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Joseph Chatt

Joseph Chatt was born in County Durham, England in 1914 of a farming family. In 1924, his family moved to a farm 10 miles S.W. of Carlisle in Cumberland where he spent his youth before receiving his tertiary education at Emmanuel College, Cambridge, B.A., M.A., Ph.D., and later D.Sc.; he is now an Honorary Fellow of the College.

Subsequent to his Ph.D. and during World War II, he spent a short time as a Research Chemist in the Woolwich Arsenal, London, then in 1942 moved to an appointment as Deputy Chief Chemist with Peter Spence and Sons Ltd., Widnes, eventually becoming Chief Chemist. When the war ended, he took a post-doctoral Imperial Chemical Industries, Ltd. (I.C.I.) Research Fellowship at Imperial College, London, but in 1947 he left that to establish an inorganic chemistry department in I.C.I.'s new fundamental research laboratories, known as 'The Butterwick', and later 'The Akers, Research Laboratories' at The Frythe, a country house outside Welwyn village in Hertfordshire. His research there was recognised by his election to the Royal Society in 1961 and at about the same time his Department became a Research Group in the Heavy Organic Chemicals Division of I.C.I., but it remained at Welwyn until 1962 when I.C.I. sold The Frythe.

Joseph then accepted an invitation from the Agricultural Research Council (A.R.C.), a quasi-Governmental body, to establish a Research Unit of chemists, biochemists and microbiologists to investigate the mechanism of the biological fixation of nitrogen, believed to take place on the molybdenum and/or iron centres in nitrogenase. The unit was established at the University of Sussex when he was appointed to a Professorship in 1964. For his work for the A.R.C. he was made a Commander of the Most Noble Order of the British Empire (C.B.E.) in 1978. He retired in 1980.

Joseph has served on many national and international committees concerned with chemistry, amongst which are the Council of the Royal Society, the Parliamentary and Scientific Committee, and the Council and most other committees of the Chemical Society. He served as its Honorary Secretary, twice as Vice President, and in 1972 was the first President of its Dalton (Inorganic) Division. He has also been a Secretary and later the President of Section B (Chemistry) of the British Association for the Advancement of Science. In 1950, he founded the well-known series of International Conferences on Coordination Chemistry, and steered their course for 9 years. He has also served for 30 years in various capacities with the Commission on the Nomenclature of Inorganic Chemistry of I.U.P.A.C. He has been a member of the American Chemical Society since 1961.

Joseph has received innumerable awards, including the gold Chandler medal from Columbia University, New York, and the Davy Medal of the Royal Society. He has also been honoured with a number of awards from the Chemical Society, including the Tilden, Organometallic Chemistry, Liversidge and Nyholm Lectureships and Medals, and by other national societies, including the American Chemical Society (Distinguished Service in the Advancement of Inorganic Chemistry) and Soviet Academy of Sciences, Kurnakov Institute of General and Inorganic Chemistry (Chugaev Commemorative Medal and Diploma); also, the Wolf Foundation Prize for Chemistry (1981, Israel).

He has been elected to Honorary Life Membership or Fellowship of the National Science Academies of India and of Portugal, of the American Academy of Arts and Sciences, Boston, the New York Academy of Sciences, the Indian Chemical Society, and the Royal Physiographical Society of Lund, Sweden. He has also received Honorary Doctorates from the Universities of Lund, Pierre et Marie Curie (Paris), East Anglia, and Sussex.

Joseph has been a Visiting or Distinguished Visiting Professor at many Universities, including Pennsylvania State, Yale, South Carolina, Western

Ontario, Cornell (Debye Lecturer), M.I.T. (Arthur D. Little), New South Wales (Dwyer Medallist and Lecturer), Chicago (Julius Steiglitz Lecturer), Southern California (John Stauffer Lecturer), Rajasthan in Jaipur (Royal Society Leverhulme Visiting Professorship), Lund (Sunner Memorial Lecturer), and many others, as well as plenary lecturer at numerous international conferences.

During his career, he has published some 400 papers. His early hobbies were cycling, fell walking, mineral collecting, and gardening, but later travel, numismatics and art.

A CELEBRATION OF INORGANIC LIVES

INTERVIEW OF JOSEPH CHATT
(University of Sussex)

G.J. LEIGH

G.J.L. Shall we begin by asking you to give us some idea of how you got interested in chemistry?

One has to dig back rather far into one's memory for this. I came from a farming family and neither my father nor mother had any real knowledge of science, except that it was mysterious and wonderful, producing all sorts of benefits. I knew nothing about science, neither did I know that chemistry was a part of science, but I had an uncle who was the Chief Chemist in a steel works near Newcastle-upon-Tyne and he and his wife, my mother's sister, had no children. My sister and I often went to stay with them during the summer vacations. During that time he took me into the works where I saw steel being produced, and into the laboratory where analytical work was going on. Initially I was amused by being shown how to turn 'water into wine', i.e. adding alkali to phenolphthalein and such, but I was soon in trouble for wasting standard reagents and so was turned off onto glassblowing. An assistant showed me how to blow Hero's engines from glass tube and mount them so that they spun around when water was boiled in them. I learnt why they spun and spent many happy hours making a stock of them to take home each summer. I learnt much more besides, and developed an interest in what was actually general science.

I soon got an interest in electricity and my uncle lent me a very lucid textbook, *Electricity and Magnetism* by Sylvanus Thompson I think. From this I got interested in making batteries, etc. Our farm was then far from electricity supplies and I fitted out my bedroom with an electric light using a 2 V bulb and a dichromate cell given by my uncle. I let the cathode down or up on a string running around the bedroom to the door to switch the light on or off. To keep the cell going I got involved in mixing concentrated sulphuric acid with water and potassium dichromate. All such chemicals, and even metallic sodium in small sticks, as I discovered some years later, were available from the local chemist's (druggist's) shop eight miles away. The zinc cathodes were replaced by casting them from scrap zinc sheet bought from our local ironmonger, and of course I had mercury to amalgamate them.

All this happened while I was at the village school in Welton, Cumberland, where the total number of pupils varied between 30 and 36, if I remember correctly. It was only after I had started secondary school that I discovered the nature of science. I had been involved in it all the time, but did not know it. It was not at all mysterious, but very interesting and exciting.

G.J.L. Eventually, of course, you studied as an undergraduate and a graduate in Cambridge. There is a long distance, in all respects, between the rural north of England and Cambridge. Perhaps you could tell us something about how you actually made that journey?

