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Tektites, Apollo, the Crust, and Planets: A Life with Trace Elements

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Abstract

Stuart Ross Taylor, MSc (University of New Zealand), PhD (Indiana University), ScD (University of Oxford), FAA, AC, always called Ross, grew up on a farm near Ashburton, New Zealand. Ross has worked on a wide variety of topics in trace element geochemistry, including the composition and evolution of the Moon, the continental crust, tektites, impact glasses, and island arc rocks. In 1969 he carried out the first chemical analysis of the first returned lunar sample at NASA in Houston. He has published 10 books and 240 papers in scientific journals. He was awarded the V.M. Goldschmidt Award of the Geochemical Society in 1993. In 1994 he was elected a Foreign Associate of the National Academy of Sciences. In 1998, he was awarded the Leonard Medal of the Meteoritical Society, in 2002 the Bucher Medal of the American Geophysical Union, and in 2012 the Shoemaker Distinguished Lunar Scientist Medal of the NASA Lunar Science Institute. Asteroid 5670 is named Rosstaylor.

ANCESTORS

My father was born in 1891. He was descended from a long line of farmers and worked on the family farm at Ashton, New Zealand, before moving with his parents to the Wakanui district, southeast of Ashburton, where my grandfather, who had emigrated from Northern Ireland, built the house (still standing) in which I grew up. My father served in the Great War, where he took part in the famous disaster involving the New Zealand Division on October 12, 1917, at Passchendaele. He was very lucky to survive, having spent the day trapped in an artillery shell hole against uncut German barbed wire. As a result of this debacle he remained skeptical of the British High Command, although he was tolerant of the German troops. He spent two years in the trenches of the Western Front and was wounded twice. Despite an aversion to things military, he commanded the local Home Guard in World War II.

FAMILY

My father met my mother, a primary school teacher at a local school, after the war. My mother, of Welsh and Irish descent, was a very strong character who had lost her favorite brother in another Great War disaster, on the Somme on September 27, 1916. A worse tragedy for my mother was the loss of her firstborn son, Lloyd, when he was killed in a traffic accident on Saturday, December 28, 1946. Mum bore this appalling loss stoically.

Nevertheless, she had a somewhat romantic Celtic view of life. She strongly encouraged us to get a good education. After my father died in 1948 at the young age of 56, worn out by the Great War, the 1930s depression, illness, and the loss of Lloyd, I offered to stay and work on the farm. I had just graduated with a BSc. She refused this offer, which in retrospect would have been greatly to her advantage, insisting that I continue to study geology and eventually largely supporting my travel overseas to Indiana University and helping me in many other ways.

My brother Lloyd and I were the first members of our family to attend a university. Lloyd, a law student, enlisted in the Royal New Zealand Air Force, became a flying officer, and served in 490 Squadron operating Sunderland flying boats on antisubmarine patrols from Freetown, Sierra Leone, in West Africa from 1944 to 1945. Forbes, my younger brother, volunteered for service in the Korean War in 1952 and became a lieutenant in the signal corps. Afterward, he became a farmer and hotel owner before retiring to Christchurch. He has now become head of the New Zealand Korean Veterans Association. Forbes was awarded the British Empire Medal in 1985.

EARLY LIFE

I was born on November 26, 1925, in Ashburton, New Zealand, and with Scottish, Irish, and Welsh ancestry, I can claim to be a Celt. I grew up in a mainly Presbyterian farming community, attending both church and Sunday school regularly, although I recall neither much attempt at indoctrination nor any mention of hell. But the Great War and its consequences permeated the whole society. I began my education at the Wakanui Primary School from 1931 to 1938. There were two teachers, usually female, and about 40 pupils. My mother had taught me to read before I started school. There is an unsubstantiated family story that I didn't begin to talk until the age of three. My mother talked a great deal, so perhaps I just listened. I was always a good student and top of my class, which usually contained three or four pupils. One of my early teachers, Miss Cowan, complimented my mother on my writing style in early essays when I was about seven or eight years old. In 1936 I developed peritonitis. Only the skill of a local doctor saved me in those preantibiotic days.

