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A celebration of inorganic lives
Interview with Erwin Weiss

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Fig. 1. Erwin Weiss.

Erwin Weiss[1] (Fig. 1) is Emeritus Professor at the University of Hamburg, Germany. He was born in Arzberg/Oberfranken in 1926. During his Secondary School (1937–1944), he had to serve as Luftwaffenhelfer and in the Labour Service, followed by a short military service. At the end of the war, he was taken as a prisoner until the end of 1947.

Subsequently, he studied chemistry at the Technische Hochschule (now Technische Universität) of München until 1956, obtaining a Diploma degree in 1954 and the Doctorate degree (PhD) in 1956 with Professor Walter Hieber. His thesis “Zur Struktur von Metallcarbonyl-verbindungen und Aromatenkomplexen” includes X-ray structure investigations of the then novel metallocenes and of dibenzenechromium, as studied in the group of the later Nobel laureate Ernst Otto Fischer. In 1956, E. Weiss had a research grant at Massachusetts Institute of Technology, working in radiochemistry with Professors Charles D. Coryell and Jack W. Irvine.

After his return to Germany, he accepted a position at the Union Carbide’s European Research Associates (ERA) in Brussels, Belgium, working on a project on the synthesis of new organometallic compounds, formed in reactions of metal carbonyls with alkynes. In the year 1965, he was asked to establish a research group at the new Cyanamid European Research Institute (CERI) in Geneva, Switzerland. He started research programs in the field of olefin complexes of metal carbonyl compounds. Also, the old mystery of the extremely reactive alkali metal carbonyls of analytical formula MCO could be solved by using X-ray diffraction methods on microcrystalline samples. These compounds turned out to be alkali metal acetylendiolates, $M[OC{\equiv}CO]M$. This led to further structural studies on the methoxides $MOCH_3$ and finally on the methyl compounds MCH_3. The discovery of the tetrameric nature of methyllithium and the polymeric nature of dimethylmagnesium initiated his general interest in alkaliorganics and many related compounds.

In 1965, he was offered a chair of Inorganic Chemistry at the University of Hamburg and remained there until his retirement in the year 1991.

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F.C.: *Could you tell us something about your education? What kind of high school have you attended*?

E.W.: I can answer your question about my education only by shortly explaining the very particular situation of a young man, being born in Germany in most unusual times, probably the worst in German history. My birthplace is a small town in Franconia, in the Northern part of Bavaria. In 1933, at the age of six, I entered Primary School, three months after Hitler's rise to power in Germany. It is needless to emphasize that this had an influence upon all my later life, especially upon the immediately following years. According to our national educational system, I entered Secondary School (Oberschule) in 1937. Two years later, Hitler started World War II by invading Poland. Surely, no German of my generation could imagine the terrible events that were to follow.

In 1943, at the age of 17, my complete school class was transferred to nearby Nürnberg to serve as "Luftwaffenhelfer". This meant that we had to replace soldiers of an anti-aircraft artillery unit, at the same time, attending the classes of the Secondary School. We were lucky that no major air raid happened during that time. Soon, thereafter, Nürnberg was destroyed to an extent of about 50%. In 1944, I had, as every young German, to serve in the "Arbeitsdienst" (Labour Service) and starting 1st August 1944, as a soldier in the "Luftwaffe" (Air Force). At that time, the Luftwaffe was already defeated. Only a few airplanes were left, most of them without fuel. Hence, my sole flying exercises were on gliders.

In May 1945, the war came finally to an end in Europe, and I had survived. I surrendered near München in Bavaria. But this was not yet the end. Now, I was taken as a prisoner by American troops, and later, together with one million other soldiers, delivered to the French authorities and transferred to Southern France to work there. After more than two and a half years, I could finally return to Germany, arriving at home on Christmas Evening 1947.

F.C.: *How did you decide to begin your studies of Chemistry? Which were the conditions of German universities, and particularly that of München, at the end of the Second World War*?

E.W.: Already as a school-boy, around 1941, I had discovered my love for chemistry and set up a small laboratory at home. Now, in 1948, it was my definite will to study chemistry at a university, despite the practically hopeless situation of the German chemical industry in those days. Moreover, getting admitted at a university was extremely difficult, if not impossible. The country was full of refugees and homeless people from the ruined cities. University life started again only very slowly, in damaged buildings, with little personnel and practically no equipment.