Before answering that question I should emphasise that my interest was in science, and physics, rather than in chemistry or just inorganic chemistry. My interest in inorganic chemistry arose in much the same way as Ron Nyholm's. He was brought up in the mineral-rich lead-mining town of Broken Hill in Australia. Our farm was near the highly mineralised area of Caldbeck Fells, so important in the development of mineralogy that, even in American collections, I have seen specimens that I recognise at once as coming from Roughton Gill near Caldbeck, Cumberland. A keen archaeologist and our French and geography master, Mr. Duff, told the class one day that the Romans had dug gold out of Roughton Gill behind our farm. Of course I went to look for it and came back with a bag of quartz smattered with patches of copper pyrites. When I turned up at the village sweet shop on my way home, someone asked me what I had been up to and I told them, displaying my 'gold'. An old man in the shop looked at it and said 'No, that's copper', but I was not convinced until I had got home and found that it did not conduct electricity.

To cut a long story short, I then started to collect minerals, and I needed to identify them. My uncle was very helpful, even doing a complete quantitative analysis of a silicate mineral for me on one occasion, and Mr. Haslam, my chemistry master, introduced me to qualitative analysis, borax bead tests, H_2S and all that. There is an enormous variety of minerals around Caldbeck Fells, ores of copper, lead, zinc, molybdenum, tungsten, arsenic, and even bismuth in tetradymite (Bi_2Te_3). It was this interest in minerals that guided me into inorganic chemistry and kept me there.

Over the years I was allowed to do all sorts of preparations, inorganic and organic, on my own in the school laboratory, something, alas, my grandchildren will never have an opportunity to do. Also, like many scientifically inclined schoolboys, I fitted out a laboratory at home. Mine was in a farm building in summer, in my bedroom in winter. I must be one of the few who has ever observed the action of sodium on aqua regia, which I tried after dark one night in the empty farm yard. A small piece of sodium produced an immediate and brilliant eruption of yellow sparks.

My sodium had an interesting end. One morning, when I was doing research in Cambridge, I saw in the *Times* that there had been an IRA attack on Silloth Dock. Silloth is a small port on the Cumberland coast. Later, I heard all about it. When I decided to disband my home laboratory I handed it all on to a younger school friend for his home laboratory. In the fullness of time he went to Manchester University and decided to disband his laboratory. He and a friend decided that the best way to get rid of the remaining sticks of sodium was to throw them into the sea, so one night after dark, when the docks were deserted, they put the sticks into a tin box, banged on the lid, made a hole or two in it, and threw it into the dock. There was a terrific bang and yellow flash which alerted all the security men. Lights appeared on the landward side of the dock and the two youths retreated seawards along the pier. The movement of lights around the docks continued until three lights started to come towards them along the pier. They could retreat no further and so they walked towards the men with the lights. When they approached, one of the men shone his light on them and recognising them at once said 'What are you two lads doing out here? Don't you know that the IRA are out. Get yourselves home'. They got home. This has not answered your question, but I tell it to straighten out a minor historical episode.

You asked how I came to go to Cambridge. I had never thought of it. My only knowledge of it was of the Oxford–Cambridge boat race, which excited most youngsters, but I never for a moment expected to get any nearer to Cambridge than that. My interest in science was so strong that it attracted the attention of my physics and chemistry masters. My maths was weak, but Mr. E.W. Burn, my maths master, took a particular interest in me and gave me private tuition. I owe all the masters, and particularly the Headmaster, Mr. Robert Sayle, and the Director of Education for Cumberland, Mr. G. Brown, an enormous debt of gratitude because they made it possible for me to get to Cambridge. I had arrived at the secondary school at age 14, rather than the normal 11 years, because my father, who was rather old and out-of-date on the education system, had thought one should go through the elementary school before starting secondary school.

In any case he expected me to continue on the farm, but he had no illusion that education was not important and entered me as a fee-paying student at The Nelson School, Wigton. However, he died within a year of the start of my secondary education. Fortunately I had already shown sufficient promise for the County Education Committee to be persuaded to give me a County Grant to continue at the school. To catch up a little with my age group, I missed out Form IV and matriculated in 2 years and 2 terms, January 1929–June 1931, instead of the usual 4 years, with the result I was often quoted as one who had taken his matriculation in 2 years.

To cut a long story short, eventually, with a little bending of the rules, because I was a month or two over age, I obtained one of the nine County Scholarships awarded each year by the County of Cumberland. However, they were geared to going to Durham or Manchester Universities and worth, I think, £120 a year, whereas the minimum recommended for Cambridge was, I believe, £220. I acquired another grant of £60 by competitive examination from a local fund and £20 from Canon Wilcox, a school governor, and so my mentors, especially Messrs. Burn and Sayle, advised that I should try to go to Cambridge because of its high reputation for science (actually for physics and biochemistry).

There were only a few weeks before the beginning of the next academic year, and my maths master decided to go to Cambridge to try to get me into St. Johns, his old college, but it had no vacancy. He tried others and finally arrived at Emmanuel College. There he met Mr. E. Welbourne and made my case to him. This was a fortunate encounter, as I learnt some seven years later when I told him the story. He replied 'Well of course we didn't have a vacancy either, but we had never had anyone from your school and we always find that the first is very good, so I decided to have you. We always lose a man, falling down an Alp or something in long vacation, and so we would have a vacancy by October'.

There was still a snag to my entry. Latin was essential for entry and my Latin and French had suffered badly from my missing the year in Form IV. A Mr. Fogden had replaced Mr. Sayle as Headmaster that summer, and he decided to give me private tuition for the few weeks before I should sit the entrance exam in Latin, usually known as 'Little Go'. I sat it and had just settled into Cambridge, even attended my first few lectures, when the result came through. I had failed the Latin-to-English translation paper by 4 marks, so back I went to school for one term in Form VI Arts to study Latin only. As a result I came out top of the form in Latin at the end-of-term examination. As my science masters said, it showed how easy Arts subjects are if you put your mind to them. I tried 'Little Go' again and passed, so I entered Emmanuel College as a by-termist in January 1935.

I tell you all this so that you can see that my going to Cambridge was a cliff-hanger in itself, and was only possible because I received a lot of help from many who have now passed on. I am also very proud that the College only a few years ago elected me to an Honorary Fellowship. This I hope answers your question. I arrived in Cambridge by a series of extraordinary chances and coincidences with much help from my school masters and the tolerance of my mother. She could well have done with me on the farm. Indeed, I helped there every vacation through my school and undergraduate days, especially during hay-time each summer.

