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HANS E. SUESS
1909–1993

A Biographical Memoir by

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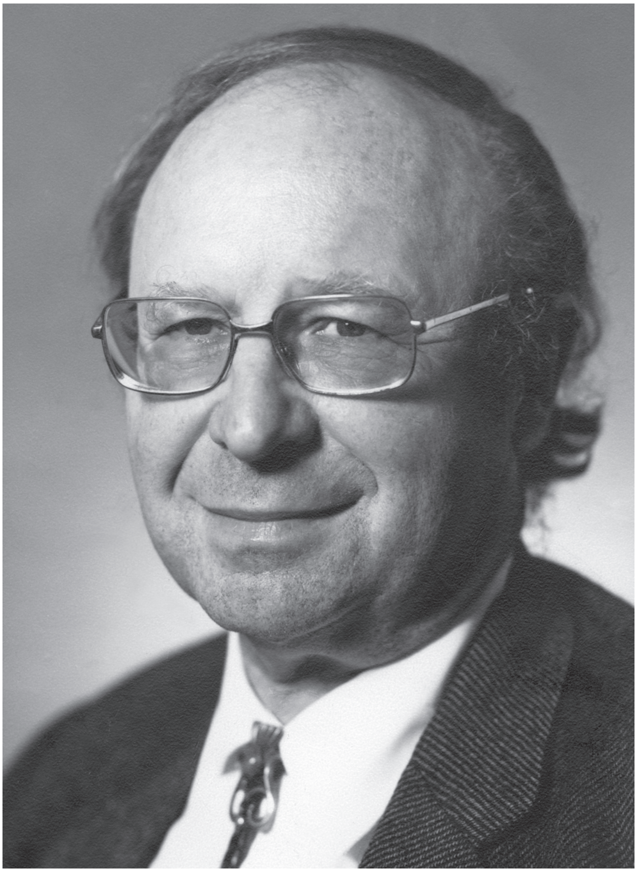

Hans Suess

HANS E. SUESS

December 16, 1909–September 20, 1993

BY HEINRICH WAENKE AND JAMES R. ARNOLD

CONTRIBUTION BY HEINRICH WAENKE

HANS E. SUESS WAS A member of a dynasty of famous Austrian scientists. The founder of this dynasty was his grandfather Eduard Suess (1838-1914). He was a professor at the University of Vienna (1857-1908) and president of the Austrian Academy of Sciences (1898-1911). He became well known in geology and paleontology, especially by his book *The Face of the Earth* (in German: *Das Anlitz der Erde*), which became fundamental in geology and geotectonics. For the first time he not only described geological phenomena but also tried to find physical and geological reasons for them. The book has been translated from German into all the major languages. Aside from his work as a scientist, Eduard Suess was engaged in politics and became the first member of the City Council of Vienna and later on a member of the Austrian Parliament. His name is also connected with a new system for the supply of water brought from the Alps to Vienna over about 200 km, which he designed and fought for its realization politically.

The father of Hans E. Suess was Franz Eduard Suess (1867-1941). His field was geology and petrography, and he became a professor at the University of Vienna in 1908.

Hans E. Suess, born in 1909 in Vienna, grew up in an environment of scientific excellence. He received in his young years a good intuition as to what could be right and what probably was wrong. In his acceptance speech for the Leonard Medal of the Meteoritical Society, he said, "When I was a little boy, I was told all about continental drift and plate tectonics, and how mountains were folded asymmetrically. Later, however, I was told by others that this was all fantasy."

Hans studied physical chemistry at the University of Vienna. He received his Ph.D. in 1936. Two years earlier appeared his first publication on experimental studies with heavy water (only discovered two years earlier), which dealt on the inversion of cane sugar in mixtures of light and heavy water (1954,1). He became especially interested in the reaction rates and equilibria in solutions of heavy water (1956; Goldschmidt, 1954), but Hans also worked on topics like the kinetic of thermal polymerization of dissolved styrene (Burbidge et al., 1957; Revelle and Suess, 1957) and other problems in physical chemistry like the thermal disintegration of dioxane (Suess, 1954b).