Germany was now divided into four zones of occupation. I lived in the American zone and could, by law, only apply for admission to one of the four universities existing at that time in Bavaria. It was sheer luck that I was finally admitted to the Technische Hochschule, TH, in München. Helmut Behrens (1915–2000), a young assistant at that time, has presented a vivid description of the situation in his report [1] "*Wissenschaft in turbulenter Zeit (Science in stormy times), Erinnerungen eines Chemikers an der Technischen Hochschule, München*". He was the person responsible for admission in chemistry. It was a lucky moment in my life when he admitted me, after having listened to my story. Only later, I understood that my luck had been two-fold. Walter Hieber (1895–1976), who was to become my scientific mentor, was also at that Institute, see Fig. 2. He was already famous and considered to be the leading chemist in the field of metal carbonyl chemistry, although the first metal carbonyl compounds, $Fe(CO)_5$, $Ni(CO)_4$ and $M(CO)_6$ (M = Cr, Mo, W), had been discovered earlier by others. Hieber was an impressive personality. By his fascinating lectures, he attracted numerous scholars to chemistry, such as the later Nobel laureate Ernst Otto Fischer, and many others. Hieber's scientific life has been described by Behrens [2].

Fig. 2. Walter Hieber (1895–1976).

My first laboratory work started in a small room in the basement of a bombed building (see Fig. 3), with no fume cupboard, and very little equipment. No textbooks were available: consequently, we attended as many lectures as possible and took many notes on rare paper of the worst quality.

However, everybody was guided by a strong will to compensate for the many lost years and to work hard for a yet unforeseeable future. There was simply no other choice, especially for the many refugees and displaced persons, who literally had lost everything. However, some were desperate. The famous chemist Hans Fischer (1881–1945, Nobel laureate 1930 for his fundamental work on the chemistry of porphyrins at the Technische Hochschule) had lost all hopes of ever being able to work again as a scientist. He commit-

Fig. 3. Technische Hochschule, München, sometimes in 1947. View from its tower towards the centre with the chemical laboratories on the right-hand side. The main lecture theatre had lost its roof, thus allowing a glimpse on damaged rows of seats. Now, the Technische Universität has large modern buildings outside the city of München and is considered as one of the finest Universities in Germany [3].

ted suicide in 1945. So did Arnold Hönigschmidt, professor at the Universität München, and famous for his work on atomic weights. Fischer's successor was Stefan Goldschmidt (1889–1971). Encouraged by W. Hieber, he returned from his exile in The Netherlands and became my teacher of Organic Chemistry.

Towards the end of the war, Walter Hieber had been wise enough to foresee its consequences. He had transferred his scientific equipment to Weihenstephan, outside München, and thus saved it from destruction. In this way, he was able, after 1946, to resume his research. A few years later, he had a relatively large and efficient research group again.

F.C.: *The school of Inorganic Chemistry of München played an important role in the development of organometallic chemistry, a role which was recognized internationally by assigning to Professor E.O. Fischer the Nobel prize in Chemistry, together with Professor G. Wilkinson. You also took part in the early observations on sandwich compounds of transition metals. Can you tell us something that gorgeous period of Inorganic Chemistry in Europe? How did you happen to become interested in X-ray structural investigations*?

E.W.: By the year 1950, chemical research was still largely dominated by synthetic and chemical–analytical methods. They had served to establish a good understanding of the structures of organic compounds. In Inorganic Chemistry, the situation was similar. Alfred Werner's ideas had become the basis of understanding the nature of the even more diversified coordination compounds. W. Hieber was a strong propagandist of these ideas and applied them widely to metal carbonyl compounds. Many chemists considered this class of compounds as curiosities, difficult to rationalize and hence of little interest. Chemists working on organometallics concentrated their interest almost exclusively on compounds of Main Group metals (Mg, Hg, As, Sb, Sn, Pb, etc.). The most complete compilation at that time can be found in "*Die Chemie der metall-organischen Verbindungen*" by Krause and von Grosse [4]. Only 40 pages out of more than 900 are devoted to transition metal compounds. Metal carbonyls are not mentioned at all.

Already earlier a few physical methods had been used to investigate the structures of chemical compounds. Concerning coordination and organometallic compounds, I should mention X-ray methods, magnetic and electric conductivity measurements. X-rays by Conrad Röntgen (1895) and their diffraction by Max von Laue and coworkers (1912), had both been discovered in Germany. Soon, thereafter, William Henry and William Lawrence Bragg (father and son), started their application to structure determinations. Surprisingly, a few chemists and physicists in Germany used these methods, and much more of the early work was done in Great Britain and in the USA. But all together, the number of investigated compounds remained limited, due to the considerable computational work involved.