G.J.L. Of course, once you graduated you started to do research which was not necessarily a very common thing to do in those days. I know that you have a very high opinion of F.G. Mann. I wonder whether you could give me some idea of what attracted you to working on phosphines with him?

Well, it was really my interest in inorganic chemistry arising from minerals, my school experience, and contact with my uncle. When I graduated in 1937 I had every intention of doing research in inorganic chemistry and the only man in Cambridge doing anything at all inorganic and interesting was F.G. Mann, whose lectures I had attended and whose lecturing I much admired. He was always very precise and gave one a lot of useful information. I had a DSIR Research Grant at, I think, £240 a year, which was sheer luxury after my undergraduate financial stringency. I had a look at a few interesting research projects, especially in physical chemistry, but inorganic still pulled. So I discussed with Mann what work he had going and started working on the reactions of halogen-bridged compounds of palladium, those containing two trialkyl phosphines, four halogens and two palladium atoms, particularly their bridge-splitting reactions. These he had opened up with Donald Purdie, but the rearrangements involved had not been noticed.

It soon became obvious that these systems are fairly labile and very soon we had sorted out the nature of their reactions. It was this work which led to my synthesis of the famous *o*-phenylenebis(dimethylarsine), so popularised by Ron Nyholm. Then I went on to the stereochemistry of organo-arsenic compounds by classical methods. This was a hangover from an aspect of Sir William Jackson Pope's work. Pope was Head of Department and officially my research supervisor, but he was then an old and ailing man, rarely seen. F.G. Mann had been his assistant and I started my Ph.D. work in the laboratory where Pope had led a team of chemists in the study of mustard gas and arsenical war gases during World War I.

It was Mann's involvement in that work which got him eventually interested in arsine and phosphine complexes. That old laboratory of Pope's thus had very efficient fume cupboards and was excellent for the preparation of trialkylphosphines and trialkylarsines by the sporting methods then available. The laboratory was rather dark, but every bench had its own gas light, as throughout the building; we students joked about the Professor having shares in the gas company. The lights were quite a fire hazard but they did impress careful working. It is interesting that the war work of Pope's was also, as recorded in Ron Nyholm's Royal Society Biographical Memoir, the more distant source of his interest in arsine complexes.

My long experience of experimental chemistry stood me in good stead during my Ph.D. work and I rolled out primary publications, which F.G.

described as substantial, at a steady rate of two a year, but all of that is published and I need say no more.

You mentioned the phosphines particularly, but these were incidental. F.G. used them initially to provide metal complexes of such low melting points that their parachors (you know, Sugden's parachor) could be measured, in the hope of providing a clue to the nature of the coordinate bond; was it double, Lowry's semipolar double bond, or single? Mann and Purdie had found that the parachor was useless for such diagnoses.

Nevertheless it opened my eyes to the study of metal complexes in non-aqueous solution, which has proved to be so important in catalysis studies since World War II, whereas Mann, becoming disillusioned after a few years by the lack of general interest in complex chemistry, turned his main attention to the study of heterocyclic compounds of phosphorus, arsenic, and such elements. Perhaps I should say that I enjoyed my research, as I had my undergraduate days, and my intention was to take up University teaching, particularly to continue fundamental research. I arranged to spend a period in St. Andrews University after my Ph.D. to teach heterocyclic chemistry, but that was cancelled owing to the outbreak of the Second World War and I found myself solving a 'war problem'.

It had been noted that *m*-dinitrobenzene was a stronger explosive than would be expected from its constitution and, I understand, Sir Robert Robinson had suggested that 1,3,5,7-tetranitronaphthalene might be an exceptional explosive. Someone was sought to prepare 200 g of this material for testing and it fell to me under the supervision of Professor W.P. Wynne, then retired and working in the Cambridge laboratory. If I remember correctly, there are 24 tetranitronaphthalenes and the required isomer was one of the 4 then unknown. You will realise that it took rather a lot of making because it had to be prepared indirectly, and I spent some eight months working out a synthesis and proving that I had the desired isomer. By the time of the Dunkerque evacuation I had obtained only 100 g. Then I was swept into the Ministry of Supply and directed into the Research Department of Woolwich Arsenal, part of which had been evacuated to Swansea.

So I went to work in Swansea and there they had the old University College chemistry laboratories packed with chemists. I was given a window sill to work on, that being the only space that was left, and told to read round the subject, during which I went to the library and read up quite a lot about explosives and of some hair-raising experiences from the past, such as the disastrous end of someone who stuck a punt pole through a pool of nitroglycerine which had leaked from an explosives factory and collected in a hollow in the bottom of a river. Having finished reading around and being still not very sure what was wanted, I decided to read Chemical Reviews and saw R.N. Keller's review on olefin complexes.

F.G. Mann had mentioned olefin complexes in his undergraduate lectures and had said that it was thought that the π-electrons of the double bond of the olefin served as did the lone pair on a normal donor atom. I thought this would be a very useful project for the future and started to read up about olefin compounds, because it seemed to me to be the only ligating substance which did not have a lone pair of electrons. However, at that time one couldn't do any work of this kind.

As they hadn't got room for me in Swansea, after about a month I was posted to Woolwich Arsenal itself, where I learnt that my 100 g of material had been detonated against an iron plate and found to have no exceptional explosive quality. There was a nice new laboratory there, almost empty because everybody had been dispersed to reduce their exposure to danger from German bombers, etc. I got digs (accommodation) in Blackheath, but Blackheath was in a very poor state. In fact many houses close to my digs had already been demolished by bombing. Perhaps it was just as well I didn't stay there too long. Anyway, my main project at Woolwich was now to find how to produce a specified substitute for glycerine from which to manufacture a nitroglycerine substitute, in case overseas supplies of glycerine were interrupted. I thought I'd found the perfect solution. Indeed when I explained it to a friend, a chemist in industry, he thought that it would make a perfect continuous process and after my lifetime's experience I also think that it would have, but the man in charge of me, who was based in Swansea, wasn't the slightest bit impressed. He moved me onto something else which was really a minor problem left over from World War I. At this point I decided to move.

