MY INTERESTS IN SCIENCE

W E Ewers

SCHOOL up to 1936

I do not know what led me to an early decision to study Science in general of Chemistry in particular. If one discounts my early experiment with the ringlet curls of the girl sitting in front of me in “1st Babies” at North Perth State School, which left me with an enduring inkling as to how a spring balance works, the earliest recollection of science at school was of a class at West Leederville when I was about 8. The teacher asked us '"What is a liquid?" My hand shot up and I came up with the somewhat inadequate definition, “something that runs". The smart-arse teacher pointed out the door. '"There's Johnny running across the playground. Is Johnny a liquid?” I certainly do not remember what his better definition was, though I assume he had one, but he did begin to teach me the importance of unambiguous definitions, not to mention the folly of speaking without due thought.

There were vaguely remembered little bits of science in my remaining years at West Leederville and at Perth Boys. In my first year at Perth Modern School there was no formal study of science but I came to second year convinced that chemistry would be my first choice, along with Physics and German, when I dropped Geography and Latin. In these choices I was undoubtedly assisted and encouraged by Keith Murray who was two years ahead of me at Perth Modern School. I am not sure that John Farrant influenced me at this stage, but we had renewed our acquaintance from West Leedervil!e days and frequently walked home together. We were in the same form and after Chemistry and Physics classes started we would sometimes finish the afternoon in Mrs Farrant’s outdoor wash-house doing chemical experiments. I remember one occasion when success in generating chlorine had us both dashing outside coughing. Our experiments with explosives were rather better controlled. John’s place had other attractions. He and his father had constructed a quite impressive 6-inch reflecting telescope, and another attraction was a working model of a steam locomotive, powered by a methylated spirits burner.

Apart from the formal teaching in chemistry there were also conservations about chemicals and their reactions that occurred in an unlikely place, a Sunday school class. Norman Walker, a cousin of the Kennedy family, was in charge of our class and in his weekday job was a chemist at the Cresco plant in Bassendean. After the class material for the day was dealt with he would talk, mainly to me, about chemistry. What the other boys in the class thought about this I have no idea. Norman would also provide me with small quantities of common chemical compounds and these made possible a few simple experiments at home. He subsequently became the Chief Chemist at Cresco and I remember thinking as I approached graduation that that was a possible line of work for me.

In my fourth year at PMS I was made a Science Cadet which entailed, for a meagre reimbursement, a few jobs assisting the teacher in preparing for, and clearing up after, practical demonstrations. A more attractive inducement was being allowed to spend time in the laboratory during lunchtime. This I used, among other things, following an early interest in growing crystals. I remember being rather pleased with the production of some hexagonal crystals of lead nitrate.

UNIVERSITY 1937-1940

The next step was starting on a Science degree at UWA with Chemistry, Geology, Mathematics and Biology as my first year units. I opted out of Biology in the second year and in the third decided on a double major in Chemistry and Maths.

Originally had intended to make Geology my second major but it became obvious that, as the syllabi were constructed, the demands of practical work in both subjects and of field work in Geology would have overloaded my timetable.

I will not attempt to give an account of my progress in these studies but it is worth noting that the syllabus in Chemistry at that time attempted to cover the major branches of the subject including Inorganic, Organic and Physical Chemistry and these were taught and examined in each year. From this melange my interests became directed to Inorganic and Physical Chemistry and I was never to become involved subsequently with Organic or Biochemistry.

During my undergraduate years I entered the annual Science Conversatione on two occasions. These were occasions when students were invited to put on exhibits for public display. Not only was something of their work to be displayed but they were required to put themselves on display dressed in dinner jackets! During my second year Tim O’Driscoll and I jointly exhibited a series of maps showing the shape of Australia at various stages through geological time. I wish I had now a photographic record of that exhibit to see how closely it would correspond to the present view. My second effort was a solo one. In my third year I decided, for reasons that escape me ow, to demonstrate how synthetic rayon could be made. I quickly eliminated the actual industrial process, too smelly to be socially acceptable, and opted for the cuprammonium process which involved dissolving the cellulose in a fine thread that could wound onto a spool. It was quite spectacular, worked a treat, and I was delighted to be awarded first prize. On the debit side I ruined the cuffs of my dinner shirt coping with broken threads in the sulphuric acid. And besides I was invited to repeat the demonstration the following year.