Walter Hieber, after 1950, became strongly interested in physical methods. He asked me to set up a dipolemeter and to start investigations on metal carbonyls and related compounds, as part of my thesis, see Fig. 4. Around 1953, the first IR spectrophotometer was installed and used for investigations of metal carbonyls. Somewhat earlier, Hieber had already acquired an X-ray generator in a black metallic box, together with a simple rotational (powder) camera. However, nobody in the laboratory was able or willing to use this equipment. Therefore, the black box got the nickname "Katastrophenwürfel" (cube of catastrophes) and stood around in a

Fig. 4. E. Weiss working on a dipolemeter, around 1954. In the background, the first IR spectrometer (Perkin-Elmer).

corner of the laboratory. Finally, my older colleague Wolfgang Pfab started to use it. He, like Ernst Otto Fischer, had served in the army during several years, occasionally interrupted by a free semester ("Studienurlaub"). In 1952, E.O. Fischer obtained his Dr. degree (PhD) and W. Pfab did the same somewhat later. E.O. Fischer soon found an interesting field of research for his planned habilitation. The surprising compound bis(cyclopentadienyl)iron(II) had just been discovered by P. Pauson and T.J. Kealy, and also by S.A. Miller, J.A. Tebboth and J.F. Tremaine in 1951. E.O. Fischer, with his training in coordination chemistry by Hieber, understood immediately that the structure initially proposed for $Fe(C_5H_5)_2$ with two localized C–Fe single bonds was incorrect and should be replaced by a "Doppelkegel-Modell", now better known as "sandwich structure". Almost simultaneously and independently Geoffrey Wilkinson and his colleagues at Harvard University came to similar conclusions and coined the name ferrocene. In München, W. Pfab undertook to investigate the crystal structure of ferrocene. From its unit cell dimensions and symmetry arguments, he could strongly support the Doppelkegel-Modell. Soon, thereafter, P.F. Eiland and R. Pepinsky as well as J.D. Dunitz (later at ETH Zürich) and coworkers carried out more complete studies.

Now, E.O. Fischer and G. Wilkinson and their coworkers started the run for other metallocenes in a strongly competitive way. This early development, unparalleled in Organometallic Chemistry, has often been described, recently by E.O. Fischer and Jira [5a] and by Cotton [5b] and is now history. Among Fischer's first co-workers were Reinhard Jira, Walter Hafner and Dietlinde Seus. Later on, Jira and Hafner took positions at the Consortium für Elektrochemische Industrie, a subsidiary of Wacker–Chemie GmbH in München. Here, W. Hafner laid the foundations for the synthesis of acetaldehyde by oxidation of ethylene, which was developed to the important industrial Wacker–Hoechst process, with essential contributions by R. Jira. While working on this project, W. Hafner discovered the first η3-allyl-metal compound.

Fig. 5. E. Weiss working at a Weissenberg–Sauter single-crystal diffractometer, ca. 1955.

Let us go back to the Technischen Hochschule München in the mid-1950s. E.O. Fischer and his coworkers synthesised further metallocenes, while Pfab did the corresponding X-ray diffraction work by demonstrating that cobaltocene and nickelocene are isostructural with ferrocene. In 1954, however, W. Pfab accepted an offer by the BASF company, which could have been the end of the short blossom of crystallography at our Institute. Therefore, W. Hieber asked me to take over. So, I started X-ray work on a cobalt carbonyl compound, practically by self-teaching. After completion of my Diplom-work in 1954, my interests turned strongly towards the sandwich compounds synthesized in Fischer's group. Hieber was very broad-minded and accepted this decision. Soon, I could demonstrate the sandwich structure of magnesocene $Mg(C_5H_5)_2$ [6a,6b], the first organomagnesium compound investigated by X-ray methods, of vanadocene $V(C_5H_5)_2$ [6a] and chromocene $Cr(C_5H_5)_2$ [7]. Manganocene $Mn(C_5H_5)_2$ [8], however, showed a polymer zig-zag structure, as we could demonstrate only much later [9].

