or more lectures and made some very valuable contacts, but not all were simply professional. I arranged to visit my Southampton friends, Brian and Cathy Odell, who by this time were in Leeds with Brian at the University. This was followed by Stirling University with Bob Hay (d. 1999) who had returned there from VUW in 1971. We thoroughly enjoyed our time and driving over Ilkley Moor (Bar T'at), and visiting York and its Minster. In Scotland, we saw Edinburgh and the Highlands near Stirling, but not Loch Lomond. The return journey took us through Cumbria and the Lake District, passing through Ambleside, where Margaret and Bernard Lancaster had run the *Boots Chemists* for many years before settling in Lytham.

The visit to Stirling University provided the best outcome for me, in that Prof. Willie Parker asked if I would like to contribute to the Royal Society of Chemistry (RSC) annual publication, *Alicyclic Chemistry*, which provided an annual summary of the literature published in the field. I accepted and it proved to be an

interesting time. The retrieval of scientific material was on the brink of electronic searching and, thanks to Willie Parker, the RSC provided me with the latest listings of article abstracts retrieved under a trial process; they gave a huge time advantage over the hard copy that came to Wellington dominantly by surface mail. Air-mail print-outs began to appear in December when I was home and they came with remarkable regularity. The collection of the *ca.* 1000 citations and requisite data over the 1976 and 1977 years was manageable. Writing was done over a short period around Easter and the book appeared some four months or so later, with the 1976 literature survey published in the northern autumn of 1977. It was an excellent experience and one that set me up for assessing and abstracting papers for the rest of my career. I had articles published in Volumes 5 and 6 of the series, the last after Parker's most untimely death.

Our final months in Reading saw us enjoying the late spring and early summertime before we dispatched surface-mail parcels home to keep our air luggage within the allowance. Our car was easily sold and we left Reading for a few days in London before our departure from Heathrow. We did not expect the welcome that we received in Los Angeles. Air New Zealand had begun flying from Auckland to London via LAX, but it was only the aircraft that made the full journey. Air NZ handled the Pacific sector and a British Airways crew flew the polar route to and from LA. We were appropriately ticketed for the single flight from LHR to AKL. On arrival at LAX, all passengers were required to pass through immigration and customs, much like the situation in the recent past. However, US officialdom designated the flight from LHR as British Airways and that from LAX as Air NZ. We had no US visas and were not to be granted entry to the US, despite the tickets and itinerary being clear; we were barred from entry and from any transit facility. Our enquiries of how we could re-board our aircraft met deaf ears until two-year old Mark decided that he had had enough and began to cry. The solution was simple: we were to be placed under armed guard and taken from arrivals to departures and held in custody until we re-embarked. The guards, however, were more humane and, once out of eyesight of the immigration officials, they carried our luggage, smoothed our way and, once at the boarding gate, simply left us to our own devices. The Pacific leg remains a blur, but we arrived home tired, yet happy to be back.

My return to VUW was greeted with enthusiasm and I began to prepare the mandatory *Leave Report* for the research and refresher leave committee, as it had become called. I was quickly into teaching and the routine cycle of research, teaching and administering began again. In September, I attended the annual meeting of the New Zealand Association of Scientists to receive the 1974 Research Medal. I had been advised of this before we left for England and its receipt, my first award, came from nomination by James Duncan, then Head of Chemistry. Naturally I was delighted, as recognition within one's own country is usually not easy to come

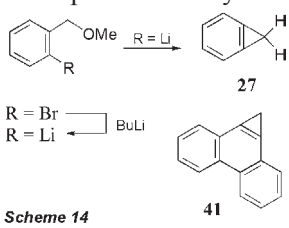
by and it was a significant addition to my CV. The surprise of the spring was my promotion into the *Extended Range* of the Senior Lectureship. At that time, the salary ranges for Senior Lecturers overlapped that of Readers through the extended section. While this was a much appreciated event, it was also a cause for concern for two reasons: it was unusual for a staff member to be promoted so soon after sabbatical and the SL Extended Range generally was regarded as difficult to move out from.

When we got back from England my research students were Alan Browne, who was approaching the end of his four-year period, Teng See Chuah, my 1974 Honours student who had returned for PhD, and a BSc Honours student, Garry Mitchell, who had had a difficult time with me absent for the bulk of the period that he was carrying out laboratory work. All three had maintained an excellent routine of reporting by air-mail and I wonder how the student body of today, with all his modern facilities, would cope if transported back to that era of pen and paper. Teng See found advanced study too demanding and she moved away, subsequently to marry our Store Manager, Peter Leighs, and to a successful career initially in business and then in law. As the end of the teaching year approached, I obtained a grant and took on a new research assistant, David Officer, who stayed with me until 1981, firstly as a paid employee and then, from 1976, for his Honours and PhD degrees.

A Young Family, a Career, and Momentum

The 1975-76 summer vacation saw David Officer as my research assistant and the only laboratory worker I had. However, he and Martin Banwell, a more than competent undergraduate, had signed up for the new 1976 BSc (Hons.) degree, with research under my direction. The reduced activity gave time to complete some projects on our Croydon Street home, especially as we were expecting our second child the following spring. Paul Derek was born on September 3 and Margaret stayed in the maternity hospital for about a week, in line with the policy of the day.

Both Officer and Banwell proved to be very competent individuals, as different as chalk and cheese, but they made the research lab hum. The pair outdid my former students as both graduated with first class honours. Alan Browne submitted his thesis on cyclopropanaphthalenes mid-year, in a time just a little longer than the four-years expected, and ended up with a tally of six publications. He went to join Leo Paquette at Ohio State for his postdoctoral period. David was attempting to extend the cycloproparene family by synthesizing cyclopropa[*l*]phenanthrene (**41**) by use of ring contraction processes. He carried out some of the critical work that showed that the 1974 report by Radlick and Crawford[43] (Scheme 14) was impossible to reproduce; the dreadful odour of **27** was the only evidence for its presence. Martin also took on aspects of mono- and dicyclopropabenzene synthesis and they both shared (with others) their first paper.



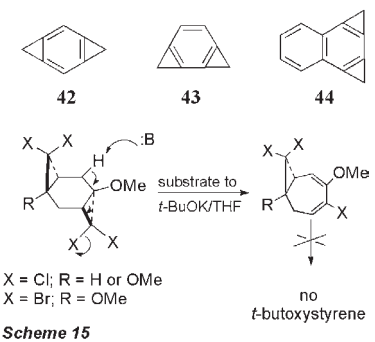
Scheme 14

In January 1977 I formally moved into the Readership, having been told of this just ahead of Paul's birth. A little later, the Higher Salaries Commission compressed the Reader's salary scale from four levels to a single rate. With a second child, everything helped and this proved a major boost to our income. Both Martin Banwell and David Officer returned for PhD study, Martin, like Tony Woolhouse, with a University Grants Committee Scholarship (of which NZ had ~100 for all disciplines) and David as a Junior Lecturer akin to Alan Browne. I also started Neil Galloway as an Honours student and was very busy again. Martin and David have been the two students whose careers have been especially pleasing to see

Martin Banwell 2010

develop as both moved into academia, Martin ascending to the senior Organic Chemistry Chair in the Southern Hemisphere, that at the Australian National University in Canberra. This followed postdoctoral study with his choosing Leo Paquette at Ohio State, a research fellowship in Adelaide (Australia), and then tenured positions in Auckland and Melbourne before moving to ANU in 1995. He was awarded an honorary Doctor of Science degree by Victoria University in May 2010. For some time I had assumed such honours would go to him, but I had neither expected VUW to be the first institution to make such an award nor to see it from the same side of the grass as Martin. By way of contrast, David had postdoctorals with Ron Warrener (at ANU) and then Emanuel Vogel (in Cologne) as a Humboldt Fellow, before accepting a lectureship in NZ at Massey University, Palmerston North, with promotions, ultimately to Professor. He accepted a Chair at the University of Wollongong in 2007, where his research is in the recently created 'Intelligent Polymer Research Institute' developing functional nanomaterials systems. Their time at VUW gave them seven and six publications each, Martin completing after a mere 30 months (like Tony Woolhouse) and David after 4 years as a Junior Lecturer, in 1981.

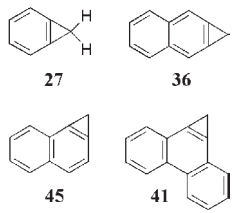
Martin began what turned out to be a failed project – the attempted synthesis of the linear and angular dicyclopropabenzenes **42** and **43**, which also was unsuccessful for Udo Brinker and his group in the 1980s when they tried to generate the naphthalene equivalent **44**. What Martin's work did was to show the ease by which halotricyclooctanes could be transformed into homotropylidenes, as depicted in Scheme 15. The reaction required addition of the substrate to the base, the reverse of what had been our normal procedure until then. The so called *normal* addition led to a series of styrenes and much of the work concerned the precise way in which these were formed. A series of papers entitled *Studies in the Tricyclooctane Series* appeared in the Australian Journal of Chemistry.



Scheme 15

Understandably, David's work was slower as he had to contend with the duties of a junior lecturer. He elected to continue attempting to extend the cyclopropabenzene ring systems to the cyclopropa[*l*]phenanthrenes. By this time, cyclopropabenzene (**27**) and -[*b*]naphthalene (**36**) had been characterized and studied in some detail.

Vogel and his students had prepared the angular -[*a*]naphthalene (**45**) from flash vacuum pyrolysis during 1976, a result that was taken much further by Paul Müller and his group using more conventional chemistry over the ensuing years. Our interest came from the strain in the molecules and its impact upon stability. Thus, while **27** and **36** were stable compounds, **45** was not. This fitted with the expected increase in π character of the bridge bond on moving from **27** to **41**. Although our efforts to obtain **45** (or a derivative) failed, we were convinced that phenanthrene **41** had to have a more double bond-like bridge and would show even less stability. Thus, David's studies laid the foundations for the ultimate synthesis of **41**, with which he was intimately involved in 1984 whilst in Vogel's Cologne laboratory.



The year 1977 also was significant for me in a different direction. I noted earlier just how much Horst Prinzbach had impressed me when I visited Freiburg in 1975. A year after returning from England in the spring of 1976, I found that the NZ Vice-Chancellors Committee had an established fund for a visiting German National Fellow. The monies available provided for travel to and from NZ, with one week to be spent at each of the four major, and half a week at the minor (Waikato and Massey) universities. Half a week at each of Rotorua and Queenstown was also included for rest and recuperation. On carefully reading the conditions and criteria, I could find nothing to limit the individual to being a German Language or Literature scholar, and so I wrote to Prinzbach seeking his permission to nominate him, got it, and then submitted the paperwork. He was selected and came in the winter of 1977 travelling via Easter Island. He gave a lecture course in each university, with that at VUW stunning in its clarity, vision and scope, and encompassing his fulvene, fulvalene and bishomobenzene chemistry. Only Roger Brown had visited me previously (1974) while on sabbatical at Canterbury University, and Horst's time in NZ did much to enhance my reputation. Margaret and I entertained him at home, showed him the Wellington area and, on his last night, took him to a *progressive dinner.* The concept of hosting an evening meal with the three courses eaten at different venues had become popular. It meant that one had each course with a different group of people in a different home. Horst was allowed to join this *Young Wives* event and it took him into different NZ homes. In the following year he was, I believe, named German of the Year, became a firm friend and, on every visit to Freiburg since (the last being in 2009), that progressive dinner has been raised by him. Subsequently, I hosted some two dozen eminent chemists who lectured in the various NZ universities. Robin Ferrier nominated Hans Paulsen, a carbohydrate chemist from Hamburg, also for the German National Fellowship and he came in 1978, the only other before the rules were changed so that they applied to language and literature experts only.

Time passed quickly as I became more involved in committee work, not least that of the University Safety and Civil Defence. I had taken on the duties of Departmental Safety Officer shortly after we returned from Reading. In itself, this was straightforward enough. However, the various science departments had quite different expectations of their staff and students when it came to safety issues and directives were frequently at odds. The effect of this was that a Science Faculty safety advisory committee was established under my chairmanship to put the house in order as it were. It succeeded in bringing about a much more logical approach to safety issues and an approach was made to the then Vice-Chancellor, Dr. Danny Taylor, for funds to produce a Laboratory Safety Handbook. Unlike modern management practices that require everything justified in triplicate, my one meeting with him was short and simple, and I emerged charged with the task of producing a manual for use in all laboratory areas. A group from the faculty committee rose to the occasion and Victoria's first Laboratory Safety Handbook appeared in 1977, by which time a University Safety and Civil Defence Committee had been established. In the years that followed I was involved with safety courses for secondary school teachers, advising the Wellington Medical School how better to address its issues and a range of other matters that provided a distraction from what I really wanted to do.

The summer of 1977 saw Mark's 5th birthday ahead of Christmas, and our first summer house sitting for Margaret's brother Ron, in Hamilton. I do not recall who initiated things but Ron preferred to have the house occupied when they were away. The idea of a quiet time relaxing in the NZ sun with a large pool and a tennis court was seriously attractive. It was the start of regular summer visits to 58 Herbert Road that provided the essential R&R after a busy year. My mother, now retired, visited us for the third time over the following summer (1978) but she was distracted by a new suitor, Victor Spencer, whom she married in March 1980. They visited us in 1984, and then again in 1987, the last time my mother was with us as she was beginning to display early signs of the dementia that took hold and forced her into permanent care for the last five years of her life. She died on February 28, 1996, ten days after a serious stroke. My flight to London for the funeral had me in a seat adjacent to John Spencer and his wife on the Auckland-Los Angeles leg; John had been out to interview for the inorganic chair vacated by Neil Curtis on retirement, was offered the position, and subsequently joined as a Professor of Chemistry with responsibilities in the inorganic area – labelled chairs were out!

For the new 1978 academic year my 1977 Honours man, Neil Galloway, returned for PhD. Neil had gained a 1st Class Honours degree and a UGC scholarship, but with added allowances for his wife and young child. He began working on the oxidative decarboxylation of bicyclo[4.1.0]heptene dicarboxylates in the search for a new and viable synthesis of the cycloproparenes. During the course of

his work he moved from Wellington city to the coastal suburban settlement of Raumati where his wife was closer to family. The subsequent daily commute did not fit to his responsibilities or the demands of the research laboratory and he transferred to an MSc degree, which he gained in early 1980. We never found a new pathway to the cycloproparenes but did synthesize the requisite bicycloheptene 1,6-diacids and follow their decarboxylations.[44] Neil subsequently joined Taubman's Paint Company (now the Peter Jackson film studios) in Wellington and had a successful career there until its closure.

I had had spells serving on the Wellington Branch Committee of the New Zealand Institute of Chemistry (NZIC), was elected to its Fellowship in 1977, and became the Branch Chairman in 1980. One David Bibby was the very competent Branch Secretary – he is now VUW's first-rate Dean of Science – and still a competent secretary. During this time, I was awarded the Institute's ICI Prize for Excellence in Chemical Research, while also serving what transpired to be the first of four lengthy terms on the National Council. The last two of these buttressed a 27 year involvement with the organization and running of the Chemical Congress of Pacific Basin Societies (*Pacifichem*). Following from the success of bringing Prinzbach to NZ, I nominated Derek Bryce-Smith for an NZIC sponsored lectureship of the country. He visited in 1979 and attracted significant media attention from his environmental stance on lead pollution. He was followed a year later by Emanuel Vogel, then Peter Sykes (Cambridge), and almost annually thereafter a distinguished chemist from the northern hemisphere.

My third international conference (I attended an RSC meeting in York while at Reading) was another RACI Organic meeting in January 1979, this time the 5th National in Hobart, Tasmania. David Officer and I both attended, and David presented our approaches to cyclopropa[*l*]phenanthrene at the subsequent meeting in Melbourne in August 1980. I was fortunate to meet and interact with Prof. Koji Nakanishi of Columbia University, fortunate not so much for his elegant chemistry or his magic (we were almost ejected from the Wrest Point Casino) as for the help he offered when VUW's 11 year old 60 MHz Hitachi NMR instrument had problems. We were unable to get any satisfaction from the manufacturers until he intervened on our behalf. Even more coincidental is that I had an excellent working relationship with Koji's brother, Atsuo, during our mutual times working on the Pacifichem meetings; he was the Executive Director of the Chemical Society of Japan during 1989-1998. The problems with the Hitachi NMR instrument signified its age and the need for a second generation machine. The organic chemists were, by far, the largest users of the facility and it fell to us to provide an appropriate case for a new instrument that could provide carbon-13 as well as proton spectra. The task was up to Ted Harvey as he was the one most familiar with the Hitachi and he did the job with aplomb. After considerable searching, a decision

was made to match Otago and Canterbury universities and acquire a Varian Instruments 80 MHz electromagnet instrument when funding became available. In those days, grants for major equipment were made by the University Grants Committee and our case was so well handled by Ted that we were given the grant at the time we needed it. The instrument was ordered, but only after some significant negotiation that had the installation done by the Varian Palo Alto expert technician and not an Australian counterpart who had quite limited experience. The order itself was placed in 1979, not by us but by the Government's Education Department. They provided their own (internal) insurance and refused to transfer monies to the US despite the almost certain probability that the NZ dollar would devalue before any account was paid. In any event, the instrument duly arrived and was installed by the Varian man in the early part of 1980. He was superb in getting the electromagnet up and running, and in training our NMR technician, the NZ Varian man (Alan Ross) and me. Ted had already moved away from the Department to the Registry, the NMR acquisition his final and lasting contribution before departure. I well recall receiving a letter from the Education Department some months after installation and payment of the account seeking something approaching $NZ 1000 from me to meet the difference between the grant and the final payment. I think it unnecessary to explain my subsequent actions, save but to say that neither I nor VUW made any additional contribution!

Brian Halton and Alan Ross – NMR 1980

All of these things seem to have happened in a flash of time, for the next focus was the second sabbatical due in late 1981 after David's departure. A decision as to where best to spend the time was coloured by thoughts of the US and Peter Stang's novel carbene chemistry, which was expected to fit nicely with new ideas for our cycloproparene work. Even in those days, Peter, who is my own age, was located in Salt Lake City and his area of expertise was simply too good to miss. We arranged for a nine month spell at the University of Utah from late September 1981 and VUW approved the programme, although I was encouraged to seek additional financial support as my VUW salary was likely to be inadequate. And so it was that I applied for a Fulbright Fellowship from the 1980-81 allocation. Shortly after the application had been lodged, I was informed that the NZ programme applied only to NZ citizens and I was asked if I had an application under consideration. I did not, but application papers were completed in short order and delivered to the NZ immigration office. There were two difficulties, the first being proof of my 1968 entry to NZ. As I commented earlier, there were no formal requirements and

the stamp in my obsolete passport was my sole record unless a letter from VUW could be provided. The major problem, however, was in having my application considered in time for the deliberations of the Fulbright Selection Panel. Yet again, fate was on my side. When I entered the Department of Labour and spoke with the officer handling applications, I detected a clear and definite Lancastrian accent; he came from a village close to Gt. Harwood. Naturally, favouritism can never be shown by any NZ Government officials as applications are dealt with strictly in order of receipt; I was shown a large pile awaiting investigation. My NZ citizenship was granted shortly afterwards and it was followed by the award of a Fulbright Fellowship ..... and I am now a member of the NZ selection committee.

Sabbatical in Salt Lake City

So it was that, in 1981, we planned the stay in Utah. Peter was generous to the extreme with his time and help. Not only did we set up a research plan, but also he provided information on apartments, cars, schools and the ways in which we could get both boys into the Utah State education system. He had me appointed as Visiting Professor, provided medical cover and assisted in many other ways. Our third foray into house renting, car sale and travel arrangements went smoothly, culminating in our October 1 departure for Los Angeles, where we spent a few days taking in all of those things that the city and its environs are famous for. By then, Peter had rented an apartment for us on the Salt Lake valley floor, a two bedroom, semi-basement, unfurnished place at 1224 SE 9th St., comparatively close to campus. It proved ideal.

Our arrival into Salt Lake City involved a flight path up the valley before a turn to the east and touchdown towards the south. The Mormon Temple and Moroni, the angel, glistened in the afternoon sun and the University stood out from its site on the foothills to the Rockies. Peter met us and whisked us to the apartment in short order. Much to our surprise the place was almost completely furnished from the University of Utah *loan closet*, set up some years earlier by University staff members' wives; all we needed do was to donate any household items we purchased or replaced. Dinner that night was with Peter, his wife Christine and their two daughters, similar in age to Mark and Paul. Although Christine had provided us with essentials (even made the beds ready for us), Margaret and the boys ventured to the grocery store next day for a few more things while I was at the univer-

sity. A day later Christine took Margaret to *Smiths*, a vast grocery store and a mind blowing experience for someone who had never been in the US before. Both Peter and Christine have remained close friends since that first encounter.

At the U of U, I completed various forms, collected my campus ID and arranged for issue of the Exchange Visitor Visa needed as a Fulbright Scholar. I met members of the research group and academic colleagues, most notably Cheves Walling (1916-2007), then editor of JACS. I was given an office desk and lab bench, and settled to the work that I would do. The boys attended Emerson Elementary, a State school just around the corner a couple of blocks away from our apartment and on E 13th S. One of Peter's neighbours ran a car dealership and he let us have a later model Mazda GLX hatchback at cost and agreed to take it back for a fair price when we left. Christine arranged for Margaret to attend the Utah Women's Faculty Club New Comers afternoon just a few days after our arrival and she won tickets to the university Pioneer Theatre for Stubby Kaye in *Guys and Dolls.*

Very soon after my arrival in the Chemistry Department, Prof. Manfred Regitz visited from Kaiserslautern in Germany, and I began to get more international contact and hear many excellent lectures. We joined the Regitz and Stangs for dinner and I was invited to visit Kaiserslautern when in Germany the following European spring. We settled to a happy life and Margaret began to explore and enjoy her new found freedom with both boys in school. The weekends were spent doing all those things that the US had and NZ did not. One of the exercises during the winter months was taking the car out of the carport safely. The apartment had no garage but a covered concrete pad. With the level of snow and ice, salting the roads was common and whenever one got home the car would be wet. Overnight, the temperature drop was such that the water froze the tyres to the car pad and so a revved up engine was needed to free the wheels and get moving. All well and good, except that the carport was orthogonal to the driveway, and as soon as the car started moving backwards, a 90º turn was vital, yet we always managed to miss the neighbour's fence directly behind.

We spent time ice skating on one of the local open air swimming pools that was set to freeze for the winter season. Skates were provided at a small cost and the boys and I enjoyed making the circuits. They became more proficient as the sessions passed and, as I had always enjoyed roller skating at St. Joseph's College, I found this experience exhilarating, despite the bumps and falls. Margaret was less of an enthusiast and, sadly, had a fall that hurt. It happened shortly after New Year

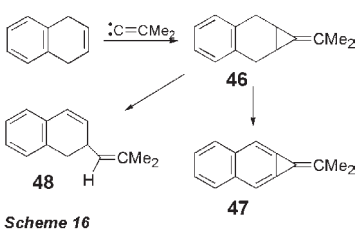
and two weeks later, on a Sunday morning, her hip began to ache. She degenerated rapidly from having some minor discomfort to total immobility and was admitted to Holy Cross Hospital (now the Salt Lake Regional Medical Center) as advised by Peter, underwent surgery for a recurrence of her childhood osteomyelitis, and remained there for almost two weeks. I contended with the boys, but as soon as our predicament became known, colleagues arrived with prepared meals and the fridge was filled, most notably with lasagne (and now I can no longer face it). The timing was far from the best as I had planned on visiting several universities to the east and, not knowing how long the recovery would take, all had to be cancelled save for visits to Texas and Florida in March.

I spent three days in Houston lecturing at both Rice University, with Ed Billups as my host, and at the University of Houston. Ed was the other chemist most active and successful in the cycloproparene area and the visit was good for both of us. At Houston I met Randy Thummel and gained much useful insight to his four-membered ring chemistry. Arrival in to Florida was to Gainesville airport on a Friday evening for a weekend with my former flatmate, Bill Eaton and his wife Cindy, before venturing back to the U of F. I spent from midday on Monday until the following Friday at the University. My visit coincided with one of the early Katritsky *Florida Heterocyclic and Synthetic Symposia* (now Flohet). He was most gracious, and involved me in as much of the programme as was feasible but I was there dominantly to see the developments in Merle Battiste's chemistry as well as catch up with former teaching colleagues. It was time well spent. There were but a few days after returning to Utah before the 183rd ACS National Meeting took place in Las Vegas. Peter had encouraged me to attend and present some VUW work. This I did, with the research going under the title *Aromatization Routes to Cycloproparenes*. The meeting was exhilarating and new contacts and friendships ensued, most notably with Larry Scott, then at Reno (University of Nevada), who I subsequently visited and with whom I have had much interaction involving the International Symposia on Nonbenzenoid Aromatics as it used to be called. The meeting was followed by a few days back in Utah and then a lecture tour in Germany that was generously hosted by Emanuel Vogel.

I flew to London, gave a lecture at University College and met Peter Garratt, and then attended to a few family matters before moving on to Cologne. Emanuel had set up an excellent speaking tour encompassing some eight universities over two weeks that began in Cologne. The tour gave me better recognition in the country, and led to many return visits. Of course, there were returns to Geneva

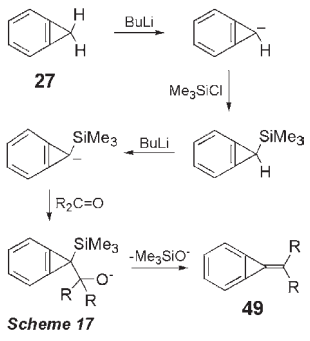
and Saarbrücken as well as time in Freiburg with Prinzbach. I made many new contacts, not least Klaus Müllen (now Director of The Max Planck Institute for Polymer Research in Mainz), and Curt Wentrup and his wife Edeline who had worked with Müller in Geneva some years earlier. I got to know Manfred Regitz better than simply over dinner in SLC and saw him a on a number of subsequent visits. Sadly, he suffered a serious stroke in Paris in the late 1990s that essentially terminated his career in chemistry and forced him into early retirement. The last stop on the tour was at Heidelberg where my host was Richard Neidlein of the Pharmacy School. We got along well and have interacted both in Germany and in NZ many times. In fact, Richard subsequently embarked on aspects of cyclopropabenzene chemistry and, being of the sort of individual who would never do things by halves, made significant quantities of the highly odoriferous cyclopropabenzene (**27**). However, there were difficulties at the University as the odour of the compound, when coupled with concerns that it advanced alopecia (baldness), led to a ban on its preparation and use there. After this, I became aware that molecule **27** can be detected nasally at the level of 1 ppb, but I am unsure just where that figure came from – perhaps the Heidelberg University health professionals who had contacted me. It is important to note here that there have been no adverse health effects in Wellington, save for the occasional headache, and this from a 35 year period of working with the compound(s).