In 1939 I started at Ashburton High School. All the teachers had master's degrees (in those pre-PhD days) from the University of New Zealand. I rode a bicycle six miles to high school in all weather, as petrol was rationed during the war. Coupled with lots of farm tasks (chopping wood and moving sheep as well as more serious work such as cutting gorse fences and stooking during harvest), this gave me a lot of stamina. I recall J.K. Galbraith's comment about a similar upbringing in Canada: "After that, everything was easy."

In high school, I took the professional class at the urging of my mother and duly passed the university entrance exam. I acquired a liking for English literature, history, and chemistry along the way. At the prize-giving ceremony in my final year of high school (December 1943), I was awarded the Drummond Prize for science. This had unforeseen consequences. I was planning to start at university in March 1944, but as World War II was in progress, I had enlisted in the air force. They had so many volunteers that I was not called up until mid-1945 and then received a notice (on V-J day, as I recall) that my services would not be required. I had some idea to start studying for a law degree as Lloyd had done, but in mid-January of 1944, I decided to study science—driven in part by the award of the Drummond Prize, showing the often hidden benefits of awards.

CHRISTCHURCH AND CANTERBURY UNIVERSITY COLLEGE

So I decided to enroll as a science student at Canterbury University College (in Christchurch), then part of the University of New Zealand. After slogging through chemistry, mathematics, and physics, and needing a fourth subject for a BSc, I enrolled in geology. The lectures were given by the professor, Robin Allan; the other staff member, Brian Mason, lectured in mineralogy. I was captivated almost immediately by Allan's lectures, delivered in a polished academic style with frequent philosophical asides—partly derived from the philosopher Karl Popper, who was at that time in Christchurch, a refugee from Vienna. For the first time, this seemed to me to be what I had come to a university to hear, rather than being just an extension of high school. I had enough sense to complete my chemistry degree and so finished with a double major in chemistry and geology.

I spent the first two undergraduate years (1944–1945) staying at College House, which in addition to providing accommodation for students was also the Church of England theological seminary. After contemplating the geological timescale, I lost interest in the absurdities of religion, with its emphasis on the past few thousand years and *Homo sapiens*. In 1946 I was very lucky to find a hospitable landlady, Anne Orchard, who provided a comfortable home. I have thrived only when comfortable and do not believe in the "genius flourishing in a garret" model.

Allan suggested mapping the Stonyhurst district in North Canterbury for a master's degree thesis (which also involved three three-hour papers on all aspects of geology). There was little supervision and hardly any lectures, but I have always been happier working on my own. Allan paid a one-day visit to the 100-square-mile area that I was mapping. After six months, I finally understood how to carry out geological mapping, as no instruction was ever given.

After finishing my MSc, I returned to work as a laboratory technician in the product control lab at the Islington Freezing Works, which produced frozen lambs for the UK export market. Here I learned to carry out analytical chemistry properly, as I had found the university laboratory practical classes to be useless. Indeed, my laboratory instruction at high school was much superior. Finally I graduated in 1951 with an MSc in Geology; I received First Class Honours and the Sir Julius von Haast Prize of the University of New Zealand for the best geology student.

INDIANA UNIVERSITY

I thought that if I was to continue in geology, I had better acquire a PhD, so I wrote for advice to my former mineralogy instructor, Brian Mason, who was now at Indiana University. Although it was the tradition for New Zealand students to go to England, usually Cambridge, most of the action then seemed to be in the United States. Brian, a former student of V.M. Goldschmidt in Oslo, offered me a scholarship at Indiana to work in the new subject of geochemistry, so I went to Indiana, as it seemed sensible to combine my chemical and geological training. I already sensed that geology was a weak subject on its own that required a backing from the hard sciences. This view has become reinforced with experience.

I found the student accommodation at Indiana good. At the university, I paid a total of $50 in fees for my PhD and was treated as an in-state student, receiving a scholarship that paid for lodging and meals on the campus. As there was a shipping strike, my family generously provided airfare to the United States, which was very expensive in 1951, as well as much other support. As Brian, who had worked with Goldschmidt, was my supervisor, I could claim to be in direct descent from the master.

At Indiana I took many graduate-level courses, and for my thesis, I worked on the trace element composition of rocks mostly from the Banks Peninsula volcanoes near Christchurch. The most valuable thing that I learned during my PhD studies was the then state-of-the-art technique for trace element analysis, emission spectroscopy. Working in the excellent laboratory of the Indiana Geological Survey, which was embedded within the Geology Department, I learned how to carry out precise analytical work.