In Summer 1955, Walter Hafner surprised everybody by his sensational synthesis of dibenzenechromium $Cr(C_6H_6)_2$ [10]. Most chemists at that time believed that a coordination compound consisting of a neutral metal and neutral ligands should be unstable. Therefore, an experimental validation was important. By December 1955, I had solved its structure (including Cr and C positions) from X-ray data [11]. To that purpose, I could use a Weissenberg single-crystal diffractometer located in another institute (see Figs. 5 and 6). The extensive calculations had to be done "by hand", using cosine and logarithm tables from my school days. Soon, thereafter, I submitted my thesis "Zur Struktur von Metallcarbonylkomplexen und Aromatenverbindungen".

Early in 1956, W. Hieber suffered from a very severe accident being overrun by a car. We were in great sorrows, hoping that he could survive. Helmut Behrens was one of the first to visit him in the hospital. He found Hieber in bed,

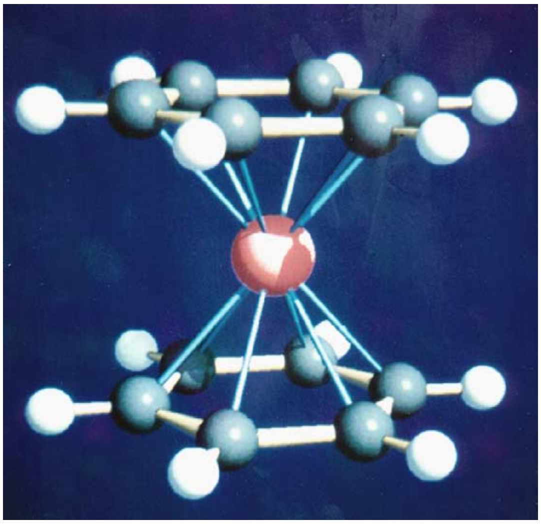

Fig. 6. Bis(benzene)chromium $Cr(C_6H_6)_2$.

dressed by heavy bandages. After perceiving the visitor, Walter Hieber addressed himself to H. Behrens with the words: “Everybody should know that my chair is not yet vacant!” These, certainly, were very happy news for all of us. Earlier, in May 1955, the Third International Conference on Coordination Chemistry (ICCC) was held in Amsterdam. Hieber’s lecture, although delivered in German, seems to have impressed many people, as Prof. Fred Basolo remembers well in his interview to this Journal [12]. After the lecture, Fred Basolo asked Hieber some additional information about reaction mechanisms. Thereafter, Hieber looked at Fred Basolo and said: “Young man, in my laboratory, we only do real chemistry!”

W. Hieber had arranged that I could attend this conference. It was my first visit of an international meeting. Later, I attended many ICCCs and 21 years later, in 1976, I had the honour and pleasure to organize the 17th ICCC, held in Hamburg, 6–10 September, the first one to be held in Germany.

In 1955, Dietmar Seyferth, a student of Eugene Rochow from Harvard University, visited our Institute as a post-doc. This inspired me to do the same in the opposite direction, and I started to contact inorganic chemists at some universities in USA. I was extremely happy when I was accepted for a “Foreign Student Summer Project (FSSP)” at Massachusetts Institute of Technology. Its short-term timing fit well my plans. I arrived in New York by ship and spent the summer of 1956 at MIT, working with Charles D. Coryell and Jack Irvine Jr. on a project in radiochemistry. At that time, practically all inorganic chemistry research at MIT was in the field of radiochemistry; only later, Frank Albert Cotton, the first student of G. Wilkinson, and Dietmar Seyferth started organometallic chemistry at that institution [13]. Nevertheless, my time in USA became one of the greatest experiences in my life. Only a few young Germans before me had such a chance and my mental horizon was much enlarged by seeing this great country. In August 1956, E.O. Fischer came over, and we both attended a “Gordon Research Conference” in New Hampton, NH, where he presented a paper on metallocenes and the sensational dibenzenechromium.

At the end of the FSSP, I was eager to see more of the United States. Together with two Dutch and a Scottish colleague, radiochemists and nuclear physicists, we acquired a second-hand car and set off for a long trip taking us to California and back. We visited a maximum of beautiful sight-seeing places, National Parks and research laboratories on our way. The unforgettable highlight was the visit of California Institute of Technology (Caltech), where Linus Pauling was kind enough to receive me. Two years earlier, he had been awarded his first Nobel prize (in Chemistry), his second one (for Peace) following in 1963. I surely felt proud when he told me of having read the two papers on the synthesis and structure of dibenzenechromium. Soon, he started to discuss the bonding in the molecule, of course in terms of his famous valence bond theory. I have always admired this outstanding scientist with many important achievements in a great variety of domains. His book “The Nature of the Chemical Bond” had already impressed me as a student.