I finished my third year with a Distinction in Chemistry, became the first winner of the Wilsmore Prize in chemistry and with an invitation to do Honours in the following year.

And that brings me to my first job as a chemist. To earn some money for the next step Professor Bayliss organised for me a job in the laboratory at the Midland Junction Railway Workshops. I was quickly confronted with professional responsibility. I was given samples of alloys from worn locomotive bearings and asked to analyse them so that their compositions could be adjusted for re-use in bearings. This was no longer a laboratory exercise but my results were going to be used. After this assignment I was entrusted with a small research project to devise a test for measuring the extent to which lubricating oils were being diluted in service with diesel oil. The test was based on viscosity which was measured in a water bath at a temperature of 100F, a temperature that could only be maintained in the hot laboratory by adding ice. Tis vacation appointment, and the recommendation I got from Dr Cyril Kent at Midland, helped me subsequently when I applied for a position at CSIR.

My honours yeas, 1940, was spent on course work and on a research project, both supervised by Bayliss. Australia was now at war and supplies of potash fertilisers were cut off. This led to a proposal to produce potash from a deposit of the mineral alunite in a lake bed at Chandler, north of Merredin. In my research project I was directed to a book entitled The Principles of Flotation by I. W. Wark and asked to explore the possibility of using flotation to separate the alunite from the clays that occurred within it, I the event it proved to be a impossible task, both alunite and clays occurring in very fine particles and showed no sign of reacting differently to the flotation reagents I tried. More of flotation later.

CSIR PERTH 1941-1946

With my Honours degree behind me I aspired to join CSIR and in particular the new Division of Industrial Chemistry which was being set up in Melbourne with Ian Wark as its first Chief. I learnt of this development through Keith Murray which was one of the first people appointed to the new Division. My application was successful and I was all set to move to Melbourne when some of the locals made representations to John Curtin, then Leader of the Opposition in the Federal Parliament, citing the importance of potash fertilisers to the nation’s agriculture and suggesting that the Division second me to continue working at UWA on the Alunite Project. So it transpired.

Over the next six years I was involved with several aspects of the Project. The alunite contained sodium as well as potassium and when it was roasted and leached the salt that crystallised from the resultant solution was glaserite, a double sulphate containing one part of sodium to three parts potassium. Tis could be used as a fertiliser and was the final product when a commercial plant was brought into production. However it seemed desirable to eliminate as much sodium as possible and I spent some time studying ways of achieving this. Then the war scene changed with the entry of Japan and it became a national priority to try to produce aluminium in Australia. Alunite was suggested as a possible raw material and I was asked to see whether sufficiently pure alumina could be produced from the residue after potash had been extracted,

The impurities that we knew we must eliminate were iron and silica and this requirement led to using acid rather than alkali to attack the alunite residue. Hydrochloric acid was the reagent of choice but the resulting solution contained iron which had to be eliminated. I argued that if I could concentrate the leach liquor with more HCl it might be possible to precipitate aluminium chloride. In the event when I passed HCl gas into the leach liquor, white crystals separated and these could easily be washed with pure HCl solution to remove the iron. The clean hydrated aluminium chloride could be decomposed by heat to produce a very pure alumina, suitable for aluminium production, and generating HCl gas to reuse in the process.

In collaboration with Grant Miles we set up a small glass apparatus in which we could produce batchwise a few kilograms of hydrated aluminium chloride and then convert it to pure alumina. The results were sufficiently encouraging that the State Government agreed to finance a pilot plant capable of producing up to 500kg alumina per day. It was a brave effort but the difficulties arising from the extremely corrosive conditions to be handled slowed our progress and ultimately the project was abandoned. In retrospect, the tonnages of residues available from the Chandler potash industry could never have supported an aluminium industry, and indeed the commercial potash production only survived for about four years after the war.

