HANS SUESS

1909 – 1993

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15

Dr. Hans Suess, 1972.

I have never known another scientist quite like Hans Suess. I hope I can convey the qualities that made him such an interesting and productive scientist, and the contributions he made to our understanding of the earth's atmosphere-ocean system.

Scholarly dynasties are much more common in Europe than in the United States. Suess was a member of such a dynasty; both his grandfather, Eduard Suess, and his father, Franz Eduard Suess, were distinguished earth scientists. Once early in our friendship Hans showed me two of his grandfather's nineteenth century field notebooks. They contained not only clear, legible notes but also meticulous pen and ink drawings of geological features, much clearer it seemed than any photograph could be. Something gained, something lost. Hans carried with him visible traces of this old world culture.

He was born in Vienna and brought up in the small town of Maertz, Austria, not far from the Hungarian border. When Harold Urey, Suess, and I attended a U.N. sponsored international conference in Vienna in 1968 on meteorites, Hans invited us to visit his mother there. It was a memorable occasion. The eastern part of Austria had only recently been liberated from Soviet occupation. The plowed, barbed-wire bounded no-man's-land, with heavily armed guards visible on the Hungarian side, was not far away.

Suess received his university education, including a Ph.D. in physical chemistry in 1935, at the University of Vienna. His first academic position was as a research associate at the University of Hamburg, Germany, beginning in 1937 and extending through the disastrous years of World War II. He spent some time in those years at the Norsk Hydro plant in Norway, representing the German nuclear project at their source of D_2O, "heavy water." He felt fortunate not to have been sent into the front lines during this period—especially the Russian front lines.

Hans and his family were living in Hamburg during the firestorm produced by saturation bombing of the city late in the war. He told one unforgettable story about that experience. It seems that all but essential workers were evacuated by train after the fire, but there was no obvious safe place to send them, so they spent long intervals stopped on track sidings while military trains went through. Once he went for a walk along the track, and happened to find a ring of keys lying on the ground. He thought he should try to find the owner, so he held them up and called out to the passengers "Whose keys are these?" The response was a roar of laughter. Everyone started throwing their keys at him as well—the doors corresponding to them no longer existed. We in the United States had no such experiences.

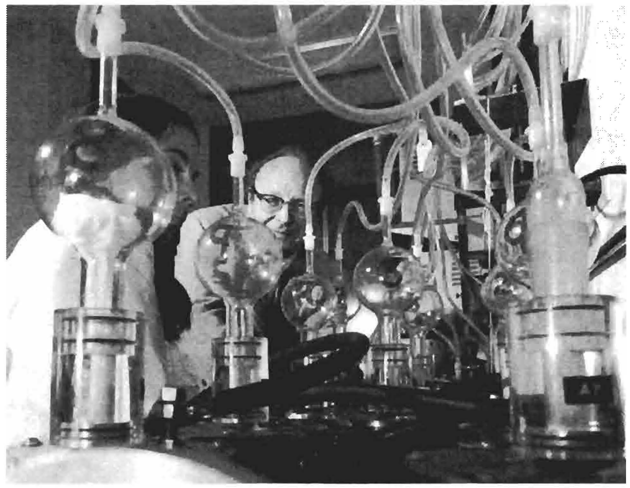
It was in the aftermath of the war, under difficult conditions, that his first major scientific paper was published.[1] By coincidence, "Zur Interpretation der ausgezeichneten Nucleonenzahlen im Bau der Atomkerne" by O. Haxel, H. Jensen, and H. Suess, *Naturwissenschaften* 36, 376 (1948), and a paper in *Physical Review* on the same subject by Maria Mayer, were submitted on the same day. Both were published promptly, and each announced not only the list of "magic numbers" of unusually stable and abundant nuclides, but the physics responsible, the so-called spin-orbit coupling. These authors published several follow-on papers, and were quickly joined by other physicists on both sides of the Atlantic. A new branch of nuclear physics had been opened, which still has work in progress today.

The role that Hans Suess played in this discovery seems clear. He was a chemist with an unusually broad understanding of his field, but he was not a specialist in nuclear physics. He could identify the magic numbers empirically, but he needed an expert to produce the theory that explained their existence. Knowing him well, I have no hesitation in accepting his own account, including his statement that he had to become "quite a pest" before Hans Jensen accepted the challenge and solved the physics problem. Haxel did not get much mention in his account. Jensen later shared the Nobel Prize with Mayer.

It was this discovery paper that led directly to his transfer from West Germany to this country. He soon was invited to visit the University of Chicago's Institute for Nuclear Studies (now the Fermi Institute) to meet his friendly competitors. Then Harold Urey invited him to spend a year working with him on problems of mutual interest. As it happened, a very young co-worker with Willard Libby on the development of C-14 dating (myself) was working in the next-door laboratory. Libby's work was just at the point of producing the first paper, which showed success in matching C-14 dates with ancient samples of known age. The institute was one of the most exciting places on the earth for a physical scientist at that time, and Suess saw the opportunity it presented.