F.C.: *After the period you have spent in Brussels in the Research Laboratories of Union Carbide, you moved to the Research Laboratories of American Cyanamid (Cyanamid European Research Institute, CERI), where I had the pleasure to be one of the components of your research group. Can you tell us how you got to move to Belgium first and then to Switzerland*?

E.W.: In November 1956, I returned to München. Walter Hieber encouraged me to start a habilitation work, but I accepted an offer from a newly established research laboratory in Brussels, Belgium. Here, the American company Union Carbide had established the research laboratories of the European Research Associates (ERA). The work in this laboratory was entirely devoted to fundamental research in selected fields of chemistry and physics. The equipment and working conditions were outstanding; no European university could compete with it. Walter Hübel, another student of W. Hieber, had already started there to investigate the reactions of metal carbonyls with different substituted alkynes, giving rise to a variety of new organometallic compounds. In all my scientific work, I always had a special interest in the fundamental aspects of Chemistry. So, I decided to do corresponding investigations with acetylene itself. This required high-pressure work, which was not so easy and occasionally risky. Some of the new compounds became themselves starting points for further studies, such as ferroles or carbonyl compounds with cyclobutadiene, cyclopentadienone, fulvene, tropone and similar ligands, themselves formed from the interaction of acetylene and CO within the coordination sphere of the metal.

In those days, there was a definite shortage of trained chemists, particularly in USA. This was one of the reasons why American companies like Union Carbide and Monsanto turned to Europe, established laboratories and made attractive offers. So did American Cyanamid by establishing the

Cyanamid European Research Institute (CERI) in Geneva, Switzerland. It was smaller by size than ERA, but had the irresistible charm of being located in a villa with a large park in one of the outstanding residential areas of that beautiful town. The Institute, which overlooks the Lake of Geneva, does not exist any longer and the building now hosts the internationally known World Economic Forum (WEF). E.O. Fischer, during his first visit to the Institute, called it “Forscherburg Bella Vista” (Researcher’s castle Bella Vista). Those were the Golden Days of chemical research!

At CERI, we had research groups in six areas, among them Theoretical Inorganic Chemistry (TIC), headed by the well-known Danish chemist Christian Klixbüll Jørgensen. He was extremely productive. Almost every week, he came to the lab with the manuscript of a new publication, mostly on ligand field theory. I was responsible for the Synthetic Inorganic Chemistry (SIC) and could appoint several young and able chemists from different European countries.

So, I was very lucky that you decided to join our group. I am glad that you obviously kept a good souvenir, as it shows up in your interview by R. Poli [14]. Not only did you perform outstanding and completely independent work in high-pressure metal carbonyl chemistry; later you also became my successor as Group Director. I should also mention Werner Büchner, coming from Heidelberg. He finished his professional career as Director of Inorganic Research at the important Bayer Company in Germany.

In Geneva, we had no General Director above us and were absolutely free of choosing our research programs. I felt that I should make a completely new start. My first choice was based on metal carbonyl compounds with olefinic ligands, such as (ethene)Fe(CO)$_4$, and on “alkali carbonyl compounds”. From Hieber’s lectures, I had learned that already in 1834, Justus Liebig had reacted potassium metal with carbon monoxide and observed an occasionally explosive black product of composition KCO. Some chemists considered it to be K$_6$(CO)$_6$, but little experimental work had been done, certainly due to its explosive nature. Just simple touching with a spatula, and of course contact with traces of oxygen, water or other protic solvents could make it decompose or even explode. A more gentle preparation consisted of bubbling CO though the blue solution of the metal in liquid ammonia until its discolouration. My coworker Werner Büchner had experience in working with liquid ammonia, and he had courage. However, solving the mystery of MCO by chemical methods turned out to be very difficult. As a last chance, I turned again to X-ray methods. Our products were exclusively microcrystalline powders; therefore, having learnt something about powder diffraction methods, I was finally successful. Thereafter, powder methods became a very important tool for much of my subsequent crystallographic work. It turned out that all MCO compounds had to be formulated as M[OC≡CO]M (M = Na, K, Rb, Cs), and they should be appropriately called alkali acetylenediolates, as they contain the linear acetylendiolate dianion $[C_2O_2]^{2-}$ (Fig. 7).

