Report

Oral histories in meteoritics and planetary science: IX. Heinrich Wänke

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Abstract—In this interview, Heinrich Wänke, a nuclear physicist, describes how he first encountered meteorite studies in 1953 when, after finishing his Ph.D. thesis in Vienna, he joined the research group of Professor Friedrich Paneth at the University of Durham, England. There, he worked on problems relating to uranium-helium ages of iron meteorites. A year later, Wänke moved with Paneth to the Max-Planck-Institut für Chemie at Mainz in Germany. He continued meteorite research but also conducted experiments to measure noble gases in the stratosphere, a project that brought him to America in 1956 where he first met Professor Harold C. Urey, with whom he formed a lasting friendship. After Paneth's early death in 1958, Wänke remained at the Institute in Mainz and pursued research on topics such as the isotopic compositions of cosmogenic noble gases in iron meteorites and the abundances of primordial rare gases implanted by solar wind particles in brecciated stony meteorites. In 1969, Wänke was appointed to fill Paneth's position as a director of the Max-Planck-Institut für Chemie just in time for him to lead a wide spectrum of research projects on the lunar rocks and soils. From the geochemical evidence these studies provided, he theorized on the formation of the Moon by the giant impact hypothesis, and proposed a two-component model for the cosmic composition of the Earth's mantle. His group also investigated the isotopic chemistry of martian meteorites and its bearing on the origin and evolution of Mars, which he viewed as a cored planet that underwent early differentiation without subsequent convective homogenization. In 1980, the Meteoritical Society awarded the Leonard Medal to Heinrich Wänke for his numerous contributions of fundamental importance to meteoritics and planetary science.

UBM: My first question, Heinrich, is how did you become interested in meteorites?

HW: I came to meteorite research just by chance.

UBM: Just by chance?

HW: Yes. I was studying in Vienna and wrote my Ph.D. thesis on alpha spectroscopy at the Institut für Radiumforschung und Kernphysik. My advisor was a woman, Professor Bertha Karlik. At that time, the situation was very bad for science in Vienna, and actually in all of Austria and in Germany, as well. Only a small amount of money was available to support science, so I tried to finish the time I would spend in the university as quickly as possible. In the summer of 1952, I went to Linz, my hometown, to write my thesis. There, I got a letter from Professor Karlik saying that I should finish at the university by the end of that year. She also said that she had received a letter from an old friend, Professor Friedrich Paneth, who had had to leave Germany in the 1930s because of the Nazis. He now was a professor at Durham University in northern England, and was looking for a young nuclear

Fig. 1. Heinrich Wänke at the Max-Planck Institut für Chemie at Mainz ca. 2001.
physicist who would like to work with him. She told me that Paneth worked on meteorites.

I had no idea what meteorites were. So, I asked a friend of mine who had a big lexicon, in 25 volumes or so, if we could look up meteorites. In it, we found three pages about them. The book said that meteorites are rare and that only a few fall during each year. Suddenly, my friend put his hand on my shoulder and said: "Oh, Heinrich, this is the job for you, you will not be overworked."

UBM: Did you take the job knowing nothing more than that about meteorites?

HW: Yes, I did. I wrote and told Professor Karlak that I was delighted with the prospect. Actually, I would have accepted more or less any kind of a job I was offered outside of Austria. But I was especially pleased to go to England because I wanted to improve my English reading and speaking skills, which were rather poor because the teaching of foreign languages in my high school was quite primitive.

UBM: Did you finish your thesis before leaving for England?

HW: Yes. I got my Ph.D. at the end of 1952 and went to Durham and started to work with Paneth early in 1953. At first, he assigned me a problem that had nothing to do with meteorites. The idea was to use helium for the calibration of standards of radium. But soon afterwards he reassigned me to meteorite research. Paneth was looking for a nuclear physicist because he and his group had been analyzing meteorites since the 1920s and had developed good, sensitive techniques for measuring trace amounts of helium in them. He studied helium in all kind of materials, potassic rocks, for example. But he soon realized that the ages derived from them are not very useful because helium obviously diffuses out of silicates quite easily.

UBM: And ages were what interested him?

HW: Yes. Ages were actually what he was trying to determine. He assumed, quite correctly, that there would be one type of samples, namely, iron meteorites, in which the diffusion of helium would be much lower than in stones. So, he and his group started analyzing large numbers of iron meteorites for helium. He got numbers that still are within a couple of percent of those we get today with the most precise techniques.

UBM: I didn't realize his early analyses were as good as that.

HW: His work was very remarkable, really. But in order to get ages, he assumed that the helium is a decay product of uranium and thorium—as everybody assumed at that time. So, once he had his helium numbers he needed uranium and thorium values. And these turned out to be a big problem. Paneth had a number of postdocs and Ph.D. students working on this. Finally, these people came out with almost concordant numbers. But, in fact, they were all wrong. As it turned out later, these values were too high by orders of magnitude. But there is a strange aspect to this. The helium numbers were correct, and by assuming that the helium was a decay product of uranium and thorium, they got ages for the iron meteorites of up to 4 Ga. Today, everybody would be happy with this. But at that time they were not at all happy because the age of the universe was believed to be only ~2 Ga.

UBM: So Paneth's meteorites appeared to be twice as old as the universe?