I read through all the legal jargon of the Essential Work Order, which controlled the work of people involved in essential industry, because most men were called up (drafted) or tied to their jobs during the war. I filled in the appropriate form, and sent it off to my boss in Swansea. He returned it to me, saying that if I'd give another reason for my leaving (I'd been rather critical of him) he wouldn't object, so I sent a rather mild reason for leaving. It went all round the chain till it finally arrived back in my laboratory in Woolwich where, by sheer good luck, I saw it. It was amazing how the estimation of one deteriorated as it went along the chain. First I was definitely a bad egg, and I got a little worse when the form got to the Shrewsbury headquarters. Finally it arrived at the Ministry of Supply at Horseferry Road, London. They looked at it and wrote on it 'As a temporary civil servant the Essential Work Order does not apply to Dr. Chatt. If he wishes to leave he should resign in the normal manner'. So I resigned in the normal manner that very day. They did say that they had no objection to my dating my resignation with the date on the original form. When my month's notice was up on the following Saturday, I handed my tin hat into the stores, and shook the dust of Woolwich Arsenal from my feet. Before I left, a clerk in the office rang me up and said 'I hear you're leaving?'. I said 'Yes, has the news got around now?'. He said, 'No it hasn't, I wouldn't have known if you hadn't handed in your tin hat, and it is my job to stop your pay', so perhaps if I hadn't had a tin hat they would still have been paying me.

I then went to Peter Spence & Sons Ltd., Widnes, as Deputy Chief Chemist, where I got involved in trying to improve the efficiency of the electrolytic reduction of titanium tetrachloride in water to give aqueous titanium trichloride. I was also put onto testing activated alumina, which was not a very common reagent then because the solid dried cake didn't have the strength to hold its own weight in the drying towers used on an industrial scale. The chemists in Peter Spence & Sons had discovered how to improve the strength and I was given the job of testing it. I spent a lot of my time first on activated alumina and then on reactions catalysed by activated alumina, such as methylating naphthalene with methanol, to produce α-methylnaphthalene, which was needed for some pharmaceutical purpose, and I was able to do this quite well.

The most important event at this time was my meeting Ethel Williams, my future wife, who has been a constant source of inspiration and support. We were married as soon as I found settled employment after the war, but immediately after the Second World War, although I had now become Peter Spence's Chief Chemist, I still hankered after pure research, preferably in a University laboratory. I applied for an ICI Fellowship at Leeds but Leeds decided to defer all appointments for a year. I then decided that I would like to go to London, so I applied to Imperial College and was taken on there by the Professor of Inorganic Chemistry, H.V.A. Briscoe. However, I had become used to an industrial environment where, even when an electric motor gave a hiccup, it had to be reported at once and put right immediately because no plant could be allowed to stop working. I found Imperial College was quite a different place. Their maintenance people could never even look at a motor until it had stopped, after which a botched repair would take three weeks or more.

In the course of 9 months there I got perhaps 2 months work out of my fume cupboard. I got very frustrated because I needed it continually to recover platinum from residues to convert into K_2PtCl_4 and on to Zeise's salt. It was then I met Professor Peacock, an old friend from Cambridge, at a reception at the Royal Society and, when he asked me how I was getting on, I told him of my frustration. He said 'If you are thinking of going back into industry the man you should talk to is over there; he is R.M. Winter, the Controller of Research in ICI'. Winter told me that if I wanted to come back into industry when I had finished my ICI Fellowship, I should write to him.

Now the next day, when I went into the laboratory, I enquired about a potentiometer which I had ordered for my work. I knew from my industrial experience that this would take about three months to come and the three months were up. Things were very difficult to get during and after the war. I remember sending an assistant from Peter Spence's down to London to sit on the doorstep of the supplier of gas meters, and telling him not to go until they gave it to him. He got one. Things were difficult, but the potentiometer should have been there in three months. However, I was always told that it had not been delivered. Fortunately, I learnt through the husband of a secretary in the laboratory that it hadn't even been ordered. They were so short of money they couldn't afford it. I felt that if they couldn't even tell me that they couldn't order it, there wasn't much point in my waiting for it. That night I wrote to R.M. Winter, he interviewed me, and I was given a place in their Butterwick Research Laboratories, a new fundamental research establishment, which had just been opened at The Frythe near Welwyn village in Hertfordshire. It was there that I did most of the work for which I am known.

When I got there I started immediately to look into the chemistry of the olefin compounds. Now I should say at this stage that I often went into the laboratory at Peter Spence & Sons on a Saturday and with permission had dissolved some old platinum crucible lids to get ammonium tetrachloroplatinate and had done a little bit of work simply making Zeise's salt, etc. to get the feel of things, but of course I hadn't done very much. I'd done a very thorough literature search using the Technical College library in Widnes, and with volumes from the Chemical Society library available to me through the post I'd been able to get all the papers that I wanted. I had prepared a very good card index of the whole of olefin complex chemistry, helped enormously by R.N. Keller's review.

One thing which struck me about Keller's review was that he found it difficult to comprehend A.D. Hel'man's view that the metal-to-olefin bond was a 4-electron bond involving two electrons from the *d*-orbitals of the metal as well as the π-electrons of the olefin. Neither was he very sympathetic to the view that the π-electrons of the olefin served the same function as the lone pairs on normal ligand donor atoms, which I had been brought up to believe. That idea does not appear to have appealed to many chemists, especially in the United States, until after the discovery of ferrocene.

G.J.L. Were you working entirely alone on this problem at this stage?

Yes, initially, I was working entirely alone. I was taken on by ICI, and sent to The Frythe where the acting manager, Mr. M.T. Sampson, was determined to place me in the organic chemistry department, whereas I had been told I would have a separate department. It was an uncomfortable position, as the head of the organic department did not want me there. Eventually it was sorted out and I became head of a new inorganic department.

First, I thought it was very important to clear up the question of whether the π-electrons could function like the lone pairs of normal ligand donor atoms. Now it was well known that donor molecules condensed with trimethylboron at low temperatures to give complexes and that they didn't require any activation energy for their formation. It seemed obvious to me I should condense ethylene onto trimethylboron and take a freezing point curve to see whether a one-to-one complex was formed. I had, in fact, built all the glass vacuum equipment and apparatus to do this at Imperial College and had to leave it there. Well, to cut a long story short, I built this again and eventually I got my potentiometer which took quite a long time, even in ICI. I discovered that there was absolutely no sign of any interaction between trimethylboron and ethylene.

A.D. Hel'man in Moscow had interpreted her proposed notion of the bonding to indicate that the platinum olefin complexes were really complexes of platinum(IV), but most of their general properties indicated that they were of platinum(II). Nevertheless, Hel'man produced redox evidence to suggest that the outer shell *d*-electrons were somehow involved in bonding to the olefin and, in my view, the absence of interaction between trimethylboron and ethylene at their freezing points lent credence to that view.

G.J.L. So eventually you started to work on complexes with transition metals, but during this period you also gathered together various people who have become well known in their own right, such as Bernard Shaw, Luigi Venanzi, and David Adams. Perhaps you could tell us a little bit about how they came to you?