With the closure of activities at the pilot plant I returned to the problem of producing potassium sulphate and not glaserite. This was another good idea that came to very little. Glaserite was the stable phase that crystallised from the leach liquors at Chandler. I wondered if the crystallisation of the stable phase, glaserite, could be inhibited by adding to the solution something that would be adsorbed on nuclei of glaserite so that the unstable phase, potassium sulphate, might crystallise. I constructed atomic models of the crystals of both materials and established that the compositions and dimension of their faces were indeed different, encouraging the notion that selective adsorbents might be found. But I had not found them when an opportunity arose to transfer to the headquarters of the Division at Fishermans Bend to join the Flotation Group and thus to extend my knowledge of the Chemistry of Surfaces.

CSIRO MELBOURNE 1947-1962

Thus, in November 1946, I moved to Melbourne into the exciting environment of the fast-developing Division of Industrial Chemistry. I was met by Keith Sutherland, a former colleague of Wark, and then Head of the Physical Chemistry Section of the Division. Keith took me in to see Wark and told him I would be sharing a room with Tony Evans. Wark's comment was 'That will be interesting' and indeed it was. Thus began a close collaboration and an enduring friendship.

My first project was to examine work that had been started by Reg Goldacre who had left the Division. He had been studying the flotation of the tungsten mineral, scheelite, which was being mined on King Island, an operation that was blighted by very low recoveries from the gravity separation methods then in use. In the event I was no more successful than Goldacre in achie3givng a useful separation from the garnet andradite also in the ore.

Then followed a more intensive study of the flotation properties of the tin mineral, cassiterite. But first let me enlarge on flotation. As applied in the separation of minerals, flotation depends on altering the surfaces of mineral particles so that they become hydrophobic so that they will attach to bubbles of air which float them to the surface of a slurry of ground rock wher they collect in a froth and can be scraped off to form a concentrate. The process was first developed commercially sat Broken Hill to separate sulphide minerals such as galena (lead), sphalerite (zinc) and chalcopyrite (copper) from their ores. Ian Wark was the first to apply surface chemistry to understanding the process. With the sulphide minerals one group of reagents was particularly successful as 'collectors' that is in making the mineral surface capable of attaching to air bubbles. Selectivity was achieved by varying the pH (acidity) and by adding metal salts in solution. The collectors for sulphide minerals are not frothers and other chemicals are added to give some stability to the froth containing the concentrate so that it pours over the lip of the flotation cell.

So much for sulphide minerals. With non-sulphide minerals, such as cassiterite and scheelite, the problems are rather difficult. The chemicals that can make them hydrophobic are generally related to detergents and it is much more difficult to achieve selectivity in affecting the desired mineral and leaving unwanted minerals unaffected. Moreover, with detergent-like chemicals in the slurry, no added frother is needed and, indeed the froth tends to be too abundant and unwanted minerals are carried mechanically into the concentrate.

We spent a lot of time studying the flotation behaviour of cassiterite, both at a fundamental level and in trying to apply our results to a practical separation of cassiterite from an ore from Maranboy, N.T. In one study, using very finely divided 'pure' synthetic cassiterite, we measured the update of the collector, sodium cetyl sulphate, on the measured surface area of the mineral ad were very surprised to find that flotation could be observed with only a small percentage of the surface covered. The practical tests on ore were plagued by the typical difficulties mentioned in the last paragraph but in the end we came to a workable process that entailed an extra non-flotation step to effect an acceptable recovery and separation of cassiterite.