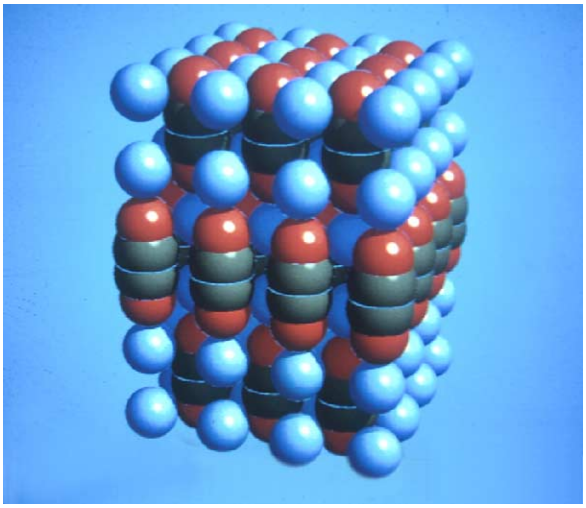

Fig. 7. Structure of $K_2[C_2O_2]$. Potassium ions are blue. The compounds with sodium, rubidium and cesium are isostructural.

A literature survey showed that structural knowledge of other metal alcoholates was almost completely lacking. Thus, I undertook a study of alkali methoxides, again using their microcrystalline samples, since no single crystals could be obtained. Following these ideas further, I came to methyl lithium. I was almost surprised that this important and most fundamental organometallic compound had not yet been investigated. It had to be attributed again due to the lack of single crystals. Most crystallographers try to avoid the more difficult work with polycrystalline samples. Here, even the first step, indexing the reflections, can be quite a problem. Fortunately, this was not the case with methyllithium (Fig. 8).

The observation of the tetrameric molecules in methyllithium [15a] was a surprise and became the starting point for all our subsequent studies on alkali-organic and related compounds. Together with my British colleague, Anthony

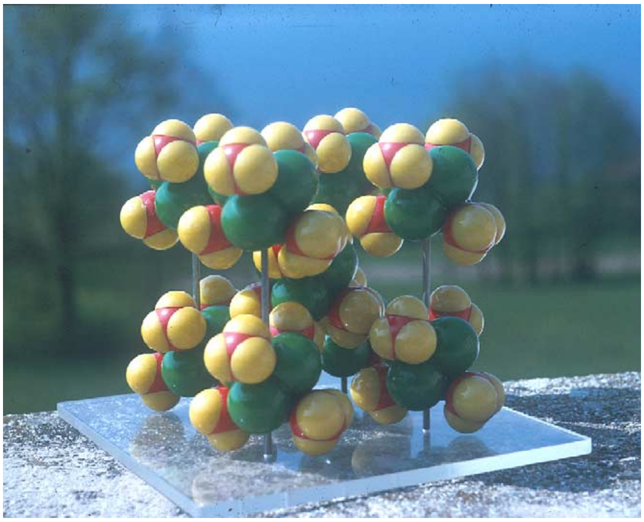

Fig. 8. The first model of the methyllithium unit cell (cubic), placed on the terrace of CERI with a view towards the Lake of Geneva.

Fig. 9. Unit cell of dimethylmagnesium (without H atoms). Picture taken in 1964 on the top of Mont Salève (near Geneva) with a view towards Mont Blanc, the highest mountain in Europe.

Lucken, director of our physical chemistry group at CERI, we described the bonding in the methyllithium tetramers by a simple molecular orbital model. At that time, the Li–C bond was considered to be largely covalent. This stimulated several theoreticians, such as Paul Ragué von Schleyer (Universität Erlangen, Germany), to carry out extensive studies in this field. Only later, with growing structural knowledge, the general opinion gradually changed to more ionic models. In this way, the radius of lithium, in comparison to carbon, became smaller and smaller. This is reflected by drawings of a structure refinement of $(LiCH_3)_4$, published in 1969 [15b]. This did not escape the sharp eyes of Linus Pauling. A few years before his death, I watched a documentary on the German TV about his life as a scientist. There were some scenes showing L. Pauling sitting in front of a book shelf at his home at Big Sur, CA. On one of them, I could notice two ball-and-stick models of $(LiCH_3)_4$, as in our publications. With the growing interest in alkali-organic compounds, particularly in the UK, in 1994, a new series of international conferences (ALKCHEM) was started in Cambridge, England. I had the privilege to open the "First International Conference on the Chemistry of the Alkali and Alkaline-Earth Metals" with a plenary lecture on their structures.