The Utah project that I had been working on had developed but, in itself, never appeared in print save for review documents. What had been decided was to couple Stang's expertise with unsaturated carbenes to our own in the cycloproparene area, with a view ultimately to deliver the first methylenecycloproparene. Our approach involved the addition of unsaturated carbene, generated from 2-methylpropenyl triflate, to 1,4-dihydronaphthalene. This gave the exocyclic alkene **46** that was expected to doubly dehydrogenate to the sought after arene **47.** This did not happen as opening of the three-membered ring was the preferred path, giving alkene **48** (Scheme 16). From these studies it became apparent that **47** and analogues **49** would need a synthetic procedure that carried the aromatic ring while the exocyclic alkene was located in place. Thus, a retrosynthetic analysis (the term was beginning to be used) led us to propose deprotonation of the arene, capture of the anion with trimethylsilyl chloride and deprotonation of the monosilylated cycloproparene so formed, so that the anion could be captured by a carbonyl-containing compound in a silyl-Wittig sequence. This was expected to deliver the alkene **49** directly as shown in Scheme 17. There was insufficient time in SLC to initiate this work except for the synthesis of **27**, which was then



Scheme 16

returned to NZ in a sealed thick-walled glass ampoule by conventional air mail delivery. It was my students in Wellington who brought things to fruition. The collaboration with Peter that my sabbatical began was ongoing and lasted some twenty-one years giving twenty communications, papers and reviews.



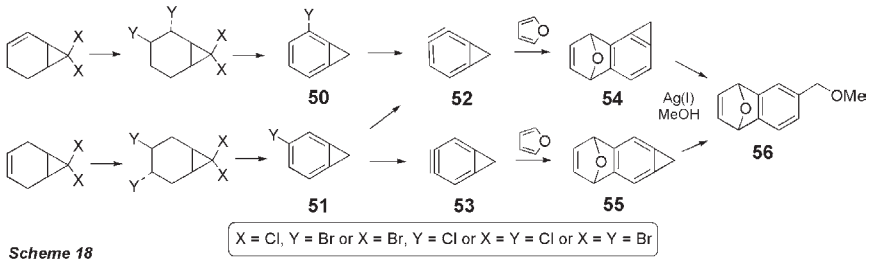
Scheme 17

Our final morning in the US started as it had begun nine months earlier – in Los Angeles and with breakfast at Denny's. Unlike our arrival, we were into pancakes and all things American and we made the most of it for the last time. Our Air New Zealand flight home provided time for reflection on what had been highly successful professionally and a truly wonderful time in our family life. Unlike my departure in 1968, this was to a homeland that I was not going to leave.

Advances in Chemistry – At What Cost?

With David Officer having left the lab in 1981 without being replaced, the hope for future research assistance had rested on the two honours students that joined me before I left for Utah. Both Barry Dent and Cliff Randall got on well with their honours research and elected to return for PhD study despite my being out of the country. Barry had gained a 1st class honours degree along with several other prestigious awards, and returned with a UGC postgraduate scholarship to take on the challenges left by David to synthesize a cyclopropa[*l*]phenanthrene. By comparison, Cliff had not achieved as highly in his honours year, but his upper second qualification was enough for him to be awarded a teaching assistantship, which he took up in late February, 1982. The provision of junior lectureships in chemistry was terminated by VUW through its Dean of Science, but the Department had retained a few assistantships to continue at least some PhD sponsorship.

Cliff had used his honours year to delineate the route to halocyclopropabenzenes from tetrahalobicyclo[4.1.0]heptanes and found that the presence of a labile bromo or chloro substituent in the six-membered ring indeed led to triple dehydrogenation. Such elimination sequences had been studied earlier but the precise route by which the product(s) were formed remained in doubt. Our studies, initiated for us by Martin Banwell, showed that, by using a mixed halide, the halogen retained in the product arene **50** or **51** was that from the six-membered ring of the *trans*-2,3- or 3,4-dihalo starting material (Scheme 18). Moreover, the 2,3-dihalo compounds had the 2-halogen on the opposite face to the three-membered as shown from X-ray crystal structure. As will be obvious from Scheme 18, the major thrust of this work was to generate the two possible didehydrobenzenes, **52** and **53**, by

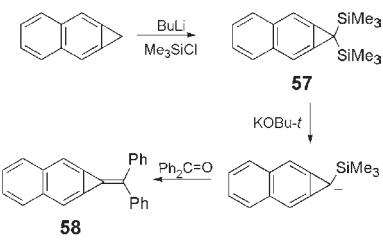


Scheme 18

subsequent dehydrohalogenation. This Cliff did using the complex base system of sodium amide/sodium *t*-butoxide in tetrahydrofuran and, in the presence of furan, the two benzynes were trapped as the angular and linear Diels-Alder adducts **54** and **55**, respectively. The structures were confirmed from Ag(I)/MeOH opening of the three-membered rings to the same naphthalenyl methyl ether **56** that was independently synthesized. The results were published in 1983.[45]

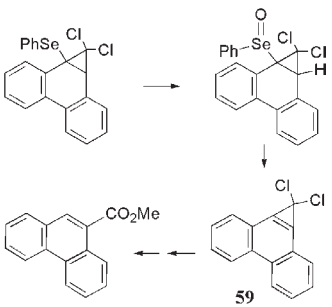
During my time with Peter Stang, the competence of Yitzhak Apeloig, both as an experimentalist and a theoretician had come to impress me. Having neither computer nor software, let alone the needed expertise to carry out a computational study of the didehydrobenzenes **52** and **53**, I approached him to see if he would be interested in assessing the compounds for us; this led to another on-going collaboration of almost 20 years. Of course, in the early–mid 1980s the personal computer had not arrived and computation, as it was, was far from easy to perform. Worse still, the basis sets then available did not replicate the cyclopropparene geometry well. Yitzhak and his students persevered with parent hydrocarbon **27**, subsequently to publish in 1986 a paper in the *Journal of the American Chemical Society* entitled *Cyclopropabenzene. Geometry, Electronic Structure, Strain, Reactivity, and the Question of Bond Fixation. A Theoretical Study*.[46] This was followed by another on **52** and **53** that showed the pair to be the most highly strained benzyne derivatives yet prepared, having strain energies in excess of 700 kJ/mol. The angular isomer **52** is the more strained (by some 10 kJ/mol) as the distortions imposed by benzyne itself sit more comfortably upon **27** in a linear rather than angular manner.[47] What is interesting here is that in the mid-1980s the time needed to complete each computation was long, and the only computer that Yitzhak had access to capable of handling the job was that in Technion's (Israel Institute of Technology's) administration. This was taken over for small ring calculations during the summer months when staff were absent. This cannot have been all bad as Yitzhak rose to become President of the Technion from 2001 to 2009. In 1999, a book *World Records in Chemistry* was published by Wiley that includes benzynes **52** and **53** as then, and even now, they remain the most highly strained benzynes recorded.

Having completed this work, Cliff moved on to the synthesis of our desired planarized cycloproparenes, namely those carrying an alkene at the C1 position, the alkylidenecycloproparenes exemplified by **49** of Scheme 17. Cliff was able to show that in the naphthalene series, methylene derivatives could easily be synthesized, not by following the path of Scheme 17, but by modifying it because monosilylation of the naphthalene could not be controlled easily. However, disilylation was readily brought about in good yield to give **57** which, in turn, underwent desilylation with *t*-BuOK to the ensuing α-silyl anion that readily coupled with aryl-containing carbonyls. This gave the much sought after olefins as highly coloured crystalline compounds in good yields, as demonstrated by the yellow-orange **58** of Scheme 19; this was obtained, ultimately, in almost quantitative yield. In this way, a range of naphthalene derivatives was soon prepared and the work extended to the benzene series. This was more problematic as the benzenoid α-silyl anion has less stability and the entire sequence, although successful, gave rise to products in yields of little more than 50%.[48] Not surprisingly, Cliff successfully completed his doctoral study. The work he did was demanding and its execution was due to his excellent 'pair of hands' that seem to have been the trademark of his career in mass spectrometry in the NZ Crown Research Institute environment. The gambit of properties of these new compounds took us in a new direction and involved us in many years of fascinating study.



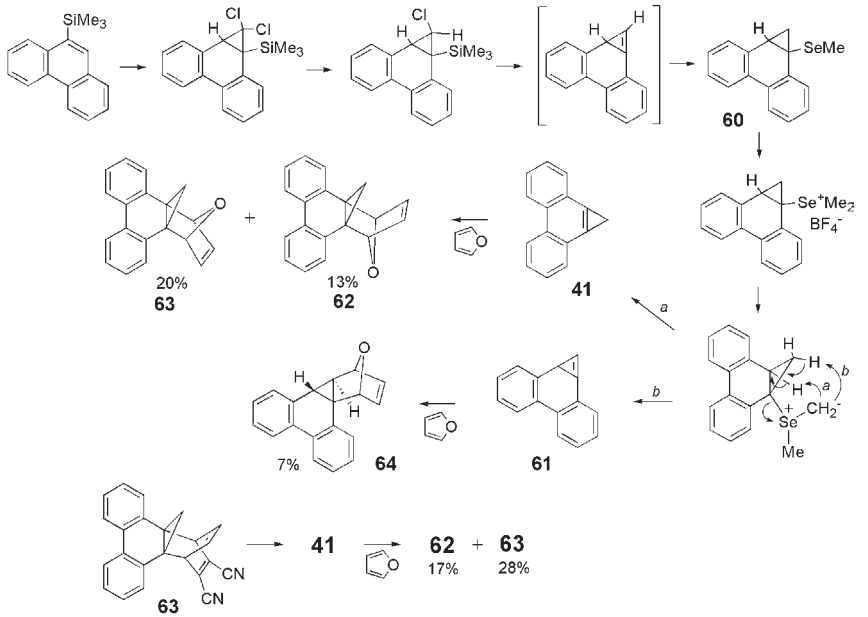
Scheme 19

While Cliff was underway with benzynes and alkenes, Barry Dent was pursuing cyclopropa[*l*]phenanthrenes. His initial studies followed from those of David Officer, and honed to the delivery of the *gem*-dichloro derivative employing a route that uses a 9- or 9,10-disubstituted phenanthrene carrying an electron-donating substituent. With a nucleofugic substituent, subsequent elimination is facilitated, with the first successes coming from the addition of dichlorocarbene to 9-phenylselenophenanthrene, oxidation of the product to selenoxide and then ejection of PhSeOH. Although not as simple as implied, methyl 9-phenanthrenecarboxylate was obtained via intermediate **59** (Scheme 20) by a route that mirrored the behaviour of **27**.[49] In order to complete the study, Barry skilfully pursued the parent molecule **41** by employing the C1 unsubstituted methylselenide **60** successfully, as shown in Scheme 21. The route to **60** involved monodechlorination of the dichlorocarbene adduct of



Scheme 20

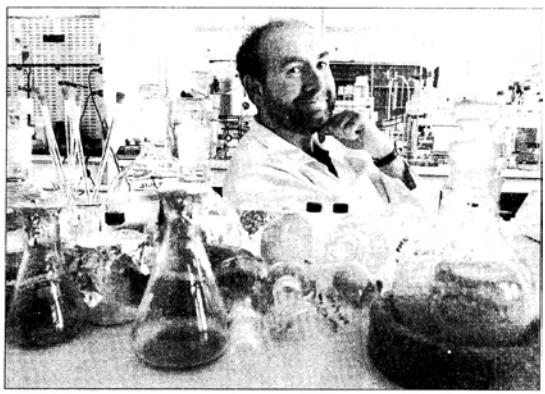
9-trimethylsilylphenanthrene, *t*-butoxide desilylation and capture of the ensuing cyclopropene by the anion of methylselenide as **60**. Methylation of this then gave the dimethylselenonium tetrafluoroborate that lost dimethylselenide when treated with *t*-BuOK at ambient temperature. In the presence of furan, a mixture of the three Diels-Alder adducts **62-64** was isolated and then separated into the independent components in yields of 13, 20 and 7%, respectively. The formation of **62** and **63** represents 83% of the isolated adducts and shows that not only is **41** formed in this reaction but that it is a kinetically more stable product than the angular (C1-C9b) alkene **61**. Work in Cologne by Vogel's group (that included David Officer) also led to **41** by their traditional flash vacuum pyrolytic retrodiene process, and this allowed for its isolation free from side products. It is a moderately stable solid at –78 °C but decomposes over several days. The results were published jointly in 1985.[50]



Scheme 21

I regard the results of Barry and Cliff as probably the most significant from my career. These two doctoral students made the advances recorded above from research that started while I was on sabbatical leave in Utah and then saw my presence in the university dwindle from March 1983 for close to a year. I had not felt well for some time and my GP had been unable to offer any solution. During the stay of Prof. Mord Rubin (Israel Institute of Technology) I was referred to the local hospital for a coronary check but discharged. That was until March 25, 1983, when

I suffered a significant heart attack – though the night duty doctor did not pick it and gave me a significant dose of morphine that kept me more than happy for the day. After diagnosis, and the mandatory stay in the coronary care unit of Wellington Hospital, my cardiologist came to the conclusion that I could be well served by arterial grafting and approached the surgical team. At that time, coronary artery bypass surgery had only been introduced recently to Wellington but I was regarded as a suitable candidate and underwent the operation some four months later, in July. Anaesthesia in 1983 was markedly more invasive than in the 21st century (as I know from experience) and it took six months before its effects finally dissipated. Although I was back to the office some six weeks after surgery, even doing some lecturing, I doubt if my presence was especially effective. During my recuperation Barry and Cliff visited, brought their results and provided much needed stimulation. That they achieved what they did, essentially unsupervised, says much about their own perseverance. Cliff married and left to take up a position with the then DSIR, while Barry spent a postdoctoral period in Alberta with Prof. Derek Clive before also joining the government laboratories. Both stayed in the service through its transition into the Crown Research Institutes. Barry developed a speciality in the synthesis of labelled reference compounds in 1987 that extended to custom organic synthesis. He bought a business unit from his then employer in 1996 and began his own company *BDG Synthesis* that now employs six PhD chemists providing custom organic synthesis; it is a well recognised organization. At the time that the pair left my group, I had made several significant decisions and refocused my life.



MAGIC POTIONS – Barry Dent: "We've been head-to-head with competitors twice that I know of – both times we made the compound when the others couldn't. That counts." Picture: CRAIG SIMCOX

Things at Victoria University were undergoing change when I took my sabbatical at Utah. My second US sojourn and European visit had brought me to the conclusion that the time was right to seek a more senior appointment elsewhere, and this I had begun to do. VUW elected to place a freeze on personal chairs, announced it in 1982 and, effective from the following year, it stayed in place until 1991, and so any advance could only come with a move. The impact of my infarct and heart surgery put an end to any thought of move, as I was no longer eligible for the essential insurance covers that would be associated with relocation within or beyond NZ; I settled to the knowledge that my career was to be

intimately linked with VUW for the remainder of my working life. 1981 had seen major restructuring of Departments with the autocratic *Head* being replaced by an elected Chairperson (limited term appointment) with an Executive that comprised the Professors and one representative of the sub-professorial staff. In Chemistry, the Chairperson (masculine and feminine were displaced and now the title 'Chair' is deemed sufficient!) was Dr. Stuart Smedley, a physical chemist. This significant change marked the beginning of a reorganization of the NZ universities during the ensuing decade, as illustrated by, ***e.g.*** each offering its own postdoctoral fellowships. As my medical advisors had given a prognosis of five years survival post-surgery were I lucky, I was faced with a rapid change in lifestyle that needed to be effective. I simply had to become more efficient and more selective in what I did so as to gain more quality time with the family, and this I did.

In the summer of 1984 I was approached by two of my compatriots from DSIR, Neil Milestone and Graeme Gainsford, to see if I would be willing to accept nomination to the NZIC 2nd Vice-Presidential role. That year, the Wellington Branch had the customary right to make such a nomination, which meant a three-year commitment to the Institute if I was elected. Having been merely a year post-coronary and with serious concerns about the work level, I was not easy to persuade. However, my former roles with NZIC had been more than worthwhile and, after some serious discussions and considerable thought, I acceded to the request and was elected unopposed (the tradition). I have never regretted the decision, as it marked the beginnings of what has now been 27 years of continuous service to the profession of chemistry by way of the national New Zealand organization in one guise or another; it has also provided much personal satisfaction. The roles took me through the Vice-Presidencies to President for the 1986-1987 year with a workload that was shared by one of the Chemistry secretaries, who attended to the entire (and significant) correspondence that was needed, much done on Saturdays. The presidential year took me to Japan for the first time. I was the NZIC observer at the 1987 planning meeting for the 1989 Honolulu pan-Pacific Chemical Society Congress that was named *Pacifichem* at that Tokyo meeting. It kept me involved as the convener of the *Young Scholars Selection Panel*. NZ and Australia were admitted as full partners in 1990 but more of that later.

During 1984, Victoria advised staff that it would fund a strictly limited number of three-year postdoctoral positions – no more than two or three across all disciplines. The selection of the projects for funding was to be made from recommendations placed by Departments. This feature has remained, although there has been a marked tightening of the criteria and the type of work that fits to the *accountability regimes* of the 21st century. In any event, Stuart Smedley solicited proposals from chemistry staff and invited those who had so done to attend for interview with those of the Departmental Executive not involved. I was fortunate

in having my proposal referred, via the Faculty, to the VUW selection committee and subsequently funded. Robin Ferrier was also a contender but he disputed the Departmental decision and, I gathered later, took his concerns to the Faculty and perhaps higher.

The fruitful times with Barry and Cliff in the laboratory saw the first of the jointly published papers with Peter Stang[48] of that valuable and viable 21 year collaboration. This work has had many successes and given much enjoyment and, as Peter Garratt of London's University College put it on one of my visits: *The methylenecycloproparenes have been good to you*. So it was that my proposal for a postdoctoral fellow was for further study of the cycloproparenes. Unlike many organizations, VUW itself called for applications to the areas chosen, ranked them and then sought the academic's approval to make an offer to the person it had selected. The best of the chemistry applicants was Simon Buckland, a graduating PhD chemist from London's City University. He had a wide range of skills that impressed, was offered and accepted the post, and joined me in February 1985. He stayed for more than two years, became engaged and married a New Zealander during his tenure, and left for a position with DSIR's Chemistry Division. His researches led to some ten publications that appeared between 1986 and 1990. After the mandatory report was submitted to the Scholarships Committee, its then convener (and subsequent University Chancellor), Prof. Tim Beaglehole, saw me to tell me that since this man had done so much good work I could not possibly need another postdoctoral ….. and I did not get funding for another until 1993 when Mark Cooney joined me from Larry Scott's lab.

The time after the 1983 surgery until 1985 was spent coming to terms with teaching and research, and retaining good health. I did a minimum of travelling, supervised four Honours students and saw Cliff and Barry through to completion. Of the Honours students, Anna de Raadt gained a 1st–class degree and went on to PhD, not with me but with Robin Ferrier. During this time and until 1989, my surgeon required that I see him on an annual basis for a check-up. Always the prognosis remained unchanged – if lucky, I might live for another five years. From about 1985, his daughter was taking chemistry at VUW and much of the consultation time was taken up discussing her progress and the prospects of subsequent graduate study; she completed an Honours degree with my colleague, David Weatherburn. I maintained the collaboration with Peter Stang and developed that with Yitzhak Apeloig, to the extent that our computational work continued into the 21st century. Peter visited NZ for the first time in 1984 to attend the 7th IUPAC Physical Organic Chemistry meeting in Auckland, which marked the major contributions of retiring Professor Peter de la Mare. He travelled much of the country giving lectures and spent time in Wellington prior to our both attending and presenting work. By the beginning of the next year, 1985, I was encouraged to do all the things that I

wanted to do and to return to full participation in all things chemical. VUW funded conferences outside NZ only once every three years and, as I had not been to a conference since the Las Vegas ACS meeting in 1981, I elected to attend ISNA-5 in St Andrews (Scotland) that summer. Yitzhak Apeloig and Mord Rubin at Israel's Technion were keen to meet with me and so my time was extended to allow for two weeks in Israel under the auspices of the Technion Vice-Presidential Fund.

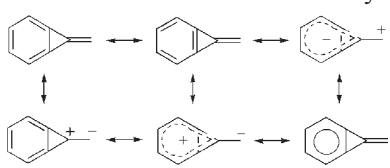
So it was that in the summer of 1985 I travelled to to Tel Aviv (amid the delays imposed by El Al the day after the aircraft hijack in Beirut, Lebanon). The time at the Technion was very valuable and allowed me to cement the collaborative relationship with Yitzhak. With visits to Jerusalem, Tel Aviv and the Weizmann Institute, the entire time was both chemically stimulating and personally satisfying. A bonus was the conducted tour of Jerusalem that I was given by Shalom Sarel (Professor of Pharmaceutical Chemistry at the Hebrew University and 1960-64 President of the Israel Chemical Society who, incidentally, still attended Israeli chemistry meetings in 2009, his 92nd year). Shalom had visited Wellington in the late 1970s while on leave at ANU and, as an amateur archaeologist, he gave me a spectacular tour that included some of the underground excavations, with access and egress through a synagogue and a home! Return to London was followed by the ISNA meeting that was chaired by the late Charles Rees with Peter Garratt as secretary. Not only were many of the attendees known to me (Vogel and Prinzbach were featured speakers) but the meeting provided an opportunity to get to know other stalwarts of novel aromatic chemistry. My attendance there was followed by an invitation to speak at the 1989 meeting in Japan and the beginning of an association with the ISNA movement that lasted through ISNA-12 also in Japan, by then Novel Aromatics rather than Nonbenzenoid Aromatics.

Chemistry and Service

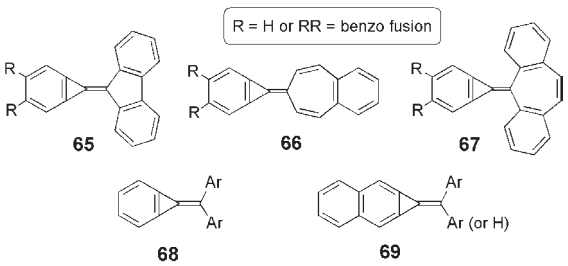
A Postdoc for Planar Strain

Simon Buckland arrived in Wellington in February 1985 and, as much of his PhD study had centred on photochemical reactions, he began his programme by attempting to synthesize some heterocyclic cycloproparenes. This did not eventuate but he was able to trap the diradical intermediates that were involved in the 3*H*-pyrazole photolyses. With Cliff in the throes of completing his degree, the time was right for Simon to move to the newly discovered exocyclic alkenes. The strong and highly unpleasant odour of cyclopropabenzene (**27**) was such that the only work with it in Salt Lake City had been limited to my own synthesis of the compound in 1982. The corresponding naphthalene **36**, however, although somewhat smelly, provided no such impediment and aspects of our programme involving derivatives of **36** were carried out in Utah by PhD student Qiu Mei.

A detailed study of the alkylidenecycloproparenes was of such fascination to us because of the strain imposed to the nucleus by the three-membered ring, with planarization of the molecular framework and the expectation of charge donation to, and withdrawal from, the aromatic core, dependent on the nature of the exocyclic substituents (Scheme 22). The schematic summary has appeared before in various guises. Simon began his contribution by extending Cliff's initial syntheses to the first of the push-pull benzocalicenes and benzotriaheptafulvalenes as depicted by **65-68** in the benzene series; Qiu Mei repeated the experiments in the naphthalene series and prepared **69** as well. These compounds clearly showed that the cycloproparene could be annulated with both donating (cycloheptatrienylidene, *cf.* **66, 67**) and electron withdrawing (cyclopentadienylidene, **65**) moieties. We studied spectrochemical, spectroelectrochemical and photoelectron properties of these compounds using the facilities available not only in Salt Lake City but also, in Eugene, Oregon and Washington DC. The behaviour of the range of compounds towards electrophiles and nucleophiles, oxidizing and reducing agents and other general reagents followed and a short review on the area was published in *Accounts of Chemical Research* by Peter and me.[51] By this time, Simon Buckland was reaching the end



Scheme 22

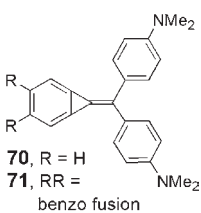


of his time at VUW and I was fortunate in having the funding for a 1987 Teaching Assistant. However, there was no VUW student qualified and interested in the position and the problem became one of finding an appropriate appointee.

By chance, a student from Fudan University in Shanghai, Qi (Chi) Lu, wrote to see if there was any possibility of studying for PhD in Wellington. As Fudan is one of the oldest and most prestigious of China's C9 League universities (China's Ivy League equivalent), competence seemed likely and the references bore this out. Chi accepted the teaching post, fully aware that he would need to communicate effectively in English, and he arrived in late 1986. He studied English hard, improved his ability with it and took on the teaching work, but not without trepidation. His chemical knowledge was far superior to his NZ peers in the laboratory and he slowly came to grips with us and we him. During the Chinese Cultural Revolution he and his family were moved forcibly to Tibet and he was required to study music. I was astonished at how quickly he gained his scientific knowledge after he was allowed to return to Shanghai. However, the transition from the rigid controls of home to the open society of NZ overwhelmed him and he was found to have taken clothing for his sister from one of Wellington's stores. The then Chairman of Chemistry, John Tomlinson, provided outstanding support and rallied the Department behind Chi. I provided the Court with a statement, attended for his hearing, and found the Bench to be occupied by the only Wellington judge that I knew. Chi received a small fine, never to err again. This did not mean that the Chinese Embassy let him off. Staff members (presumably their secret service) were sent to the university in search of him and we responded by putting Chi in a secure location. I made an appointment with the Embassy's First Secretary Lin Xinglan, and sought to smooth matters so that Chi could continue his studies. The one vivid recollection of that meeting that I retain is that had the slight Mrs. Lin decided I was not to leave, I would never have got past her! Chi stayed, and I have kept the New Year card the Lin's sent at the end of 1987 to remind me of that auspicious meeting. To the best of my knowledge, Chi was the first Chinese national to study for a higher degree at VUW, certainly in chemistry and the sciences.