The next challenge took us out of the mineral separation business. Australia's woolclip, most of which was (and still is) baled and exported without processing, contains in its crude form up to 15% of wool grease which is a potentially valuable product that can be refined to lanoline and contains a variety of interesting compounds. The wool treated in Australia was being scoured I soapy water and their scour liquors with their wool grease content became a noxious waste. It was suggested to us that the wool grease might be amenable to recovery by flotation.

The process Tony Evans and I developed resembled mineral flotation only to the extent that modified flotation cells were used. With a cooled scour liquor, in which some of the wool wax had solidified, more than 50% of the wax could be collected in the froth could be increased by feeding it back to the impellor of the flotation cell to 'churn' the froth in a process that had characteristics in common with butter churning. The froth could be cleansed of solid matter and excess soap by passing it through a bank of cells with a counter current flow of water added to the last cell. Further refining was achieved by adding phosphates to the froth dispersed in hot water and then centrifuging to separate clean lanoline from remaining solids and water. The process was briefly operated commercially, but soap scouring has long since been overtaken by the more environmentally friendly solvent scouring.

In 1954 I was given the opportunity to travel to England, France, Germany and the United States to visit mines and research groups where flotation was being used and/or studied.

Our studies pertaining to flotation extended to some of the more basic science involved. Tony looked at the way in which a bubble made contact with a plane surface, showing that a lens of water was trapped between the bubbles and the surface and contact was first made on the periphery of this lens, after which the air rapidly displaced water over the area of contact. The mechanism explained the relative ease of contact between particles with sharp edges compared with smooth spherical particles.

We also studied the stability of froths, some of the factors that affect the stability of thin films between bubbles as water drains from them. Popular wisdom at that time stated that surface active materials, such as detergents, stabilised froths by lowering the surface tension of water. They do this by becoming concentrated in the air/water interface, an ability that they owe to the fact that their molecules have one end that is attracted to water and at the other end a long hydrocarbon chain that is more comfortable in air. Keith Sutherland and I published a paper which added to the understanding of foam stability. We showed that it was not the lowering of surface tension *per se* that was important. It takes a small but significant time for the molecules of the agent to enter the surface and when film between two bubbles is stretched locally there will e a temporary diminution in the concentration of the agent at that point causing the surface tension in that area temporarily to increase. This in turns causes molecules of the surface active agent to flow into the area of stretching bring with them a flow of the underlying water. The film is thus self-healing and a result of the variability of the surface tension. A reverse process accounts for the activity of some foam breaks which are insoluble compounds capable of spreading on the surface of a film taking underlying fluid with them and causing the film to rupture.

Another factor in froth stability is surface viscosity, which is the two-dimensional equivalent of the resistance to flow in a three dimensional fluid. This property of films on water surfaces is very difficult to measure because of the variations of surface tension when flow film surfaces are caused to flow. I proposed a new method for measuring surface tension when flow film surfaces are caused to flow. I proposed a new method for measuring surface viscosity which overcame this difficulty and was able to persuade Robert Sack to make the difficult hydrodynamic calculations which legitimised this method.

There was still much to do on flotation when in 1955 Lewis Lewis, Secretary of the Division and Wark's administrative side-kick, was transferred to the Head Office of CSIRO and I accepted an invitation to take his place. This gave me an invaluable opportunity to understudy Wark, and administrator of research for whom I had great admiration. The job put a temporary stop to my research activity but I was able to tale a vicarious interest in the aspirations and successes of many of the talented research staff then working at Fishermens Bend. Allan Walsh was developing his atomic Absorption Spectrometer; Bill Mansfield was demonstrating how to reduce evaporation in dams; Don Weiss was coming up with a strong of processes for purifying water and recovering metals and Dave Wadsley was making important advances in the understanding the structure of complex solids. All of these and many others would pop into my office and tell me about ideas and advances. I was also involved sometimes when the results of research work led to patents and to arrangements with industry people for practical applications. Other aspects of my job included general responsibility for the library and the general workshop, oversight of the preparation of estimates and the collation of the Annual Report. All of these jobs brought me into contact with research staff and that they were doing. Soon after I took over this job I was brought back to earth when John Farrant asked Graeme what he was going to do when he grew up. “I want to be what Dad was” replied my 10 year old son.