The first 10 papers Hans published in the first five years of his career show the wide spectrum of his interests. Aside from the papers just mentioned, there is one dealing with photochemistry of the Earth's atmosphere (1959), two on the radioactivity of potassium and its use for the determination of the age of elements in meteorites (1960,1,2), and three on capture reactions of thermal neutrons. He had irradiated gaseous ethylbromide with thermal neutrons and found that in the gas phase all activated bromine atoms were set free and could be separated to 100 percent. The later investigations were carried out in Hamburg at the Institute for Physical Chemistry, to which Hans had moved in 1938. Earlier he had visited Zurich to do research at the ETH (Swiss Technical University).

During World War II, Hans Suess worked especially on exchange equilibria of $H_2 + HDO \leftrightarrow HD + H_2O$. He became an expert on heavy water and a scientific advisor to Norsk Hydro, the Norwegian plant in Vemork, producing hydrogen by electrolyzing water and as a by-product of heavy water. In this capacity Suess was sent several times to the heavy-water plant in Norway. For these trips he was allowed to travel through Sweden. That is why he liked these trips very much: He was able to buy goods in Sweden that had long since disappeared from German shops.

Aside from his work with heavy water he started working on the cosmic abundance of the elements, a topic that he continued to work on for decades. Following the pioneering work of V. M. Goldschmidt by plotting mass numbers versus cosmic abundances, Suess found smooth curves for both nuclei with even mass numbers as well as for odd mass numbers. In this way he was able to correct the abundances of elements for which the experimental data were uncertain. In the graphs that were made with the corrected abundance values, the nuclei with certain proton and neutron numbers (magic numbers) were easy to recognize. The two most distinct numbers N and Z = 50 and 82 had been pointed out previously by Elsasser in 1933 and 1934.

The papers of Hans Suess dealing with the cosmic abundances of elements played a fundamental role in the physical explanation of "magic numbers." These numbers not only showed up in the cosmic abundances but also in various properties of the nuclei like the binding energy or the neutron absorption cross-sections. At that point the topic became more a problem of nuclear physics. For that reason Hans joined forces with two physicists, first with Hans Jensen (Heidelberg) and later also with Otto Haxel (Göttingen).

The breakthrough came with the assumption of a strong spin-orbit coupling in the nuclei. The energy levels of a single nucleon in the potential of the rest-nuclei split due to the strong spin-orbit coupling in such with parallel and antiparallel position of spin and orbit. Filling the energy levels with protons or neutrons Haxel, Jensen, and Suess (1949, 1950) could with this model show that binding energy for nuclei with a magic number becomes larger compared with neighboring nuclei just as observed. They could also show that the magic numbers (2, 8, 20, and 28, 50, 82, 126) in fact belonged to two different series (the smaller three and the larger four). This break in the series of magic numbers was explained in the following way. Nuclei with smaller mass numbers have a weak spin-orbit coupling, and the corresponding shells follow the energy levels given by the orbital momenta. For mass numbers higher than 20 the spin-orbit coupling becomes dominant, and the energy levels are governed by the total angular momenta.

In his book *Chemistry of the Solar System* (1987) Hans Suess downplayed his own contributions in respect to the magic numbers and the related breakthrough of the shell model for atomic nuclei by writing in his book: "In 1948, Maria G. Mayer published convincing evidence for the significance of the magic numbers in nuclear structure, and two years later, she succeeded in postulating a theory to explain them. The same explanation was proposed at exactly the same time completely independently by Haxel, Jensen, and Suess" (1949, 1950).