One of the things that we had wanted to do in extending our exocyclic alkene chemistry was to make meaningful measurements of the polarities of the compounds. However, there was no suitable cell available for the small quantities of the compounds that we had. The original Guggenheim procedure had used the now impractical 100 mL of *ca.* 0.1 M solutions of the compounds, and conse-

quently a miniature capacitance cell had to be designed. Our then highly skilled glassblower, Ian Crighton, rose to the challenge, obtained the remaining supply of small variable capacitors from DSIR and built two cells (see opposite) with which readings were taken from *ca.* 2 mL of 0.05 – 0.15 M benzene solutions. Impedance readings were taken with the cell open and closed (done by rotating the glass socket spanner shown protruding above the cap) and the necessary refractive index measurements were from an Abbe refractometer. The cell gave results for known compounds that lay within 5% of the literature values, and for every alkylidenecyclopropparene whose dipole moment was determined, a standard was measured before and afterwards. Eventually, the dipole moments of almost 90 compounds were obtained in this way. It was Chi Lu who began expanding the series of compounds and he made the first dipole measurements on the known and new compounds. In 1987, when he started this work, we had no personal computers, but my good friend and physical chemistry colleague, Jim Pearce, lent us his HP 75C programmed microprocessor to make the mathematical transformations. Our initial report covered the dipole moments of the then known compounds and this was followed by one on the ambiphilicity of the system.[52] It had been our intention to compare the polarities of cyclopentadienylidene and cycloheptatrienylidene **65** and **67**. Indeed, the dipole moment data on **65** (R = H; μ 2.6 D) and **67** (R = H; μ 1.2 D) could easily be taken as supporting cyclopentadienyl anion and cycloheptatrienyl cation (tropylium ion) character, respectively. However, further computational results from Apeloig were convincing in having *both* dipoles caused by electron donation from the cyclopropparene core; the seven-membered ring was sustaining an increase in electron density. Thus, we turned our attention to the well recognised electron donor, *N*,*N*-dimethylaminophenyl. Chi made the benzo and naphtho derivatives **70** and **71** in short order and recorded their dipoles as 3.0 and 2.2 D, respectively. The ability of the cyclopropparene to sustain both positive and negative charge was clear, especially when the effects of UV-vis data and nitrogen quaternization were considered. The dual nature of the cyclopropparene was confirmed by subsequent experimental, crystallographic and computational study. I collected my first Recognition of Research award and a DSc degree from Victoria University that year.

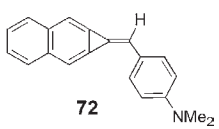


70, R = H
71, RR = benzo fusion

The year of 1987 was notable for several other reasons: the textbook *Organic Photochemistry* underwent revision for a second edition (published in 1988), I was President of the NZIC, and we had visits from Al Padwa from Emory University, Atlanta (with whom Tony Woolhouse had spent a period of leave from DSIR)

and Merle Battiste. Merle spent his sabbatical at Canterbury as an Erskine Fellow with Jim Coxon as bequest funds have always allowed Canterbury to attract more international visitors than Victoria. Jim had spent his first leave with Merle in Florida in the 1970s after discussing the U of F and Merle with me. Merle's first visit to NZ was all the more memorable as he met his (subsequent) second wife, Jan, while in Christchurch. While both of these occasions were stimulating in their own right, a third visitor sought permission to come and meet with me. He distinguished himself with Margaret, becoming known as '*the vegetarian*' – he only told me of his dietary needs on the way home to stay with us with a lamb-based dinner almost ready! He was a crystallographer, Roland Boese by name, and his visit heralded not just 20 years of collaboration but a strong lifelong friendship with him and his family. He arrived from Essen in Germany *en route* to the International Union of Crystallography meeting in Western Australia, gave us a great lecture, and asked me if he might have some of our small-ring compounds as he wished to focus his crystallographic study towards strained molecules. Our samples requiring structural characterization were on his desk before he got back home! What his analyses showed us was that, while the aryl substituents of **71** were twisted out of plane, the extent of the distortion was much less than in comparable compounds – the absence of pendant hydrogen atoms adjacent to the ring fusion sites allowed for the aryl rings to be closer to planarity with a twist angle of *ca.* 28° rather than the *ca.* 45° of diarylethenes. The expectation of small twist angles had had us seek monoaryl derivatives and the synthesis of **72** had much in its favour.

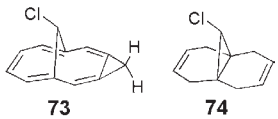
Chi Lu appeared in my office one morning to seek advice on how to get his glassware clean which, despite much washing, glowed green! This was not his inability, rather the exceptional fluorescence of **72** that he had just prepared. It triggered a further aspect of our study, namely an examination of the absorption/emission characteristics of the alkylidene compounds. In that era, VUW had no fluorescence spectrophotometers and there was nothing available to allow us to study the excited states involved. However, fluorescence measurements were made using equipment at the Otago Medical School in Wellington's Public Hospital, and we enlisted the services of DSIR's Hugh Melluish, NZ's leader in the field of photophysical measurement and a member of the IUPAC Commission on Photochemistry. His work provided a reliable absolute quantum yield (Φ_{72} = 96 in both cyclohexane and acetonitrile) and led to the conclusion that the exceptional fluorescence was due to a twisted internal charge transfer state. The compound and its potential as a laser dye were placed under provisional patent in 1988 by the University at the instigation of Ted Harvey. In those days, the recognition of intellectual property rights and the facility to promote their exploitation had not been thought of in most academic institutions and it advanced no further. Multinationals



72

overseas with minimal exposure in New Zealand were interested in having access to the compound but no one was willing to provide financial support to an academic institution here to advance the study. The data were published subsequently in the chemical literature.[53]

That same year of 1987 saw Sarah Russell begin her PhD studies as my last University Grants Committee postgraduate scholar. Her work was focussed on our continuing exploration of structural types carrying a fused cyclopropene ring. Specifically, she studied nonbenzenoid cycloproparenes and was able to provide the sole air-stable cyclopropa[10]annulene, **73**, whilst exploiting much three-membered ring chemistry in polycyclic assemblies. Chlorine-containing **73** was easily reduced to parent cyclopropa[10]annulene, a molecule synthesized by Vogel using the Billups route many years earlier. An interesting consequence of her work was the π-facial selectivity of the chloro-substituted tricyclic diene **74** recorded in, for example, carbene additions. Following a lecture at Imperial College in London late in 1991, Henry Rezpa was keen to perform some calculations and they led to a *Perkin Communication* that was prepared over three days by circulating the draft manuscript via e-mail from Wellington to London to Essen and back; electronic mail had certainly made its impact!



73 **74**

Service in New Zealand and Beyond

My year as President of the New Zealand Institute of Chemistry started on October 1, 1986. At that time, the Institute was a fully professional organization requiring appropriate qualification in the subject to gain membership and job advertisements had (traditionally) carried the qualification in chemistry needed for the job in question. All of the various professional demands of qualification and registration were dealt with by long standing Registrar, Denis Hogan (1927-2006). However, society and professionalism were beginning to change and much of my year was spent holding the two factions of the organization at bay. On the one hand, there existed those who felt that professionalism and all its ramifications had to be retained to ensure that standards were maintained and the profession appropriately recognised. On the other hand, a more vocal group foresaw a decline in membership, a departure from professional needs, and a requirement for the organization to become more open. Membership and number predictions dominated and the debate was rampant. For my part, I saw the environment changing but felt that it was too soon for NZIC to make major change. In fact, it did not until after Denis had resigned as Registrar some while later. The growth in membership that deregulation had predicted did not eventuate and current numbers are somewhat lower than they were more than 20 years ago. As 1987 President, I was required to visit all of the

NZIC's six Branches and lecture at their main and outlying centres, ten lectures in all, and I did this giving an overview of small rings in chemistry. As for most incumbents to the position, making these visits was the highlight of my year. I was shown generous hospitality and everyone went out of their way for me. I was given tours of the Ivan-Watkins-Dow plant at New Plymouth that had the notoriety of being the last site to make 2,4-D and 2,4,5-T, NZ's cottage acid industry producing hydrochloric and sulfuric acids as Scientific Services Laboratory Ltd. in Napier and run by Gerry Hussher, Nelson's Cawthron Institute and Invercargill's aluminium smelter at Tiwai Point. In those days Institute Presidents did not stay on the Council for a subsequent year and the end of my tenure provided more time for other things.

What I will always associate with my 1987 NZIC visit to Invercargill and Tiwai Point is the fact that it was in mid-winter with a howling gale blowing from the west, difficult to walk against and chilling to the bone. After taking the train to Dunedin and meeting the main Otago Branch members, I was back in Wellington for a day or so before making my first visit to Japan. The transition from the cold southernmost point of NZ to Tokyo's July heat and humidity was something more than dramatic! As NZIC President and an academic, I was the one chosen to represent the organization as an observer at a Pacific Basin Societies' Planning Meeting for the 1989 Congress to be held in Hawaii. I had know of this meeting sufficiently far in advance to be able to set up a meeting with Prof. Renji Okazaki at Tokyo University as he, too, had become involved with cycloproparenes. In correspondence, he asked me to allow him to set visits to a couple of other universities and so I also attended Waseda and Osaka. The visit with Okazaki initiated much correspondence and personal contact, and Renji came to Wellington in 1998. Waseda University provided my first experience of a Japanese private institution and the formalities that go with such visits. After my lecture a selected group of graduate students each presented a summary of the research they were involved with, but this was decidedly formal and in front of the Faculty Dean, the Head of School and several chemistry staff. As one with no experience of the country or its customs, it was not at all clear if I should simply thank the student or question him/her. I elected to put questions, found the session to take more than the allotted time, and secured a place in history! For Osaka, I was met at Tokyo Railway Station by Ichiro Murata and taken to Kyoto by train where I spent a Sunday at the end of the hurricane season – and got thoroughly drenched seeing the magnificent sights – even the Shinkansen (bullet trains) were cancelled by the ferocious downpour. Osaka University was reached easily by bus on the Monday morning and the discussions and lecture there was the first of many years of interaction with Murata-san. The late Monday afternoon bullet train had me in Tokyo well ahead of the morning committee meeting and, as is so common with our Japanese hosts,

I was met at the station and escorted to the Hilltop Hotel in Ochanomizu, close to the Chemical Society of Japan building.

The Pacific Basin Societies meeting was a significant affair. The three organizing societies were the American (ACS), Canadian (CSC) and Japanese (CSJ) chemical societies, with the last as the host society. Michinori Oki, a Professor of Chemistry from Tokyo University and former CSJ President was congress chairman. Both NZIC and RACI had had observers for the 1984 Pan-Pacific meeting, and so we were invited in the same guise for the 1989 event. The meeting was held in the new CSJ building and the banner entry board welcomed all the delegates by name; Bob Breakspear was my RACI counterpart and President of that organization, and we were accompanied by Sang Up Choi (Korea) and Zhand Dehe (China). The last two took little part in the proceedings and were not further involved. The morning session was dominated by the search for an appropriate, easily recognisable name for the congress and it was the Japanese hosts that proposed the name *Pacifichem*; it could not be bettered, was unanimously accepted, and is now known to almost every chemist the world over. The various discussions that shaped the nature of the 1989 meeting were wide ranging and open to all. However, at an early break, Guy Dutton (1923-1997), Head of the Canadian delegation, took me aside to remind me that I was but an observer, that I had no voting rights, and that it was inappropriate of me to attempt to contribute. Those who know me will recognize that such comments would never win me over. Indeed, I took precisely the opposite tack, contributed to my limit and came away as Chairman of the Young Scholars Fund that was set to bring some 25 young chemistry professionals from emerging countries to the 1989 meeting.

But the comments and the meeting itself ran deeper than that with me. To me, it was clear that if Pacifichem was to survive as a truly Pacific Basin meeting then its organization had to involve more than three northern hemisphere societies. I spent much of the non-meeting time in Tokyo with the American delegation, Norm LeBel (1931-2003) and Chris Pruitt in particular. Norm, a Chemistry Professor at Wayne State University for more than four decades, is regarded as the *'Father of Pacifichem'* as he was involved from its inception to his death; we became firm friends. Chris, on the other hand, was an administrator *par excellence* and became Congress Manager for the 1995 and 2000 meetings. They both recognised the need to expand the organizing committee and saw involvement from Australasia as the only way forward. Early suggestions were for one member selected from between the two societies, but that would have had little chance of success in any early involvement. I left Tokyo with the view that an increase in the organizing societies from three was critical and, as NZ is but a small country, the expansion needed to encompass both NZ and Australia. Over the next two years, having gained NZIC approval, I made approaches to the RACI in Melbourne and secured an agreement

for that organization also to be an organizing society of Pacifichem were the ACS, CSC and CSJ willing to accept us. Securing the support of the ACS was easy as Norm was all for the expansion and he persuaded the Canadians for me. The more difficult task was to convince the Japanese that an increase in the size of the structure would serve them well in the future. Norm LeBel and Chris Pruitt were generous in ensuring that my time at Pacifichem 1989 was spent meeting individuals who could further our cause. They ensured that I left appreciating that Pacifichem extended beyond chemistry and national boundaries to international collaboration, cooperation and a demonstration of how peoples could live and work alongside one another in peace.

I mentioned earlier that I was an invited speaker at ISNA-6 in Toyonaka, Osaka in 1989. I am sure that this came about from my 1987 interactions with Renji Okazaki, his colleague Hiizu Iwamura (then at Tokyo University), and Ichiro Murata. Murata was Conference Chairman and Iwamura its Treasurer. Returning to Japan to attend that meeting provided the chance to meet with Renji again in a more relaxed atmosphere than the conference – a fact that has been a feature of my career, initially because conference leave overseas was only available every three years. This I did, but I also spent time discussing the Pacifichem issues with CSJ staff in Tokyo and then, much more seriously, at a meeting with Michinori Oki during the ISNA meeting and some months ahead of 'his' Pacifichem 1989. I had been told that Michinori Oki was of Samurai warrior descent and that was entirely believable from his tall solid stature and voice. My recollections of the meeting are that he was not too enthusiastic in expanding the organization as Australasia had few chemists and would be unable to contribute at an appropriate level. Nonetheless, we concluded with a favourable agreement and, in late 1990, after the post-congress 'wrap-up' meeting, I was informed that the membership bid had succeeded. Both NZIC and RACI have been involved continuously ever since and Korea (2005) and China (2010) joined subsequently. NZIC needed look no further than me for a willing Pacifichem representative. I was elected to the role in 1990 and attended the 1991 meeting to sign the agreement for Pacifichem 1995. I held the position until after my retirement (and inability to secure medical travel insurance for the US) in 2004. Participation at Pacifichem climbed from 1989 through the 1995 and 2000 meetings for it to become the largest chemistry event worldwide. There were well in excess of 11,000 registrants for Pacifichem 2010 and some 11,500 presentations. Those meetings involved a lot of hard work, none less than that done by Chris Pruitt and Stan Israel

(1944–2004), who joined the group with me in 1991. The pair had typed reports and programme proposals in everyone's room by the time we woke up, having not completed the typing and photocopying until well after midnight. The committee meeting in December 2001, at which the 2005 agreement was signed, was a clear demonstration of Pacifichem unity, being just two months after the shock of 9/11.

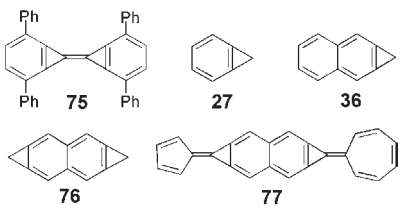
I am left with many memories of this work, of the terms of Michinori Oki, Norm LeBel and Shinji Murai as Congress Chairman for 1989, 2000 and 2005, respectively, and of friendships strengthened, especially that with Paul Walter, congress treasurer supérieur and an ACS president during that time. Indeed, at the 1996 signing ceremony meeting Paul Walter was President-elect of the ACS and Ron Breslow, of Columbia University in New York City, was the then President close to the end of his term of office. I was more than aware of Ron's work in the small ring area of organic chemistry and that Merle Battiste had been his first PhD student with Bob Grubbs moving to Ron from Florida for his PhD in the mid-late 1960s; we had not met and the opportunity was more than enjoyable. The sad loss of Norm LeBel in 2003 was superseded only by the untimely death of Stan Israel in 2004; he was, by then, a very close friend. In fact, he had wanted me to transfer to Texas State University in San Marcos, where monies for research would be provided without applications. Such a move from the comparatively low salary base of NZ would have meant major mortgage commitments, a feature I could not take on. As for the congresses themselves, I gather that I hold some sort of record in asking the waiting staff at Sheraton Waikiki to serve me dinner so that I could leave within 30 minutes, and also in setting judges on the Student Poster Award trail with minimal criteria to follow. I was given the duty only the day before the competition began and, as judges could only be gained at the last minute, the simplest of criteria, *visual impact of the poster* had to apply; the number of young lady winners proved high! I lost Christmas Day 2000 as the only flight I could get from Honolulu was on Christmas Eve crossing the date line and arriving on Boxing Day. Peter Stang joined the American delegation for the 2005 meeting and had to carry Stan's load as well as his own for the last year.

Stan Israel 1944-2004

More Chemistry and Some Recognition

I have described Chi Lu's discovery of the highly fluorescent cycloproparene **72** and Victoria placing the results under patent. He continued his studies after my NZIC Presidential year and laid the foundation of the invited lecture to ISNA-6. A substantial component of his PhD thesis work covered the synthesis of a large

range of exocyclic methylene compounds and then recording a range of physicochemical properties including the IR and laser Raman spectra, absorption and emission spectra, the molecular polarities and the ^{1}H and ^{13}C NMR chemical shift data. A range of the compounds were subjected to crystallographic study by Boese and his students and, as was noted earlier, the twist of the diaryl substituted derivatives is a mere 28°, while the monoaryl-substituted compounds have the exocyclic bound aromatic ring essentially coplanar with the cyclopropene core; fluorenylidene compound **65** is almost flat. Chi Lu completed his study by synthesizing dimer **75** from the C1 carbene derived from our first cyclopropene **29**, in higher yield than recorded in the literature and then explored the chemistry of dicyclopropanaphthalene **76**, again prepared in better yield than formerly recorded. Comparisons and correlations of the structural parameters of the three hydrocarbons show the geometry of **76** to lie between those of **27** and **36**. Sadly, we were never able to transform **76** into its corresponding anion and provide exocyclic alkenes of either one or both of the three-membered rings; dialkylidene derivatives could have provided extended push-pull systems as illustrated by, ***e.g.*** **77.**



Two of Roland's students came to our NZ labs over our summer months, forerunners of the now common German exchange students that Jim Johnston has regularly. In the late 1980s my students from Essen brought their own monies with them as VUW had nothing in place, though I was able to glean some summer research funds to assist a little.

During 1988 one of the most dedicated of Honours students worked with me. Janet Bridle set to work attempting to generate cyclopropa-fused furans, a piece of work that provided interesting chemistry but not the desired compounds. It was brought to a publication state by Dr. Eva Lovett, who was here on leave from Petrolite Corporation of St. Louis (Missouri) in 1989. Janet gained a IIi Honours degree, spent time in the UK and married and, most distressingly, developed Toxic Shock Syndrome from which she never recovered and has been under continuous medical supervision for many years. Before her debilitating illness she was always keen to hear of developments, even thought seriously of returning for PhD study and was a joy to have had as a member of the team. During her year, Richard Neidlein visited NZ under the auspices of NZIC. It was the first of several visits that he made and our discussions were always profitable. However, Richard distinguished himself in more ways than he might have wished and caused an element of notoriety in Wellington, and especially at our home where he was supposed to stay; his unannounced travelling companion was a female PhD student.

As it was then six years since my last sabbatical, 1988 denoted the maximum

accumulation of leave entitlement. By this time, our eldest son, Mark, was in the latter part of his school education and, with public examinations, it was impractical to take leave as a family overseas. My programme involved an October return to Utah for a few days with Peter Stang, visits to various places in the US, and then on to London and another lecture tour of Germany and Switzerland. This was followed by a return to the Southern Hemisphere to give a plenary lecture at the Melbourne Synthesis meeting in early December and then the bulk of the summer at Monash University carrying out some flash vacuum pyrolysis studies with Roger Brown. The northern lecture tour was useful in several ways. Firstly, it provided more time with Peter to settle future directions of the cycloproparene work face to face, and that was coupled with a desire that I try to spend a longer period there the following year. I saw Al Padwa at Emory University as he had encouraged the visit when he was in Wellington the previous year, and that meeting changed the direction of my 'spare time' for most of my remaining career. By 1988, Al was the Series Editor of the JAI Press texts on organic chemistry and he was keen for me to take on the editorship of a volume on strained organic molecules. The invitation was tantalizing, not least because he was so adept and prolific in his own chemistry to think me up to the task. Despite the pressure Al applied, I promised to give the matter serious consideration and over the following months did just that. Eventually, I wrote and accepted, giving an undertaking to have the first volume in the series published in early 1991.

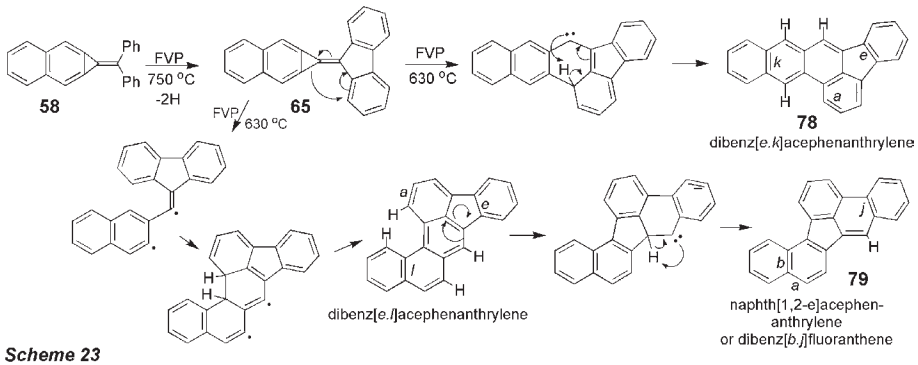
In Germany, Richard Neidlein had put together a three week lecturing schedule but it was by no means a circular tour, rather a trekking back and forth across the country from Essen in the West, to Regensburg in the East, Hamburg in the North and then to Geneva in the south. The train travel became tedious and a stretch of railway line near Mannheim reminded me of Jerome K. Jerome's book *Three Men in a Boat*, but rather than repeatedly seeing a bun it was washing on a clothes line! Nonetheless, the travel had a bright spot. On the sector between Cologne and Essen my compartment was shared by another chemist and north country Englishman. He was the then President of the Royal Society of Chemistry, Sir Jack Lewis (now Baron Lewis of Nownham). We had an exhilarating discussion, brought to a close all too soon by arrival in Essen.

The meetings that Neidlein provided for me were by far the most comprehensive I have ever had. Thus, I met, and subsequently came to know, Klaus Hafner in Darmstadt, renewed my acquaintance with Klaus Müllen, then at the Johannes Gutenberg University in Mainz, that has evolved into a true friendship. I met Armin de Meijere in Hamburg, renewed my 1985 ISNA-5 acquaintance with Neuenschwander in Bern, and made my first visit to the Max Plank Institute for Coal Research in Mulheim. Visits to my well established contacts and collaborators were also included. My last visit was to Heinz Dürr in Saarbrücken and,

with a day and a half in hand before my flight to Australia, I returned to Mainz to have dinner with Klaus Müllen and discuss future interactions. As my departure was the next evening, I was able to meet Rolf and Angelica Rehberg at Frankfurt Airport when they returned from an overseas holiday. It was a happy reunion, our first since they left Gainesville in 1967. Rolf spent his entire career working for Hoechst AG (it became Aventis after its merger with Rhône-Poulenc in 1999), initially in Frankfurt and as a senior representative for the company in Japan for a period that covered my earlier visit to Germany. It was the beginning of infrequent but enjoyable times with them. He moved to Cologne after being relocated to the Knapsack plant, which I visited in 1991 to see the 500 L scale diazotizations used in the dyestuffs industry.

Once in Australia, I spent a few days at the University of Melbourne staying with Martin and Cathy Banwell. The first couple of days provided time out after the long flight and then it was the RACI Victoria Section *13th Annual Synthesis Symposium* at Melbourne University on Friday, December 2. The meeting has become a focal point of the RACI Victorian calendar and continues to attract full representation from the region. The 80+ registrants were stimulated by the opening plenary from Prof. Bernd Giese, at the time a colleague of Hafner's in Darmstadt and already in Australia when I had visited there just a few weeks earlier. The last morning lecture was from Prof. Frank Bachelor (Calgary, Canada) and I had the pleasure of presenting the closing plenary *Methylenecyclopropar*enes: *Strained and Polar Aromatics*. There were about a dozen posters that were judged during the session breaks and after my lecture. The student prizes were sponsored by *The Australian Journal of Chemistry* and presented by its then editor, John Zdysiewicz (John Z; 1943-2010); that was the first time that I met this remarkable and humble man. The time at MU served me well as discussions with Martin, David Kelly and Don Cameron influenced some of our subsequent work.

On my first Sunday in Melbourne, I moved south of the Yarra River to stay with Roger Brown and his wife Mary in their home in the suburb of Prahran. Roger had arranged a university apartment for when Margaret and the boys came to join me at the end of the NZ school year, but it was not immediately available. I had the pleasure of sharing the Brown home for a little over a week when we commuted south to Clayton and the Monash campus each day and I slowly acclimatised, not just to the Australian summer but to a campus that was comparable in size to that of the University Florida. The first task was to finalise my manuscript of the third review on the cycloproparenes and get it off to Josef Michl, then editor of *Chemical Reviews*, and this I did. David Black had moved from Monash to Sydney by this time but the summer gave me ample opportunity to interact with Roy Jackson (another Lancastrian who held the Organic Chair), Ian Rae, David Collins, Syd Middleton (who had spent a leave with Robin Ferrier in Wellington)



Scheme 23

and, of course, Frank Eastwood. I say 'of course' as it was the pairing of Frank and Roger that had established Monash as *the* centre for pyrolysis chemistry. My time was to be spent assessing the behaviour of the simplest of the alkylidenecycloproparenes under flash vacuum pyrolytic conditions. The fluorenylidene **65** (Scheme 23) and diphenyl **58** derivatives were those chosen as they were available in sufficient quantity for study. The pyrolysis work used the very simple, yet especially functional and well used equipment that Frank and Roger had. Both compounds gave a series of products, the latter converting into the former by cyclodehydrogenation at 750 °C prior to opening of the three-membered ring and expansion to a series of polycyclic aromatics (Scheme 23). As we were aware of the potential carcinogenicity of the products, the work was performed on small scale and it was not possible, at that time, to be sure which products we actually had, other than the known dibenz[*e.k*]acephenanthrylene **78**. As the years passed, the number of polycyclic aromatics with data matching our own, *e.g.* **79**, increased, but it was not until 2006, some ten years after Roger had retired from Monash as professor that the study appeared in print. It formed a *Tetrahedron Letter*, for which I received surprisingly generous reviews from its assessors. I had asked Roger to co-author the paper but he declined and, in so doing, gave me my only primary publication as sole author.[54]

Returning to Wellington in late February 1989, I hosted Eva Lovett who had leave from her St. Louis job as I noted above. She was able to bring the bicyclo[4.1.0]hept-1(7)-ene work to a nice conclusion that, subsequently, I was able to incorporate into my Pacifichem '89 presentation in the symposium on *Strain Effects.* However, I needed to think more carefully about the invited lecture I was to give at the August ISNA-6 meeting in Toyonaka, Osaka, and to see if it would be possible to revisit Peter Stang in Salt Lake City, as he had been encouraging me to do. Sarah Russell and Chi Lu were, by then, sufficiently advanced with their studies that my absence late in the year would be non-problematic and so I sought funding to spend time in Utah ahead of the 1989 Pacific Basin Congress in Honolulu.