HW: Yes. He gave talks about this to the Royal Society in London and there were big discussions about the discrepancy. But the astrophysicists soon made little adjustments to the Hubble constant and the age of the universe went up to ~4 or 5 Ga. At first, that appeared to solve the problem, and everything seemed to be fine. But this, of course, was not the end of the story. In the late 1940s, two people outside of Paneth's group, one was Carl Bauer, at the Harvard College Observatory, and the other was H. E. Huntley, at the University of the Witwatersrand in Johannesburg, South Africa, had suggested that the helium Paneth was measuring in iron meteorites might not all be a decay product of uranium and thorium; some of it might be due to cosmic-ray interaction in space—spallation. The question was, how could this be proved or disproved?

UBM: This was still at least 10 years before the discovery of cosmic-ray particle tracks, wasn't it?

HW: Yes, just about. Bauer had suggested three or four approaches to solving this problem. They included determinations of the radial variation of helium in iron meteorites and of the ratio of $^3$He to $^4$He. If the helium was produced by cosmic-ray spallation the amount of it should decrease with depth in an iron meteorite. So if you were to cut a slice through the meteorite, or take a core of it, you could document the decrease in helium from the outside to the inside. But Paneth's group actually had tried this before I came there and had found very little difference with depth. One has to say, however, that at that time the nuclear physics of this whole process wasn't very well understood. So, the penetration depth they measured of cosmic-ray particles in the metallic iron was not wrong, but there was much less difference than they had expected to see at that time.

UBM: Was this true regardless of whether the iron meteorites were small or large?

HW: Yes. The problem was that if you just consider the primary cosmic-ray particles, the interaction length should have been visible. The reason why Paneth didn't see it, we realized later, was due to the activity of secondary particles. Primaries make a cascade of secondary particles that create a cross-over effect. So, later on it became quite clear why he couldn't see a gradient in helium content in, for example, the meteorite Tresya, that he studied. It was not large enough to show such cross-over effects.

Paneth also realized that the decay of uranium and thorium produced almost entirely $^4$He ($\alpha$ particles), but cosmic-ray spallation had been predicted to produce $^3$He. He had no way of doing mass spectographic analyses to identify which he was finding, so he wrote to the number one person in the world
who was skilled in mass spectrography at that time, Alfred Nier at the University of Minnesota. Very often when I met Nier in later years, he would say to me, oh, I was so stupid because when Paneth asked me if I could measure the helium he would extract from iron meteorites, and then told me how small the amounts would be, I said this is a very interesting proposal but I'm sorry, I cannot help you. At that time, Nier had just measured the $^3\text{He}/^4\text{He}$ ratio in the atmosphere, and found it to be $\sim 10^{-7}$. He thought that if Paneth expected to find maybe $10^{-5}$ STP of helium, he would have to measure $10^{-12}$ cc of $^3\text{He}$, and that would be hopeless. However, there was a young Ph.D. student at Oxford who was just building a mass spectrometer for analyzing argon. K. I. Mayne was his name. He had a very straightforward approach. When Paneth asked him, he said: "Sure, I can measure your helium."

UBM: Even if Al Nier couldn't?

HW: Yes, the difference was that Mayne was not misled by assuming far too low $^3\text{He}/^4\text{He}$ ratios for the samples he got from Paneth's group, and using the mass spectrometer he found they contained $\sim 30\%$ of $^3\text{He}$. This was easy to measure because he had the order of say, $10^{-5}$ cc of $^3\text{He}$ instead of $10^{-12}$ cc, which Nier had estimated. Nier estimated wrongly, of course, without much thinking. That is why he was so unhappy about it afterwards because he could have done it easily. So, Paneth sent his samples to Mayne at Oxford. The results made it quite clear that a significant part of the helium Paneth's group had measured during many, many years was not a decay product of the uranium or thorium but was produced by cosmic rays.

UBM: I am rather pleased that the credit for making these analyses went to the young student at Oxford.

HW: Yes, definitely. It started off a new player in the field without really doing any harm to Nier.

UBM: I recently reread Paneth's paper of 1952 in which he published these results along with Mayne and P. Reasbeck, a colleague of his at Durham. Paneth regarded the measurements of $^3\text{He}$ as a step forward. He said that knowing the isotopic composition of the helium in iron meteorites made it possible to eliminate the contribution of cosmic rays and put the uranium–helium method of age determinations on a sounder basis.

HW: Yes, he was satisfied with this result at that time. But of course, the next question we faced was: what is the source of all the uranium and thorium Paneth's group has been measuring all this time? It took a couple of years until this question was settled and I will return to it later.

In the summer of 1953, Paneth took a vacation in Austria and Germany and I took one in Austria. When I came back, Paneth called me into his office and said: "Herr Wänke, I have signed a contract; I will go to Mainz in Germany and you, of course, will come with me." I said: "Herr Professor, do you know Mainz? I have been there recently; it was bombed completely flat, absolutely destroyed." He said that was correct but the rebuilding was going on, particularly on the periphery of the city where the Max-Planck Institute of Chemistry was located. He assured me that there were great research possibilities there and as a young scientist I couldn't have a better place anywhere than in the Max-Planck Institute.

UBM: In hindsight he seems to have been right.

HW: He was right. But I was not at all happy because the contract I had with the head of Durham University was for 2 years. Although I had been there only a few months I had just received an increase in salary and so I wasn't at all favorable to leaving. But every second or third day Paneth would come and ask me: "Have you thought it over? Will you come with me?" He brought up all kinds of arguments. I remember one, in particular, which puzzled me. He said that Mainz is close to the Frankfurt Airport; actually, it's not much farther than from the center of the city of Frankfurt, and Frankfurt is certain to become the center of air traffic in central Europe. At that time, I never had flown in an airplane. I went to Durham by train and ferry and at that time I think 98% of the people crossing the Atlantic did it by boat.