Back to Geneva and the year 1964. I then turned to Grignard compounds and could determine, again from microcrystalline samples, the crystal structures of dimethylmagnesium and diethylmagnesium. Both form polymer chains with pairs of alkyl groups between adjacent Mg atoms (Fig. 9).

Structural calculations became more and more tedious, thus I learned to use computer methods. Geneva is the seat of the world famous European Nuclear Research Institute (CERN). Naturally, very advanced computer technologies have been used there; and later, the World Wide Net, www, has been developed at CERN. In 1964, I could make use of part of these facilities, learn some programming and use the FORTRAN language, in those days, very important for scientific problems.

F.C.: In 1965, you moved from Geneva to the University of Hamburg, where you have been selected to become Professor of Inorganic Chemistry. Can you tell us something about your major scientific achievements there?

My decision to accept the offer of the University of Hamburg was not an easy one. But nobody at that time could foresee that the Golden Days of industrial research institutes in Europe would soon come to an end, mainly due to changing economic conditions. In Hamburg, I had to start again under every respect. However, the group was steadily growing, thanks to very able and enthusiastic coworkers. The University had just established a new Computing Centre, and we soon were among its main users. Program and data input/output were handled on punched cards, using "home-made" programs. Soon, we could add to our laboratories the first computer-controlled single-crystal diffractometers (Hilger&Watts, Syntex P21), and the first NMR spectrometer.

We developed two research lines: organic compounds of transition metals and organic compounds of alkali and other Main-Group metals. The first group embraces compounds of Fe, Co, Ni, V, Cr and other transition metals with a great variety of organic ligands (olefins, allyl, enyl, acyl and others), compounds with multiple bonds, such as M=Ge, M=Ge=M, M≡S≡M, M≡Se≡M [16] or linear carbonyl groups between metals MC≡OM.

The second group is the largest one. We could extend our investigations on organic compounds to all alkali metals, to magnesium and to a large number of organometallates with Cu (cuprates), Be (beryllates), Mg (magnesates), Zn (zincates), Cd (cadmates), B (borates), Al (aluminates), Ga

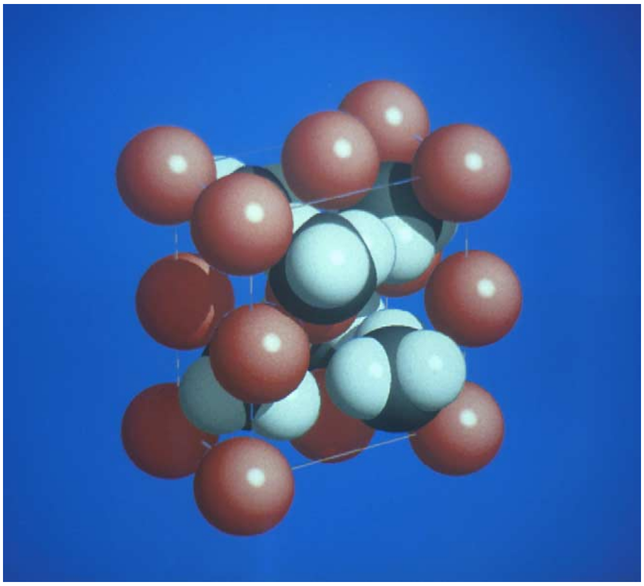

Fig. 10. Unit cell of methylpotassium according to X-ray and neutron diffaction studies of KCH_3 and KCD_3. The methyl compounds of rubidium and cesium are isostructural.