There is a long time gap between my investigations of the possible trimethylboron complexes and my being joined by the people you mention. When I started to investigate the platinum(II) olefin complexes I had only one assistant, Alan Williams, who had joined me from Peter Spence & Sons because he wished to take a part-time London degree by working at Birkbeck College. At this point I realised that I had to get something published and the olefin work was going to take much too long, so I decided to make an investigation of the tertiary phosphine, halogen-bridged complexes of platinum which were then unknown. Ralph Wilkins also joined me at about this time and increased enormously the rate of development of this early work.

The platinum-bridged complexes could not be prepared in boiling alcohol in the simple manner of their palladium analogues, but using considerably higher temperatures, first by fusing the bis(trialkylphosphine)platinum(II) chloride complexes with solid platinum(II) chloride, and later by use of very high boiling solvents, I got the bridged compounds in very nice orange crystals. Luigi Venanzi joined me about this time and the work started to become more of a team effort. We started to split the platinum-bridged compounds with various potential ligands and to do the work which is the subject of your question.

Also, I had been preparing the [$PtCl_2$(amine)(olefin)] compounds, which have a *trans*-configuration, and decided that it would be useful to see whether the coordinate bond from the olefin balanced that from the amine, which would require measuring dipole moments. If the *d*-electrons of the metal were involved in one bond but not the other, there would probably be marked electrical asymmetry across the molecule. Dr. Leslie Sutton in Oxford had measured a lot of dipole moments and I got in touch with him. He gave me the details of the equipment he used, none of which was available commercially, but ICI had a good instrument department at the Butterwick Laboratories and they produced some for me. I found that the mixed *trans*-compounds with amines opposed to ethylene had an appreciable dipole moment with the amine the more positive, apparent confirmation of some electrically more symmetrical system of bonding to the olefin than to the amine.

Just before the war, Pauling's book on chemical bonding had appeared, but in fact I hadn't studied it until now. There he describes his double-bonding idea for the carbonyl-to-metal bonds. Also, Ron Nyholm had just arrived in University College and got in touch with me so we were able to discuss this problem. He was of the opinion, since he had worked mainly with arsine complexes and noted their similarity to the carbonyl complexes, that the *d*-electrons of the arsenic formed a double bond by overlap with the vacant *p*-orbital of the platinum. As I pointed out, the arsine complexes were so like the phosphine complexes that if there was any such double bonding it had to be by donating *d*-electrons from the platinum back into the *d*-orbitals of the phosphine or arsine.

We agreed to work separately along that idea, and it was this that led to my study of phosphorus trifluoride as a ligand. The electronegative fluorine would be expected to weaken the normal donor properties of the phosphorus and to enhance the back-donation of electrons from the metal, so conferring on PF_3 coordinating qualities even closer to those of CO, as I was able to show. The idea of double bonding to the olefins arose from that, but it was very difficult to prove, especially as then infrared spectroscopy had not entered general use.

G.J.L. Of course, the currently accepted model for metal-to-olefin bonding is normally termed nowadays the Chatt–Dewar model. I wonder if you could say a little bit about that aspect?

Well, finally determined to get a double bond between the carbon and the platinum, as in Pauling's picture of the metal–carbonyl bond, I came forward with the ethylidene structure which was published as a note in *Research*. About that time I was asked to read a paper to the Chemical Society in London. I decided to discuss that particular structure with the intention to submit a paper on it later. However, at that meeting Professor C.K. Ingold was able to convince me to have another look at the proposed structure and I never submitted the paper.

Of course, I had discussed my ideas on the olefin–platinum bond in many places before this and it was after the reading of the Chemical Society paper that Michael Dewar, who was not at the paper reading, visited me and we discussed the problem again. He asked me whether I had considered the antibonding orbitals of the olefin to receive the *d*-electrons in the metal, and pointed out his publication in *Comptes Rendues* on the silver ion–olefin combination, which I hadn't seen. It was easy to adapt that to the olefin platinum complex structure using *dp*-hybridization of the bonds on the platinum, so giving the structure you enquired about. The problem was to prove that this type of structure was correct.

Fortunately, at this point our laboratory had acquired its first infrared spectrometer for the use of the organic chemists and a bright young man fresh from University College, London, to operate it, Dr. L. Andrew Duncanson (Dunc.). He suggested that perhaps, if I could make the propylene complex, we might see the $C=C$ stretch. We did, and the whole story with all supporting evidence was finally published. This was the first time that any structure for the olefin compounds had been suggested and reasonably proven. Of course there were many others, all suggestions, as listed in R.N. Keller's review and in a review by A.D. Hel'man, none proven any more than had been Dewar's proposed silver ion–olefin structure.

G.J.L. The problem of olefin complexes was only one part of a big development of organometallic chemistry generally with alkyls and aryls and hydrides. Perhaps you could tell us a little bit of how that work actually started?

Well, again it has roots well back. A.D. Hel'man in Moscow had claimed that the platinum in the olefin complex was tetravalent, and apart from the redox evidence she quoted the production of trimethylplatinum(IV) iodide by treatment of the olefin complex with methylmagnesium iodide. Of course I repeated this experiment, and even from Na_2PtCl_4 I obtained trimethylplatinum iodide. She had used an enormous excess of methyl iodide, so I guessed that she had obtained dimethylplatinum which had a sufficiently long life to add the methyl iodide, a process which many years later was dubbed oxidative addition.

It seemed to me that perhaps with trialkylphosphines attached to the platinum(II) chloride I might get stable phosphine platinum(II) alkyl complexes and indeed I obtained a methylplatinum complex. I then handed this problem over to Dr. M.E. Foss in Gloucester because I was too busy working on the olefin compounds and I was still working essentially on a one-man-and-his-boy level. Foss did get some stable methyl derivatives but got no higher homologues and was too busy to continue. Thus the whole problem lay fallow for some years until we'd finished the investigation of the olefin compounds and derived topics such as the *trans*-effect and relative affinities of ligand atoms for metal ions, in which I was joined by Sten Ahrland and Norman Davies.

Bernard Shaw had then joined me and during a lull in his work on hydride complexes I thought that he might as well finish Foss's work. I told him that it was a nice little project he could finish off fairly quickly; there didn't seem to be much in it, but to tidy it up. Well, in his hands it grew and grew, so that before long we had organo-complexes of platinum, nickel, cobalt, and iron. Just after finishing making the platinum compounds, I learnt that Geoff Coates in Durham had prepared some palladium analogues and so we passed over palladium.