UBM: I'm sure they did.

HW: But during World War II Paneth had been with the British Canadian Atomic Energy Team in Montreal and he had crossed the Atlantic by plane. So, he was used to air travel and believed in a great future for it. But air travel wasn't in very wide use at that time, and I said to myself this does not really attract me very much—to be close to an airport. Finally, Paneth came to me and said: "Well, Herr Wänke, I brought you here to Durham and you came because of me and because you needed me to help you get a start in a position. Now I need you to transfer the technique to Mainz, and so I really ask you come with me." I said: "Herr Professor, why didn't you say this in the first place? This is an argument I cannot contradict at all. Of course, I will come with you." I never have thought that this was a wrong decision.

UBM: So you went to Mainz with Paneth at the end of 1953? What research projects did you work on there?

HW: Together with my colleague, Dr. Klaus Ebert, I was busy for ~2 years setting up an improved vacuum apparatus for the microanalysis of rare gases, especially of helium and neon. To do this and to do all the necessary calibrations was a time-consuming process. When we were almost ready to start work on iron meteorites again, Paneth gave us another problem, analyzing stratospheric air which was collected first using German V-2 rockets shot up into the stratosphere and later with American rockets. The idea was to measure helium, neon, and argon relative to nitrogen. From such analyses you can check to see if there is any separation of the lighter gases from the heavier ones. Some people had reported such a separation, which, according to theoretical considerations, should not take place because due to the temperature gradient convective mixing should occur up to an altitude of ~100 km. I worked on this for awhile and then I was sent to the University of Michigan at Ann Arbor where an American group led by Prof. Johnson was investigating the same problem, with the support of the U.S. Signal Corps, by sending up devices for the
collection of stratospheric samples. So, I visited the United States for the first time in 1956.

UBM: What did you find out from your work in Ann Arbor?

HW: Actually most of the work was done in Mainz. It turned out that all the previous data obtained in this project were wrong. The steel bottles used to collect the air had been made vacuum tight using a paint to close pinholes along the weld-seams. So the bottles were tight for oxygen and nitrogen, but not for helium and neon which entered the bottles through the paint by diffusion. There was no way to measure all four gases.

UBM: Did you travel around the country a bit after you finished at Ann Arbor?

HW: Yes, of course. From Ann Arbor I went to Chicago and later on to Boston. At the University of Chicago, I visited the Enrico Fermi Institute and came across a door marked with the name: H. C. Urey.

UBM: I assume that you knocked on that door.

HW: I did. Of course, he was a very eminent figure and I was nothing in comparison, but I knocked on the door and he opened it for me. I told him I was from Germany, and he gave me a very, very cordial and friendly greeting. After we talked awhile, he brought me to meet Johannes Geiss, who was working with him. Then he said there was a German in the laboratory, Friedrich Begemann, but he was on vacation in Mexico at the moment. Eventually, Urey asked me: "By the way, does your boss, Paneth, still believe in those high uranium figures he has been reporting in iron meteorites?" I said: "Well, I think so." Urey responded: "Then you have to understand, my young friend, that this is hopeless; impossible. Uranium is a strictly lithophile element. It never would go into the metal phase in such concentrations."

I kept this in mind, and coming home, to Mainz I decided to do something about uranium determinations by a technique that would not be subject to contamination. The answer, obviously, was neutron activation. So, at our laboratory in Mainz I built the apparatus needed for measuring small quantities of uranium by neutron activation. For our first uranium determinations we used the fission product, barium, but later on we used xenon isotopes, as we were experts in rare gas handling.

UBM: That was a rather new technique at that time, wasn't it?

HW: Yes, and I think ours were the first such measurements made in Germany. I had to send my samples to England to Harwell for neutron irradiation in a nuclear reactor. After irradiation the samples were sent back by plane. I found that I couldn't detect any uranium in the iron meteorites, but I got nice numbers in stone meteorites. We could have contributed something new in these studies except that Hamaguchi, Reed and Turkevitch in Chicago had just published a paper about uranium in stony meteorites. That left us with not much left to do except to confirm their data, which we did a few months later. It also became clear that all the uranium and thorium Paneth's group had measured over all the years were due to contamination, which we showed to be derived mainly from the glassware that was used for the chemistry.

Having worked with radioactive xenon isotopes for the determination of uranium, I thought I could use radioactive argon isotopes for K-Ar age dating in connection with neutron activation. I sealed the sample in small quartz tubes before the neutron activation and later on extracted three argon isotopes $^{37}$Ar, $^{39}$Ar, and $^{41}$Ar and measured their activity. Argon-41 was produced by neutron capture of $^{40}$Ar, the decay product of $^{40}$K, while $^{39}$Ar was formed by (n,p) reaction from $^{39}$K. In this way, K-Ar ages could be determined by just measuring the ratio of $^{41}$Ar/$^{39}$Ar. Today, this technique is very much used, except that the amount of $^{40}$Ar is not determined by neutron activation but by mass spectroscopy. But the principle to determine potassium via $^{39}$Ar is identical. Argon-37 is made by (n,$\alpha$) reaction on Ca, yielding the concentration of calcium in the samples.

UBM: What was your next project?

HW: I mainly started to work on cosmic-ray interactions in iron meteorites. I thought that if a melting event were to take place, the helium would be lost from the metal phase—if, for example, an iron were melted by coming too close to the Sun. This was a stupid idea, of course, but at least I was thinking in that direction.

UBM: It didn't seem so stupid back then; Harold Urey speculated on the same thing.