Within the first few weeks of becoming Divisional Secretary, I was invited tone day to meet Maurice Mawby, then CEO of ConZinc Rio Tinto. He asked me if I would be interested in joining the Company as head of their Research Laboratory in Broken Hill. After discussing the prospect at home and with Wark I declined. It was one of those decisions that one looks back on and wonders.

In 1959 there was a major reorganisation. The Division of Industrial Chemistry (DIC) had grown to more than 160 professional staff in its six Sections. Several of the Sections were as large as some of the Divisions in other parts of CSIRO and it was decided to give Divisional status to three of them. Thus DIC became the Chemical Research Laboratories (CRL) with Ian Wark as Director, and Chemical Physics, Physical Chemistry and Mineral Chemistry became Divisions with Lloyd Rees, Keith Sutherland and Dick Thomas as their respective Chiefs. The other three Sections, Organic Chemistry, Chemical Engineering and Cement and Ceramics, renamed Applied Mineralogy, under Harold Hatt, Clive Pratt and Arthur Gaskin remained as Sections.

These changes brought changes to my responsibilities as the new Divisions, to varying degrees, used their autonomy to take over administrative functions that had been handled centrally hitherto. This trend gathered pace in the year 1960/61 when Ian Wark was appointed to the Executive of CSIRO and Organic Chemistry was given Divisional status, with Jerry Price as Chief, and again two years later when Chemical Engineering and Applied Mineralogy (formerly Cement and Ceramics) became Divisions under Pratt and Gaskin. Over this period CRL was managed by a Committee of six Chiefs and Officers-in-Charge, the Choics Committee, with Lloyd Rees as Chairman. This was a transitional period with my role to look after central functions and, where invited, to fill in the gaps in the developing Divisional administrations. This role was different for each group and inevitably shrinking as time went by.

It was with this background that I first learnt of and became increasingly interested in the proposal to set up a new laboratory in Perth. The proposal originated in Perth where the local State Committee of CSIRO, no doubt stimulated by the political climate for development which Charles Court was preaching, urged the Executive to extend its research activities in Perth beyond the areas of relevance to Primary Industry (Plant Industry, Soils, Fisheries and Wild-Life). Thus it was that the first unofficial name of the new laboratory became The Secondary Industry Laboratory. In 1962 I was given the nod that I would be transferring to Perth Charged with the task of setting up a branch laboratory of the Division of Applied Mineralogy. We sold our house in Melbourne, gave Graeme time to sit his Matriculation examination, packed up and travelled to Fremantle by sea.

CSIRO PERTH 1963-1981

The Western Australian Committee and the State Government wanted a secondary industry CSIRO presence in WA but there was no detailed indication as to what area of activity was expected of us. The terms of reference given me by CSIRO were very broad – “to work on minerals and related topics”. As a part of a Division of Applied

Mineralogy, with Gaskin a geologist which chemical interests as my Chief, and myself a chemist with some geology, my first task was to acquaint myself with the mineral industry in the State, establish connections with the industry, the University and the Geological Survey and to give myself a refresher course in geology. This I did over the next six months from an office in the old Chemistry building at UWA, kindly provided by Noel Bayliss.

There were old friends who helped me in these endeavours, notably Joe Lord, Director of the Survey, Rex Prider, Professor of Geology, and Andy Cole and other chemical colleagues. I made many visits to mining and exploration sites sometimes accompanied by Divisional colleagues from Melbourne and occasionally by members of the Survey. We were generously received by geologists from the mining companies and the friendships made with such people as Roy Woodall and his WMC team were to prove enormously valuable to the future development of the Laboratory.