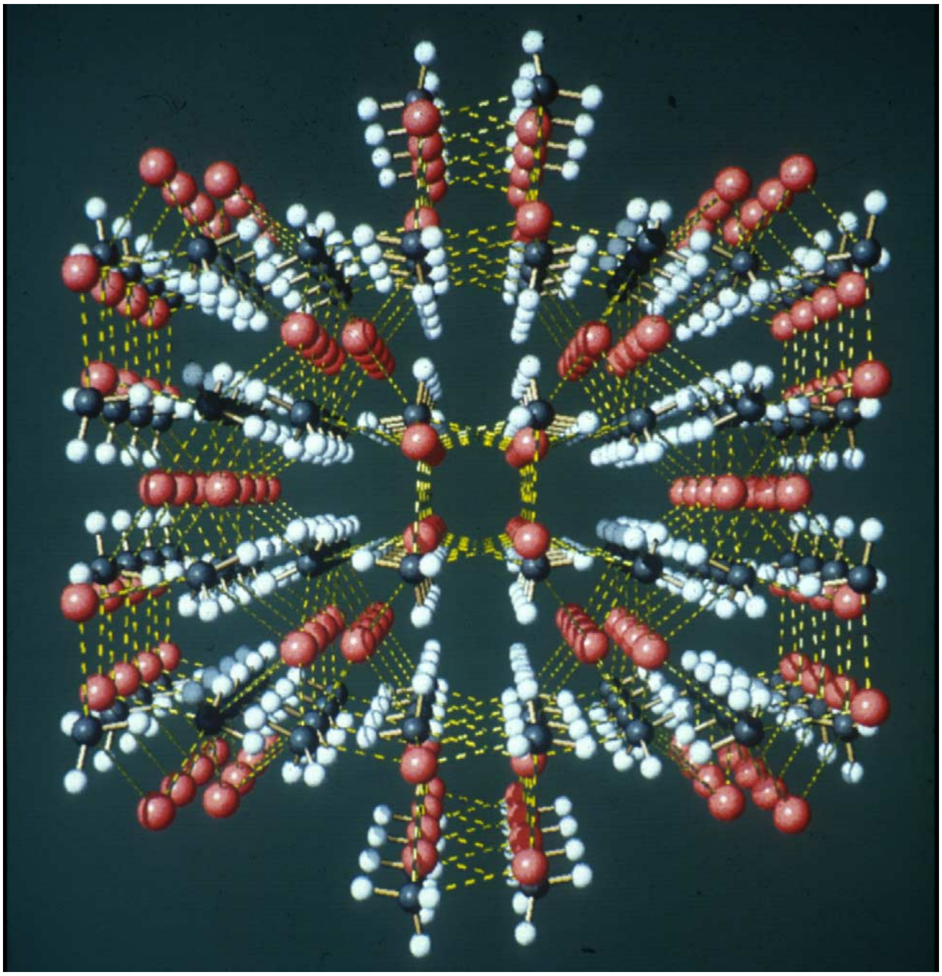

Fig. 11. Several (orthorhombic) unit cells of methylsodium according to X-ray, synchrotron and neutron diffraction studies of $NaCD_3$.

(gallates), In (indates) and Tl (thallates). On the other hand, the organic ligands were replaced by various groups, such as alkoxides OR, amides (NH_2 and NR_2), silyl (SiH_3) and germyl (GeH_3). The most extensive studies, however, were on organolithium derivatives and related compounds by studying their solvation by ethers and (chelating) amine ligands. They gave much information on the nature of organometallic compounds in solution. In collaboration with colleagues at the Institute Laue-Langevin in Grenoble, France, we carried out neutron diffraction studies of microcrystalline compounds (methyllithium, methylpotassium, methylsodium), in order to get precise hydrogen positions. For technical reasons, these very air-sensitive compounds had to be prepared as fully deuterated samples.

F.C.: *Which scientific observations of yours you like best and/or may be regarded to be relevant for the future development of Inorganic Chemistry*?

Here again, my preference lies in the most simple compounds, which may have significance in understanding some basic principles of structural organometallic chemistry. Apart from bis(benzene)chromium, I should mention our powder diffraction studies of the methyl derivatives of alkali metals, such as KCH_3, $RbCH_3$, $CsCH_3$, of NaC_2H_5, and last but not least, of $NaCH_3$ (Fig. 10).

Methyl sodium was for a long time our “missing link”, even from a purely synthetic point of view. Finally, we found a way to prepare the mixed $NaCH_3/LiCH_3$ compounds. They turned out to be inclusion compounds (host–guest compounds of methyllithium tetramers in a lattice formed by methylsodium tetramers). Only after many efforts, we could prepare pure methylsodium. It showed a rather complicated structure, different from all its analogues. Even the determination of its rather large unit cell from powder data was quite a problem. After years, we finally succeeded by using a combination of the best diffraction methods available at that time, X-ray, synchrotron radiation and neutron diffraction of the deuterated compound $NaCD_3$. This probably was the most difficult ab initio structure determination carried out up to that time. Details and a survey of most of our work on organo alkali metal complexes and related compounds can be found in the literature [17] (Fig. 11).

I always found great satisfaction in solving problems, particularly long-standing ones and of fundamental interest. This attitude can be traced back to the influence of Walter Hieber.

From him, we learned that the search for knowledge should be the primary goal of Science and that one should not look first for immediate useful applications. They will follow by themselves if the observations turn out to be important.

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