HW: Another issue raised was the possible effect of heating due to the storage of radiation defects in the metallic crystals—reactors blow up sometimes because of radiation damage in the graphite. So, I thought I should look for a cosmic-ray product that is not a rare gas but an isotope of a stable element and, hence, could not be lost in a heating event. I looked for an element close to iron and found that scandium should be a useful element. It is close to iron and I found that not many determinations of scandium were reported in the literature. I started measuring scandium by neutron activation and found that it is directly proportional to the concentrations of helium, and neon, and argon in iron meteorites. So scandium definitely is a cosmic-ray product—almost exclusively a cosmic-ray product—in iron meteorites. After making lots of analyses of scandium, helium, and neon, I published a paper on the effect of the primary particles and secondary particles in the production of cosmic-ray-induced isotopes. This, I think was the first paper that made me somewhat known outside of the country.

UBM: When did you publish it?

HW: In 1958. Next, I started to look for helium or other rare gases in stony meteorites. I wanted to get an age distribution, not just radiogenic ages but also cosmic-ray exposure ages. So I measured many, many stony meteorites and we got nice statistics on them. In the process, we found four chondrites—Pantar, Breitscheid, Tabor, and Pul'tusk—which contained huge amounts of rare gases. These gases were
not decay products of potassium or uranium or thorium, and certainly were not made by cosmic-ray interactions, so that time, they were called "primordial rare gases". The first report of such gases in stones had been published in 1956 by E. K. Gerling and L. K. Levskii in Leningrad (St. Petersburg). They found them in Staroe Pesyanoe. In 1961, Josef Zähringer and Wolfgang Gentner at Heidelberg found them in Kaopta. Both of these two gas-rich meteorites were achondrites.

UBM: The idea of achondrites being rich in primordial gases is so counter intuitive that it came as a great surprise to many of us.

HW: It did to all of us. For a while it was assumed that this is a phenomenon that occurs mainly in achondrites, objects which were once molten and then solidified. Gerling actually suggested that the rare gases came in from an atmosphere that dissolved in the melt. I asked myself if that were possible and concluded that the absolute pressures would have to have been too extraordinary high. I think it was calculated as four atmospheres for helium, or something close. Except in Jupiter, you wouldn't find such atmospheric pressures of helium. So this obviously was not the right explanation. But by then we had found primordial gases in ordinary chondrites.

UBM: Were you measuring bulk samples or selected fractions?

HW: At first, we did only bulk samples. So, some of my coworkers in Mainz became very eager to do mineral separations and look for the sites where the rare gases were concentrated. They made their separations and found the gases were nowhere: neither in the metal grains nor in the troilite, nor in olivine, nor in the pyroxene. The bulk samples had much higher concentrations than any of the separated fractions. I asked them just how they made the separations; what was the grain size. They said the grain size of all fractions was >30 μm. I said this probably makes all the difference, between the gas contents of the separates and of the bulk samples, because the bulk samples have grains down to below 1 μm. Yes, they said, but we cannot do mineral separations on such fine-grained samples. I decided we had to find a technique to analyze all the constituents of the meteorites without making standard mineral separations. So I developed, together with Elsa Vilseck, a technique of dissolving the meteorite in a series of acids in a vacuum. The first step always was to dissolve the particles of metallic Ni-Fe. The second was to dissolve the troilite, the third was to dissolve the olivine, and the rest was mainly pyroxene. From the very beginning, we found that ~50% of the helium was in the grains of metallic Ni-Fe.

UBM: In the finest grained metal?

HW: In the metal of all sizes, but always in outermost regions. Hence, the concentrations of helium were proportional to the surface to volume ratio of the samples used. I knew this could not be helium that had been dissolved into the molten metal because I had learned from my previous work on helium that rare gases, almost by definition, do not dissolve in metals. Therefore, there must have been another process at work.

UBM: These meteorites were all breccias, weren't they?

HW: Yes, and both the chondrites and the achondrites had a light-dark structure in them, and only the dark portion contained large amounts of primordial gases. This discovery caused great excitement. Several groups besides ours were working on it. I've already mentioned Josef Zähringer's group at Heidelberg, but there also were Peter Eberhardt's at Bern, Peter Siguer's at Zürich, and Heinz Stauffer at La Jolla. In fact, as more and more work was done on these meteorites we all noticed that the noble gases were concentrated almost entirely in the outermost layers of practically all mineral components in the fine-grained dark material in which light-colored, gas-free fragments were embedded. Between 1961 and 1963 members of our group including Heinrich Hintenberger, Hans König, Klaus Keil, Frank Wlotzka, and Friedrich Begemann described gas-rich dark structures in the chondrites Pantar, Breitscheid, and Tabor. Helium and neon were extracted in our apparatus for the microanalysis of rare gases and Hintenberger measured their isotopic composition by mass spectrometer. The rest of us determined the chemistry and mineralogy of the breccia clasts and matrix. Then, in 1965, we assembled our data in two papers that described the radiogenic, cosmogenic, and primordial gases in 47 stone and 22 iron meteorites.

The biggest problem we all still faced was to account for the origin of the primordial rare gases. The only possibility I could think of was irradiation, corpuscular radiation. Then I realized that the best means of introducing gases into meteorites would be by solar wind implantation.

UBM: Solar wind implantation which must have taken place after the stones were brecciated and before the breccia fragments were aggregated into rocks?

HW: Yes. No other explanation seemed plausible. But I was not alone with this idea. I must add that, since I never was very fast in writing papers (and I'm still not) Peter Eberhardt and his group in Bern published the first paper on solar wind implantation of primordial gases in 1965. Our paper appeared about the same time. However, by that time, I already had presented a paper on solar wind origin of implanted gases at a cosmic-ray conference in Jaipur in December 1963.