It was a very hectic time for us as by then Margaret had gained her essential *Teacher Training Certificate* that she was able to do while teaching part-time. This she had done at Karori's Samuel Marsden Collegiate School, a job to which she was encouraged and led into by one of New Zealand's famed chemical immigrants, Dr. Dora Suuring, then about to leave her role there as the temporary Chemistry Teacher. Dora came to NZ after WWII and, it transpires, had been actively involved in the Dutch underground movement providing falsified documents and the like; she is one of the longest surviving Wellington members of the NZIC as far as I am aware. With her registration and our boys in secondary school, Margaret had become a full-time teacher and accepted the role as Head of Science from the beginning of 1989. She rose to become Dean of Student Achievement some years later, retiring in 2005 as she had been worked to the bone and no longer felt that she should keep me in the lifestyle to which I had become accustomed! My time at ISNA-6 included a visit to Okazaki in Tokyo before travelling to Osaka and then the Post-Conference meeting in Sendai, for which I was also asked to give a lecture. However, not being sufficiently familiar with Japan at that time, I declined the invitation as, with only a small group by international standards, I did not feel able to deliver two highly relevant first-rate lectures and did not want to deliver the same one twice, as did others.

The author and Emanuel Vogel (1927-2011)

ISNA-6 was held in August 1989 in the heat and humidity of the Japanese summer. Simply exiting the air-conditioned environment of the meeting for lunch took me back to my time in Florida. The lectures were stimulating, none less than that by (now Sir) Harry Kroto on the C_{60} discovery and subsequent study. However, the plenary lectures and subsequent interactions with Fritz Bickelhaupt, Fumio Toda and, to a lesser extent, Siegfried Hünig led to ongoing enjoyable chemical and personal interactions. Of course, this second visit to Japan extended my appreciation for the culture of the country and my friendship with a number of its people. One of the things that the ISNA-6 organizers did was to place registrants new to Japan in contact with Osaka chemists who were area members of bilateral societies. Thus, I received a delightful invitation from one Haruyasu Shiota to meet with him at the end of the meeting. I did and we have maintained contact since. We exchange cards at New Year and have dinner whenever I visit Osaka. His son spent time in our home on a visit to New Zealand but, although Haruy has been to NZ as an executive of the Osaka Japan-NZ Society, he has yet to come to Wellington. He recently retired from his role as a flavour chemist in Shiono Flavor and Fragrance Co.

The Osaka meeting provided an opportunity to discuss more chemistry with Ed Billups from Rice University, who was also an attendee. In fact, we both went on to Sendai to be hosted by one of Chemistry's junior staff, Tadayoshi Morita, as his English was better than that of Tsutomu Kumagai. They took us to Shiogama for an excursion on Matsushima Bay on the boat shown in the photograph. The more relaxed atmosphere gave a great opportunity to get to know our Japanese hosts and explore our common interest. In fact, it was this meeting more than ISNA-6 that aligned me with Koichi Komatsu who has looked after me in Japan on many occasions – but more of that later.

Morita, Billups, Halton & Kumagai

Back in NZ, I applied for a Claude McCarthy scholarship as, having gained a New Zealand DSc, I met the criteria, namely *to enable any university staff member who is a graduate of a New Zealand university to take leave and travel overseas, other than at the time of their normal entitlement for refresher leave and additional to it, to undertake short periods of research in important recent developments relevant to their specialties*. Why I raise this is that for the first time I was asked to submit a budget for the period, with costing geared to public service rates – something that does not form any part of a university leave scheme. My application was successfully submitted and I spent some six weeks at the University of Utah over November-December of 1989. We planned what we could continue to collaborate on, given US funding agencies were becoming more sensitive to the nature of the work involved and Peter less likely to gain support for fused aromatic chemistry. In contrast, the NZ scene still had no funding for fundamental studies except through university monies. Funding though the Ministry of Science was being established but it emerged to be only for applied science in areas dictated by the Ministry. The more important task in SLC was to writeup the studies that had been completed and the time passed quickly with the completion of four manuscripts. These covered the remaining work of Chi Lu in Wellington and that of Lingsheng Song in Utah, which had concentrated on reactions with organometallic reagents. Song's departure left Aileen McNichols as the last researcher in the cycloproparene area in Utah. My departure from SLC to Hawaii was by way of Reno and a visit with Larry Scott at the University of Nevada, my last before his move to Boston College. I had got to know Larry even better at ISNA-6.

At Pacifichem, my oral presentation was in the Symposium on Strain Effects, scheduled for the first full day of the meeting. Almost all of those at the forefront of the field were present; my range of contacts was extended and the opportunity to

meet many of those I had not seen for some time arose. The symposium included a luncheon organized by Phil Eaton (Chicago) to which a good number of the participants were invited. I had run across Phil in passing but this gave us a good personal introduction. This aside, Norm LeBel went out of his way to be my host, introducing me to the working of the conference, and he led me to the conclusion that the NZ and Australia bid to become formal partners in the organization would be sympathetically heard and, as I have said earlier, it succeeded. The inclusion of the Australian and New Zealand chemical societies among those organizing Pacifichem has to be my most significant contribution to NZ chemistry. It has served to formalize internationalization, given the profession added visibility and strengthened ties worldwide. I received a VUW *Research Recognition* award that year as well.

The return from the McCarthy Fellowship coincided with Christmas, the new decade, the expectation that Chi Lu would soon defend his PhD work, and that Sarah (Russell) would have her's submitted by about the middle of the year – but no prospects of replacements for them. NZ science was undergoing major restructuring and university funding, never great, was suffering as academics had the greatest difficulty in obtaining monies from the emerging contestable fund … and, on the personal front, I was advised that my mother was showing increased signs of dementia.

The year was comparatively quiet but set to a good start by Chi gaining excellent reports from his PhD examiners and performing well at his oral in February of 1990. Had awards been offered and theses graded in that era, I am sure that he would have collected a distinction and an award. Without doubt, he was the most productive PhD student in the Department at that time, and indeed for many years, but that was partly the project and not just him. His demeanour changed and he was less than happy tying up the ends of his work while waiting for his move from NZ. In 1990, any return to China would have resulted in serious repercussions for his 1987 indiscretion and he was very keen to get to the US and work there for some time. Chi had made contact with Professor J. H. Boyer in the University of New Orleans, an expert in fluorescence, who offered to sponsor him for a 'green card'. This he did and the time came for Chi's all important interview at the US embassy in Wellington. His most prodigious hobby was the game of bridge, which he played at the Wellington Bridge Club (the largest and most prestigious in the country it is claimed). I recall his departing for the interview in trepidation, and his sheer elation on return – the immigration interview had been conducted by the only American he knew, his bridge partner! He was granted the green card, went to New Orleans University and then spent a second postdoctoral at Tulane before joining Monsanto in St. Louis (Missouri). When Chi arrived in NZ, he was adamant that chemistry was what he had always wanted to study but by the time he

left this had changed to medicine, and the early period of his time with Monsanto was spent seeking admission to a US medical school. As far as I am aware this never happened and he left Monsanto sometime after 1996 and may well now be back in China, but I have lost contact with him.

Sarah Russell had her thesis completed by the middle of the year and was examined by Frank Eastwood at Monash and Con Cambie in Auckland. Sarah experienced no difficulties through her examination process and left Wellington in mid-August 1990 for a postdoctoral with Prof. Jaeger in Laramie, Wyoming. He would have kept her on for a second year had the project not been declined further funding. Her approaches for a postdoctoral position had problems in that the reference from a colleague was, in her view, biased. It is not that she obtained a copy in an untoward manner rather than being given it by one of the Department secretaries because she, too, was concerned. The issue was a difficult one and the then Head of Department, the late Professor John Tomlinson, handled the matter with much skill and diplomacy. Sarah returned to New Zealand and secured a position in Forensic Science in Auckland, with what was DSIR and transformed into Environmental Science and Research Ltd. (ESR) with the reorganization of NZ government science shortly after she joined. She transferred to the Wellington (Porirua) facility and has had a distinguished career with them.

From Recognition to Retirement

A New Direction

The year of 1990 had my research work largely on hold as the laboratory was empty, save for one Honours student, and I had even less time available to devote to serious laboratory work myself. Thus, it was largely a time of taking stock and transforming the idea of *Advances in Strain in Organic Chemistry* that I had agreed to edit under Al Padwa in JAI Press series, into reality. I had selected a range of recognized authors –including Padwa by way of retribution – Mark Baird (who moved to Bangor that year), Martin Banwell, Ed Lee-Ruff (with whom I had been associated at Pacifichem '89) and Herbert Meier, who I had met in Mainz in 1988: everyone agreed to contribute. The manuscripts arrived as expected and gave me my first experience at personal editorship. As all academics do, I had served apprenticeship as a referee for a number of periodicals in my area, not least those from the US and UK. Thus, the standard for the articles was much less of an issue than presentation and level for the targeted senior undergraduate/graduate student audience. I had had a reasonable amount of experience in writing and in having review articles accepted with minimal amendment, so I stumbled along. Al was particularly supportive as the volume took shape, and proof was eventually issued for publication in 1991; Ed Billups provided a nice review of the volume in the *Journal of the American Chemical Society*, but that did not appear until 2003. The most difficult aspect of editing the first volume was in deciding just how much of Herbert Meier's English should be altered. It was not an easy decision to make. I did not wish to offend him, but I came to the conclusion that if the series was to succeed, then it needed internationally recognised researchers summarizing their own field of research, and for this to be edited, heavily if necessary, to provide a uniform standard with easily understandable English. This concept led me to Volume II that carried four of the five chapters from authors in Continental Europe, with English as their second language. Volume III included the first of the Japanese contributions and the series continued to progress well. My title in the JAI series was initially produced out of the UK office and then moved to its US counterpart before the series was terminated. That happened after JAI was taken over by Elsevier who felt that *Strain* generated insufficient profit. But the experience and camaraderie that I got from the editorial role and the frequent communications and

meetings with the contributors persuaded me that this was a role that I could happily revisit should the occasion arise. Indeed, comments from some of the authors served to encourage me into what has become a major part of my professional life in retirement.

By the middle of 1990, my step-father had advised us that my mother's dementia was becoming more serious and that it would be best to make a visit during the year, if at all possible. Sadly, it was not possible for all four of us to get to London, but as December of that year signified Mark's eighteenth birthday, he and I spent Christmas in London. As it so happened, the well respected Fritz Bickelhaupt (Vrije University, Amsterdam) had encouraged me to spend some time in Holland and so a short lecture tour was set up for me in December 1990. Although I was ineligible for sabbatical leave from VUW, the then convener of the Leave and Grants committees, Librarian Vic Elliot, was always sympathetic to my lecture tours and provided some financial support to assist. However, before I left, I started a summer student, one Andy Kay, who worked that summer in my absence and then continued on for Honours in 1991 and PhD training from 1992.

The lectures in Holland were preceded by a short visit to Essen and Roland Boese, as our structural work had reached an interesting stage. The bond lengths within the aromatic cycloproparene moieties were showing some consistency; the internuclear bond separation of the bridge bond was short while the electron density path of the bent π and σ bonds matched very closely those of a normal benzenoid and σ bond, respectively. The 1990 visit to Essen has more than an element of notoriety associated with it. My flight from London was to Amsterdam (for the Netherlands tour) and then by direct train to Essen. Although the aircraft on which I was a passenger was fitted with all the modern equipment to allow it to take off in fog, it was held in line behind all the aircraft not so equipped until the fog lifted from Heathrow; arrival into Schiphol was late in the afternoon and without lunch. I was quickly ticketed for a train to Essen but it had no dining facilities. Being without food so soon after my departure from Wellington did not augur well and I was not in such good shape, even though Roland had me into a café very quickly. It was only the next morning that I realised I was without passport, tickets and money. All that I could imagine was that my documents case must have been left in the restaurant, but the morning staff knew nothing of it and it was only just before lunch that the owner made contact to say that it was safely locked away. We went to collect the essentials, only to have the proprietor insist on us drinking Schnapps to settle matters before my lecture.

That evening I returned to Amsterdam to be met by Fritz, dined and entertained at his home and then settled to an apartment at the Vrije University for the duration of my visit to the country. The arrangements were excellent as I had the weekend free to see the canals, markets and, of course, the van Gogh gallery. The downside

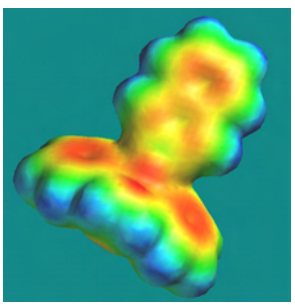
was that I had to take the tram to the centre of the city and collect a worker's train pass for the following week. In itself, this was simple enough. However, when I tried to pay my fare to the tram driver as I had been instructed, I was told to board. There was nowhere to purchase a ticket but numerous machines in which a ticket could be validated – all emblazoned with the penalties for riding without one! I survived without challenge. The lectures went well and my hosts provided me with much fascinating chemistry that once again led to subsequent visitors to the NZ scene, most notably Binne Zwanenburg from Nijmegen, who brought his research group on an Australasian tour some eight years later.

The year of 1991 was significant for the Department and also me in more ways than one. Firstly, John Craig, my organic colleague, retired at the end of January. In those days, the university maintained continuity in teaching by having staff retire on January 31st after their 65th birthday, unless it fell in February when retirement was on January 31 beforehand. It meant that academics were employed for the full teaching year with a known retirement date. What was even more notable with John's position – formally a lectureship in chemistry – was that it was advertised in 1990 ahead of his departure, not as a permanent tenured post but a limited term three year appointment; student numbers in chemistry had declined and the Department was under pressure to contract. By the time I left for my visit to the Netherlands, the shortlist had been drawn up with the leading applicant, Christina Chai, a Chinese Malay with Australian citizenship. She had taken her PhD under Athel Beckwith's supervision at the Australian National and was part way through a postdoctoral in Oxford. As I was to be in London in very early December, I was asked to interview her and report back. The interview was held in a University College office, arranged for me by Peter Garratt; I was suitably impressed by Christina and returned my summary of the interview. I was told that she would be offered the position. However, by the time the offer was to be formalized, the University terminated the post. Robin Ferrier was adamant that the teaching of organic chemistry demanded a minimum of three people and he was successful in negotiating a joint appointment between Victoria and what had just become *DSIR Chemistry*. By the time Christina received her offer of appointment, it was not for the limited term position for which she had been interviewed but as a joint VUW/DSIR job for three years. She accepted, joined us for the beginning of the 1991 teaching year, and quickly established a first-rate teaching and research reputation. Her free radical chemistry had much emphasis directed towards the biological/medicinal environment, but we jointly supervised a Masters student, Malcolm Starr, who attempted to detect the cycloproparenyl radical. The work overlapped with some of Neidlein's in Heidelberg and was jointly published.

Sadly, Christina's lot in Wellington was far from easy. She was given a normal full-time teaching allocation as the 50% VUW component of her role, with the

other 50% at DSIR for research where she had no student assistance; but she was encouraged to start students at VUW. She did her very best to keep everyone happy, but the task was too great, as it would have been for just about anyone knowing that full-time teaching demands more than 60% of one's time. She was discouraged from conference or other leave, and from seeking promotion even by way of an accelerated salary increment, all because she was part-time. Such features no longer play a part in VUW ethos and, indeed, have been contrary to a part-time limited term position for a long time now; likely they were not the norm even then. The increase in part-time positions demanded that the duties be consistent from area to area and no one is now allowed to be given more than a 50% teaching load in a half-time position. By the time her three year term was coming to an end, a full-time permanent lectureship in organic chemistry was reinstated and Christina should have been appointed to it. However, with an equivalent job offered in the School of General Studies at ANU in Canberra, and a refusal by Victoria to give her advancement, she left in early 1994. She was a great colleague and a dedicated teacher whose research opportunity had been limited by her Wellington joint position. Robin Ferrier was the Departmental Chairman when Christina was appointed and just how much of her lot resulted from his decisions is not known.

Two other major events occurred in 1991. The first, that of Curt Wentrup's first *Reactive Intermediates* meeting on Heron Island, Great Barrier Reef, to which I was invited and this was a stunning event both chemically and scenically. Most of the chemists with significant research in intermediate/reactive molecule chemistry were there. I got to know Leo Radom better as we were billeted together, then Maitland Jones and Akira Oku, who I met for the first time. Friendships were renewed with Peter Stang, Roger Brown, Armin de Meijere and Leiv Sydnes to name but four. The last 1991 event at Victoria University was the removal of the 1983 embargo on personal chairs. I was encouraged to let my name go forward – it was not a matter of completing forms and submitting an application as now, but neither was it the same as the nomination process followed by Departmental Heads in the 1970s. That year Robin Ferrier had taken over as Departmental Chairman and he was careful to ensure that I was nominated and that it received the support needed. Reports were sought from some outside New Zealand who knew me, but with no formal connection to me, and others with whom I had had contact for some time. Peter Stang was more than willing to give support and the late Professors Charles Rees (of Imperial College, London) and Athel Beckwith (of ANU) were named as ones who could comment independently on my standing in the profession. The outcome of the first personal chair ex-



A Chair of Chemistry

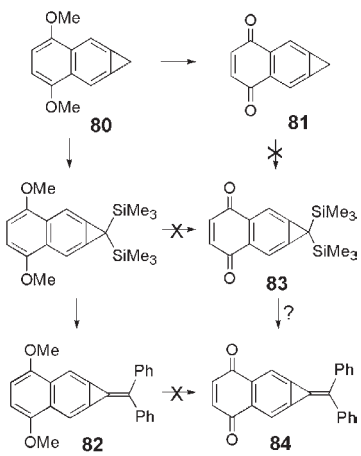
ercise at VUW for some eight years was two appointments, both in science – they were to John Lekner in Physics and me in Chemistry. We each accepted the chair in our respective subject, effective from September 1991. The elevations were treated with great aplomb by the university and there followed a series of interviews and lectures, not least that of the inaugural lecture, which I entitled *Battered Benzenes and Beautiful Bicycles*. Of the interviews, one for Radio New Zealand was conducted by Shona Willis, one of the better New Zealand radio journalists and she came to my office to make the recording. Once again, fate reared its head as Shona's mother-in-law became our delightful next door neighbour when we moved to our second home.

A further recognition came a year later as I was elected to Fellowship of the Royal Society of New Zealand in November 1992. My good friend Jim Coxon had been elected in 1991 and he was awarded a personal chair in chemistry at about the time as my 1992 RSNZ election. As was customary in those days, newly elected Fellows were appointed to the selection panel for their area and I served my time through two terms that took me to 2001. The donation of accolades two years in a row had a clear message – I had to be close to my highest level of incompetence! And so it was. As Robin Ferrier's term as Departmental Chairman drew to a close, the academics were encouraged to put their names forward for election to the position. There was a dearth of activity and so, after some considerable thought, I saw John Wells (the then Dean of Science) and gave him my name. The chairperson of a department or school at Victoria has always been a difficult one because of the bureaucracy involved and the need to do a good job. I had always felt that this was a task for those close to retirement, as research could be reduced over the last years as opposed to having the role early, losing track of one's research field and falling behind with the chemical literature. However, the Dean released my name ahead of the closing date for the chairmanship role, and it proved an excellent move as a number of names were submitted in short order. With such keen departmental interest evident, I then did the unthinkable; I withdrew from the race and infuriated the Dean.

During the time that I was distracted from the Wellington lab, Andy Kay made good progress, initially in his 1990-91 summer studentship and then through his Honours year. Part way through that time Mrs Zha Zhi-mei arrived from Sichuan Normal University for a year of research experience. She settled into the exocyclic olefin and quinone chemistry that was then carried on by Andy, when he returned for PhD after his gaining a IIi Honours degree. Before coming, and while seeking acceptance to join us as a Visiting Fellow, Zha had communicated well in written English; it was only after she was here she told us that her Russian was better than her English, and asked if we could communicate in that language. That was not an option as I have never been competent with languages and so Zha struggled, but

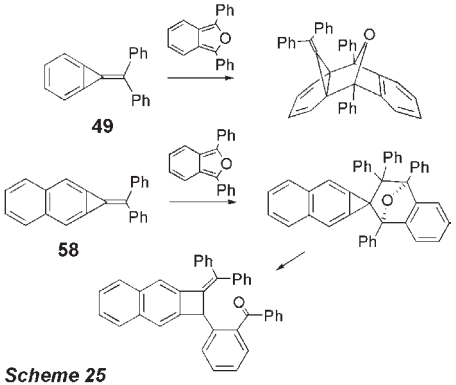
improved enough to cope, although she never mastered her spoken English. Nonetheless, she proved a convivial lady to have around and she tried hard to succeed at the bench. The only difficulty that she presented was with help to get permission for her husband to visit for a few days. Her visa did not allow her to leave NZ, while her husband, who was involved in official trade with Hong Kong, was not allowed to travel beyond there from China. A second session with staff in the Chinese embassy (the Lins had left by then) served to settle things there and they agreed that Mr. Zha could come provided that he was chaperoned by embassy staff once here. The next hurdle was Immigration NZ. Only through the good graces of Mr Schroff was Zha's husband allowed into the country, and then only for a few days – but enough for Zha to persuade him that she should go to the US after her time in Wellington. On our day out sightseeing, we had a Chinese embassy staffer in the car and his presence created a somewhat different atmosphere to the norm when I am host.

Andy's actual research carved out two major areas of cycloproparene chemistry and showed us the limitations in another. He completed Zha's efforts and synthesised **81**, the first stable quinone in the cycloproparene series. Our hope was that this compound would lead to more extended conjugation and give rise to extended 'push-pull' polar molecules. In the process of pursuing this, Andy found cerium ammonium nitrate to be most efficacious for demethylation of **80** and obtained **81** in 85% yield. This provided sufficient compound for the full range of spectroscopic, crystallographic and chemical properties to be recorded, including the electrochemical reduction potentials. What we found totally frustrating was that, while the diether **80** could be silylated and transformed into alkylidene derivatives such as **82** (Scheme 24), neither **81** nor the disilylated diether could be converted into quinone **83** (and hence **84**). Moreover, olefin **82** did not lose its ether moieties without also involving reaction of the exocyclic alkene; extensions into the 'push-pull' arena had to wait.



Scheme 24

The second part of Andy's study combined with Utah work in examining the behaviour of exocyclic olefins to cycloaddition. What was found was that the alkylidene compounds add dienophiles in a Diels-Alder sense. However, the benzene derivatives underwent addition exclusively across the bridge bond while the naphthalene analogues gave products only from addition to the exocyclic double bond (Scheme 25). The nature and orientation of the benzene adduct was proved by



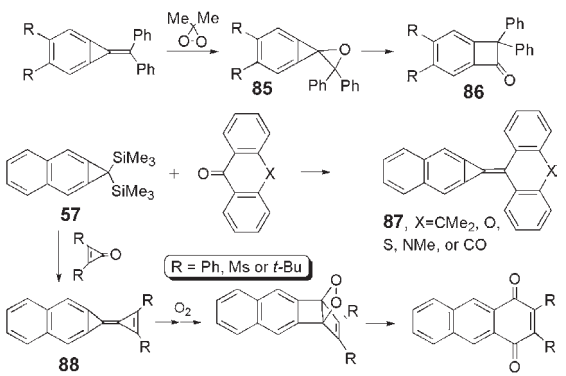
Scheme 25

crystal structure analysis in Essen and the additions were further rationalized by computation, also studied in Essen. By then one of Roland students, Andreas Maulitz, had spent time with Apeloig and was trained in computational chemistry. He put this to good use on our behalf. As Scheme 25 shows, there is a basic difference in the bridge bond character in moving from benzo to naphtho fusion. The last part of Andy's study was to extend conjugation in the exocyclic olefins through functionalization at the carbon atom adjacent to the ring fusion (C2), but this did not eventuate. He made the passage to a PhD chemist by serving as a teaching assistant, not in the traditional half-time post, but by taking a fractional position that allowed four rather than three students to have some funding. He completed his PhD in 1996, had a postdoctoral with Hiizu Iwamura, then at Kyushu University, and subsequently returned to what was by then the metamorphosis of DSIR Chemistry – Industrial Research Limited; he is now the Manager of the Energy and Materials Group.

It was during Andy's time that the funding of science in NZ was dramatically altered and government science reorganized. The various parts of Scientific and Industrial Research were carved into Crown Research Institutes, each charged with returning a profit on the public investment. Science funding was diminishing and the catch phrase became 'accountability'. The universities were no exception and the push for a 'blue skies' research fund gained momentum, ultimately to succeed with the creation of the Marsden Fund largely as a result of the work of Simon Upton, then Minister of Science. He was subsequently elected to Fellowship of RSNZ for this. National Postgraduate Scholarships through the University Grants Committee disappeared, to be replaced by awards offered by each institution, initially on a simple application basis and then by the institution selecting the project work that it would sponsor. The effect of this was (and is) that despite a run of good PhD candidates, Victoria had insufficient scholarships to retain them. Not surprisingly, students with very good upper second class honours degrees gained PhD scholarships from other places in NZ and left VUW. Thus, over three years from 1993, I saw four of my BSc (Hons) graduates leave for PhD training elsewhere, two in NZ, one at ANU, and one in Boston College, USA. Each had made a genuine contribution to my research, but the writing was being etched indelibly into the wall, namely, unless one's work was deemed *relevant* then funding was not going to be made available. As best I recall it, the postdoctoral fellowships were the first

subjected to project selection. Much to my surprise, I was able to put forward a proposal directly to the faculty in 1992 for scholarship consideration, a process that was quickly terminated in favour of internal departmental selection. Be that as it may, I succeeded and gained my second and last postdoctoral. Moreover, I was able to solicit applications by writing to colleagues overseas and then ranking the applicants gained that way with those who applied directly to the university. Larry Scott, who was in the process of moving from Reno to Boston College, had a man keen to join me and, with the relevant paperwork completed, Mark Cooney arrived in Wellington for a two year postdoctoral at the end of August 1993.