The hydride complexes were a completely separate issue and an accidental discovery, a real example of serendipity. They arose from attempts to obtain substituted cyclobutadiene complexes of platinum(0) well before any cyclobutadiene complexes were known, although they had been predicted by Leslie Orgel, with whom I was in close contact. Lamberto Malatesta had noted that the reduction of triphenylphosphine complexes of platinum(II) chloride with hydrazine in the presence of triphenylphosphine, in alcohol I think, led to complexes of platinum(0). It seemed likely that if the free triphenylphosphine was replaced by an electronegative acetylene, such as diphenylacetylene, a complex of platinum in the zero oxidation state and containing the two triphenylphosphine ligands and two molecules of the acetylene might result. The latter we hoped would be present as a tetraphenylcyclobutadiene ligand. When I asked George Rowe, who had recently joined me, to try this he isolated complexes of the type $[Pt(PPh_3)_2(R_2C_2)]$. Bernard Shaw isolated the corresponding, but much less stable, olefin complexes.

However, when I asked Bernard to try to get the triethylphosphine analogues of the acetylene complexes, he obtained a completely different material. It was very difficult to isolate from the alcohol solution. Because known metal(0) complexes tended to be volatile, the oily product was finally sublimed in high vacuum onto a cold finger. It provided colourless crystals of a low melting solid which analysed as $Pt(PEt_3)_2Cl$ and was monomeric. We could not believe that it contained platinum(I) because it was colourless, nor that it was a hydride, because it was so stable even to air and dilute acid. Also, we had been taught that transition metals never formed stable molecular hydrides, but Duncanson suggested he should look at the infrared. There, sure enough, was a sharp strong absorption which could only be from the Pt–H vibration. The substance was *trans*-$[Pt(PEt_3)_2HCl]$ and its preparation in the absence of alcohol by warming a solution of dichlorobis(triethylphosphine)platinum in dilute aqueous hydrazine immediately gave us a precipitate of the solid hydride complex.

It was the first stable transition metal hydride which did not contain carbon as a ligand atom. Of course, some carbonyl hydride complexes had been known and their structures argued about since the 1930s. A few cyclopentadienyl metal hydride complexes had also recently appeared, but this compound was unique in its ease of preparation in aqueous solution and its unexpectedly high stability. As Philip Owston had joined me to establish a small X-ray crystallographic structure group, we were eventually able to show that the hydride ion apparently behaved as a normal monoatomic ligand. From the time of the discovery of the hydride, my group of coworkers started to grow and it would be difficult to quote all of the newcomers. They became a strong and productive group. We started to prepare phosphine-stabilised metal hydrides by the obvious method of reducing phosphine chloro-complexes with lithium aluminium hydride, and extended our study of organo-transition metal complexes containing small organic ligands. Indeed, the only part of this work never to be written up was the descriptions of the compounds of various acetylenes of type $[Pd(PPh_3)_2(acetylene)]$. George Rowe prepared many of these, but when I left ICI in 1962 it had not been recorded and eventually became outdated. I handed a bit of it on to Bert Allen, of sad memory, in Toronto and I have watched little bits of it appear independently in other places over the years, but so far as I know, not yet all of it.

G.J.L. After all these academic successes it came as a big surprise to the world at large when the Frythe laboratory was closed down. Perhaps you could tell us a little bit about that?

Well, there had always been dissent from the ICI Divisions to the establishment of a research laboratory by Head Office at The Frythe. At the time of the closure, the Company's main profits had been coming from petrochemicals and there was then a decided downturn in that market. It was necessary to think of economies and the Frythe was one of the first casualties. It was decided to disperse the staff at the Frythe throughout the Divisions. Many of my people left for academic work, and I had been invited to a Visiting Professorship at Yale University. Within the Company I was to be transferred to the new Petrochemical and Polymer Laboratory of ICI to be established in Runcorn but vibes within the Company caused me to think that its life would be even shorter than that of the laboratory at The Frythe.

However, before I left to go to Yale, rumour had got around that I was about to emigrate. At that time Professor G.E. Cox, now Sir Gordon Cox, the then Secretary of the Agricultural Research Council, was looking for a man to establish a Research Unit to study biological nitrogen fixation, which, it was believed, involved reactions at metal centres in nitrogenase. Sir Gordon was a chemist and X-ray crystallographer at Leeds before he became Secretary to the Agricultural Research Council. To have a chemist as its head is, I think, possibly unique in the history of the Agricultural Research Council (ARC), because most people involved in its research are biologists. He approached me to start this new, and to me exciting, project. Thus I was appointed to be in charge of the new ARC Unit, with the intention of trying to adapt the chemistry of the biological reaction to the manufacture of ammonia by some less extravagant method than the Haber process, which uses high pressures of nitrogen and very expensive hydrogen. I had accepted his offer before I went to Yale, where I had a very pleasant term in that exciting University.

G.J.L. It must have been a considerable challenge for an inorganic chemist to set up what was essentially a biological research unit. Can you tell us something about how you went about collecting staff and setting up the organisation?

It was intended that I should have an expert microbiologist as my second-in-charge, and indeed one was nominated, but I wished to establish the unit in London and he wished to stay where he was. Neither I nor Cox was happy with this idea. We felt that the chemists and biologists should work together in the same building, and as you know it has turned out to be a very fruitful interaction. Gordon Cox therefore decided to look around for another Assistant Director for me and had heard of John Postgate, who was then just about to return from a period in the United States. It was arranged that I should interview him at the Abbey Hotel in New York. He was passing through on his way back to England and I was on my way to Yale. We took to each other at once, and thus began the very happy collaboration over many years in which we have both built up the Unit of Nitrogen

Fixation to become one of the main international centres, perhaps the main international centre, of interdisciplinary research in this area. Only the future will judge.

G.J.L. After one or two uncertainties, the Unit of Nitrogen Fixation finally became established at the University of Sussex. It would be interesting to know what you consider to have been the highlights of the 25 years or so of work at the Unit of Nitrogen Fixation.

Well our first effort was to obtain complexes of rhenium, molybdenum, tungsten, all related metals, which we knew could form stable nitride complexes and thus might give us some indication as to how the molybdenum reacted to convert molecular nitrogen into ammonia, if it was indeed the element which interacted with molecular nitrogen in nitrogenase. When Allen and Senoff discovered the first characterised complex of molecular nitrogen in 1965, our efforts were turned very much to obtaining molecular nitrogen complexes of molybdenum and its near analogues, but of course one had to get into the complex chemistry of those elements because, particularly so far as molybdenum was concerned, there was no useful complex chemistry.