UBM: And you had written this idea in your abstract?

HW: Yes, not only in an abstract, but also in a full paper, which is in the conference proceedings. Also there was a Gordon Conference in the summer of 1963 and I presented it there. Peter Signer was there, too, and we suddenly realized that we both had the same idea about the solar wind origin of rare gases and other species in meteorites. Hans Suess was there and so was Harold Urey, and both of them got very excited about this subject. Suess said we should have a little workshop in La Jolla on solar wind implantation, so he arranged a one-day meeting in August 1963, so that those of us from Europe could simply stay in America until then.

UBM: Did it add to your confidence to have other groups working on the same subject?
HW: I think it did, at least it showed that ours wasn't a totally absurd idea. All our findings fit together very nicely and in 1964 Hans Suess, Frank Wlotzka and I authored a paper about the solar wind implantation of the primordial rare gases.

UBM: The solar wind, itself, was a fairly new phenomenon wasn't it? I think it had been discovered in 1951.

HW: That's right. Then, as one of my next problems I began to wonder just where the solar wind implantation takes place. Today, one would simply say that it takes place on asteroids. But at that time it was generally thought that the surfaces of asteroids were free of dust. Although impacts on them produce dust, the dust was supposed to be quickly lost to space.

UBM: And asteroids weren't supposed to have enough force of gravity to attract or hold dust?

HW: No they weren't. So I needed an object with a strong enough gravitational field so that the impact dust and debris wouldn't be lost completely. However, this object could not have an atmosphere and could not have a magnetic field, because very low-energy, solar-wind-type particles would not penetrate through any but the thinnest atmosphere. On thinking this over, the Moon seemed to me to be a very nice and convenient site for solar wind implantation of gases.

UBM: So, you got this idea before the Apollo missions were announced?

HW: Yes, I did. In fact, I wrote a number of papers back then suggesting the Moon as the parent body of broncite chondrites, because a substantial portion of the bronzite chondrites we measured at that time contained solar wind implanted rare gases. Then, I decided to look into some hypersthene chondrites and found no solar wind type gases in them. So I concluded that the rare gases in the meteorites originated by solar wind implantation, and that the unconsolidated soil on the surface of the Moon was a very likely place for this to take place. Those were my two cosmic predictions. I didn't know the name "regolith", being a nuclear physicist, but I thought the lunar soil would be loaded with solar wind particles and that the chemical composition of the lunar crust would be that of the bronzite (H-group) chondrites.

Harold Urey approved both of my hypotheses and looked at me like a father to his son. He helped me a lot and was very much interested in our deductions about the Moon, which was his favorite object. Then, the Apollo 11 mission showed that both of us were right about the lunar soil being loaded with solar wind implanted gases but absolutely wrong about getting bronzite chondrites from the Moon.

UBM: Even so, you must have been happy to be right about one of your two predictions.

HW: Yes, we were very happy about that. Next I began to realize that the solar wind irradiation could also have occurred in space; Peter Signer was more for having it happen on the surface of a body. However, because of our proposal of the lunar soil being loaded with solar wind particles, we also had measured a number of radioactive and cosmic-ray products in meteorites, similar to the work Jim Arnold was doing at that time at La Jolla.

UBM: Speaking of Jim Arnold's work you also did some research on terrestrial ages of meteorites. Jim credits you with determining the oldest terrestrial age ever measured on a meteorite—5 Ma on an iron meteorite from Chile.

HW: I read that in Jim's interview with you. That was the Tamarugal iron meteorite, and I think we actually dated it as being just 3 Ma. What we measured in this meteorite was $^{10}$Be, and we found only about half the activity that we expected. So, we said this must represent one half-life of $^{10}$Be, which was thought, at that time, to be 3 Ma. But now the half-life of $^{10}$Be is known to be closer to 2 Ma. However, this still may be the oldest terrestrial age for a meteorite.

UBM: There are two story meteorites from Antarctica with terrestrial ages close to 2 Ma.

HW: Yes, that's right. So it would be hard to ascertain that Tamarugal is older. You know, it is quite interesting. When I started this research project, very little was known about terrestrial ages. I didn't understand much chemistry at that time, but we could measure $^{39}$Ar with a half-life of ~300 years. With this I thought we could find the terrestrial ages of iron meteorites. I needed large samples because, at that time, we did low-level counting. This meant that 100, 200, or 300 g was the absolute minimum of sample size we could use, depending on its activity. So, I went to Ed Henderson, the curator of meteorites at the National Museum of Natural History in Washington, who became a very good friend of mine. I told him my story and he said, well, OK, but I don't believe you will see much. Iron meteorites, just rust away.

UBM: Some of them do; and some of them definitely don't.

HW: I said that perhaps we could take some that are only partly corroded. But Ed said: "No, no, I'll give you good samples. We will go through the list of what we have in our collection, and you tell me what you would like to have." So, I picked out about a dozen reasonably fresh-looking iron meteorites and he gave me samples of them. In our first publication on this subject, I think we had numbers on 20 iron meteorites. All except one had zero $^{39}$Ar.

UBM: Zero, really?

HW: Yes, the exception was Keen Mountain. We calculated a terrestrial age for it of ~1400 years. When we dissolved the meteorites in a vacuum to catch the $^{39}$Ar, we added carriers for chlorine, beryllium, and aluminum because we wanted to measure $^{36}$Cl, $^{10}$Be and $^{26}$Al in these same samples. And though we came across Tamarugal in which even $^{36}$Cl with a half-life of 300 000 years was below our detection limit, we found $^{36}$Cl in others indicating ages of ~200 000 years.