Aside from detailed papers on the shell model of atomic nuclei by Haxel, Jensen, and Suess (authors in varying sequences), there are among the papers Hans Suess published after the war (before he left Hamburg to move to the United States) two on the radioactivity of potassium-40. He questioned the then-new measurements on the branching ratio of the decay of potassium-40, in which for the ratio of K capture to total decay rate values of up to 0.78

had been obtained. Using geochemical evidence, Suess concluded that a value of 5 ± 2 percent for the K capture fraction to be more likely, or definitely less than 10 percent. In this respect F. G. Houtermans once joked, "Suess is noted for the fact that he comes to the right conclusions on the basis of very scanty evidence or no evidence at all."

Another important paper of this period was the one on the abundance of rare gases in the Earth's atmosphere. Suess stated that the ratio of the number of xenon atoms in the atmosphere to the number of silicon atoms in the bulk earth is about 10^7 times smaller on the Earth than in the universe, whereas for neon this figure exceeds 10^{11}. He further found that abundances of Ne, Ar-36 and Ar-38, Kr and Xe follow an exponential function suggesting loss to space by selective diffusion and stated that a proof could be obtained by studying the isotopic ratios of neon and argon, as the lighter isotopes should be preferentially lost leading to a decrease in the neon-20/neon-22 and argon-36/argon-38 ratios. The proof came from the discovery of the solar wind implanted rare gases, which clearly showed that in the terrestrial atmosphere the light isotopes of neon and argon are indeed depleted.

In 1949 Harteck and Suess published a short note on deuterium content of the hydrogen of the Earth's atmosphere. The Linde Company producing rare gases by separation from air provided a fraction of what they called "raw" neon, which contained besides helium and neon also hydrogen from the atmosphere. From a total of several $10^5 m^3$ air each; two samples of 30 cm^3 water were obtained. From the density of the water a 25 ± 7 percent higher deuterium content was deduced and explained as preferential removal of the lighter isotope in the continuous loss of hydrogen from the atmosphere to space.

CONTRIBUTION BY JAMES R. ARNOLD

Scholarly dynasties are much commoner in Europe than in this country. Hans Suess was fortunate as a member of such a dynasty. Both his grandfather Eduard Suess and his father Franz Eduard Suess were distinguished earth scientists in their day. Hans Suess was born on December 16, 1909, and was brought up in the center of Vienna. His vacations were spent frequently in Maerz, a small town not far from the Hungarian border, where his grandfather had built a representative summer villa within a large park.

On one occasion he showed us two of his grandfather's nineteenth-century field notebooks. They contained not only clear, legible notes but also skillful pen and ink drawings of geological features, much clearer it seemed than any photograph could be. Something gained, something lost. Hans Suess carried with him visible traces of this old world culture.

His first trip to the United States in the aftermath of World War II resulted directly from the coincidence of the two papers announcing the shell theory of nuclear structure, which were submitted on both sides of the Atlantic Ocean on the same day. It was the German discovery paper that led after some time to his transfer from West Germany to this country. He was soon invited to visit the University of Chicago's Institute for Nuclear Studies (now for many years the Fermi Institute) and to meet his friendly competitors there. He met with Maria Mayer at that time and gave a seminar that drew a large audience.

During that first extended visit he became acquainted with the work of Prof. Willard Libby on the development of C-14 dating, which was being carried on (in part) in the next-door laboratory to his own. 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 Earth for a physical scientist at that time, and among the many interesting projects under way there this one attracted his particular interest.

Suess saw the possibilities of the technique at once, especially for its application to the climatic history of the Earth, most of all that portion of the record that can be reached by the radioactive isotope C-14, that is to say, the period from the later portion of the most recent ice age to the modern era. This subject of climatic history was familiar to him from the researches of his father and grandfather.