The year 1951 signaled the start of his new career. Suess's first papers in English appeared in U.S. journals, some in collaboration with Chicago authors. In later years, only an occasional work was published in German. He continued to visit German laboratories, particularly the Max Planck Institute at Mainz University, for the rest of his life, but his research center was henceforth on this side of the Atlantic.

This was also the year in which Dr. Libby gave me my last assignment under his direction, which was to instruct a few other scientists (I believe four) in the techniques we had developed for C-14 dating, to enable them to establish laboratories of their own. Libby was very clear that he wanted no part of a monopoly on his dating method. "I don't want to be the pope of archeological dating" was the way he sometimes put it.



Laboratory technician Karen Dockins and Dr. H. Suess check the operation of an electrolysis plant. The detection of tritium in certain water samples involves delicate measurements.

Typically my sessions with these newcomers consisted first of demonstrations of the procedures step by step, followed by their repeating each step with suggestions and corrections from me. That is how it went in all cases but one—Hans Suess. He let me run through our procedures all right, but then he lost interest. He already had a better idea for the main step, converting the carbon in the sample to a form suitable for the decay counting rate step.

By then I was familiar with Libby's style in such matters: keep trying until we found a method that worked, then never make the smallest change not demanded by some serious problem. The other "students" wanted to get going as quickly as possible, so they followed us in every detail. Not Hans. His method was original, and found very few followers as the number of dating labs increased. It was to convert the sample to be counted not to a solid (finely divided carbon black) such as Libby used for many years thereafter, but to a gas, acetylene, C_2H_2. Why? Suess liked it because it had two atoms of carbon per molecule, while other labs chose other gases, mostly methane, CH_4, or sometimes carbon dioxide, CO_2, with only one. So he got twice as many carbon atoms and twice the number of decay events at a given pressure and volume. Why did others not follow him? Because by itself acetylene is explosive under some conditions, an event that Hans successfully avoided throughout his career.

His whole approach to laboratory work was to think calmly and deeply about the plan, and then do it right the first time. He might repeat the process a few times to become sure, then turn the day-to-day lab work over to a technician, or sometimes a student. His role was to calculate the results and plot them, and to write the papers.

Some details of his laboratory technique made good stories. I'll give two examples. His first successful C-14 lab was established at the U.S. Geological Survey in Washington D.C. Meyer Rubin, his first assistant, became his successor there when Hans moved to Scripps in the mid-1950s. The counting system worked well for about a year, after which it was necessary to call in an expert to diagnose and cure the malfunction. As they were taking it apart, Rubin remarked "Probably the C-14 counter's high voltage center wire will be attached to the electronics by a paper clip." It was.

While still in Washington, Hans had published his first C-14 papers. The most striking of these reported a set of results from wood samples associated with the retreat of the most recent North American ice sheet.[2] He sent a draft of the paper to Libby in Chicago, who showed it to me. Libby was most pleased, because it was the first important C-14 paper produced by a laboratory outside Chicago. He felt his work was now secure.

Roger Revelle recruited Hans Suess to join Scripps in 1955. After his arrival it took several years before his complete C-14 system was ready for use. I arrived in 1958, and was present one day when workmen, who were soldering his iron counter shield together using an acetylene torch, had gone off for lunch. Hans came over to my lunch table on the grass looking very pleased with himself. He had just disconnected the acetylene tank from the solder gun and directed the acetylene gas flow through the counting system inside. Then he had turned on the counter and it worked! By the time the workmen returned he had concealed the evidence.

Another major paper in this period authored by Suess and Harold Urey treated the abundance of the chemical elements in nature.[3] The famous paper called "B^2FH" (by Burbidge, Burbidge, Fowler, and Hoyle) on the origin of the chemical elements in stars, published in the following year, acknowledged this paper as the inspiration and main source of data for their work.

The next important product of his presence at Scripps was a paper written jointly by Revelle and Suess on exchange rates of CO_2 among the major reservoirs in the biosphere, particularly the atmosphere and the oceans.[4] When Revelle was awarded the Tyler Prize late in his life the citation listed this paper as the first to call attention to the likelihood of global warming as a consequence of increasing fossil fuel burning.

Another paper by Suess on C-14 measurements was published in 1959.[5] It reported the record of increase (briefly a doubling) in worldwide atmospheric C-14 as the result of the hydrogen bomb testing by the U.S. and the Soviet Union. This is also the first paper on which the name of George Bien appears as a co-author. The close collaboration between Suess and Bien continued for many years thereafter. Generally Bien did the measurements and Suess the calculations and interpretation.