UBM: But nothing approaching 2 Ma except the Tamarugal iron?

HW: No, none of the others were nearly that old.

UBM: To put the events of your career in their proper sequence, were you still working with Paneth when you did your research on solar wind implantation of gases?
HW: No, Paneth had died before this. He died in 1958.

UBM: That means that you and Paneth were in Mainz together for only ~5 years.

HW: Yes, he died quite suddenly and unexpectedly. And this created a problem because, at that time, if your boss passed away, this was very, very bad for a young scientist anywhere in Europe. It was a problem for me and also for Friedrich Begemann who was in Mainz by that time. We two sat together and discussed what we should do. Of course, we could try to get positions at Heidelberg or at Bern, where there were groups working on such things as we were, but it was Fred who came up with the best idea. He said it probably would take years for the Max-Planck Society to find a successor for Paneth, and perhaps we should just try to stay for awhile.

UBM: Could you do that? Would they let you keep on working there and keep on paying you?

HW: Yes, they would. Begemann was quite right. I was somehow seen as the leader of the group and I was called to meet with Professor Josef Mattauch, the Director of the Institute. (Paneth was the director of a separate division.) Mattauch said to me: "I don't understand anything of the projects you are working on. But you obviously do quite well, so I would say that you should continue. You can have all the money the division receives and you also have the staff, so just continue for a while until we find a successor. Then, of course, I cannot promise anything."

UBM: That sounds like a remarkably agreeable arrangement.

HW: Yes, but you know, at that time, you easily could be fired, but we said OK, we'll stay. Then we tried to make good names for ourselves in the time we had.

UBM: So you carried on with research on cosmic radiation and solar wind implantation of gases.

HW: Yes, but meanwhile I got an offer from the University of Chicago.

UBM: To join the Enrico Fermi Institute?

HW: Yes. Edward Anders, Anthony Turkevich and Nathan Sugarman were behind this and I got a very nice offer from Chicago. I think that was in 1963. I almost accepted it. My wife, Grita, and I went to Chicago, looked around in the city, and even looked for an apartment. The Twin Towers close to the lake had just been built and I was very much impressed. It had a layer-cake structure. The first four floors, I think, were parking garages, and below them was a boat harbor with a tunnel from which one could go by boat straight out onto Lake Michigan. We were quite excited about it.

UBM: I would think so.

HW: I almost signed a contract, except that Herr Mattauch, who was still the acting director in Mainz, said: "Be careful and wait. We will do everything we can to get you Paneth's position." So, I waited. Ed Anders was a little unhappy that I didn't sign immediately, but I think, over the years, he has forgiven me.

UBM: I'm sure he must have. Eventually they found the new director for that division of the Max-Planck Institute right there at home, didn't they?

HW: Yes, but Begemann was right. It took years and years, while this interregnum went on. Finally, they offered it to Hans Suess. Hans came to Mainz for half a year to see if he would like it. Then he decided, no.

UBM: He liked La Jolla better?

HW: I think it was his wife who liked La Jolla better. Perhaps that's understandable.

UBM: I suppose it is. The Suesses had a lovely home overlooking the Pacific Ocean. I'm sure you remember that they held The Meteoritical Society banquet on their terrace in 1980, the year you were given the Leonard Medal. Just as you began your acceptance speech a fireball appeared behind you and broke into three or four pieces as it fell into the ocean.

HW: I heard people exclaiming about it, but I didn't see it.

UBM: So Paneth's job at the Institute was still open at that time?

HW: Yes. After Suess refused it they looked for a theoretical chemist, and after that an organic chemist. In the meantime, I had a gained quite a good reputation and so the President of the Max-Planck Society, Prof. Dr. A. Butenandt, came to Mainz and said to me that the future of the division was completely independent from my future. He promised to get me money for my group and make it independent of the division. This was towards the end of 1963.

UBM: A rearrangement of the bureaucracy? Very good.

HW: Very good. Finally I got the offer from Mainz to assume Paneth's position.

UBM: Eleven years or so after Paneth's death?

HW: Yes, the offer came in 1969. That was 3 years after NASA announced that they would go to the Moon in 1969 and issued a call for research proposals. We got very excited when this occurred. I think that we sent in 12 separate proposals from Mainz, explaining what we would like to do with samples from the Moon. And to our surprise, seven of them were accepted. The other five were very good, but the review panel thought they were outside our fields of expertise. Anyway, I have been told that in the initial allocations we got the largest amount of lunar material of any laboratory outside the United States. I became very heavily involved in the lunar analysis program of NASA.

UBM: Until the lunar samples came back no one could be quite sure that none of our familiar types of meteorites came from the Moon.

HW: Right, right. There were people who thought the achondrites come from the Moon. I thought the bronzite chondrites came from the Moon, and we all found out we were wrong. But the Moon is an interesting object in itself. I thought we should do as much as possible to analyze these highly valuable samples whatever they might be, so while we were waiting for them we developed a multi-element analysis program, in which we would do the least destructive analyses first and then more and more destructive ones in succession. First, we did instrumental neutron activation by fast neutrons and then by slow neutrons at different irradiation times. At
the very end, the samples were slightly radioactive but still present in their original forms.

UBM: Then, you could study their mineralogy optically or by x-ray or whatever method you wished.

HW: Yes, all the things that involved slowly taking samples apart. The multi-element analysis program we developed turned out to be very useful. It is written up in the Lunar Sourcebook.