My experience had largely been with the phosphine complexes of platinum and during my period with ICI I had worked slowly backwards across the Periodic Table towards Group VI. Getting into that Group itself was not very easy at that time because the affinity of such metals as tungsten and molybdenum for oxygen is high and our aim was to get it into combination with phosphines and chlorine. We approached it, however, from the platinum metal side and very soon found that we could obtain molecular nitrogen complexes of both osmium and rhenium, but neither of these series reacted with reducing agents or acids to produce ammonia. However, by steadily chipping away at the molybdenum and tungsten complex chemistry we eventually obtained compounds of those metals in the zero oxidation state containing the equivalent of four tertiary phosphine molecules and two molecules of molecular nitrogen. Then finally, and for the first time, we were able to get ammonia in a well-defined manner and in high yield by the reaction of ligating molecular nitrogen with acids.

Also, and it was a prior discovery in my laboratory, we were able to add organic acid chlorides such as acetyl chloride across the molecular nitrogen complex, chlorine on the metal and acyl group on the molecular nitrogen, to give acetyl diazenido-complexes. I think that the reduction of molecular nitrogen in a well-controlled manner, and the fact that we were able to work out the whole sequence of reactions leading to ammonia or organonitrogen derivatives from the molecular nitrogen complexes of molybdenum(0) and tungsten(0) was one of the highlights of the work. This demonstrated a perfectly feasible mechanism for the conversion of molecular nitrogen to ammonia on a molybdenum site, say, in nitrogenase. Of course this work involved us in developing the whole of the halophosphine complex chemistry of the heavier metals of Groups VI and VII and particularly, of course, the investigation of nitride complexes as well as those of molecular nitrogen. All of this was new.

I think the other very important aspect of the Unit's work was a long and systematic investigation of the kinetics of the reactions of nitrogenase and, as knowledge of gene transfer between microorganisms became available, the transfer of the nitrogen-fixing genes from *Klebsiella pneumoniae* into *Escherichia coli* by the biologists in my Unit and their small genetics team. As a result, John Postgate's section of the Unit had *E. coli* fixing its own nitrogen and retaining the ability to fix nitrogen on division even some years before I retired from the Unit as its Director in 1980.

I would like to add that when the Unit started in 1962, the chemical and biological programmes were essentially distinct, but by getting chemists, biochemists, and eventually geneticists all working together in the same building and in close collaboration and discussion, the chemical and biological programmes grew together and interacted, one helping the other, to give the Unit the enormous prestige which it developed and for which I can scarcely claim the credit. All of my staff cooperated and worked together with a single aim of improving our understanding of the process of nitrogen fixation in nature and how this might be applied eventually to agriculture.

During the course of those years the aim has changed. We started by looking for a new less energy-intensive chemical process of nitrogen fixation based on the mechanism of the natural process, but we now know that the natural process is also energy-intensive. The emphasis has shifted towards developing nitrogen-fixing bacteria symbiotic with the main food crops such as wheat and rice or, in the extreme, modifying those plants to fix their own nitrogen and, of course, the energy would then come from the sun. Another strong factor in favour of such development is the fact that nitrogen-fixing organisms only fix nitrogen when they must. If ammonia is available to them, they do not fix more. The genes determining nitrogen fixation and its control must all be transferred into any biologically engineered organism.

G.J.L. When you were still at The Frythe you arranged a meeting of coordination chemists which was to have international consequences. Would you care to tell us a little bit about that?

Yes, the meeting you mention was to become the first of what are now known as the International Conferences on Coordination Chemistry

(I.C.C.C.) which have been run by Stan Kirschner for many years. I described their beginnings at the 25th anniversary meeting of the I.C.C.C. in Hamburg and they are published in its Proceedings, so I can be very brief. During the 1930s, there was very little interest in coordination chemistry or in inorganic chemistry generally. After the war there was even less. Around 1948 or 1949, I was grumbling to Mr. M.T. Sampson, the manager of the ICI laboratory where I worked, that there were never any meetings of The Chemical Society in London devoted to coordination chemistry. He suggested that I should call a meeting myself, and that the laboratory would finance it. So with Ralph Wilkins we got down to the literature and issued an invitation to all those resident in the United Kingdom who had published anything on coordination chemistry since about 1930. It was quite a small number, about 30 I think. I had also got to know K.A. Jensen in Copenhagen personally, and Gerold Schwarzenbach was visiting the U.K. from Zurich at the proposed time of the 'discussion', so we invited them. It was a small meeting of single sessions lasting only 2 days held during September 1950. Its size appealed to K.A. Jensen who said 'Ah, the Danish Chemical Society is very small but it could manage a meeting of this size' and he undertook to call another discussion meeting in 2 years time. In fact it took 3 years to find the money, so that in 1953 the Danes called participants to a 'Discussion on Coordination Chemistry' in Copenhagen. It attracted about 60 non-Danish participants. It was the first conference that I had ever attended overseas and I must say that I found it extremely stimulating.

Now it was the Dutch who noticed that this was a small conference and with their small Chemical Society they thought they could manage another in 2 years time, which they did. So the meeting now entitled the 'International Conference on Coordination Compounds' moved to Holland in the spring of 1955. The Italians picked it up from Holland (1957) and we wanted it back again in London in 1959. The Rome meeting was much larger and there were many bids for future conferences. The thing had grown much too big and required some sort of organisation. It was agreed there that the General Secretaryship should 'rotate around the nationalities', the meeting should come to London in 1959 where we had about 750 active participants, and then to the United States in 1961. The local organisation of the latter was carried out by Stan Kirschner in Detroit. From this point Stan Kirschner became General Secretary and eventually Permanent Secretary of the I.C.C.C., the position which he has just abandoned to Jan Reedijk this year. I am now looking forward to attending the half-centennial meeting in London in the year 2000.

G.J.L. One of the great early generalisations on coordination chemistry concerned the division of metal ions into the classes a and b. This has had very *important consequences for the development of our chemical understanding and it would be interesting to know how you got into this area.*

The idea behind this really arose from the first I.C.C.C., the one at The Frythe in 1950. There, Harry Irving gave a paper on the relative stabilities of complexes and the Irving–Williams series. This, of course, applied only to the oxygen–nitrogen donor ligands and was based on stability constants measured in aqueous solution. Qualitatively, it was obvious that it did not apply to the organic phosphines, sulphides, etc., the sorts of ligand that I had been working with in combination with the heavier of the later transition metals. To put it beyond all doubt, I wished to get comparable stability constants of my sorts of complexes.