This paper was followed by a first Scripps date line.[6] The next C-14 paper was another landmark.[7] It reported the first extensive series of measurements of C-14 activity in carbonate from sea water, specifically in the Pacific Ocean, at various locations and depths accessible during cruises of the Scripps research fleet. The purpose was to gather data that could shed light on the movement and mixing of ocean water on a wide scale. The techniques for gathering seawater samples, and extracting the CO_2 from them on board ship, while avoiding contamination or loss, had to be developed first. In modeling the data, the effect of bomb-produced C-14 had to be taken into account, especially in the surface layers of the ocean.

The exchange of CO_2 across the air-sea boundary was shown to require a significant time. The transport of carbonate ions to deep Pacific water, and horizontally as well, required much longer, more than a thousand years in some cases. Suess's pioneering measurements provided a framework for the much more extensive surveys that followed. By now he had published in a number of important fields, and he continued to widen his interests. A paper with Heinrich Waenke on C-14 in meteorites was the first of a long and valuable series of papers on meteorites and the light they shed on various processes in the solar system.[8] It was soon followed by another paper proposing a mechanism for the synthesis and accumulation of organic compounds in planetary atmospheres, with implications for the origin of life.[9]

A field that preoccupied Suess until the end of his scientific career first surfaced in the mid 1960s.[10,11] Its practical side was to produce precise corrections of C-14 dates for variations in the production of C-14 in the atmosphere. This could be determined by counting rings in suitable tree ring sections, an idea first developed at the University of Northern Arizona into a reliable dating tool. Hessel De Vries in the Netherlands had been the first to show that in the 17th century AD (more precisely during the reign of King Louis XIV) there was a measurable increase in the flux of cosmic rays entering the earth's atmosphere, making C-14 ages in this period significantly too young. Suess undertook to extend this work backward in time, using overlapping tree samples made available by experts in Arizona and in Europe. After many years he was to produce a widely accepted calibration curve going back eventually over 8,000 years. He demonstrated the existence of several other disturbances resembling that of the Louis XIV period. The shape of the calibration curve in such times has one unexpectedly ugly feature: for some decades during each such event the same level of activity can result from three separate dates, which may be spaced more than a hundred years apart.

In 1971 the "water library" may have been the most extensive collection of ocean and rainwater. Samples from around the world, from land stations and ships were received weekly at the library. Pictured is Dr. Suess at the Scripps Radiocarbon and Tritium Laboratory with Dockins logging in a sample just received from Majuro, an atoll in the Marshall Islands.

In addition to their practical utility, these data suggested that a number of interesting processes might be responsible for the departures observed. Variations in the strength of the earth's magnetic field are known to occur, especially in periods when the polarity of the field is reversing. A decrease in field strength lets more cosmic rays reach the earth (and conversely). Another is sunspot intensity variations, because it is recorded that in the period reported by de Vries, the "Maunder Minimum," sunspots became very rare, again changing the flux of particles entering our atmosphere. This was a valuable contribution to the subject now called C-14 geophysics.

These pages should give at least some idea of the strength and breadth of Hans Suess's scientific interests and achievements. I have been much less successful, I fear, in portraying his remarkable personal style. Perhaps a couple of examples may help.

He was very much interested in the terms that others used to characterize particular phenomena. For example, at one period the leading students studying rare gases embedded in stone or iron meteorites called one component "primordial rare gases." He saw that the word "primordial," meaning "present from the beginning," implied a model for which the evidence was weak, or even nonexistent. His response was to identify a colleague highly regarded in that field, in this case Peter Signer, and write a joint paper that introduced the term "trapped rare gases" instead. This paper changed the culture in the field quickly, because it eliminated what could now be seen to have been a false assumption.

Another example is closer to home. For a few years after a core group of chemistry professors (including Suess) formed UCSD's chemistry department, I taught the graduate course in quantum mechanics, an esoteric subject underlying all of chemistry. I taught from a textbook I'd studied in college, which in some ways seemed to me more like a cookbook than an insightful memoir. Then someone showed me a wonderful book by two Soviet theorists, L. D. Landau and E. M. Lifschitz, which introduced the subject from very clear first principles. So I used their text the next year. The students' eyes glazed over. No interest whatever. I told Hans my sad story, and he explained the effect for me. "People often confuse two unrelated ideas" he said. "One is 'simple' and the other is 'elementary'." The new book was simple—and so was his explanation.

Hans Suess was a professor at Scripps and in the Department of Chemistry at UCSD during most of his scientific career. He was a member of the National Academy of Sciences and received many other honors. However, he always felt he was under-appreciated, and as a friend I shared that opinion. I believe that the main root of this problem was that he had some of his best ideas "too soon," that is, before the rest of us had seen the steps between what was familiar and his new perception. He was going too fast for us. Still he enriched the lives of those of us who had the good sense to admire him, and to listen to him.

Footnotes

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