By the time Libby and coworkers had established the basic validity of the method in the early 1950s, Suess had begun to publish papers in English and to establish himself in the United States. Libby was beginning to offer instruction in the technique of the method to a few interested persons, and Suess was among them. In Libby's laboratory the samples for C-14 dating were prepared for counting by converting their carbon content to solid carbon, which was inserted in counters designed for their measurement. All but one of the early users of the method followed his procedures in detail. The exception was Hans Suess, who saw a virtue in counting the samples in the gas phase. The gas he chose was acetylene, C_2H_2, because of its high content of carbon (1954,1). Soon many others chose to use gases, but they all avoided acetylene because it is known to explode under some conditions. Suess quietly used it without problems throughout his career.

Although his research work from then on was carried out mainly in the United States, he continued to visit Germany and the German laboratories in his field, particularly the Max-Planck-Institut for Chemistry at Mainz, for the rest of his active life.

His own group's first successful C-14 laboratory was created at the U.S. Geological Survey in Washington, D.C. His first "date list" (1954,2) was published in 1954; three years after Libby's first list had appeared. His early results came mainly from samples of wood collected from locations in the northern U.S. states, usually found as stumps or logs knocked over by the advancing ice sheet. The dates clarified and extended the few measurements earlier reported by Libby's group. This subject continued to be central to his studies in the few years he remained in Washington.

Libby was particularly pleased by this first paper, as it provided proof that important C-14 results could be obtained by other workers than him. His remark to Suess and others was, "I never wanted to be the pope of C-14 dates." The number of productive C-14 laboratories increased rather rapidly thereafter in countries around the world.

Suess's work in this period was by no means confined to C-14 measurements. Another strong interest was the abundance in the sun and in meteorites of the chemical elements and their isotopes. This subject had made important progress through the work of V. M. Goldschmidt, which was summarized in a book (Goldschmidt, 1954) providing him with a starting point. However, the literature still contained many erroneous values, most of them too high, as was becoming apparent.

Suess's approach to laboratory work was to think calmly and thoroughly about the plan, and then do it right the first time. He might modify the system in small ways and then turn the day-to-day lab work over to a technician, or later sometimes to a student. His role was to calculate the results and then to write the paper.

Some details of his laboratory technique made good stories. I'll give two examples. His first successful C-14 laboratory 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 California. The counting system worked well for about a year, after which it was necessary to call in an expert to diagnose and cure a 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.

With the improvements in analytical techniques and progress in understanding the structure of the sun and other stars, improved chemical abundance data were becoming available. A paper by Suess and Harold Urey (1956) produced a further large step forward. Here one of Suess's most striking qualities was demonstrated, namely his remarkable ability to pick out correct values from a mass of unreliable numbers. His intuition in such matters was proverbial, and seldom failed. It was helped by his earlier work on the "magic numbers," which had led to the shell theory of nuclear structure and which also pointed to elements with especially high abundance. The graphs in that paper set the style for further improvements as data and theories became more reliable. Especially notable was the paper universally referenced since as B^2FH (Burbidge et al., 1957), which created the modern theory of the origin of the elements in stars, making much use of the Suess-Urey data.

It was soon after the appearance of these papers that Suess was recruited by Roger Revelle to join the small group of geochemists and geophysicists at what was soon to be known as the University of California, San Diego. Still housed in the Scripps Institution of Oceanography, this small group was the nucleus of an idea that emerged in response to the shock the United States had experienced when the Soviet Union was the first nation to launch a satellite into orbit around the Earth. It took some years for Suess's new C-14 laboratory to come into full existence, but in the meantime he could do some interesting theoretical work.

He joined Scripps Institution of Oceanography in 1955. After his arrival, it required years before his complete C-14 system was ready for use. I arrived in 1958 and was present one day when workers who were soldering his iron counter shield together using an acetylene torch had gone off for lunch. Hans came over to our lunch table on the grass looking very pleased. He had just disconnected the workers' acetylene tank from their 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 from lunch he had concealed the evidence.