UBM: Oh, yes, the User's Guide to the Moon, a collection of highlights of lunar information put together in 1991 by Grant Heiken, Dave Vaniman, and Bevan French. I have used it often to check back on all sorts of lunar research projects.

HW: In all, I think our Mainz group measured up to 56 elements on at least one of our lunar samples, and this was very useful. This is why our data look more coherent when compared to other data where major elements were measured by this group and a trace element from that group, and the other trace element from the third group. So it is no wonder that we found many of what we now call element correlations, which at that time were practically unknown in geochemistry. I must admit, though, that we were not the ones who found the first important one—the correlation of iron and manganese in the lunar samples, and how distinct it is from that in meteorites. J. C. Laul, of Oregon State University, discovered that. But we did find the potassium-lanthanum correlation, and many, many other element correlations that now are well understood.

UBM: I know you used these element correlations to make estimates on the bulk composition of the Moon.

HW: Yes, this is correct. Of course, I wanted to compare the data we obtained for the bulk composition of the Moon with that of the Earth's mantle. At that time all that was known about the composition of the Earth's mantle was based on the empirical pyrolite model of Ted Ringwood from Canberra, Australia. At that time I had a young, very bright student, Emil Jagoutz, with whom I had discussed my wish to have more precise data on the bulk composition of the Earth's mantle, including data on trace elements. He contacted several professors of the Geology Department of the Mainz University. It was Prof. Volker Lorenz who suggested that we should study spinel lherzolites—nodules brought from the upper mantle to the surface in violent volcanic eruptions.

UBM: Had not many people made analyses of such nodules at that time?

HW: Yes, but the data showed large variations. The problem was to find the most primitive of these nodules. I had the idea to use lithophile refractory elements with a low degree of incompatibility to the major mantle minerals to find unaltered primitive nodules, which should come close to the CI carbonaceous chondrite patterns for these elements. Emil Jagoutz developed a petrography scheme to distinguish such nodules. In 1969, Jagoutz was the lead author of a paper that reported our findings of the composition of the terrestrial mantle, which in fact turned out to come very close to Ted Ringwood's pyrolite composition. As a result, I got in contact with Ted and we became very close friends. Unfortunately, Ted died in 1993.

UBM: Can you give me a brief outline of your model?

HW: Inspired by Ted Ringwood, I proposed in 1981 the following two-component model to explain the element pattern of the Earth's mantle.

Component A: highly reduced and free of all elements with equal or higher volatility than Na, but containing all other elements in CI abundance ratios. Fe and all siderophile elements are in metallic form, and even part of Si occurs as metal.

Component B: oxidized and containing all elements, including the volatiles, in CI abundances. Fe and all siderophile and lithophile elements are present mainly as oxides.

For the Earth, we deduced a mixing ratio of components A to B of 85:15, and postulated an inhomogeneous accretion scenario. According to this model the accretion of the Earth started with the highly reduced Component A. We thought that Component A formed inside the present orbit of Mars where temperatures in the primordial solar nebula were high enough to reduce the matter and simultaneously to prevent condensation of volatile and moderately volatile elements. Segregation of the metallic FeNi phase, that is core formation, was supposed to be almost contemporaneous with accretion, leading to an FeO-free mantle during this stage. The high present-day mantle abundances of siderophile elements like Ni and Co, which in the case of Ni exceeds by far the amount to be expected from the metal-silicate partition coefficients, exclude equilibration with a pure FeNi phase. This is the reason for our conclusion that the oxidized Component B, which supplied FeO, NiO, CoO, and so on, to the mantle, was added in substantial amounts only after the Earth had reached about two-thirds of its present mass. In this inhomogeneous accretion model the major portion of the matter from Component B never was in equilibrium with larger amounts of metallic FeNi. We assumed that Component B formed outside the present orbit of Mars where water condensed as ice or formed hydrates.

UBM: How did the composition of the Earth's mantle compare with your estimates of the lunar composition?

HW: They agreed very well, especially in respect to some crucial elements. It turned out that the Moon showed a depletion in manganese and chromium similar to that of the Earth's mantle. This point became even more significant when we could show later on that the martian mantle is not depleted in these elements. Of course, volatile and moderately volatile elements showed a higher depletion on the Moon as compared to the Earth.

Another observation which pointed to a close genetic relationship between Earth and Moon was the almost identical depletion of the partially siderophile element, tungsten, in the silicate portions of the two bodies. At first there was no physical model, which could explain this similarity. However, the hypothesis of the impact-induced fission of the Earth for the
formation of the Moon accounts for almost all the observations. In 1986, together with Gerlind Dreibus, who became my close coworker for many years, I wrote the paper "Geochemical Evidence for the Formation of the Moon by Impact-Induced Fission of the Proto-Earth."

UBM: And this still is your favored theory?
HW: It is.

UBM: I know you also have investigated martian meteorites.

HW: We did. I was very interested when Don Bogard and Philip Johnson, of the Johnson Space Center, published their paper in 1983 on martian noble gases in an Antarctic meteorite.

UBM: The paper in which they reported a close match between the gases they extracted from bubbles in the shergottite, Elephant Moraine 79001, to those measured in the martian atmosphere by the Viking landers in 1976.

HW: Yes. They found an exact correspondence in relative abundances and isotopic species of the gases. There was an earlier paper, published in 1979 by John Wasson and George Wetherill, of the University of California at Los Angeles, that discussed a possible martian origin for the so-called SNC meteorites (shergottites, nakhlitites, and Chassigny). I was fascinated by these ideas. I said to my people, look, we must work on these meteorites. If they are from Mars, we will learn from them about Mars. If they are not from Mars, this might be even more interesting.