The Division's Annual Report for the year ending June 30, 1963 states that the Laboratory 'will investigate the feasibility of developing glauconite as an economic source of potash'. It also referred to visits to the south western corner of the state revealing several possible topics for future research. In the following year Ted Davis and Jim Graham joined me from Melbourne and we had started on the glauconite work, using material from drilling by the Geological Survey at Gin Gin and Dandaragon. We had also looked at rocks from near Wundowie as a source of vanadium and others from two sites, Ravensthorpe and south of Coolgardie, as sources of lithium. All of these investigations had a common feature; in each case we were looking at the geological setting of rocks and considering whether the way the rocks had formed, or how they had been altered by weathering, might suggest a way of treating them to recover their valuable components. In the event none of this work led to commercial application but they helped to establish a theme for the laboratory. Most geological processes are basically chemical in nature and the laboratory's theme became geochemistry in the broadest sense, the Chemistry of Geological Processes. I remember giving a talk to a group of geologists in which I made the point that the index of a well-known book on petrology listed many terms ending in the letters "isation". Examples are sericitisation and granitisation. I concluded that all of these words has two things in common; one that each described the process of formation of a rock or mineral from some precursor, and second that the chemical nature of this change was not understood.

The move to establish a mineral research laboratory in Perth was most timely. In the late 1950s Western mining had shown that large reserves of bauxite in the Darling Range were suitable for the production of alumina for aluminium smelting. A few years later the enormous potential of the Hamersleys as a source for iron ores was revealed spurring a flurry of exploration and geological mapping. I recall a talk by Bill McLeod of the Survey in which he turned upside down the former notions of the geological succession in the area. In the same talk he staggered his listeners with estimates of the amount of iron that had been deposited in an area about the same size of Tasmania. This would have been in about 1964 and it was only a few years later that Western Mining announced their discovery of nickel ores at Kambalda, an announcement that triggered the widespread exploration of Yilgarn.

Although we had only a peripheral interest in these beginnings of very significant developments, by the time of the nickel discoveries we had established close connections with the Survey and with mining companies, notably Western Mining.

The nickel discovery was a most important geological advance as the occurrence at Kambalda established a new pattern for exploration for nickel worldwide. With Russell Hudson, a recent

appointee to the staff, I was able to start a detailed analysis of a continuous drill core, provided by WMC, which intersected the whole sequence through the silicate host rock, the disseminated ore containing some nickel sulphides and the massive sulphide ore below. This analysis enabled us to propose that the ores had been introduced as a partially solidified lava containing crystals of the silicate mineral, olivine, and a separate liquid sulphide phase made up of the mixed sulphides of iron, nickel and cobalt. In its final resting place the dense sulphide phase would finish at the bottom, to form the future massive ore, with the lighter crystalline olivine floating on it except where the downward pressure of the overlying olivine forced some of the olivine down into the sulphide matrix. One can see the same effect in a glass when too much ice is added to a scotch. At home one night I realised that a test of this model would be to show by electrical conductivity that the sulphides were the continuous phase throughout the ore. And so it provided to be. However sparse the sulphide grains appeared to be each grain was in electrical continuity with every other grain in a block of ore. We therefore proposed that the ‘disseminated’ ore should be renamed the ‘matrix’ ore.

Further changes were seen to occur after the lava cooled to the point where the sulphides solidified. Within the massive sulphide ore there was a band of pyrite which had evidently crystallised first leaving a solid solution that subsequently separated into the main nickel ore mineral, pentlandite, and the second iron sulphide, pyrrhotite. There was evidence of ready mobility of nickel on other metals in the hot sulphide and this led me subsequently to experiments that showed that nickel could be recovered from solution by heating pyrrhotite. This work resulted in a patented process which was tested by WMC when they were considering processing laterite nickel ores.

The work on nickel ores continued in the Laboratory for some time with Russell Hudson, Ernie Nickel, Mike Thornber and Rob Hill making important contributions. The work attracted attention from overseas and in 1975, when there was an attempt (fortunately unsuccessful) to transfer our Division to merge with the BMR, Tony Naldrett, a world authority on nickel sulphide ores, came to our defence with a statement that our group was the outstanding laboratory in his field in the world. I gained great personal satisfaction in particular from the very successful collaboration between Nickel and Thornber. It vindicated bringing chemists together with geologists in close working relationship to understand geological processes.