Mark was, and probably still is, a quiet individual, particularly knowledgeable in the art of science and exceptionally adept in the laboratory. Not only did he bring a range of modern techniques to the Department, but also he gave his time to those around him who needed assistance. He worked well, initially coming to terms with the area and then completing and delving into a raft of topics in our field. His publications numbered eight from a mere twenty-two months at VUW. They included the synthesis and spectral characterization of the first oxaspiropentene **85** (RR = benzo fusion) that was much more stable than we had expected, but it transformed into the ring expanded cyclobutarenone **86** on standing.[55] Moreover, the simpler starting material (**85**, R = H) underwent an identical series of reactions showing that it is only cycloaddition that involves the bridge bond. It served to remind us that small ring systems annelated to an aromatic core have increased stability over their non-annelated analogues, allowing for easy characterization and study. A range of heterocyclic-substituted methylenecyclopropanes were made and the series of polar dyes **87**, *e.g* xanthone (X = O), obtained. They were subjected to detailed crystallographic and theoretical study, again involving the Boese and Apeloig groups, and shown to be essentially planar. Initial work by Mark, involving the silyl-Wittig reaction of **57** with diphenylcyclopropenone (the dimesityl and di-*t*-butyl derivatives followed), ultimately led us to settle on a reaction sequence to 1,4-anthraquinones that proceeds via bicyclopropenylidene **88** (R = Ph), formally a valence bond isomer of benzyne (C_6H_4), and the first non-annulated derivative to be spectroscopically characterized. Mark's other contributions involved chemistry directed to the cyclopropenyl radical and radical anion. The synthesis of a dioxacyclopropanene



provided extended conjugation and set in train subsequent work on π-extended alkylidenecycloproparenes, which somehow did not get published until 2006. He left Wellington in mid-1995 to settle to life as an industrial research chemist in his home country. He was very much our loss and industry's gain.

One thing that I remember after Mark's departure was the need to keep the administration happy with his appointment by way of a formal report – it was no longer appropriate to say he came, he saw and he conquered. At that point in time, bureaucracy was in the early stage of proliferation and corporatization, and a concise account that included publication off-prints was not quite what the Assistant Vice-Chancellor demanded. The clear requirement, as I saw it, was for a substantial report, with the all-important précis attached. I decided that this was an ideal time to test the waters and so, rather than send double-sided copy of article preprints, double spaced, single-sided copy at a minimum print width and page length was the order of the day. And so it was that a very substantial 'report' was handed in, which probably had had a few extraneous pages inserted for good measure. In due course, I received a glowing letter of thanks for my very detailed summary of Dr. Cooney's tenure and successes. As far as I am aware, that very heavy folder lies buried in a filing cabinet in the (corporate) archives of the university, most likely never opened beyond the précis. It seemed to me that this was fair recompense for having been classified as one with 'no overseas collaborators' from an earlier survey when, in fact, I had more than most. The 'zero' came from the fact that none of my overseas collaborators funded Victoria – only those that put money into the institution's coffers were counted!

Later in that year of 1993 I took my fourth sabbatical but, unlike the earlier ones, the pressures meant that I could not be away for long periods and somewhat contrived absences became the norm. Instead of spending most of the available time in one place, I chose to accept invitations to lecture and to couple those with new experiences, and so I asked Koichi Komatsu if he would be able to co-ordinate a visit to Japan for me. This he did, and I spent almost two weeks there with meetings around Tokyo, Kyoto and on to Hiroshima, and then across the Ituski-nada Sea and into Matsuyama, where I was a guest at Fumio Toda's symposium on Solid State Organic Chemistry. I ended with Sho Ito (who I first met at ISNA-6) bestowing me with the Dean's Lecture at Tokushima Bunri University, the first of many meetings with him as will become apparent later. From there I flew to Germany and spent a little less than two months with Klaus Müllen, who by then had formally moved from the Gutenberg Univer-

Fumio Toda

sity to the Max Planck Institute for Polymer Research (MPI–P), also on the Mainz campus. It was a very happy time as Klaus and I have got on well since we first met in 1982, and my time in his institute proved no different. On a chemical note, I tried to gain evidence for a cyclopropparenyl radical anion from conventional electron transfer using the expertise of Martin Baumgarten, but all we were able to show was that cyclopropa[*b*]naphthalene (**58**) ring opens and dimerizes before the cyclopropparenyl anion radical (or its ring-cleaved naphthalene analogue) could be detected; only the radical anion of pentacene was characterized. Mark Cooney completed the study by carrying out equivalent conventional chemistry prior to publication.

New Staff, New Challenges, and a New Home

I made comment earlier about the need to gain funding for research and how this was beginning to become more and more important in NZ. It seems to have continued unabated in this country from the early 1990s to a situation that now makes it almost impossible to perform any meaningful research, or gain promotion, without it. What is lacking is an adequate size of fund to meet needs beyond those of areas deemed of national importance. It was at the time that this was being advanced that Christina Chai's replacement, and permanent appointee, arrived in the first part of 1994. The position had been widely advertised and it attracted a good range of serious contenders, from which Peter Northcote was duly appointed. Peter, a Canadian marine natural products chemist, who had done some school teaching in Tonga prior to his PhD training and a postdoctoral position with the Blunt-Munro combination at Canterbury University, arrived from his post with a New York pharmaceuticals research organization. He was thrown into first-year teaching and had his independent research initiated by gaining an MSc student, Anthony Fake, who needed serious supervision. Peter settled amazingly well and started what has been a very decisive foray into NZ marine sponge natural products chemistry. He has attracted attention internationally with peloruside A, a serendipitous discovery from a native sponge. He gained pharmaceutical funding because of its potency as an anticancer treatment and is now on the board of Reata Pharmaceuticals Inc., who licensed the compound. What quickly became clear with Peter's work was that the aged 80 MHz Varian electromagnet NMR was not going to meet the needs of a modern natural products chemist intent on structure identification from tiny amounts of sample, rather than the minimum 20 mg for a good ^{13}C spectrum on our instrument.

The aging VUW instrument had survived being deluged in a flood caused by failure of an elbow bend of the mains water inlet pipe on the top floor of the Easterfield Building early in the 1980's and then, later, smoke damage from a fire in

the adjacent chemical store. It had come through these events well and continued to serve us despite its limitations. One of the interesting impacts of science on the local workforce followed the flood, in that a new mains server for Chemistry was installed on Kelburn Parade, outside the Department. At that time, charging for water used by the University was being introduced, and so the new system came not only with a water meter but also an ability to cut the supply to the building. When I asked how a permanent low flow-rate supply to the NMR could be guaranteed, the city workmen responded simply: apparently they installed a bypass that avoided the cut-off and likely the meter. The effect was that the FT80A had uninterrupted water for life, presumably unknown to anyone but the workmen and me! The lifetime of the machine was enhanced by our acquiring the Chemistry Division of DSIR's sister FT80A when the government lab upgraded to a superconducting instrument. This was another interesting acquisition. I resisted a request for payment, saying that it would be just as easy to transfer the machine to our truck at the Hutt Valley rubbish disposal site! In the end, DSIR transported it to us and even managed to damage the tray of their truck in the process, all at no cost!

Despite all of this, Peter's presence was just the trigger needed to set in motion a move for a replacement instrument. It was not so much a case of the FT80A not operating to its specifications, as to it being a second generation instrument still in use in the third generation era; 'supercons' with multinuclear and 2D NMR facilities were in every place that serious chemistry was researched. In a short time, Peter and I had surveyed the range of instruments available, made a case for major instrument funding to the Department, the Faculty and the Vice-Chancellor, and utilised the opportunity to seek the best possible financial arrangements and instrument capability that we could get. Apart from his natural products expertise, Peter was (and is) an order of magnitude up the NMR ladder of knowledge than me, and so it was that he went to Brucker's NMR facility in Germany, while the Varian sales and engineering people came to us, as NZ was dominantly Varian at that time. The final hurdle turned out to be not so much the fund-raising as having to gain support from Robin Ferrier, who felt that our electromagnet instrument was adequate and wanted to know the cost of a straight replacement for it. During our negotiations, a team of Varian's senior executives had happened to pass though NZ and called in to meet us. They were especially enthused when I told them that our electromagnet was working and they insisted on seeing the instrument. This was a new experience for them, as they were under the impression that the only remaining FT80 in operating condition was in interior NW China. The Chinese instrument could have been available to us second-hand, while the cost of building a new FT80 specifically for us was markedly higher than the 300 MHz supercon instrument we wanted. Approval for the latter became automatic, the monies (including a grant from the NZ Lotteries Commission) became available,

and a multinuclear Varian 300 instrument duly arrived with its cryogenic probes manufactured by Nalorac Corp., which was subsequently taken over by Varian (itself now a part of Agilent Technologies). It was installed in late 1996, became fully operational early the next year and is still in excellent working order. However, it was not located with Chemistry in the Easterfield building but with the physicists in Laby. The size of the superconducting magnet and the need for access to its core from underneath, coupled with the external magnetic field, meant that no room in Chemistry was suitable. I cannot remember just what favours I was owed by the Works and Services Section (now the more grandiose *Facilities Management*) but, after some considerable bartering, I was offered a large room on the ground floor of the Laby Building alongside the physicists; it was modified and fitted out for the new instrument.

Installing a superconducting magnet in New Zealand in the mid-late 1990s was no easy task. All the necessary equipment had (and still has) to be imported into the country and, depending upon the whim of NZ Customs, the port of entry could vary. Varian had competent technicians in Australia and their senior men did the installation. The large quantity of helium needed to initiate the superconducting magnet arrived by air from Sydney (NZ does not have its own liquefaction plant) and the instrument came up with the minimum of effort. Peter mastered the instrument quickly and had superior spectra produced routinely because of the attention that he had paid to having the best probes possible as part of our package. Of course, routine maintenance was not easy and even having the vital supply of liquid helium here when needed always caused a missed heart beat or two. This was no more so than when the 'can of helium' sat on the tarmac of Sydney's Kingsford Smith airport because the pilot of the daily freighter elected not to carry it (yes, they can make that decision!); no one took it out of the summer 30+ ^{o}C heat and its volume on arrival was markedly short of the needed quantity! Beyond this, the instrument needed a continuous supply of compressed air and this had to be supplied and installed by our Works and Service personnel. Sadly, they tried to minimize costs by ordering an inferior compressor and not allowing adequately for the storage of the helium and nitrogen cylinders that had to be kept outside the NMR room; much instrument time was lost with more still invested by Peter to ensure that this vital need was met. One of the matters that Victoria University has never come to grips with adequately is the time commitment involved by staff in maintaining an instrument. The responsibility for a new piece of equipment is always passed to a staff member and, until quite recently, it was presumed that this was part of the research commitment with minimal additional cost involved in running the facility. For the life of the FT80A, I was the one with that responsibility and changed NMR samples most weekends and evenings until the then organic technician, Rhys Batchelor, was able to assist. Rhys moved over to the 300 MHz

instrument and provided a good service for that. Nonetheless, problems were not alien to us as the supercon quenched more than once, the last time damaging the magnet and putting the instrument out of action for six months or so; that single event had major consequence for organic chemical research as we shall see.

But to return to the time that the NMR replacement was being planned, the northern summer of 1995, I attended my fourth ISNA meeting, that time in Germany. I had been on the International Advisory Board (IAB) and had attended the meetings since the 1985 conference in Scotland. With the cost of travel from NZ to Europe, there was little point in simply attending the conference and I asked Manfred Regitz (Kaiserslautern) if he could arrange a few visits for me. Once again I was well served, meeting Gerhard Maas in Ulm and Paul Binger at MPI–Coal Research. The ISNA meeting, organized by Klaus Müllen and Henning Hopf, was held in Braunschweig, Henning's home territory, and it was a little more intensive than the earlier events. Nonetheless, it was more than pleasant as I had enjoyed Henning's company in Victoria (Canada) in 1993 where, inadvertently, we managed to take breakfast at the same time most days. There were three particularly notable events at this meeting, leaving aside the meeting of colleagues from the past. The first was that the IAB set ISNA-9 for Hong Kong under the stewardship of Henry Wong of the Chinese University. Henry wanted to have a Regional Organizing Committee in place for this 1998 meeting and I became its convener. Secondly, the subsequent meeting was proposed for Europe, with Leo Jenneskens of Utrecht in Holland offering to put things in place. The last, and most significant for me, was that after the meeting I had a free day before an evening flight to London to visit my mother in her hospice for what proved to be the final time. I spent that free day alone revisiting Göttingen, only to find that the main Japanese party escorting Professor Tetsuo Nozoe, the father of ISNA and in his 93rd year but still active, was also sightseeing. We crossed paths several times including the same restaurant for lunch. At the railway station, waiting for a train to Hamburg and the flight to London, I found the opposite platform to be occupied by my Japanese friends awaiting their train south. My train arrived first and, after boarding, I found the entire group, Nozoe included, lined up along their platform where they courteously bowed as my train pulled away. That was the last time that I saw Nozoe as he died on April 4th in 1996, two months after our eldest son's wedding.

With involvement in the upcoming 1998 ISNA-9 in Hong Kong, the news of Nozoe's death came not just as the loss of a mentor but as the loss of the doyen of Japanese organic chemistry and the founder of ISNA. It meant that ISNA-9 could not take place without some significant recognition of the great man. Henry Wong and I saw a Nozoe symposium as one possibility but a commemorative Nozoe Lecture as the better choice. Sho Ito, an early Nozoe student who had spent his formal teaching years at Sendai University, was the next senior Japanese and he has

ably filled Nozoe's shoes, though unable to attend all the ISNA meetings. Thus, I wrote to Sho suggesting that we included a *Nozoe Memorial Lecture* to open the ISNA-9 meeting, and that the Nozoe students themselves select the speaker. Not only was this accepted but it went one better. The group established a fund to commemorate Tetsuo Nozoe and set about raising money to secure *The Nozoe Memorial Lecture* and hold the *Nozoe Memorial International Symposium on Organic Chemistry*, the latter taking place in Sendai in August 1997. A book commemorating Nozoe's life was issued after the symposium and some 255 people were involved in getting the commemorative events started; just one American and three European names appear (Ron Breslow, Klaus Hafner, Brian Halton and Emanuel Vogel). Naturally, to establish the lectureship there needed to be a nomination process, an international advisory board (to select the lecturer and prevent the current ISNA organizer dominating), and stipulations on what the stipend would cover.

This, in essence, comprised my contribution to ISNA-9 with the various friends that I had in Japan. As it happened, Fumio Toda invited me to his 1997 November Mini-International Symposium in Okudogo, just outside Matsuyama on Shikoku Island, and this provided an ideal opportunity to meet with Sho Ito as he was still in Tokushima. So, after the wonderful conference at the Okudogo onsen, I again took the train to Tokushima, where I had the potency of the Japanese puffer fish (fugu) demonstrated in one of Sho's favourite sushi restaurants. The owner stirred the tank containing one fugu with a range of other fish, all available for eating. The impact of the ejected toxin on the other species was spectacular, with water from the tank being splashed a fair distance. Before my departure home, we both travelled to the new Osaka airport by boat where we met Ichiro Murata (by then retired from Osaka University) and transposed our ideas into the final details for the Nozoe Memorial Lecture. The first of the series was notified, nominations/applications were sought, and the panel of Japanese Nozoe students decided that the inaugural lecturer should be Larry Scott of Boston College. I was privileged to chair that inaugural lecture as the opening event of ISNA-9. As an aside, the new Hong Kong airport opened just a few days before my arrival for ISNA, but I was assured by Roland Boese that my luggage would not be lost as his brother had designed the excellent facility.

The ISNA series continued, with Klaus Müllen opening ISNA-10 under Jay Siegel's stewardship in San Diego in 2001, and again I was asked to chair his Nozoe lecture. At that meeting, Leo Jenneskens of Utrecht University accepted

the challenge of running the next novel aromatics meeting somewhere in Holland (to be the ISNA-11 mooted in Hong Kong in 1998) but it never eventuated. The Canada-based Graham Bodwell and Rik Tykwinski rose to the challenge and saved things by running the meeting in St. John's in 2005, and my good friend Koichi Komatsu of Kyoto gave the third Nozoe lecture. I had been invited to attend as Guest of Honour but it was not medically possible for me to travel at that time and so, unbeknown to me, Graham Bodwell had a photograph printed at life size and mounted (below). I was told that I chaired one session, attended the dinner and visited one of the local hostelries one evening. Larry Scott has told me that I was alive and well in Graham's Newfoundland office in late 2010! It was a great privilege for me to be involved with the series and to have three of my personal friends selected as Nozoe speakers. The funds raised by the Japanese covered the series including ISNA-12, which was back in Japan under the auspices of Yoshito Tobe at Osaka University. At that meeting François Diederich of the ETH in Zurich, Switzerland, was the Nozoe speaker. Fortunately, and sensibly, the IAB decided that the name of Tetsuo Nozoe was too closely associated with the congress series, to be allowed to disappear and it enshrined the Nozoe Memorial Lecture in the series, with each conference offering some sponsorship. Europe was redeemed by Diederich as he organized the 2009 meeting that was held in Luxembourg, while the 2011 meeting is being held in Oregon in late July.

Koichi Komatsu

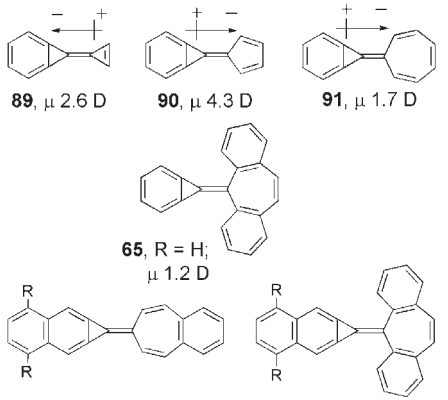
Not too long after my return from Okudogo and Toda's mini-conference in 1997, Robin Ferrier retired. At that time, 65 was the mandatory retirement age (scrapped in NZ in 1999) and his formal departure was on Jan 31 of the following year in accord with the 'old contract'. Robin's departure was expected but he made no comment to Peter or me about it, his plans for the future, or when he would leave. He had often taken his summer holiday later than most and, when I went to see him a few days before he was due to depart, I found that he had already left. He did return a little while later to give a 'post-departure' lecture and was hosted to a farewell function, but there was no personal word of encouragement for the future to those left behind. For the entire 1998 teaching year, Peter Northcote and I were left to cope with all of the organic chemistry lecture, laboratory and tutorial work. In its 'all too customary' manner, the university chose not to decide on whether there would be a replacement until long after the incumbent had departed. In Robin's case, it was clear that there would be no continuation of an organic chair

and, with the advent of the Departmental Chairperson, it was not within my mandate to become the *Organic Professor* as had been the case with Neil Curtis, who become Professor of Inorganic Chemistry on Duncan's retirement. Eventually, a chemistry lectureship was advertised in mid-1998 and among the applicants was one John Hoberg, an American carbohydrate chemist from the National Renewable Energy Laboratory in Colorado. After telephone interview (by then accepted for non-senior applicants from overseas) and appropriate discussion, he was appointed. He arrived in late 1998 but took some time to settle to the NZ way of things. Sometime later, the senior organic laboratory programme was reorganized and the communal research group meeting was replaced by area specific ones. I assume that Jim Johnston, then chairman of SCPS, had some part in this but it was only after the events that I became aware of them. It had always been my hope to introduce change to our syllabus but, other than for comparatively minor adjustments with new courses and the like, I never had adequate opportunity to discuss or evolve the sub-discipline as I might wish.

John Hoberg rapidly gathered a sizeable research group, did some good chemistry using the equipment that had been available to all the organic people prior to his arrival, and alienated both Peter and me. To assist his independent start in academia, I encouraged John to write an article on strained carbohydrates for my book series, which he did with one of his PhD students, Ghislane Cousins (now employed by NZ Pharmaceuticals). John had his first child in NZ but his wife never settled to life here and they left Wellington just before Christmas in 2003. Joanne Harvey, his replacement, arrived six months later having gained her Honours degree at Victoria with Robin Ferrier and PhD with my own former student, Martin Banwell, at the ANU.

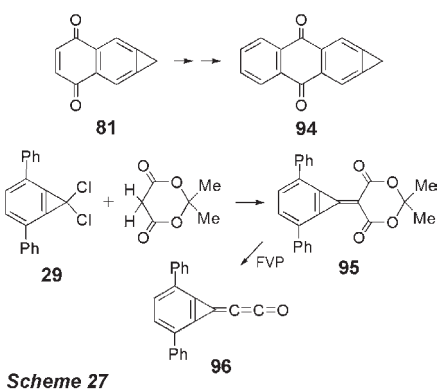
But to return to chemistry. At the time the new NMR arrived I took on Gareth Dixon as an Honours student. He stayed with me until 2002 and was the last of my PhD students to finish. It is not that Gareth took his time rather than the circumstances that he found himself in. He started his PhD training in 1997 after an honours year that gained him a IIi from researching some new polar alkylidenecyclopropanes. He continued in the same area for his PhD degree but with an initial foray into the precise conditions needed for the silyl-Wittig reaction. We had moved to an era of completing the studies we had done and determining the most appropriate ways of obtaining the various products. From his study, Gareth made a range of conjugated and cross-conjugated alkylidenes, recorded the dipole moments of all the ones still needing measurement, and completed a significant and thorough survey of the polar and spectroscopic properties of these compounds; the work provided him with a number of publications. Most notable among these was confirmation that the cyclopropanyl core acts as both an electron donor and an electron acceptor from synthesis of various *p*-substituted phenyl derivatives

of, *e.g.* **58** (Scheme 25), and that the ability of the cycloheptatrienyl moiety to donate electrons is lower than that of the cycloproparene. Coupled with a theoretical study, the last of the results were decisive in showing that in the unknown cyclopropenylidene compound **89** (Scheme 26), it is the cyclopropenyl moiety that is the stronger electron donor. With a cyclopentadienylidene attachment as in **90**, donation is to the five-membered ring as expected from first principles, but intuition argued that the seven-membered ring of **91** should be the donor. Theoretical calculations in Haifa and Essen differed with this and served to dispel such thought as did Gareth's subsequent work with substituted compounds. Thus, by preparing the previously known seven-membered ring-containing hydrocarbons **66** and **67** and the new ethers **92** and **93**, the dipole moments were all telling (Scheme 26); the presence of the electron donating ether moieties in **92** and **93** results in an *increase* in polarity. Even more important was the X-ray crystal structure of **93** that has the seven-membered ring bent out of the plane of the cycloproparene to a greater extent than in non-ether **65**; the cycloheptatriene resists the increase in electron density that would require it to become an 8π7C antiaromatic ring.[56]



Scheme 26

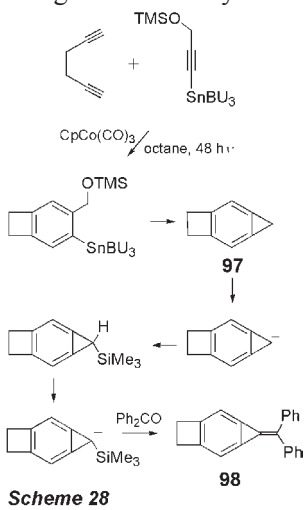
As Gareth started his PhD training, Carissa Jones entered the BSc (Hons) programme and spent most of her time delineating the detail of our reactions with silylcycloproparenyl anions, which she continued into her PhD that started the following year. She had much success, inevitable failures, but discovered side products not detected by earlier workers, and she was able to provide a series of cross-conjugated π-extended compounds. Her final contributions transformed cyclopropaquinone **81** into its higher anthracene homologue **94**, and facilitated the synthesis of the precursor to our cyclopropabenzenylidenethenone **96** (Scheme 27) that was obtained by a 'reverse electron mode' process. This used our original cyclopropabenzene **29**, which ionized and let its cation be captured by the anion



Scheme 27

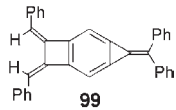
of Meldrum's acid. In work performed in conjunction with Curt Wentrup in Brisbane – we did not have flash vacuum pyrolysis and low temperature IR facilities at VUW – the decomposition of **95** was monitored and **96** isolated at low temperature (Scheme 27).[57] After leaving Wellington, Carissa had postdoctoral positions in San Diego, with Jay Siegel, and then Oregon, with Mike Haley, both of whom I had come to know well.

Gareth's PhD languished for some time, but not all of it of his making. When he registered for his degree, VUW was in the process of added change, this time directed at the postgraduate student. Accountability and a mandatory research proposal for all PhD candidates had been set. Until this point in time it was assumed that chemists could direct research and supervise PhD students in a responsible fashion merely from reputation in one's discipline, assuming that university funds were available. Gareth registered shortly before the changeover and, although the offer of a proposal and cost analysis/budget for his programme was made, it was declined; we were still under the old rules. One of the important phases set for the cycloproparene programme was to extend the exocyclic olefin series of strained cyclopropabenzenes into molecules that also incorporated a four-membered ring, *e.g.* **97**, that we named 'rocketene'. This compound and its angularly substituted isomer had been known for almost 25 years but their chemistries had not been studied. In the early part of the programme we were able to obtain a supply of hexa-1,5-diyne, the essential starting material for the new route reported by Peter Stang and Aileen McNichols in 1992 for **97** (Scheme 28).



Scheme 28

Our difficulties began with the diyne-to-propyne cycloaddition that failed to give the organotin substituted benzocyclobutene in measureable quantities under the conditions stated; most of our diyne was used up attempting to persuade the reaction to behave. Eventually, we learned from Mike Haley that the reaction was likely best performed with external irradiation from an air cooled halogen lamp, and this proved to be the case; a 46% conversion was effected. The subsequent 1,3-elimination proceeded without difficulty and gave us rocketene (**97**), but we had little starting material left and no way of synthesizing the compound ourselves given local safety regulations. Worse still, the financial situation had deteriorated and we were not allowed to renew our supply. Garth was left in limbo, active in other aspects of our programme but so close to the end of this appealing synthesis. With no softening of local attitudes but monies being dispensed elsewhere, I had little option but to beg. This I did, securing funds in the US to provide the



99

vital hexadiyne for us. The synthesis of **97** was repeated and the chemistry advanced to generate the derived anion, silylate it and provide **98**, the first and only methylenecylobutacyclopropabenzene.[58] Sadly, we were unable to proceed to the range of derivatives that we had hoped for, nor to pursue the ramblings of an aging chemist towards cross-conjugated trienes as illustrated by **99.**

Attempted silver(I)-mediated dimerization of various alkylidene compounds never led to any of the hoped for products but charge-transfer complexation did provide some interesting results that await others to confirm and extend. These complexes were generated by my final honours student, Jarrod Ward (2001), who subsequently worked in UK industry before undertaking his PhD at Auckland University and, like Gareth Dixon, is now a thriving patent attorney.