The first paper in a series, written by Roger Revelle and Suess (Revelle and Suess, 1957), was one of three that appeared in a single issue of the European journal *Tellus.* The others, covering much the same ground, were by Ernest Anderson and myself, and by Harmon Craig. These papers signaled the creation of a field with a new name, C-14 geochemistry, by analyzing the distribution of CO_2 among the major terrestrial reservoirs of carbon dioxide, the atmosphere, land vegetation, and the ocean. All the papers calculated in various ways the time rate of exchange of carbon dioxide among the reservoirs.

The Suess-Revelle paper was, however, the only one of the three to stress the growing quantity of CO_2 contributed by our burning of fossil fuel, and to call attention to the fact that it might cause global warming over time. This was later mentioned in the prize awarded to Revelle as the effect became visible.

Another paper by Suess on C-14 measurements was published in 1959. It reported the record of increase (briefly a doubling) in worldwide atmospheric C-14 as the result of the hydrogen bomb testing by the United States and the Soviet Union. This was also the first paper on which the name of George Bien appeared as a coauthor. 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 Institution of Oceanography date list (1960,1). The next C-14 paper (1960,2) was another landmark. It reported the first extensive series of measurements of C-14 activity in carbonate from seawater, specifically in the Pacific Ocean, at various locations and depths accessible during cruises of the institution's research fleet. The purpose was to gather data that could be used to 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 onboard 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 in this paper to require a significant time. The transport of carbonate ions to deep Pacific water, and horizontally as well, required a much longer period, 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 (1962,2) 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. It was soon followed by another (Suess, 1962) proposing a mechanism for the synthesis and accumulation of organic compounds in planetary atmospheres, with implications for the origin of life.

A field that preoccupied him until the end of his scientific career first surfaced in the mid-1960s (Suess, 1965; Stuiver and Suess, 1966). 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 Arizona into a reliable dating tool. De Vries in the Netherlands had been the first to show that in the seventeenth century (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. Over 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 this period. The shape of the calibration curve in such times has one unexpectedly ugly feature: For some decades during each such event the same activity can result from three separate dates, which may be spaced more than a hundred years apart.

In addition to their practical utility, these data suggested that a number of interesting processes might be responsible for the departures seen. 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, since 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.

A theme that occurs often in the course of Suess's scientific work was "too soon." He often saw the deeper implications of his own work and that of others before they did. He often found frustration in their inability to grasp the clues that led him forward. The writers of this memoir more than once lacked vision in this situation. A related quality that was more widely appreciated was his ability to estimate quantities not yet determined by reliable experiments, for example, the abundance of some important elements in our sun (Revelle and Suess, 1957). The reason, perhaps, might have been the shorter time between his estimates and their experimental confirmation, but also that it is easier to grasp a successful estimate of a numerical quantity than that of a broadly applicable concept.

He was very much interested in the terms that others used to characterize particular phenomena. For example, at one period the leading students of 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 (1963,2) that introduced the term "trapped rare gases" instead. This paper changed the culture in the field quickly, since 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 the Chemistry Department of the University of California, San Diego, I taught the graduate course in quantum mechanics, an esoteric subject underlying all of chemistry. I taught from a textbook I'd learned it from 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, Landau and Lifschitz, which introduced the subject from very clear first principles. So I used their text the next year. The students' eyes glazed over. They had 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.

Those meeting him for the first time often came to the conclusion that he didn't work very hard. This resulted rather from the fact that he operated by picking important research problems, and spent a great deal of time thinking about them, with experimental work only used to confirm insights already arrived at, and to set the stage for the next steps. Not rarely he would ask his American friends, busy with proposals, organizing conferences, department meetings, and so on, "When do you have time to think?" There was no good answer.

Though his later scientific work was carried out almost entirely in the United States, he maintained close ties with friends in Europe. He traveled there often, and when that was not possible the transatlantic telephone was pressed into service.

Hans Suess was a professor at SIO and in the Department of Chemistry at UCSD during most of his scientific career. He was elected to the National Academy of Sciences in 1966 and received other honors as well. 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.

He lived in quiet retirement in his last few years and died of complex causes on September 20, 1990, at the age of 83.

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