Apart from nickel the interests of the group ranged to problems of exploration in the deeply weathered ancient landscape of Western Australia, to understanding the transport of metals such as gold and uranium in the geological environment and the localisation of the deposits of these metals and to developing new approaches to geochemical exploration. My role in these activities was mainly in facilitating and encouraging.

I did however have a few bees in my bonnet. One was the role of temperature gradients in providing the driving for solution transport of materials over relatively short distances enabling recrystallisation of minerals and the changing of rock types, for example a sediment to granite. This mechanism would allow, at high temperature and pressure, a relatively small amount of water to effect the transport without being used up. A note published in *Economic Geology* pointed out the potential of this mechanism in the hydrothermal processes.

Another enduring interest was the extraordinary accumulation of iron in the Hamersleys. I had been excited by the Survey's mapping of the area and by Alec Trendall's classic *Bulletin 119*, The Iron Formations of the Precambrian Hamersley Group. I had had the good fortune to visit the area with Alec on several occasions and regretted that there did not seem to be a role for our involvement because there seemed to be so much iron ore already discovered.

The we learned that the companies were concerned that the really high-grade deposits, with the low phosphorus content demanded by the market, were limited. Enquiries determined that the companies would welcome research on what caused the deposits at Mt Newman, Tom Price and Paraburdoo to have acceptably low phosphorus contents in contrast with many other deposits discovered up to that time. After a short preliminary investigation, a research proposal was made to AMIRA and support was gained from Hamersley and BHP enabling us to appoint Dick Morris initially for a period of two years. Dick's speciality was a very well-developed skill in the use of microscope in mineralogy and petrology and an ability to use it in interpreting the sequence of changes during a rock's history.

We were fortunate to obtain from the Geological Survey a complete core from Dale's Gorge Member of the Brockman Iron Formation which was the hist rock of most of the high-grade hematite ores at Mt Tom Price and Mt Whaleback. The core had been drilled in unweathered rock near Wittenoom during exploration for blue asbestos. This core was sectioned along its entire length and analysed in short lengths to give a compositional profile. We also developed a new technique using adhesive tape to transfer a picture of the surface of the core with the main minerals identified by their streak (their colour in powdered form). These enabled us to compare our Wittenoom core with parts of the Survey's Type Core (drilled 10 metres away) and another core from Paraburdoo (130km away).

The Dale's Gorge Member, as described by Trendall comprises 33 distinct units, 17 of them dense hard banded rocks (BIF) alternating with 16 softer and more friable units designated as shale (S). This sequence occurs throughout the Hamersleys, and, where exposed on mountain sides or in gorges, is recognised by the stepped alternation of vertical cliffs of BIF and the slopes of the shale. The whole member does not vary very much in thickness (up to ca. 130 metres) over the whole area (about that of Tasmania). The BIF units are characterised by a sequence of mesobands, between 1 and 80 millimetres in thickness, which are differentiated by variations in composition, mainly in the ratio of iron oxide to quartz and silicate minerals. Many of the mesobands are further divided by finer bands, microbands (0.3 to 1.7mm), which are essentially alternations of layers of iron oxide and quartz. Trendall had noted that the sequence of mesobands could be recognised in cores drilled at sites 50km apart. Our streak prints enabled us to recognise identical sequences even to the microband level in cores taken from sites 130km apart.

The chemical analysis of the core was undertaken to provide background information and to see whether there was a pattern of phosphorus content that might persist in the ore after silica and other components had been removed in the process of enrichment to ore. The general conclusion was that the phosphorus in the mineral apatite occurred sporadically through both BIF and shale units, slightly more unabundant in the former. During the enrichment process, which at Mt Tom Price and Mt Whaleback had occurred at considerable depth, the iron oxides had recrystallised to form haematite excluding phosphorus to a greater extent than had occurred in the more abundant ores in other parts of the province that had been enriched by weathering nearer to the surface. The latter type are exemplified by the as yet under developed ores at Boolgeeda and Rhodes Ridge.