It was during Gareth's tenure as a student that academic members were strongly encouraged to apply for monies for research from the Marsden Fund. Like most of my chemistry colleagues, I had made applications from the earliest time and the new 'push' was easily satisfied by putting the development to tri-olefins, as illustrated by **99**, before the selection panel. The effect of this was that my 1999 application passed through the preliminary round and I was required to submit the detailed 'round two' application. The procedure remains one of submitting a brief project synopsis for assessment and then, if invited, a full formal application to the second round, from which projects are selected for funding. As it happened, my application was the first from a VUW chemist to go to the second round and I was sent a glowing letter of appreciation from Dr. John Morrow, the then Assistant Vice-Chancellor (Research). Nonetheless, the project was turned down because one referee had questioned the predicted outcome of one particular reaction. While this might seem harsh to some, it needs to be noted that, even today, the success rate for chemists from this (nominal) blue skies research fund is a mere 5%, 1 in 20; it is rightly regarded as a lotto more than a serious fund. This is not to criticize the few who rank the applications, as their task in a small country such as NZ is to use the slightest reason for rejection so as to reduce numbers to fit the paucity of funding available.

The decade of the 1990s had many things happening as I have discussed already. It also initiated me further into the role of editor, not simply from the 'Strain' series but from being appointed to the International Advisory Boards of (in sequence) the *Journal of the Chemical Society, Perkin 1* (1993-97), *Perkin 2* (1997-2002) and the *Australian Journal of Chemistry* (1994-99). With my involvement in Pacifichem, it was pleasing to find that those members of the UK RSC publication boards present in Honolulu for both 1995 (*Perkin 1*) and 2000 (*Perkin 2*), met with Sheila Buxton who was, in my view, an excellent managing editor for RSC. By comparison, *AJC*, being closer to home, meant a more 'hands-on' involvement and it proved to be

a delightful time working with John Zdysiewicz and the board chairmen, initially Don Cameron and then co-ordination chemist Len Lindoy. John and Don I have referred to before, while Len, who moved from the James Cook University of North Queensland in Townsville to Sydney, was more than familiar with the forces of change that were blowing through Australia's CSIRO in the late 1990s. He was an astute and perceptive Board Chairman. For me, the most frequent task was to assess manuscripts from emerging country authors whose language ability was poor, and it was from this that my patience with English-as-second language authors evolved. I continue to offer to rewrite such papers in the hope that subsequent submissions are improved, but I do this only once for a given author. There was an annual face-to-face meeting that John Z organized, usually late in the year and always a pleasure to attend. It was not simply the generous hospitality that John showed, but the camaraderie and dedication to maintaining *AJC* as the flagship CSIRO periodical that made the time so worthwhile – the AJC subscribers more than doubled the number of its next best rival in the stable. Back in those days it was accepted as the pre-eminent chemistry periodical of the region and although its editors would accept material from other countries, that was dominantly by invitation. All of that has changed since John retired. AJC now strives to excel as an(other) international publication; the regionalism that made it so recognizable (and known) is gone. The moves to government agency ratings and staff assessments based upon publication and citation numbers have done nothing but exacerbate matters.

John Z in his office

One of the other tasks that emerged in the mid-to-late 1990s after Peter Northcote's arrival was the serious need to upgrade our mass spectral facilities. My personal involvement with mass spectrometers and mass spectrometry date to my faculty position at Florida and those research outcomes have already been discussed. What, in fact, was involved in Florida was a first generation mass spectrometer that arrived in the Chemistry Department in the summer of 1967. It was assembled by the manufacturers and a graduate student gained his PhD degree from work using and maintaining it; I hate to think what my own work vapourizing organic salts did to the instrument's interior! Nonetheless, I had a grounding in instrument operation and so when VUW obtained its own instrument (again brought in by Ted Harvey) in 1974 from Micromass in Manchester (UK), I was an operator. The instrument was housed in its own sizeable room adjacent to one of the lecture rooms in the McLaurin Lecture Block, now the Mail Room. The Micromass 12F was a

second generation electron impact instrument with a large flight tube, huge pumps and was difficult to service, but it was state-of-the-art. I geared myself up to using the machine and spent a week with Micromass becoming familiar with its intricacies in 1975, whilst at Reading. It served us well, but advances in the art of mass spectrometry were fast and all too soon the 12F was outdated. By this time, John Craig had become interested in volatiles chemistry and after some negotiation, the NZ Racing Conference (the organization that oversees the country's horse racing) placed a Hewlett-Packard 5595 desktop mass spectrometer in Chemistry. The old Micromass instrument was decommissioned in the mid-1980's and its various parts put to good use in science around Wellington – even the flight tube went to a good home. The Racing Conference contracted a technician to perform the necessary horse urine analyses and we had the instrument for the balance of the time. Eventually these analyses were transferred to Auckland and, after some protracted discussions, the aging desktop 5595 was left with us as our MS instrument.

The general research of Chemistry and the Northcote marine natural products programme really needed a modern instrument and so one was mooted in 1997, especially since the biochemists were keen to have a MALDI time-of-flight instrument. Serious negotiations began late that year and ran well into 1998. After much discussion with VUW's senior administration, including the Dean, as well as a raft of manufacturers, our decision was narrowed to either one from PE Biosystems (near Boston) or Finnigan (in San Francisco). The Biosystems instrument, a Mariner, seemed better suited to the natural products chemists than an ion-trap instrument from Finnigan that was ideal for me. Someone had to inspect the instruments and put them through a strenuous test process. As I had had the most experience (and am able to shut down most instruments and computers in minimal time!), I was the one to make the trip. The Mariner was in the process of final development and the timing for my visit was put back a few times, eventually taking place in a one week break from teaching. The manufacturers split the cost of my travel (which I had insisted to be business class as I would only return the day before lectures recommenced) and the Biosystems salesman accommodated me in a Venice Beach hotel near LAX for two nights. I flew to Boston on the Sunday and met him at the airport there. The time was hectic but the instrument performed well and handled the samples that I had brought with me with flying colours. On the Wednesday morning we arose before 7 am, drove back to Boston and I left for San Francisco, arriving late morning local time. I was whisked to my hotel, lunched and then taken to the Finnigan facility to 'play' with the ion-trap. Just as I was about to ask to be returned to the hotel (it was by then 6 pm Pacific time, 9 pm Eastern) I was told that I was being taken to dinner … and so the long day progressed. The time with Finnigan was well spent and although I preferred to have their instrument for my own needs, it was the Biosystems Mariner that won

out. My return to Wellington was on the 14 hour United Airlines flight to Sydney and then home, arriving mid-afternoon, 18 hours before my Monday lecture. The Biosystems man gave me an upgrade coupon and the United flight was in 1st class; the food was awful but the wine superb – I slept for 11 of the 14 hours.

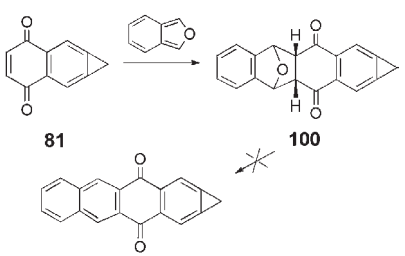
With decisions made, a combined Biochemistry-Chemistry order was placed once the price finally had been settled for this 'first-in-NZ' purchase and the monies confirmed available. At that time Les Holborow was in his last year as Vice-Chancellor and, as happened for the NMR, payment was to be met by the central administration. However, with Holborow's departure at the end of his term and following the arrival of Michael Irving in January 1999, all of the arrangements were subsequently negated and the instrument costs were levied against the Science Faculty. There was significant impact as the funds for routine equipment replacements were swallowed up and no science department was able to get anything new for more than a year. As far as I was concerned, this was a reversal, not simply of the policy, but the reneging on an agreement. I am sure that Les Holborow would never have initiated it. John Hoberg took charge of the instrument after its arrival and attended a training course in Boston, which had been negotiated into the all up cost. For the first time, Victoria had accurate mass measurement facilities. It signified the end of a long standing arrangement with Canterbury University and its first-rate technician, Bruce Clark, who had provided me with high resolution mass spectra (at least 150) for many years; they were run at a cost that was close to zero, a far cry from today's charging regimes that pervade every sector of academia.

In the early days of the Mariner's short life, we had a Russian immigrant, Oleg Zubkov, master the instrument and have it perform to specification. However, during 2002, the Varian NMR instrument suffered a quench that damaged the magnet and it was out of service for some six months; this impacted on the Mariner. Whereas an NMR instrument accepts crude samples within a sample tube, a mass spectrometer is only as good as the sample that is inserted into it, unless separation is done by way of gas chromatography. Unfortunately, research students were allowed to use the instrument as a 'crude sample analyzer' whilst the NMR was down and the inlet, source and ion-tube became clogged; costly repairs were needed. The manufacturers did not help matters by not maintaining a supply of spare parts and the quality of the mass spectra never returned to the former level. Ultimately, after Oleg's departure in 2006, the instrument became almost impossible to maintain (and certainly too expensive), not because it was inferior, but because of the maltreatment that it had been subjected to. It was decommissioned in 2008 and its replacement is due soon.

The period that covered the Mariner installation was one of further change for the Chemistry. For many years the chemists had held the view that amalgamation with a larger unit was desirable, the organic staff favouring an affiliation with the

biological sciences and the rest of the Department preferring Physics. A couple of the earlier Departmental reviews had recommended amalgamation with Physics but the university administration rejected the calls. However, from 1997 there was a change of heart which, when coupled with the paucity of undergraduate numbers, led to amalgamation to 'The School of Chemical and Physical Sciences' (SCPS) during the last part of John Well's term as Dean. Not surprisingly, there was considerable concern about the name of the new unit and, recognising that it would never be accepted, I was to the forefront pushing "The School of Physics and Chemistry" as the order reads and sounds better than the reverse. Of course it was rejected as being insufficiently appropriate to define a new collaborative entity. And so my preferred (but previously unannounced) *SCPS*, with chemistry first, won the day. After this, and when the facilities in Physics were being modified to provide teaching and research laboratories for chemistry, my time for 'research and refresher leave' (as it had become known) came around again. For a variety of reasons, it was no longer possible to spend long periods away and so this particular sabbatical was divided into three. The first segment was to be a two month spell in Ron Warrener's *Centre for Molecular Architecture* at Central Queensland University in Rockhampton, the second a similar time at the Australian National with Martin Banwell and the last, another two months with Klaus Müllen in Mainz.

The first component, with Ron and his delightful colleague Doug Butler, was set for the NZ winter over June-July 1999. On my way to 'Rocky', I stopped in Brisbane to see Curt Wentrup and then made the 8 hour drive north the next day. My arrival was carefully scheduled so that I could meet John Warkentin (McMaster University, Ontario) before he left. John had begun some work on the addition of dimethoxycarbene to ring-fused cyclobutenes as a part of the elegant Warrener-Butler programme that led the way to large, rigid molecules with functional units precisely located on the carbon framework. It is continued by others even today. I spent time modifying the reaction conditions and characterizing the products in order to bring the work to a successful conclusion; it gave rise to my second publication with full-time academics as its only authors.[59] Ron had rented a small house for me on Ocean Parade by the beach in Yeppoon, a 30-40 minute drive from the University. The location was glorious and a far cry from winter in Wellington. I took early morning walks along the beach and became familiar with the sea eagles and ibis, saw the brolgas in the marshlands to the north, and enjoyed the local old mining town to the interior. Margaret joined me for the winter holiday break that the NZ schools had during July. But it was not holiday for



Scheme 29

me. I had forwarded a sample of cyclopropaquinone **81** in order to use Ron's high temperature/pressure facilities for cycloaddition. What we found was that the olefinic bond of **81** did not add furan under 14 x 10^5 kPa pressure, even at 65 ^oC. By comparison, the more reactive isobenzofuran added at 40 ^oC and atmospheric pressure giving **100**, but attempts to deoxygenate the furan ring to liberate the corresponding tetracenedione (Scheme 29) failed. Nonetheless, I was more than pleased to have had two publications from my last sojourn at the laboratory bench.

The second component, at ANU, was hosted by Martin Banwell over October-November 1999 at the time he was appointed to his Chair. *En route* to Canberra, I stopped over in Sydney to visit my old friends at the University of New South Wales, especially David Black, and the newer ones at Sydney University, particularly Len Lindoy, who was in the last period of his AJC Board chairmanship. In Canberra, I stayed with Martin and his wife Cathy for almost a week and got to know their two boys, the elder one then 11, but only a baby when I had been with them in Melbourne in 1988. Martin had arranged an on-campus apartment for me but it was unavailable for the first two weeks of my stay, and so Christina Chai made a room in her home available for my second week. The time I spent on the ANU campus provided a great renewal of chemistry with international visitors and their seminars, and the RACI (Royal Australian Chemical Institute) evening meetings and the camaraderie of the organic chemists. It was especially gratifying to meet up again with Leo Radom and the late Athel Beckwith (1930-2010) as we had enjoyed our time on Heron Island some eight years earlier.

The plan for my stay was to use the time, not in the laboratory as in Rockhampton, but by writing a review with Martin on the use of *gem*-dihalocyclopropanes in natural products synthesis. Many hours were spent in ANU's excellent Chemistry Library searching *Chemical Abstracts* as electronic searching with SciFinder®, routine today, had not yet advanced adequately at that time. In any event, the literature was collated, the references read and the draft manuscript prepared. Not surprisingly, Martin had had little time to contribute and felt that were the review to be published, he would need to make a significant contribution. The project had arisen because Martin had been approached by Peter Vollhardt for a review to appear as an *Account* in *Synlett*. When I left in late November 1999, Martin was to draw most of schematics but he never found the necessary time and the text lay dormant until late 2004.

During my stay in Canberra, I was advised by Jim Johnston, still Chairman of SCPS, that the comparatively new Dean had placed the School under review and that staff numbers were to be reduced with all positions under scrutiny. Asked if I should return to better cope with the issues, Jim's response was non-committal and offered little by way of hope or expectation; it was bureaucratic, but it did tell me that I needed to make a submission, which I did. When I got back I needed to move

from my home of 31 years in the Easterfield Building to a new office in the School of Chemical and Physical Sciences located in the Laby Building, and this I did, leaving Gareth to clean up and vacate the Easterfield laboratory. In itself a simple task one would imagine, but one simple slip – omitting to 'kill' the remnants of our cyclopropabenzene with an alkali wash – resulted in one of the Psychology staff calling the fire brigade because of a chlorine leak. The psychologists evacuated but when Easterfield was checked no chlorine was found – only those chemists yet to move happily having afternoon tea.

The revamped facilities in Laby gave the chemists about half of the space that they had had and teaching laboratories that were markedly smaller. The renovations were Science's major project for the new millennium and, while nice and new, there remains insufficient space for storage and too few fume cupboards for the number of students involved in synthetic work. Of course, these deficiencies only emerged when numbers increased significantly a couple of years later. As one would imagine, the design and construction of the modifications to Laby had been under contract and, whilst visually impressive, the reality was not. We had to have a light well to the central area that borders a small instrument room and student computing facilities on one side and a teaching lab on the other, with the remaining sides corridors; within days, the blinds in the computer unit were shut. Offices had their computer work stations set back-to-back to minimize wiring costs – but the effect was that one side was in the sun with its computer screen impossible to read. Main support columns pierce work areas making sections inaccessible and, as has happened throughout my lengthy time at VUW, the join of the 'add-on' to the existing roof was anything but watertight. What NZ has never learnt is that countries far larger have had bigger science laboratories operating for many years with true and tested designs. Despite their availability, VUW seems incapable of accepting such designs, preferring instead to see if the wheel can be reinvented. My office is in a section of the revamped old structure that had its heating decommissioned for the renovation. Reconnection, though perhaps obvious, was not attended to and so heaters were supplied, supposedly to be removed when the connection was reinstated over a year later. The temperature dips below 15 ^{o}C much of the time and the heaters remain. What was new in Laby was the provision of a separate laboratory for each of the organic staff to locate their students. This marked the beginnings of a divide into separate and independent operations. In the early stages, the major concern was with the few fume cupboards available and I was more than happy for one of those in my lab to be used by the Hoberg group, but the new environment led to strained relationships as much of what had been communal equipment was amassed into just one area.

The final part of my sabbatical was to be a period with Klaus Müllen at the Polymer Institute over February-March of the new millennium. Unfortunately, it

did not eventuate, not because I had had to reapply for my position as all those in the organic area were renewed, but because I suffered a second heart attack in January 2000, while on holiday. Margaret drove me back to Wellington and I was immediately hospitalized. By this time, the procedure of stent implantation was to the fore and my blockage was treated speedily and efficiently in this manner. Since my return from Canberra, I had not been feeling as well as usual and my then GP had suggested seeing my cardiologist, Ron Easthope. Unfortunately this did not happen, not that the NZ health system failed but because Ron had made attempts to reach me when we were away. In the event, he suggested that, since his retirement was imminent, I should accept a more recently trained and competent man – and I have been in the care of Phil Matsis ever since. Because of the infarct and keyhole surgery, I was not able to obtain international health insurance for a six-month period and so the visit to Mainz had to be put off and, when coupled with other university commitments, it could not be rescheduled until mid-2002.

My hospitalization and convalescence period also had an impact on my leave allowance. Since about 1998, the NZ government had required employers to keep a record of the leave taken by their staff members; Victoria was no exception and, like every academic, I was asked how much leave, if any, I was owed. After about 30 years in the employ of Victoria University, I came to the conclusion that the 40 hour working week insisted upon by the government agencies (and university) over that length of time led to a considerable period of leave due – especially allowing for all the weekend time changing NMR samples on the FT80A. The 40 hour week had brought me into disrepute with one government agency earlier. I cannot remember when, but there was a survey of academic duties, responsibilities and times taken performing the various tasks. This was no simple exercise as staff were randomly selected for interview and my name was on the list. I recall the interview being lengthy by the time all the boxes had been ticked, crossed and expanded, and questions answered. Finally, I was asked to say what proportion of my time was spent on teaching, research and administration. This was about 33% each but, not surprisingly, the interviewer insisted on expressing this as 33% of 40 hours and entered these into the boxes. The truth was that it needed to read 50% each if 40 hours was taken as the norm; this did not fit to the survey being applied and its entry was refused. So I took the forms, tore them up and put the pieces in the wastepaper bin. I have always felt that ⅓ teaching, ⅓ research and ⅓ administration is a fair rule of thumb, but never at 13 hours 20 minutes each per week! Thus, my simple calculation of the time in excess of the assumed 40-hour week over the 30 years for which untaken holiday was to be claimed led to insufficient time in one calendar year for this to be taken! I suggested that my entitlement be recorded as 90 days and there was no argument. At the time that I was hospitalized, I was taking ‘leave due’ but this had to be cancelled and ‘sick leave’ inserted in its

place for this and the subsequent recuperation dictated by the medics; my arrears did little but grow!

The impact of this second heart attack was more significant than just preventing leave in Germany. The six-month 'no insurance' period also covered the time of the major Pacifichem 2000 planning meeting scheduled for Washington, DC. Sadly, I missed it and a hosted tour of The White House that ACS had arranged for the delegates. This meeting scheduled the accepted symposia and allocated the various rooms to be used in Waikiki. Despite my inability to attend the DC meeting, I was back to normal by the time of the congress and I attended. It was at the final planning meeting, the day before the congress opened, when I found that the student poster competition had attracted far more entrants than had been presumed. Rich Oakley of Waterloo University in Ontario asked me if I would assist with the event. At that time, papers were scheduled within the various symposia rather than being a student event at one place with a predetermined number of sessions. We both worked harder than ever before with the outcomes as I indicated earlier. The congress was another outstanding success and there was no dissension in holding another in the series in 2005. Again, I signed the agreement for the NZIC and was delighted to support the Korean chemists' move to join the organizing committee. These duties took place at the first of the planning meetings for Pacifichem 2005 that was held in Hawaii a few months after the dramatic events of September 11, 2001. Stan Israel took over as Head of the ACS delegation and Peter Stang was the replacement for Paul Walter as treasurer. That 2001 meeting and the subsequent one in Tokyo had great camaraderie. It was agreed that the congress must move into the modern era as its growth had almost outstripped the facilities we had available. Registration and abstract submission went to an on-line format for the 2005 meeting through the good graces of Rich Oakley and some Canadian software colleagues. However, someone was needed to test the Canadian-designed software and, of the committee members, the task fell to me. I managed to shut the system down rapidly and then with increasing difficulty until the software became idiot proof! That Tokyo planning meeting was the last time that I was with Stan Israel, whereas Peter Stang has visited NZ a number of times since then.

For VUW staff, the Pacifichem meeting in 2000 was more than interesting because one of its former chemistry graduates, Alan MacDiarmid, had just been awarded the Nobel Prize (with Heeger and Shirakawa) for his work on conducting polymers; VUW's name was recognised more than usual. In a rare case of 'getting it right', VUW had awarded an honorary DSc to MacDiarmid in December 1999, a year ahead of the Nobel Prize. After the auspicious occasion, Alan revisited VUW Chemistry a number of times and had some of our staff, including John Spencer, the Head of School since 2005, visit him at his home-base in the University of Pennsylvania. Alan gave his name to a number of institutes and organizations,

The MacDiarmid Building, VUW

none less than the MacDiarmid Institute for Advanced Materials and Nanotechnology based on the VUW campus. By the time of his Nobel Prize, NZ science was being concentrated and focussed even more, with government pushing for centres of research excellence (CoREs). Eventually, it was decided that there should be six of these. Perhaps as a result of the Nobel, or perhaps from good advice, the decision was for one of these to encompass new materials. 'Bids' by reputed scientists in conjunction with others elsewhere in the country were sought for the six centres. By that time, Prof. (now Sir) Paul Callaghan had been appointed to a Chair in Physics and he led the initiative for a materials science and nanotechnology centre to be located at VUW. The VUW bid included Massey, Canterbury, and Otago Universities, as well as two Crown Research Institutes (GNS Science and IRL Ltd.), and it succeeded. The MacDiarmid Institute was opened in 2002 as a partnership among these organizations. What the significant funding did for Chemistry, unintentionally, was to create a two tier system: those with MacDiarmid monies and those without. Fortunately, as time has passed, the discrepancies have become less significant, but the School's doors still carry signs designating the nature of the laboratory. The CoRE not only survived its initial six-year funding period, but retained an equally significant budget for its second six year period. Victoria named its newest (2010) science building, after Alan.

In 2001, Carissa Jones departed for her postdoctoral with Jay Siegel in San Diego and I saw her there as Jay hosted ISNA-10 that year. It was also the year that Jarrod Ward examined the charge transfer complexation of our π-extended cycloproparenes for his Honours year. Moreover, despite the lack of monies for chemicals, I was allowed to purchase the School's only copy of the Warren Hehre Spartan software for *ab initio* molecular orbital calculations. I then began to use the software to compute properties of many of our molecules; the most notable outcome was a delightful illustration on the cover of No. 23 of the *European Journal of Organic Chemistry* in November 2003.[60] The year also allowed me to reschedule the visit to the Max Planck Polymer Institute and Klaus Müllen for the following June. Over the years Leiv Sydnes, initially at Tromsø then Bergen in Norway, had been to

Scott and Ito – ISNA-10

visit, always staying with us at home and always with a mutual interest in three-membered ring chemistry. Leiv was keen for me to go to Norway. With this in mind, a two month period from mid-May 2002 was set to include almost two weeks there and several days with Roland Boese in Essen, prior to some six weeks back at the MPI–P in Mainz. The time in Norway was most rewarding, not simply because I had over a week in Leiv's comfortable home, but because I was able to meet Lars Skattebøl in Oslo. Many years earlier he had invited me to a NATO chemistry meeting but I had been unable to attend because of funding and the 1-in-3-year overseas conference regime. As any small ring chemist will know, Lars is acclaimed for his pioneering work with cyclopropanes and their derived carbenes, and the time with him was rather special. Not only this, but Leiv had arranged for me to fly to Trondheim and lecture as well. However, the Norwegian custom of a visitor spending time only with the host group surprised me as remarkably few other staff were seen. I then returned to Bergen on the overnight ferry, the maiden voyage of the new boat MS Trollfjord. The experience of daylight well into the small hours of the morning was exhilarating and the time in Bergen stimulating. The weekend was spent with Leiv and wife Kirsty at a cabin on Hardanger fjord, with friends Terry and Ruth.

L-R: Terry, Ruth, Leiv and Kirsty

From Bergen it was a short hop to Dortmund. Roland Boese met me at the airport and took me back to his home as it had become the norm to stay there when in Essen, in the same way that he always stayed with us in Wellington. I remember arriving on a Saturday afternoon and being told that he and Renate were to have a group of chemists round for lunch the next day. This proved to be a most enjoyable occasion as Frank-Garret Klarner and other organic chemists were there. I stayed with Roland and Renate until the middle of the following week, lecturing at what is now the old University of Essen (the University of Duisburg-Essen since the 2003 amalgamation) and spending a day at Bochum with Wolfram Sander. He had been a contributor to my 'Strain' series and had moved to Bochum some time earlier to establish what has been a spectacularly successful group studying matrix isolated molecules and other reactive intermediates. Then it was a longer than usual train journey to Mainz. The railway lines on the southern side of the Rhine were being renovated and a slow winding trip on the left side of the river was needed before crossing back upstream ahead of the Mainz station.

As in 1993, Klaus had set up accommodation in an MPI-P guest room. Life

was easy to adapt to and the newly constructed organic lab wing well designed. My first lecture was in Heidelberg, but with Rolf Gleiter as Neidlein had, by then, retired. I had first met Rolf in Utah, and then at conferences before he came to NZ in 1998, but my visit to him was the same as in Norway. Of the people that I was keen to see again, Klaus Hafner in Darmstadt rated highly. He is another of the distinguished German chemists, but with a realistic and perceptive grasp of the discipline in the country and of the individuals involved. Indeed, I well recall him describing one national as a 'me too' chemist that precisely described this person's ability to take and use whatever had been discovered elsewhere. It was a delightful day, no lecture commitments and time to simply enjoy his company. However, after returning to Mainz by train and then dining at a Greek restaurant, I began to feel uncomfortable but, early the next morning, a Friday, I was up and off to Utrecht to see Leo Jenneskens and deliver a lecture. Unfortunately, the overnight discomfort continued and I suffered another heart attack, the third, somewhere *en route*. After Leo had collected me from the station and taken me to the Debye Institute, I decided that I was in no state to do anything other than call the NZ insurance company and get to a hospital.