UBM: That's almost exactly what Don Bogard said in 1982 when he first reported his results to the Antarctic Meteorite Working Group. "If they don't come from Mars, they come from some very interesting planet."

HW: Yes, some other body large enough to keep erupting volcanic lavas for most of the age of the solar system.

UBM: There really wasn't any likely place except Mars.
HW: No, there wasn't. But many people didn't believe in this at all.

UBM: I know they didn't. Jim Arnold mentioned in his interview in this series that he didn't believe it for a long time...
HW: Right, right.

UBM: ...until you asked him to tell you just exactly where he thought they did come from.
HW: Right. He couldn't think of another place. So this makes a nice connection between the two interviews.

UBM: Numerous topics overlap in the interviews, so I get to hear interesting versions of the same subject from different points of view. So you went to work on SNCs?
HW: Yes, we went to work on SNCs and found them to be a small group of meteorites some of which are closely related to each other by their mineralogy and chemistry, and all have unusually low radiogenic ages. We began work on this, and then I gave a talk about our work at a meeting of the Visiting Scientific Committee on Cosmochemical Research at Mainz and Heidelberg. One member was the astrophysicist J. Mayo Greenberg from Leiden, The Netherlands. After I finished discussing the SNCs and the arguments for a martian origin, he told me that I talked as though I were describing an animal: it has four legs, it has a tail which it likes to wiggle, it also barks, but if it is a dog you are not absolutely sure. "But," he said, "from your arguments I will be sure: these meteorites are rocks from Mars." I answered that I was very glad he felt this way, but we still had a number of disbelievers close to us.

UBM: There were other papers published in 1979 that argued for martian origin or came close to doing so. One of them by David Walker, Edward Stolper, and Jim Hays, of Harvard, discussed the importance of the size of a planet that is required to keep volcanism active until relatively recent times. There also were papers by Hap McSween at the University of Tennessee and his colleagues and one by Larry Nyquist and his at the Johnson Space Center. This is an excellent example of how several groups were at work on the same problem separately but simultaneously in 1978–1979.

HW: Yes. But from the beginning, the need for a fairly large planet was one of the strongest arguments for martian origin. How can you have volcanic melting processes on anything except a large, well-insulated object as recently as...

UBM: 300 million years ago.
HW: Yes.

UBM: Or even 1300 million years ago.
HW: Even 1300 million years is still young compared to the age of the solar system.

UBM: What did you conclude from your studies of the SNCs?

HW: From the SNCs, we developed ideas about the chemical composition of Mars. It is a first step and I'm rather sure that what we've learned is correct, at least for the silicate mantle of the planet. We also predicted the size and composition of the core. I'm not so sure about that because, since the rocks tell us nothing about the core, I had to make certain assumptions. Our group published a number of papers on the composition of Mars. Our figures improved a little bit over the years but really didn't change much from those in the first paper we wrote in 1985.

Knowing the composition of the martian mantle, I was pleased that it fitted nicely into the two-component model, which we had developed for the Earth. Except that the portion of Component B was found to be ~40%, compared to 15% in the case of the Earth. Furthermore, Mars located at the border of the formation region of Components A and B, obviously was fed from both regions simultaneously, so we found for Mars an almost homogeneous accretion. The large amount of Component B results in a sulfur-rich core for Mars.

For our estimate about the core of Mars, we assumed that the iron to silicon ratio for the whole planet would be the solar (or CI chondrite) ratio. With this assumption, I could estimate the amount of metal in the core. We took the content of moderately volatile elements as a measure for sulfur, so that gave the sulfur content of the core. Our paper of 1988 is cited frequently and it became a basis of the martian exploration plans, so I think we understand Mars pretty well.
UBM: If I remember correctly, your group published on the Antarctic meteorite, Allan Hills 84001, in 1994 very shortly after David Mittlefeldt, at the Johnson Space Center, reported his identification of it as a martian meteorite. He said it was one that did not fit into the SNC group but showed certain resemblances to them. You thought it might possibly be a progenitor of the SNCs because, unlike every other known martian meteorite, this one was 4560 Ma.

HW: We did. We thought it might be a sample from an early reservoir that gave rise to the youthful igneous rocks, but we couldn't pinpoint a specific line of descent because, for example, its neodymium isotopes suggested that it could be ancestral to Nakhla, but its radiogenic strontium isotopes were closer to those of the shergottites.

UBM: Then, in 1996, the group led by David McKay, at the Johnson Space Center, announced possible evidence for past life on Mars that they discovered in the same meteorite.

HW: Yes, and overnight it became the most famous meteorite in the world. We left that problem to the life sciences people. But that meteorite certainly added a new rock type to the others we have from Mars. Now we need some sample return missions.

UBM: Three years ago we were thinking about possibly receiving samples from Mars in 2005 or 2008, or soon thereafter.

HW: Now we will have to wait much, much longer than that.

UBM: But it will happen.

HW: It will happen. This is an exciting time—a very exciting time. Enormous progress has been made during my career and it still is going on. So there you have my career. It started with a Ph.D. in Vienna, a short time in Durham, England, and a lifetime in Mainz, with, of course, visits here and there including 1 year in La Jolla in 1961.

UBM: Now I know how you happened, just by chance, to get interested in meteorites and you went, very reluctantly, to the Max-Planck Institute for Chemistry in Mainz, but then made your long and highly productive career there. Thank you very much, Heinrich.

HW: My pleasure, Ursula.

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SELECTED REFERENCES