In order to measure these it was necessary to prepare some suitable and analogous water-soluble phosphines, arsines, sulphides, etc., and to measure their stability constants with a later transition metal ion from the second or third long Period. I chose the silver ion for its stability and its ease of measurement, but I had had no experience of measuring stability constants. However, on a visit to Sweden and finding that Sten Ahrland had a similar interest and had a sabbatical coming up, we agreed to get together on this. I found ICI agreeable and the whole idea was sealed, if my memory serves me right, at the second I.C.C.C. in Copenhagen. He spent a year with me at The Frythe.

With Alan Williams, I started to prepare the various suitable and analogous water-soluble phosphines, etc., and Sten had already measured the stability constants of silver ion with the halide ions and started to measure those with our ligands. Norman Davies later joined from Australia to complete the series of measurements and extend the investigation to the cadmium ion. It took two to three years to collect all the data. Although the complexes with silver ion formed by ligands attached through donor atoms from the second and subsequent rows of the Periodic Table followed different patterns within each Group, in every case the second donor atom, phosphorus, sulphur and chlorine, formed very much more stable complexes than the first. The outcome was the exposition of the 'a' and 'b' classification of metal acceptors, as set out in our *Quarterly Review* which became a *Citation Classic* many years ago.

That very short review was prepared as an introduction to the papers containing the measurements and submitted to the *Journal of the Chemical Society*, hence its condensed style. The referee thought it unsuitable for the journal and suggested an article for *Quarterly Reviews*, but I wanted my time for other things so I submitted it as it was. Were it that all non-comprehensive reviews were as concise as that one! The 'a' and 'b' classification is so well known, even if it has been overlaid by 'hard and soft', that I need say nothing more about it, except to point out a subtle difference. 'a' is not the opposite of 'b' and it is somewhat specific in its application; 'hard' is the opposite of 'soft', and developed to become all-embracing. I wonder whether this provides the explanation of a statement concerning 'hard and soft' ascribed to Ralph Pearson as it was retold to me, 'Some say the idea is no good. Some say that it is Chatt's anyway. But I never hear anyone saying both things together'.

G.J.L. Looking back over your life in science, do you have any remarks to make about the way science has been developing and about your role in the development of inorganic chemistry?

During my working life, science has changed and the attitude of the general public to science, especially to chemistry, has changed dramatically. I have now been retired for 10 years and I think it was the right time, though I didn't really choose it, to retire. When I was a research student in Cambridge, money for science was extremely scarce. I remember it was a great triumph even to get £10 for a piece of equipment. After the war, under the influence of such things as excess profits tax and the euphoria of what science had done in the development of weapons and other instruments of war, the governments started to look with great favour on the development of science, and money became much more readily available. Many large international companies started to put money into fundamental science and building fundamental science laboratories such as that I joined at ICI.

With passage of time, industry and the government became more parsimonious, until now scientists spend almost as much, if not more, time foraging for the money to support their work than they do in creative science. The legislators talk of relevance, but the most important discoveries of science were rarely planned. They are real discoveries, just as unexpected as Christopher Columbus's discovery of a new continent, and like him the scientist who discovers it may never know the value or extent of what he has found. A prime example is Kipping's discovery of the silicones. I am sure that I have lived through some of the best years for science, and especially for inorganic chemistry, because as I said, it was held in very low esteem during the 1930s and even when I was working on olefin complexes they were regarded by the very few chemists who knew of their existence as being of very little interest to anyone. The growth of the petrochemicals industry changed all of that and the development of catalysts, which of course usually contain metallic active centres, led to a vast development and interest in organometallic and inorganic chemistry. Now, of course, we are beginning to see the great importance of metals in enzymic systems, but it is in the more complex enzymes that the metals serve some of their more specific functions. Undoubtedly the development and understanding of metallo-enzymes is one of the directions in which inorganic chemistry is going to go, or should we now call it bioinorganic chemistry? It makes no difference; chemistry is all one with some physics and biology thrown in.

What about my own contribution? I think I played a great part in keeping inorganic and organometallic chemistry alive whilst it was in the doldrums. My subject, as it happened, was one which became of very great importance in the developing petrochemical industry but I wasn't to know that when I started and hadn't even thought of such. Even ICI's Billingham Division, which was then developing petrochemicals, showed very little interest in it at the beginning. Indeed, the ICI Billingham Division's Director of Research told me after I had got going on my work into olefin complexes, 'There is nothing you can do down there (meaning in Welwyn) that would interest us up here'. I may say that I found a much greater interest in my work in other Divisions of ICI and in other companies, especially in the United States. As you know, nearly all my work in ICI was published in primary journals and my contributions to my subject are, I think, well known. I deplore the idea of listing firsts. I see some of mine listed by others quoting later references than mine and I have no doubt that it may happen the other way around. I shall leave all that to posterity, if it matters. The awards and honours that it has been my pleasure to receive from national and international organisations in scientifically highly developed parts of the world are evidence that my efforts have been appreciated and not in vain.

G.J.L. Would you like to elaborate on the awards and honours?

Not really. They are all mentioned in *Who's Who*, and it would be invidious of me to say which I prized the most, but there is no doubt about the most valuable. It was the 1981 Wolf Prize in Chemistry, the first to be awarded to someone resident outside of North America and, to the best of my knowledge, the most valuable chemistry prize to any individual in that year.

There is a final point I would like to make. It concerns the present atmosphere of 'relevance' of research to national or commercial needs. My work on the olefin complexes would never have been launched if it had been subject to peer review in that context. Even after I had got going, an older and highly respected member of our profession, who later became a Nobel Laureate, told me in the nicest possible way that it was of 'little interest to anybody'. Yet you see how wrong he was on that score. It provided background knowledge valuable to the developing petrochemicals industry, pure serendipity. My second example relates to my work in Sussex University where I had two small chemical research groups, one in my Unit of Nitrogen Fixation and the other in the University Chemical Laboratory. From industrial experience I felt I would keep my paymasters happier if I tended to have the group in the Unit working in areas of more obvious interest to nitrogen fixation, and to put those of more peripheral relevance into the University group. Nevertheless, again and again, the real initial breakthrough tended to come from the work of the less circumscribed University group. Its development was, nevertheless, better taken at the appropriate time into the more experienced Unit group. The moral of this is that, if you want really good new science, choose a dedicated scientist and let him get on with it. Do not keep pestering him for reports and programmes. I was lucky. When I joined ICI and later the Unit, I was each time allowed a slack rein to range where I would, within a problem of personal interest to me and to my paymasters.