Klaus Hafner

What happened has to be the worst nightmare any academic can face, namely that of having a visitor taken ill while host. Leo and his wife were more than generous. Initially, Leo got me to the university hospital where an infarct was confirmed and hospitalization mandated. The difficulty that Leo had was that, in Holland, being a geographically small country, the patient is located at that hospital best able to take him, not the closest. However, because I knew few people in the country, he was able (eventually) to have me admitted to the hospital in the small town of Zeist, not far outside Utrecht and close to his home. One of the cardiologists was Oxford-trained Dr. Bredero, with impeccable English, and I was assigned to him. I also had a nurse whose command of the language was good. This was Lilian Ollimans who went out of her way to be of assistance and she has become a close contact. I was well cared for; Leo went and bought me pyjamas and other essentials as I had arrived only with enough clothes for the weekend stay, and he was instrumental in having a bed provided for Margaret in my room when she arrived from NZ. The only other non-native to have stayed in the hospital was a Russian and his treatment set the precedent. The system in the Netherlands at that time was for angioplasty and open heart surgeries to be performed in a few centres with diagnostic testing done elsewhere; the patient was moved as needed. For me, the angiogram dictated changed medication and a probable second by-pass surgery at home, a journey to be taken only under medical supervision.

As a long journey by air was designated in Zeist as 'Amsterdam to New York',

the 24+ hours needed to return to NZ could only be agreed to after being symptom free for a week. This time was spend in a hotel in the neighbourhood as I was not allowed to take up the Zwannenburg's offer of accommodation, but to remain close to the hospital should its services subsequently be needed. The NZ insurance company, by this time one that covered all the Victoria University travelling employees, was good to deal with and they, in turn, liaised with Margaret. The only time the service fell down was when payments were needed in Holland.

I was visited by Klaus and Renate Müllen, who brought my belongings from Mainz, the Rehbergs from Cologne, and the Zwannenburgs from Nijmegen. But it was Roland, who drove from Essen twice each week, who kept me in good spirits. It was he who introduced me to Henning Mankell and the 'Kurt Wallender' series of crime novels (see frontispiece) saying that he thought I might like them; I have read every one in the series and many others by the same author. Roland went even further, offering to cover any expense that I could not meet; a very generous gesture but not needed as it happened. The hotel that Margaret and I had to stay in was by no means inexpensive and the account due at the time I left had to be significant. However, the insurers had agreed to this place and their NZ medic settled all the accounts before we eventually left. Margaret was unable to get flights back with the medic and me, and so she had a weekend in London with Mark and his family (who had come across to Zeist for a weekend to see me) before flying back to NZ via Los Angeles, arriving home some six hours ahead of me. My journey was by way of Singapore where we had a daytime layover. After checking in for the overnight flight to Auckland (and then down to Wellington), we ran into David Officer in the Singapore Airlines lounge! Upon return, the videotape of my Dutch angioplasty was left with my cardiologist and a follow-up appointment scheduled. This led to another angiogram and Phil Matsis' decision to open the blockage and insert stents number three and four.

Unfortunately, Phil was proved wrong and the Dutch right, as the blockage stayed open only for a few weeks before causing infarct number four in September. With this, the decision was that I should have a 'redo' by-pass and so I underwent surgery in late November of 2002; the surgeon proved to be Mr. Riordan, the assistant during my first such surgery. By 2002, the impact of anaesthesia was markedly less significant than in 1983 and the procedure itself was much faster. In any event, my anaesthetist's daughter was one of the chemistry students (Joanne Wojnar) and she brought flowers to the hospital for me. This caused all sorts of confusion as the hospital staff thought that they had to be for her father and not a patient. I recovered quickly, edited the subsequent issue of *Chemistry in New Zealand*, (for which I became the editor in 2001) as therapy, and then took a short break to sample some of the Hawkes Bay wines and see the Mike Ponder paintings that were being exhibited at the Silini vineyard before returning to the office.

Gareth Dixon had been in the process of writing his thesis before I left for the ill-fated sabbatical in Europe and his examination procedure 'ticked along' during my northern hemisphere hospitalization. His completion happened after I had returned home but before the subsequent by-pass surgery. He left to undertake successful preliminary patent attorney training in Manchester. This was followed by further courses in Wellington before being appointed to Baldwin-Shelston patent attorneys (now ShelstonIP) in Sydney. Another notable event before the surgery was notification and presentation of the New Zealand Association of Scientists 'Shorland Medal' to me. It is awarded annually in recognition of major and continued contribution to basic or applied research that has added significantly to scientific understanding over one's career. Its importance to me stems from the fact that Brian Shorland (1909-1999), a doyen of NZ science, spent his 'honorary career' in retirement (in biochemistry) at Victoria University for 30 years from 1969. He always supported my endeavours and I cherish the times we had when he was a passenger in my car on the way from local NZIC meetings, always insisting that he take the bus home once we reached the Wellington terminus at the railway station.

A consequence of my medical misdemeanours was that I had no research student assigned for 2003 and so the year gave me an opportunity to take stock once again. My step-father had died a few weeks after I had returned from Zeist and the family home in Lee, London SE 12, which he had occupied since my mother's death, was put on the market, a task eased with the help of friend Peter Civil and his son Roland. In short order, the place was emptied of its contents, put up for sale and a settlement reached. Once the various UK taxes had been paid and the lawyer done with the paperwork, it was well into 2003, but the consequences were significant. Whereas I had assumed that I would have to stay at VUW at least until the 'traditional' retiring age of 65 years, I now found myself in the position of being able to retire and not be the pauper I might have been. There seemed little virtue in continuing as a staff member at the university because funding was being targeted more and more, and staff without such monies or unable to attract students were going to have increasing difficulties with their research efforts. Indeed, such academics have become less and less able to make a meaningful research contribution without bastardizing their work and twisting their real interest to fit to the modern idiom. Given my health situation, the demands of targeted research, and the paucity of external funds for very fundamental studies, the direction was clear, but the ultimate decision to retire was most difficult. Eventually it was made, as retire I must, and it was in the full knowledge that my ties to the laboratory bench would be severed. I could never raise the monies needed to rent a bench and purchase the chemicals necessary to push back further the frontiers in strained molecule chemistry. And so, towards the end of October 2003, it was with much sadness that I submitted my resignation. Jim Johnston was still Head of School and he instigated

the process of my becoming an emeritus professor and retaining my office, with access to the associated facilities. My resignation proved to be a little ahead of that from John Hoberg, who returned to the US with his family just before Christmas in 2003 some months ahead of my own departure.

One of the last duties that befell me in VUW's employ was the need to complete a portfolio for the newly instigated *performance based research funding* (PBRF) model. By mid-2003, New Zealand had chosen to follow world trends and assess the performance of academic researchers, and this seems soon to be extended separately to teaching. There is little doubt that having an assessment of a given discipline can be a good thing. However, as soon as it was announced that the exercise would grade each academic according to performance (output) with the individual results returned to the institution involved, that arrows were drawn and the opportunity for misuse legion. The concept was simple enough, assess staff research across all the institutions and then divide the government money for research to fit the profile. Of course, none of this has evolved as was intended and the universities now use the PBRF outcome at will for any purpose which suits. Thus, it was announced in the spring of 2010 that, henceforth, no new academic staff member will be appointed to VUW unless the individual's PBRF rating is B or better, at least until after the next (2012) round – the NZ universities are moving to buying in staff to raise their profile for a subsequent round(s).

But to return to the first such exercise and my involvement in it: by the time the need to produce a *portfolio* for the newly established Tertiary Education Commission was known, I had tendered my resignation and felt that, should the institution require a submission from me, then it would need to make its views known in a polite manner. I took the various electronic communications as of little relevance and awaited the personal touch – my research output as measured by the number of published papers in the peer reviewed literature was one of the highest in Chemistry. Eventually, it was the Dean who asked if I would be willing to complete a portfolio; it was very definitely a situation of the institution needing me far more than I, it. Whereas colleagues had attempted to 'fill boxes' as prescribed, I felt it important to know precisely what the bureaucrats meant in specifying the maximum size of an entry by way of characters. I had seen enough nonsensical exercises to suspect that the characters would also include spaces, and thus minimize the opportunity to say precisely what one wanted; I was proved correct. In 2004, there were no good comparative data to allow for science and non-science publication to be matched effectively. This was rectified through a subsequent major exercise undertaken in Australia (which showed that almost every journal that carried my colleagues and my own refereed papers ranked 'A'). Thus, we were required to classify papers by the nature of the review process, the locality of the publication house, *etc.*, *etc.* What I found highly amusing was the absence of a suitable box

in which one could place one's peer review exercises of refereeing. I maintain the view that, if by my actions of scientific review, a paper is subsequently published and the author gains an 'A' grade for that paper in his PRBF exercise, then my action as referee is at least as good, if not better. I included my list of referee reports among the highest ranked papers. Not only did this cause a stir among the pencil-pushing administrators but it elicited a request for the actual reports. The request was vehemently refused (naturally, all hard and electronic copies had been destroyed) and the information that my portfolio would be altered if I did not relocate the offending material was treated by a simple deletion of the entire folder (backed-up of course but who was to know). The point had been made, my personally completed portfolio was duly submitted without change and the 'A' grade outcome, which Victoria had so desperately wanted, gained. Vindication for my stand followed when a separate category of output, namely, material refereed, was included in subsequent exercises.

The decision to retire proved to be a wise one as the sole remaining 'original' by-pass faltered just after Christmas 2003, a little more than a year after surgery. It gave rise to infarct number five and the implantation of a new stent, also number five. Statistically, one in five stents block within the first six months and mine did just that. Shortly after my formal retirement, I awoke in the early hours one April morning feeling unwell but capable of recognizing the heart symptoms. Margaret called the ambulance service and I was (again) admitted to Wellington Hospital. It was later that same morning, while having an ECG taken by the coronary care unit (CCU) medical manager that I was told the pattern was changing as the plot emerged from the machine. I was having an(other) heart attack but felt nothing untoward. That single event had more people moving faster that I had seen previously in the CCU, and I was in the procedures facility for an(other) angioplasty within fifty minutes. The blockage to stent five was opened and two more stents successfully located. At that point in time, an anti-platelet medication had become available in NZ, but only for about three weeks after an uncoated metal stent was implanted; Canada had their patients on the drug for about six months. The cardiology team were of the view that I needed to be medicated for at least a month but the NZ drug purchasing agency 'Pharmac', whose medical director earlier had been my GP for several years, declined the request even for a slightly extended period. The hospital staff manipulated matters so as to provide the medication for the longest possible time, and I purchased the remainder to cover their preferred time period.

It took only a short time to recover and I was soon back to my office at Victoria and into teaching. Peter Northcote was the sole full-time organic staff member and, although assisted by recent PhD graduate Bridget Stocker (prior to her postdoctoral appointment), it was a difficult time for him. My health was then stable from that

time until November 2008. While pruning a small tree, my blood pressure dropped to the extent that I blacked out, fell from the tree, and regained semi-consciousness finding myself in total darkness and with no sensation. The question in my mind was: *had I died?* I tried to move a finger, could do so, and ultimately regained full mobility on the ground in our garden. Margaret found me with no recollection of what had happened. She got the ambulance, and I was off to hospital again. I then had a series of black-outs but all attempts to monitor an 'event' failed and, after an incident in which I scraped the skin off the left side of my face, I was readmitted to hospital and advised that a pacemaker was essential. As I was declared a danger not just to myself but to everyone around me, I was required to stay there until one was inserted. This was done but, being an active chemist, the local anaesthetic did not numb my sensations sufficiently and I was able to discuss just where the electrodes were during the insertions; I had several doses of anaesthetic more than the norm, but to little effect. Since the 2004 blockage and this 2008 're-wiring', I have had no further troubles with my array of expensive metalwork.

The Joys of Emeritus Professor – What Better Role?

When the time came for my formal retirement on March 31, 2004, there was no replacement here for John Hoberg, although the position had been advertised and an appointment made; my own post was about to be advertised as a lectureship in chemistry (organic). On my final day, Jim Johnston organized a delightful School gathering in the late afternoon and marked my retirement with the presentation of *Yearling Sale*, a print by local Hutt Valley artist Mike Ponder. It has pride of place in our home as I have been a Ponder fan for some time. I had negotiated with Jim, who still retained the chairmanship, to retain my office, its facilities with the computer, and university services so that I could continue with writing, editing and student assistance. I was appointed an emeritus professor some months later. I was also asked if I would agree to the School holding a formal retirement symposium in my honour, to which I acceded, but this could not happen until 2005. It seemed prudent to separate my formal retirement from the new 'paper chemist' activities I had planned, and this was to have been by way of a month away from the university. The period was extended for it was during April 2004 that my fifth stent blocked as I have already described.

Joanne Harvey, a Victoria graduate, was the replacement for John Hoberg, but she did not arrive until mid-May of 2004 as she was completing a postdoctoral with Richard Taylor at York in England. For the second time Peter and I covered much of the organic teaching, Peter as a formal employee, me as one under contract, though some of the courses were taught by Crown Research Institute staff and others. My vacant lectureship was subsequently filled by Brendan Burkett, another ANU graduate, but this time from my former colleague Christina Chai. He arrived in the early part of 2005 and, together with Joanne, proved to be a delightful colleague, each active in establishing their respective research groups.

I regard my senior unpaid role as one that allows me to continue with writing, refereeing, editing and assisting any student who wants it – providing only that s/he makes an honest effort. Additionally, it has given me a number of excursions into computational chemistry, some published, some not. What the status of *emeritus* has done for me is to provide complete freedom of choice to do what I most

enjoy, in the time that I wish to devote to it, and at a pace that I find pleasing. As a staff member I was always in to the office well before 9 am, but now I prefer the corridors of knowledge to be aired before I enter at about 10 am, after a leisurely breakfast, the newspaper and morning crossword.

My first tasks were to complete a review that Henning Hopf had invited me to write and to finish off all those papers remaining from our research work. The review was on the fulvalenes and it appeared in print in 2005. With that done, and as Joanne Harvey had been in Canberra at the time that I was on sabbatical, the ill-fated review from 1999 on *gem*-dihalocyclopropanes reared its head. The content encompassed much of Joanne's ANU PhD work and it seemed an ideal opportunity to complete the exercise with her, providing that she was willing and that Martin (Banwell) had no objections. Peter Vollhardt and *Synlett* were still interested in publishing the item while Martin was happy to leave the task to Joanne and me. Thus, it was in late 2004 that Joanne and I began to think about just how, and in what way, the manuscript should be revised and updated. The exercise itself showed Joanne to be adept at the task at hand, effective and efficient, and it was a pleasure to be involved with her in this way. The duly completed article went to Peter Vollhardt, received accolades from him, and subsequently appeared in *Synlett* in 2006.[61] I recall making an enquiry of Peter's secretary, only to receive a rebuke that said 'with so few amendments required what more did I want'! I think it worth noting here that I had not wanted to complete this review for any personal kudos, rather than to provide Joanne with her first article as a VUW staff member.

As the time passed, I took great pleasure in being able to explore the industrial uses to which so many small-ring organic compounds had been put, something that never quite made it past the 'wish list' whilst I was employed. What I found fascinated me and led to a series of articles for *Chemistry in New Zealand* (all externally refereed since I was editor!) under the title of '*From small rings to big things*'. Not only this, but one manuscript provided the NZ readers with a synopsis of Alfred Nobel's life and work that encompassed the nitroglycerin commercialization and its pharmacological (heart) use. What better than to dedicate this to the trio of cardiologists who have tended me over the past 28 years, and I did just that. Another in the small ring series covered the use of 1-methylcyclopropene as a fruit/flower preservative and that fell to my surgeon, Bede Squire, who in his retirement is growing and grafting New Zealand native shrubs. The Nobel piece came from my (editorial) policy of including a synopsis each year of the Nobel Prize winning chemistry with someone subsequently saying just how little he knew about Nobel himself.

Several of the students I had taught continued on to postgraduate study and my final senior teaching in 2004 was to an exceptionally competent group individuals. Joanne Wojnar, who I have mentioned previously, took a year away from study

before returning for PhD training with Peter Northcote, a postdoctoral in Chicago and then back to NZ for a fellowship with Margaret Brimble in Auckland. Maria Matvenko, a Russian immigrant, impressed and it has been a pleasure to support her through the choice of PhD supervisor, her study, and her postdoctoral as a Humboldt Fellow in Munich. Teck Lim, a Malaysian Chinese, completed his PhD with Richard Tilley (in my view, Richard is an emerging nanoparticle star), then went to Oxford as a postdoctoral and is now employed as a research chemist in Resene's Hutt Valley (Wellington) plant. Jonathan Singh, a local man who has taken time to display his true talents, is about to complete his PhD with Peter Northcote as this is written. Indeed, one of the joys of retirement has been to interact with those students who have sought some form of assistance and to watch them develop.

The retirement symposium that the School hosted for me took place over two days at the end of June in 2005. The School asked me to provide a (limited) list of overseas chemists who could be invited and extended the invitations around

Upper: Andy Kay & Leiv Sydnes; Roger Brown, the author, Jun Nishimura and Yoshito Tobe.
Lower: Barry Dent, Andy Kay, Tony Woolhouse, Brian Halton, Ted Harvey, David Officer, Martin Banwell and Jarrod Ward

NZ itself. I was humbled by the number of colleagues who came. They included all the senior organic academics: Jim Coxon, Rob Smith and Margaret Brimble, my former students Tony Woolhouse, Martin Banwell, David Officer, Barry Dent, Andy Kay, and my last Honours man Jarrod Ward, then a PhD student in Auckland. The visitors from overseas included Martin, Roger Brown (himself retired for some years), Jun Nishimura and Yoshito Tobe from Gunma and Osaka Universities, respectively (in Japan), Andrew Grimsdale (who was in Melbourne at that time and represented Klaus Müllen of MPI–P), and Leiv Sydnes (then President of the International Union of Pure and Applied Chemistry) who travelled the furthest (from Norway), specifically for the event. Koichi Komatsu was unable to attend but he visited with his wife in 2008 and, like Leiv, stayed with us at home. Graham Bowmaker of Auckland University was also there as President of the NZIC.

Immediately after the symposium opening by the Vice-Chancellor, Graham took the floor, ostensibly to chair the opening session with Martin Banwell as speaker. However, he diverted and advised the gathering that the NZ Institute wished to confer upon me its highest honour, namely that of Honorary Fellowship, and proceeded to present me with the framed certificate. This election by my peers is the most meaningful award I have received as it formalizes my lifetime attempts to foster chemistry beyond the classroom and laboratory, and my service to the profession in this country and beyond. At the time of writing, it is the only such award this century and the first since the early 1990s. Following this, Martin proceeded to give the opening paper and I was designated the last slot with a title *Life in the Strain Lane.* The first day was capped off by a delightful symposium dinner to which many of the technicians and secretaries from the School came, as did Margaret, our son Mark and his wife Debra. What surprised me was the fact that Yoshito Tobe took the floor to represent the Chemical Society of Japan. He presented me with a framed citation marking my involvement in Pacifichem (Shinji Murai was then President of the CSJ) and, later, the American Chemical Society did a similar thing, giving me a cut glass globe on a Pacifichem-inscribed stand.

The CSJ Plaque

Some fifteen weeks after the retirement symposium the School was visited by

Clockwise: Andrew Grimsdale, Peter Northcote, Jim Johnston, Yoko & Koichi Komatsu, Jim Coxon, and John Spencer

Bob Grubbs, Merle Battiste's most renowned student, who, as I mentioned previously, I had met on a visit to Caltech in the early 1990s. Bobby, as he was known in Florida, gave an inspirational lecture to a large audience and influenced many of our graduate and undergraduate students. I had had the impression that Bob was likely to be awarded a Nobel Prize for some while and my thoughts were that 2005 was the right time. It was because of this that I had Bob sign an offprint of that original joint Chemical Communication in my office, ostensibly for Merle Battiste. Bob received the call from Stockholm advising of his Prize later that night, after returning to Christchurch where he was a visiting Erskine Fellow. Merle never got the offprint, but Bob took him to Stockholm for the ceremonies and that became the highlight of Merle's career.

The presence of Brendan Burkett from early 2005 reintroduced some biologically oriented heterocyclic research into the School and Brendan supervised one Robert O'Reilly as his first student. Robert had entered VUW in 2002, just after his 17th birthday and before he had formal university entry requirements, as his chemi-

cal knowledge was ahead of his peers. He had been brought to my attention by the university's occupational health nurse because he was tutoring the 7th-form class at his school although he was only a 6th-former. He had a better grasp of chemistry than even the teacher involved. After meeting him in 2002, Jim Pearce and I arranged for the Dean of Students, Shona de Sain (a former chemistry teacher), to interview him. He impressed and was enrolled for an introductory chemistry course while still completing his senior school programme and examinations. His VUW undergraduate degree was completed at the end of 2004 and he then taught in Thailand for a year before returning for Honours with Brendan in 2006. Robert's interests were (and are) dominantly theoretically oriented but with affiliated experimental study; he gained a 1st class honours degree and is set to complete his PhD under Leo Radom's supervision at Sydney University during 2011. I regard Robert as my protégé student as he has continually performed well above the average with a chemical maturity ahead of his 25 years; likely his PhD will be completed ahead of his peers in this institution.

But let me return to Brendan Burkett and the difficulties that he experienced from changing attitudes in the university. He was caught up in a major change in policy and protocol. Because Joanne took maternity leave, it was Brendan who had the responsibility for much of the organic chemistry outreach work, lots of teaching, and the expectation that his research would still flourish. Indeed, his proposal for a postdoctoral research fellow succeeded and Mattie Timmer was offered and accepted the job. After his arrival, Mattie worked on Brendan's project as expected, but the Dean of Science and the School encouraged him to follow his major interest that allied with the Malaghan Institute of Medical Research, a private research organization housed in rented space on the VUW campus and alongside the School of Chemical and Physical Sciences. As I saw it, what this did was simple – it essentially terminated Brendan's research and made it difficult for him to make significant progress. Worse still, Mattie was encouraged to seek external funding and it seemed that he spent too little time in Brendan's laboratory, writing research proposals instead. With but 2.7 organic staff, it was obvious to me that another appointment was needed, even if it was to be limited to a three-year term. A post was created and Mattie was duly appointed, likely with the Malaghan Institute meeting a part of the salary. Brendan then sought approval to apply the monies remaining from the postdoctoral position to employ a research assistant (there were neither funds nor time to seek a new postdoctoral researcher) and, even with the intercession of the Head of School, these were not well received.

Current policy now regards postdoctorals as independent researchers rather than a recently qualified individual gaining new experiences under the guidance of an appropriate member of staff whose research proposal was deemed relevant prior to the appointment. The circle is completed with the absence of a fair and

equitable career structure for the recent doctoral graduate, and insufficient funding either to provide the vital postdoctoral fellow for a research group or to provide additional support should limited monies be gained from some other external source. The postdoctoral now is encouraged to seek independent monies from the already overstretched research funds, such as the Marsden, whose criteria have changed to include them. What is so insidious here is that the postdoctoral (or other short-term appointee) appears to be given promises of a position beyond the length of any successful grant application and spends much time preparing submissions, as did Mattie. In comparison, the staff member whose postdoctoral may be diverted from the 'real' job is still expected to raise external monies for research and gain a high ranking in the PBRF exercises that have become a fixture in NZ academia. It has a major impact on the 'doing' of science, while leaving the administrators free to criticize and 'grow' their own area almost without limit. The University used to proffer its core business as teaching and research but it seems that this is now closer to *the management of teaching and research*. It is not surprising, then, that Brendan became disenchanted with the institution and its demands for increased research output: it showed little concern for his extensive teaching and outreach work. He tendered his resignation and is now in the Singapore Institute of Chemical and Engineering Sciences. He has been replaced by Rob Keyzers, one of Peter Northcote's doctoral graduates who gained additional experience in South Africa and South Australia, and who, like Brendan, is being diverted into outreach and other duties above the norm. Mattie's limited term appointment became permanent at the same time.

Martin Banwell

Apart from the writings that I described, the editorial work with *Chemistry in New Zealand*, and the various peer review editing that I continue with, I gain much pleasure from assessing the discipline of chemistry locally, and supporting those chemists who appear worthy of nomination for New Zealand prizes and awards. This no less so than the nomination of Martin Banwell for an honorary DSc degree of Victoria University. With appropriate support internationally, Martin was nominated for and subsequently awarded the degree in May 2010. He graced us by being in Wellington for the week that included the graduation ceremony and the School made the occasion grand by hosting a one day symposium in his honour. I was allowed to entitle the event '*From Small Rings to Big Things'*. Prof. Peter Steel from Canterbury University was the invited external speaker as he had interacted with Martin in Christchurch when he was an Erskine Fellow and even earlier during Martin's PhD study when he was there to use the Canterbury NMR facilities.

L-R: Halton, Banwell, Harvey, Hewitt (18 May 2010)

The local speakers included Joanne Harvey and her 'just graduated' PhD student, Russell Hewitt. I was fortunate enough to be able to outline Martin's early career and chair his closing lecture. The symposium was completed with a dinner at which the day's proceedings and the degree award were commemorated in a bound book that was presented to our guest of honour. The graduation ceremony itself was special, not just for the award to Martin, but because the official seating housed four generations of 'Halton chemists': Russell Hewitt (PhD supervisor: Joanne Harvey), Joanne (PhD supervisor: Martin Banwell), Martin (PhD supervisor Brian Halton), and the author.

Finally, it is worth recording that my last invited lecture on the strained molecule chemistry took place in 2009. Our younger son, Paul, was married to his German fiancée Sandra, in Winnenden, north of Stuttgart, in October of that year. Much to my surprise, I was able to gain international medical insurance to visit Europe and, when Roland Boese heard of this he insisted that I revisit Essen and give a final lecture. This I did under the title *From Cyclopropenes to Novel Molecules and New Ring Systems*. It was a delightful occasion that provided much needed time to catch up with this good and generous friend before his own retirement in July of 2010.

So what about the Bread and Butter?

This biographical account has run its course with little mention of teaching work, academically oriented extracurricular activities, or the way in which Victoria University has evolved in its approach to its staff, and their duties and responsibilities, save for the occasional comment. What follows is no less a personal view than those which have already appeared, but I feel that comment is justified.