Subsequently it emerged that the Marra Mamba Formation, which is lower in the sequence than the Dale's Gorge Member, is the source of another set of iron ore deposits that are now being mined. Although these ores do not match the dense lump ore of the older mines they are also desirable for their low phosphorus content, reflecting that of the Formation from which they are derived.

The original 2-year project was extended to another two years up to mu retirement in January 1981, and beyond that under the leadership of Dick Morris and with the support of other companies. Dick went on to establish an impressive reputation as an authority on the processes if iron ore formation. Along the way we had become involved in an International Geological Correlation Program, Project 132 (Basins of Iron-Formation Deposition). The results of Project 132 were published in 1983 with Alec Trendall and Dick Morris as editors of a monograph entitled Iron formations: facts and problems. I was invited to contribute a chapter in this book on the topic "Chemical factors in the Deposition and Diagenesis of banded iron formations". Preparation of the chapter commenced before my retirement and continued well beyond it. This work gave me a good opportunity to indulge my fascination with the whole phenomenon of the extraordinary concentration of iron in the Hamersley area, which it can be argued, constituted a major turning point in the history of the planet and the evolution of life.

It is generally conceded that these rocks were precipitated, probably initially as gel from the ocean. How they finished up as a finely branded rock with bands down to a millimetre or so in thickness and each band containing a layer rich in iron and another rich in silica, and how these bands persist in recognisable sequences over very large distances, pose fascinating problems which I discussed, leaving most of them unsolved. The sheer bulk of iron precipitated over 100,000 square kilometres demanded concentrations of iron in the ocean that could only been achieved if there was virtually no oxygen present in the atmosphere. The oxidation process which caused the iron to precipitate may have been a by-product of the photosynthesis of primitive algae or it may have been caused by ultraviolet radiation from the sun. It can be argued that the precipitation of the iron ultimately exhausted the supply of available dissolved iron in the ocean and after that molecular oxygen produced from photosynthesis would start the build-up of atmospheric oxygen. With abundant oxygen in the atmosphere ozone would also be produced which would shield organisms from ultraviolet radiation and would allow the process of organic evolution to lead to more complex organisms. It would also explain the disappearance of banded iron formations from the subsequent geological record.

These grand ideas were not mine. Preston Cloud, for one, had proposed the general theme. What I tried to do was to fill in some of the chemical detail and set in in a quantitative framework.

I was delighted, after the publication of the monograph, to receive a note from Preston Cloud congratulating me on what he considered to be the best chapter in it ‘by a good margin’.

After my retirement in 1981, I was invited to join the Council of WAIT on which I served for six years, chairing several of its committees including one that reformed the promotion system for academic staff and finally becoming Deputy-Chairman of the Council. This service was recognised when, in 1987, WAIT became the Curtin University of Technology and appointed me as a Foundation Fellow of the University.

Also, in 1981 the WA Government appointed me to the first Board of the Western Australian Mining and Petroleum Research Institute, WAMPRI, and then to its successor, the Mining and Energy Research Institute of Western Australia, MERIWA. These were set up to foster research with Government money as the seed but requiring substantial contributions in funds and logistics from Companied that might benefit from the investigations. With old friend the late Joe Lord as Chairman these were very successful and MERIWA is still going. I retired from the Board on reaching seventy.

Throughout my working life, I was active in various professional bodies, the Royal Australian Chemical Institute (RACI), the Australasian Institute of Mining and Metallurgy (AusIMM) and the Geological Society of Australia (GSA). My first interest was the RACI of which I was awarded Fellowship in 1957, elected WA Branch President in 1969 and 1970 and Federal President in 1974.