As I have said, my teaching began in the University of Florida in 1967 and continued beyond retirement. Some of my colleagues have preferred to focus their teaching to the senior years, leaving others to carry the onus of the larger more junior classes. I have always held the view that it is the senior staff who should instruct the new student at the beginning level, not on a continuing basis, but among the group of that particular chemical specialization, typically the organic, inorganic and physical chemistries in NZ. Moreover, I maintain that every organic chem-

ist ought to be able to teach any part of the curriculum save for more specialized course work in the final undergraduate year and beyond. From my earliest days in Florida, through my introduction to work at VUW by Ted Harvey, and for my lifetime, the protocol has been simple: teaching is the first responsibility with lectures always met unless health makes it impossible. In comparison, laboratory supervision and tutorial work can be delegated if the need to be elsewhere becomes an essential duty. I do not subscribe to the concept of 'smart teaching' whereby much of the job is handed to research students and postdoctorals as may be justified on paper. I am convinced that it is important for the academic to be available for discussion in laboratory sessions, unlike the US norm. Research lines up as second and administration a poor third. This allies with my 1:1:1 time division that I provided to the witless public service interviewer. Of course, there is always flexibility and when research is not going well respite can be found in teaching and *vice versa.* To me, teaching has always been a pleasure, with lecturing never a chore but something enjoyable. This is despite one student seeing me after a particular class to tell me that it was the best lecture he had heard me give; little did he know that a total lapse of memory had my attendance there at the last minute and without preparation! With undergraduate and graduate course work, the quest always has been to impart a balanced approach to organic chemistry and provide the essential base from which the interested student can evolve, never spoon fed. This requires that the courses are given over an appropriate time period that allows the candidate to assimilate the material, digest it, and come to grips with it ….. and this is markedly different from the current 'put it in a box, test it and discard it' that the fractionalization and marginalization of the 21st century now demand.

Another part of my role in academia has been with extracurricular activities over and above service to the discipline and profession of chemistry, and these have gone beyond the NZIC. Regular outreach courses on laboratory safety were given to school teachers in a 'two man show' with my friend and colleague Jim Pearce, even advising the Wellington School of Medicine on it safety protocols many years ago. I spent three or four terms of service on the University Library Committee that covered periods of financial constraint, library redevelopment and budget reductions for the sciences accomplished by reducing text and periodical spending, as well as a range of (limited) activities for the Science Faculty. Further afield, I was involved with the Catholic Chaplaincy from the time that the Student Union officers ousted the chaplains from the union complex through to the Anglican and Catholic chaplaincies having their own properties on Kelburn Parade. I found one of the chaplains (a Capuchin friar) appear in the university without introduction or direction to the institution by the Church and I became involved establishing a Chaplaincy Trust Board. I was appointed the Victoria nominated Newman Trustee to join two illustrious 'legal eagles', supreme court judge Sir

Barry O'Regan and Maurice O'Brien QC, when the trust expanded from its two inaugural members to oversee a significant added bequest and then through it, to the joint Anglican-Catholic Trinity-Newman Trust Board. The Board saw the development of student accommodation using VUW-owned properties that it renovated using a timely government summer employment initiative. Ultimately, it became the largest single non-sectarian accommodation provider for university students as Trinity-Newman Hall. Rentals were set at no more than market rates, often below, and a part-time warden and support staff were employed. It became a 25 year involvement but I never felt that I had given to it as much as I should. Ultimately, the accommodation facilities were taken back by the university authorities by not renewing the lease that the Trust Board held. VUW felt that the enterprise could be better managed and return a (good) profit if it ran things. The impact of this has been to decimate the work of the Anglican and Catholic trust boards that continue as Trinity-Newman. More importantly, there is now notably less support for the student interns whose rentals have added to the institutions coffers (except for 2010 which, through needed major maintenance, seems to have run at a significant loss). The entire VUW accommodation service has undergone a 'change review', the terminology that administrators use to reduce staff and services in the cause of fiscal efficiency. The outcome of this is that University Flats – the name given to the former Trinity Newman Hall – has lost staff and is now centralized, thereby removing its operating personnel from within the environment of the flats where they were easily accessible to the students. The long standing and dedicated manager of the facility, with whom I worked on Trinity-Newman for many years, has taken voluntary redundancy rather than be forced to lose the student contact that she had – another case of the administration seeing victory in change – *in victoria mutatio*!

The institution that saw me as a formal employee for 34+ years, and to which I owe my career, has, like many, evolved to one in which the corporate model of administration rules. Curriculum and research matters have become confused during the evolution of a university from one with a small administration, which regarded its role as assisting the (all important) academic staff, to one that has its enclaves (and here I do mean this as dictionary defined) each insulating itself from the others by becoming pernicious and almost autonomous. The corporate administration, and indeed the like-minded governmental agencies, impose a document requirement and paper trail so long that they take up too significant a proportion of the academic's time, if the now regulated 37½ hours per week is used as measure. What is most destructive is the one rule for all, whereby every minor problem seen in one sector of the institution is solved by imposing its solution institution-wide. It is sad to see precious research time eroded by bureaucracy, the benefits of serious study reduced because of statutes, regulations and rules introduced for all. Far

too often, a minor problem in one area could have been solved quietly – but instead the largest mallet available is sought to squash the smallest ant! As I complete this, I find that the PhD supervisor is no longer allowed to select appropriate examiners for his/her student, rather having to comply with a School Committee who make the judgement – a principle that might work in a large organization but one that will be fraught with more than frustration in a small one such as VUW. What is wrong with the supervisor nominating those world leading experts best able to assess his student's work? After all, PhD study in science is a training in which the ideas of the supervisor in his or her sphere of activity are advanced: one's reputation depends upon it. Worse still, the Chairperson for the PhD oral examination is now appointed by the Faculty of Graduate Studies with little recourse to the discipline and subject of the incumbent. As I complete this writing, an invitation to Readers, Associate Professors and Professors to seek appointment to the panel of 'PhD Oral Chairpersons' has been issued. Those who do not seek appointment seem doomed to be labelled 'unwilling to do their bit' for the institution, while the mediocre researchers will become resplendent in chairing orals in disciplines that they know nothing about. The institution in which I have spent my career has as its motto *sapientia magis auro desideranda* – wisdom is preferable to gold. As I said before, VUW's core business was teaching and research whereas now it seems to be the management of them, with research in the small time that the management allows. Given this, and the administrator not to be questioned in public, acclaim by its staff may now better be served by *aequalitas sine discrimine* or, more cynically: *regat volgus nequissimum*.

But I am retired from the serious doing of chemistry, the production of reports and the keeping of paper trails, and so it falls to the next generations to fight their battles and serve this same discipline that has served me so well. What is important is that they fight adversity, treat each day as gift, and use each as though it will never end. They must do this in a way that best fits their time and not mine. What remains for me is to keep the memories and to continue serve my discipline as best I am able.

In its latest motto-creating initiative, Victoria University advises its staff to '*Get Amongst the Best'* when they log on to their personal computer each morning. I am of the firm opinion that my life and career in chemistry *has been amongst the best* – and I could never have wished for anything more.

Postscript

Keep the *Memories* - Don't *Send in the Clowns*

That Which I Regard as the Best

In completing this autobiographical history of my working life, I feel a need to summarize what I regard as the best of my best – memories that will serve to keep my mind functioning by rolling through the years of pleasure doing what to me has been a hobby and a way of life, not simply a job. Indeed, I am reminded of words that Merle Battiste used the last time that I saw him: *I am a chemist.*

My view of my top ten papers (in sequence)

1. The isolation of the first conformational isomers.[14]
2. The thermal rearrangement of tetra-arylcyclopropenes and Grubbs first paper.[17]
3. The mass spectra of carbocations.[19]
4. The first cycloproparene in New Zealand and the start of a lifetime of their study.[29]
5. The least stable cycloproparene: cyclopropa[*l*]phenanthrene.[50]
6. The most highly strained benzynes generated: the cyclopropabenzynes.[45]
7. The synthesis of olefins from the cycloproparenes.[48]
8. The oxaspiropentene.[55]
9. The cyclopropabenzenylidenethenone (propadienone).[57]
10. The bent cycloheptatrienes.[56]

The ten best lectures I heard (in sequence)

1. Sir Lawrence Bragg at the Royal Institution, London, 1958 (and 1959).
2. Sir Christopher Ingold at Southampton, 1965.
3. Bob Woodward at the ACS Biennial Organic meeting in Vermont, 1967.
4. Horst Prinzbach at VUW, 1977.
5. Emanuel Vogel at VUW, 1979.
6. Sir John Cadogan, former research director of BP, at the Massey ANZAS conference, 1988.

7. Sir Harry Kroto at ISNA 6 in Toyonaka, Osaka, 1989.
8. Larry Scott at ISNA-6 in Hong Kong, 1997.
9. Klaus Müllen at ISNA-7 in San Diego, 2001.
10. Bobby Grubbs at VUW on 5 October 2005.

My top twenty professional memories (in sequence)

1. My interview to enter Southampton University, 1960.
2. The Southampton lecturers Ken Webb, Ishbel Campbell, Eric Parker and Richard Cookson.
3. Arriving at Gainesville railway station, 1966.
4. Thanksgiving dinner in Miami, 1966.
5. Using a stretched Pan Am limo at Kennedy airport, 1967.
6. My welcome aboard the Oriana on departing Accapulco, Mexico, 1968.
7. The journey up Dixon Street in Stan Slater's car, 1968.
8. Meeting Heinz Dürr on Saarbrücken railway station, 1975.
9. Seeing my suitcase off-loaded at the wrong terminal in Houston airport, 1982.
10. My taxi ride from Tel Aviv to Haifa when all transport was on strike, June 1985.
11. Having a DB Bhan train held for me at Heidelburg station by Richard Neidlein, 1988.
12. The washing on a clothes line north of Mannheim repeatedly seen from trains, 1988.
13. The dinners in Tokushima with Sho Ito, 1988-1997.
14. Securing a place for New Zealand and Australia on the Pacifichem Organizing Committee, 1990.
15. Being awarded a personal chair at VUW, 1991.
16. The farewell from Tetsuo Nozoe and his Japanese friends on Göttingen railway station, 1993.
17. The early morning walks along the Yeppoon foreshore, 1999.
18. Eating reindeer in Norway and then returning business class to NZ from Holland with a seat for the O_2 cylinder, 2002.
19. Having Bob Grubbs lecture 10 hours before being advised of his Nobel Prize, October 2005.
20. Having four generations of Halton chemists at the May 2010 Science graduation.

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60. Halton, B.; Boese, R.; Dixon, G.M. *Eur. J. Org. Chem.* **2003**, 4507-4512.
61. Halton, B.; Harvey, J. *Synlett* **2006**, 1975-2000.

Appendices

Appendix I

Qualifications, Awards & Employment

Qualifications:

1963	BSc (Hons.) Southampton University
1966	PhD Southampton University
1987	DSc Victoria University
1977	FNZIC – Fellow, New Zealand Institute of Chemistry
1992	FRSNZ – Fellow of the Royal Society of New Zealand

Selected Awards:

1974	Research Medal, NZ Association of Scientists
1974	British Council Travel Award
1980	ICI Prize for Excellence in Chemical Research, NZIC
1981-1982	Fulbright Research Scholar and Visiting Professor, University of Utah, USA
1987	Victoria University Recognition of Research Award
1988	NZIC Nominee for Federation of Asian Chemical Societies Award
1989	Victoria University Recognition of Research Award
1989	Claude McCarthy Fellowship (NZ) and Visiting Professorship, University of Utah, USA
1996	NZIC In-Service Award for International Services
2002	Shorland Medal, New Zealand Association of Scientists
2003	NZIC Mellor Lecture – Wellington Branch: *From Electrostatic Attraction to Molecular Machines*
2005	Hon. FNZIC – Honorary Fellow, New Zealand Institute of Chemistry

Employment:

1966-1967	Postdoctoral Fellow, University of Florida
1967-1968	Assistant Professor of Chemistry, University of Florida
1968-1971	Lecturer in Chemistry, Victoria University of Wellington
1972-1976	Senior Lecturer in Chemistry, Victoria University of Wellington
1977-1991	Reader in Chemistry, Victoria University of Wellington
1991-2004	Professor of Chemistry, Victoria University of Wellington
2004-	Emeritus Professor of Chemistry, Victoria University of Wellington

Appendix II

Publications: 1966-2011 (TOTAL 185)

Reviews:

1. B. Halton and M. P. Halton, *The Triplet State*, *Chem. in NZ*, **1971**, *35*, 153-157.
2. B. Halton, *Benzocyclopropenes*, *Chem. Rev.*, **1973**, *73*, 113-126.
3. B. Halton, *The Cycloproparenes*, *Ind. Eng. Chem., Prod. Res. Dev.*, **1980**, *19*, 349-364.
4. B. Halton, *Stress and Strain: A Century of Closed Carbon Chains*, *Chem in NZ*, **1981**, *45*, 87-89.
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10. B. Halton, *Long Bonds—Weak Bonds: A Century of Free Radicals*, *Chem NZ*, **2000**, *No. 80*, 11-14.
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14. B. Halton and J. Harvey, *Electrocyclic Ring Opening Reaction of* gem-*Dibromocyclopropanes: Application of Nucleophilic Trapping to the Synthesis of Natural Products and Related Compounds*, *Synlett*, **2006**, 1975-2000.
15. B. Halton, *From Three-Membered Rings to Important Things: Cyclopropanoids*, *Chem in NZ*, **2007**, *71*, 53-60.
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17. B. Halton. *From Small Rings to Big Things: Xerography, Sensors, and the Squaraines*, *Chem in NZ*, **2008**, *72*, 57-62.
18. B. Halton, *From Small Rigs to Big Things: Benzocyclobutenes and High Performance Polymers*, *Chem. in NZ*, **2008**, *72*, 155-159.
19. B. Halton, *From Small Rigs to Big Things: Cyclopropenes, Fruit and Flowers*. *Chem. in NZ*, **2009**, *73*, 34-37.

Book Chapters:

20. B. Halton, *Three-membered Rings.* In Alicyclic Chemistry, Vol. 5 (W. Parker, Senior Reporter), The Chemical Society, London, 1977, 1-99.
21. B. Halton, *Three-membered Rings.* In Alicyclic Chemistry, Vol. 6 (M. A. McKervey, Senior Reporter), The Chemical Society, London, 1978, 1-93.
22. B. Halton and M. G. Banwell, *Cyclopropenes*. In *The Chemistry of the Cyclopropyl Group* (Z. Rappoport, Ed.), Pt. 2, Wiley, Chichester, 1987, 1223-1339.
23. B. Halton, *Strain in Organic Chemistry: A Perspective*. In *Advances in Strain in Organic Chemistry,* (B. Halton, Ed.), Vol. 1, JAI Press, London, 1991, 1-17.
24. B. Halton, *Cyclopropa*renes. In *The Chemistry of the Cyclopropyl Group* (Z. Rappoport, Ed.), Vol. 2, Wiley, Chichester, 1995, 707-772.
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Books:

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Edited Books:

29. Advances in Strain in Organic Chemistry, Vol. 1 (B. Halton, Ed.), JAI Press, London, 1991, xii, 1-283.
30. Advances in Strain in Organic Chemistry, Vol. 2 (B. Halton, Ed.), JAI Press, London, 1992, x, 1-268.
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42. M. A. Battiste and B. Halton, *Mass Spectrometry of Carbonium Ion Salts: 3-Halogeno-1,2,3-triphenylcyclopropanes, J. Chem. Soc., Chem. Commun.*, **1968**, 1368-1370.
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60. B. Halton, P. J. Milsom, and A. D. Woolhouse, *Dehydrohalogenation of Some Tetrahalogenobicyclo[4.1.0]hept-3-enes: Formation and Decomposition of 1,1-Dihalogenocyclopropabenzenes, J. Chem. Soc., Perkin Trans. 1*, **1977**, 731-735.

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Non-Refereed Professional Publications:

160. B. Halton, *Guest Editorial, Chem. in NZ,* **1986**, *50*, 166.

161. B. Halton, *Comment, ChemNZ,* **1987**, *No. 34*, 1.

162. B. Halton, *Annual Report of the New Zealand Institute of Chemistry, Chem. in NZ,* **1987**, *51*, 101-104.

163. B. Halton, *Science and Society: Chemistry and the Community*, NZIC Presidential Address, Aug. 1987, *Chem. in NZ,* **1987**, *51*, 151-153.

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165. B. Halton, Book Review of *Writing Organic Reaction Mechanisms: A Practical Guide,* M. Edenborough, *Chem. in NZ,* **1995**, *59*, 31.

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168. B. Halton, *Editorial*, *Chem. in NZ*, **2001**, *65*(2), 7.
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170. A. Blackman and B. Halton, *The 2002 Nobel Prize in Chemistry*, *Chem. in NZ*, **2002**, *66*(4), 20-24.
171. B. Halton, *The 2003 Nobel Prizes - Chemistry and Physiology or Medicine*, *Chem. in NZ*, **2003**, *67*(4), 41-46.
172. B. Halton, *An Interview with Leiv K. Sydnes – IUPAC President 2002-2004*, *Chem. in NZ*, **2004**, *68*(1), 40-41.
173. B. Halton, *The 2004 Nobel Prizes - Chemistry and Physiology or Medicin*e, *Chem. in NZ*, **2004**, *68*(4), 12-15.
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176. B. Halton, *The 2006 Nobel Prize in Chemistry*, *Chem. in NZ*, **2006**, *70*, 121-123.
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178. B. Halton, *Obituary: W. E. (Ted) Harvey*, *Chem. in NZ*, **2009,** *73*, 41-42.
179. B. Halton, *The 2008 Nobel Prize in Chemistry*, *Chem. in NZ*, **2009**, *73*, 39-41.
180. B. Halton, *The 2009 Nobel Prize for Chemistry*. *Chem. in NZ*, **2010,** *74*, 32-38.
181. B. Halton, *Editorial*, *Chem. in NZ*, **2011**, *75*, 1.
182. J. E. Harvey and B. Halton, *The 2010 Nobel Prize for Chemistry*, *Chem. in NZ*, **2011**, *75*, 49-51.
183. B. Halton. Book review of *Letters to a Young Chemist* (A. Ghosh Ed.), *Chem. in NZ*, **2011**, *75*, 158.
184. B. Halton, *Editorial*, *Chem. in NZ*, ***2011***, *75*, in press.
185. B. Halton, Dora Suuring – 75 Years a Chemist, *Chem. in NZ*, **2011**, *75*, in press.

Appendix III

Research Personnel 1969-2009

Period	Name	Programme
1969	COVENY, Rosemary	Research Assistant
1969	ANG, Weng Soon	MSc
1969-70	MILSOM, Paul, J.	Research Assistant
1971	MILSOM, Paul, J.	MSc
1971-73	WOOLHOUSE, Anthony, D.	PhD
1972-76	BROWNE, Alan, R.	PhD (JL)
1973	SPANGLER, Charles, W.	Visiting Professor
1973	ANG, Weng, Hiong	MSc
1973	TASKER, Steven	MSc (incomplete)
1974	HARRISON, Sylvana, R.R.	MSc
1974	CHUAH, Teng See	MSc
1975	CHUAH, Teng See	PhD (incomplete)
1975	MITCHELL, Gary, M.	BSc (Hons)
1975	OFFICER, David, L.	Research Assistant
1976	OFFICER, David, L.	BSc (Hons)I
1977-81	OFFICER, David, L.	PhD (JL)
1976	BANWELL, Martin, G.	BSc (Hons)I
1977-79	BANWELL, Martin, G.	PhD. 1979
1977	GALLOWAY, Neil	BSc (Hons)I
1978-79	GALLOWAY, Neil	MSc (by thesis)
1978	SLEEMAN, John, R.	BSc (Hons)
1979	ROMIJN, Simon, J.	BSc (Hons)
1979	JAMES, Mark, A.	BSc (Hons)
1980	VIATOS, James	BSc (Hons)
1981	DENT, Barry, R.	BSc (Hons)I
1982-85	DENT, Barry, R.	PhD
1981	RANDALL, Clifford, J.	BSc (Hons)
1982-85	RANDALL, Clifford, J.	PhD (TA)
1983	WARNES, Jeremy, M.	BSc (Hons)
1984	MAIDMENT, Anthony, I.	Research Assistant

1984	COATES, Helen, P.	BSc (Hons)
1984	de RAADT, Anna	BSc (Hons)I
1985	SMITH, Anthony, F.M.	BSc (Hons)
1985-87	BUCKLAND, Simon	Postdoctoral Fellow
1986-	RUSSELL, Sarah, G.G.	BSc (Hons)
1987-90	RUSSELL, Sarah, G.G.	PhD
1986(11)-90	LU Qi (Chi) (now Q. Ronnie)	PhD (TA)
1988	BRIDLE, Janet	BSc (Hons)
1989	CLARK, Heather	BSc (Hons) (aegrotat)
1989	LOVETT, Eva	Visiting Fellow
1990	DIGGINS, Mathew, D.	BSc (Hons)
1990-91	KAY, Andrew J.	Summer Student
1991	KAY, Andrew J.	BSc (Hons)
1991-92	ZHA, Zhi-mei	Visiting Fellow
1992-96	KAY, Andrew J.	PhD
1992-93	DAVEY, Tim, W.	Summer Student
1993	DAVEY, Tim, W.	BSc (Hons)I (PhD Otago)
1993	STEINER, Antony	BSc (Hons) (PhD Boston Coll)
1993-94	STARR, Malcolm, A.E.*	Summer Student
1993-94	STARR, Malcolm, A.E.*	MSc (Hons)
1993-95	COONEY, Mark, J.	Postdoctoral Fellow
1994	FORMAN, Grant, S.	BSc (Hons) (PhD ANU)
1994-95	CUTTLER, Charlotte, A.	Summer Student
1995	CUTTLER, Charlotte, A.	BSc (Hons)
1995-96	CUTTLER, Charlotte, A.	Summer Student (PhD Wollongong)
1996	DIXON, Gareth	BSc (Hons)
1997-2002	DIXON, Gareth (TA)	PhD
2002	DIXON, Gareth (TA)	Research Assistant
1996/7	JONES, Carrisa, S.	Summer Student
1997	JONES, Carrisa, S.	BSc (Hons)
1998-2001	JONES, Carrisa, S.	PhD
1997-98	STRETENOVIC, Sanja,	MSc (by Thesis)
1998	BICKERS, Peter W. T.	BSc (Hons)I (aegrotat)
1998-99	BICKERS, Peter W. T.	Reserach Assistant

2000	PARKIN, Chris T	BSc (Hons)
2001	WARD, Jarrod, M.	BSc Tech (PhD AKL)
2008	SYDNES, Leiv	Sabbatical Visitor

*Jointly supervised with Dr, C. C. L. Chai. JL: Junior lecturer. TA: Teaching assistant.

TOTAL: 47 Graduate Students as per:

6 MSc (Hons) 1969-1974; 26 BSc (Hons) 1975-2000; 1 BSc Tech (Hons) 2001; 1 MSc (Hons) 1993-4;

2 MSc (by thesis); 11 PhD; 2 (x 2 year) Postdoctorals; 4 Visiting Academics.

Appendix IV

Overseas Visitors With NZ Lecture Tours

Year	Name	Institution/Sponsor
1974	Dr. R.F.C. Brown	(Monash, Australia)
1977	Prof. H. Prinzbach	(Freiburg, Germany), 1977 German National Fellow
1979	Prof. D. Bryce-Smith	(Reading, UK)
1980	Prof. E. Vogel	(Cologne, Germany), NZIC Sponsored Visitor
1983	Prof. M.B. Rubin	(Technion, Isreal), NZIC Sponsored Visitor
1984	Prof. S. Sarel	(Hebrew University, Israel)
1984	Dr. P. Sykes	(Cambridge, UK), NZIC Sponsored Visitor
1987	Dr. R. Boese*	(Essen, Germany)
1987	Prof. M.A.Battiste*	(Florida, USA)
1987	Dr. L.A. Cohen*	(NIH, Bethesda, USA)
1987	Prof. A. Padwa*	(Emory, Atlanta, USA)
1988	Dr. M.G. Banwell*	(Melbourne, Australia)
1988	Prof. R. Neidlein	(Heidelberg, Germany), NZIC Sponsored Visitor
1989	Prof. Y. Mazur	(Weizmann Institute, Israel), NZIC Sponsored Visitor
1990	Prof. L.K. Sydnes	(Tromso, Norway)
1991	Prof. U. Brinker	(SUNY-Binghamton, USA)
1993	Prof. R. Keese	(Bern, Switzerland), NZIC Sponsored Visitor
1993	Dr. M.G. Banwell*	(Melbourne, Australia)
1994	Prof. M. Regitz	(Kaiserslautern, Germany), NZIC Visitor
1995	Dr. M.G. Banwell*	(Melbourne, Australia)
1997	Prof. S. Hünig	(Würzburg, Germany)
1998	Prof. R. Gleiter	(Heidelberg, Germany)
1998	Prof. R. Okazaki*	(University of Tokyo)
1999	Prof. L.K. Sydnes*	(Tromso, Norway)
1999	Prof. B. Zwannenberg	(Neijmegan, Holland)
2000	Prof. C. Wentrup*	(Queensland, Brisbane)
2001	Prof. M. A. Battiste*	(Florida, USA)
2001	Prof. C. Wentrup*	(Queensland, Brisbane)

2001	Prof. Z. Rappoport*	(Hebrew, Jerusalem)
2002	Prof. L.K. Sydnes*	(Bergen, Norway)
2002	Prof. S. Polanc*	(Ljubljana, Slovenia)
2002	Prof. M. Rabinovitz*	(Hebrew, Jerusalem)
2002	Prof. M. M. Haley	(Oregon, USA)
2003	Prof. R. Boese	(Essen, Germany)
2005	Prof. J. Nishimura	(Gunma, Japan)
2005	Prof. L. Sydnes	(Bergen, Norway)
2005	Prof. M. G. Banwell	(Australian National)
2006	Prof. M.A. Battiste*	(Florida, USA)
2007	Prof. E Schaumann	(Clausthal TU, Germany)
2008	Prof. A. de Meijere	(Götttingen, Germany)
2008	Prof. K. Komatsu	(Kyoto, Japan)
2008	Prof. L.K. Sydnes	(Bergen, Norway; sabbatical visitor May-June)
2011	Prof. M. G. Banwell*	(Australian National)

*VUW only

About the Author

Brian Halton was born in Lancashire, England and attended St. Joseph's College (Blackpool) and St. Joseph's Academy (Blackheath) grammar schools. It was while at the latter that he was able to attend Royal Society Schools Christmas lectures given by the late Sir Lawrence Bragg. His undergraduate and graduate education was gained at the Southampton University between 1960 and 1966 when he moved to a postdoctoral and then Assistant Professorship at the University of Florida in Gainesville. In 1968 he was appointed to a lectureship at the Victoria University of Wellington arriving in New Zealand late September that year. Although he had the intention of establishing himself in academia and then returning to the northern hemisphere a few years later, he has remained in Wellington ever since. His autobiography surveys his seventy years, fifty as a practising organic chemist.

He has had terms on the international advisory board of Perkin 1, Perkin 2 and the Australian Journal of Chemistry. He serves as a referee for many international publications, is a member of the New Zealand Institute of Chemistry Council and is one of its Honorary Fellows. He is now emeritus professor at Victoria University of Wellington.

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