After finishing my PhD, I worked for about a year (September 1953 to June 1954) in the Indiana Geological Survey laboratory before going to Oxford, so I really came to understand the nuts and bolts of the spectroscopic technique. I retain fond memories of Indiana University and of the kindness and hospitality that I received in America, a view reinforced during my many later visits.

UNIVERSITY OF OXFORD

It was Henrich Neuman from Oslo—filling in at Indiana for Brian, who had left for the American Museum of Natural History in New York—who encouraged me to apply to Oxford for an academic position. Oxford was attractive, with the combination of Bill Wager, the famous petrologist, and Louis Ahrens, arguably the most famous geochemist at the time. The upshot was that I arrived to work in Oxford with Louis Ahrens fully up to date on the emission spectroscopic technique. From Louis I learned the philosophy of trace element analysis and set up the primitive (by US standards) Hilger equipment, again even more educational. A major lesson from Louis was the need to get both accurate and precise data, and I was introduced to the famous international rock standards, G-1 and W-1. During my subsequent career, I spent much time in the pursuit of accurate data, using both emission and spark source mass spectrometry, continuously trying to improve these somewhat intrinsically imprecise techniques.

Louis recruited me as a coauthor on the second edition of *Spectrochemical Analysis* (this led to my later involvement with NASA), and I wrote some papers with him and followed him to Cape Town in 1958. This association lasted until I came to Canberra in 1961. After a year or so in Oxford I was promoted and thus needed to join a college. As I came from Canterbury in New Zealand, which Christ Church had been influential in founding, Christ Church was an obvious choice. As I already had a PhD, Oxford also kindly presented me with an MA, a requirement for giving lectures at the university.

About the time I got the optical spectroscopic laboratory in Oxford into operational shape and could produce accurate data, Knut Heier appeared in Oxford from Norway, so we began work on trace elements in feldspars. This collaboration resulted in several papers and continued after I went to Cape Town. We developed a close friendship and lived in the same "digs," and he was best man at my wedding in 1958. He followed me to the Australian National University (ANU) in 1962, and we attempted briefly to write a joint book on trace element geochemistry, but I soon realized that it was not going to work and we went separate ways. Knut soon returned to Norway as a professor at Oslo University and later became director of the Geological Survey of Norway.

So by now I was beginning to get a feel for scientific work, realizing that one could make no progress in geology without accurate new data and bearing in mind Rutherford's dictum that "a scientist must make measurements," something that both Wager and Ahrens also appreciated. After all, conventional geological mapping and optical microscopic techniques had already been around for generations.

Then a chance event occurred. Harold Urey, who had won a Nobel Prize for discovering deuterium, came to Oxford on leave, and I learned two things from him: first, that the Moon was an interesting geological body, and second, that there was a great controversy over whether tektites came from Earth or the Moon. Harold Urey had a remarkable influence on my career. Listening to him at Oxford, where he was spending a sabbatical year in 1956, I first realized that the Moon could be investigated scientifically and that the origin of tektites might be within my reach with trace element analysis. Urey told me to "always work on significant problems" and "don't admit that you are wrong too soon."

David Vincent, the professor of geology succeeding Wager, wrote a very entertaining book, *Geology and Mineralogy at Oxford, 1860–1986: History and Reminiscence*, which records my time in the department. This is an engaging and readable account of the problems besetting the head of an active department.

MARRIAGE

In Oxford, I met Noel White, a postgraduate student in X-ray crystallography with Dorothy Hodgkin, later a Nobel laureate, and we were married in Christ Church Cathedral on May 21, 1958. Noel had a distinguished academic record at Perth Modern School and the University of Western Australia. She had two older brothers—Jeff, a psychologist, and Peter—both of whom served in World War II.

After completing her PhD, Noel divided her intellectual abilities between the care of the family, a career in trademark law, and completing a half-built house while I was absent at NASA in Houston for six months in 1969. She was born on March 29, 1931. Her parents were Victor White (1890–1937), headmaster of the school at Kalgoorlie, Western Australia, and Ida Comfort White (1897–1992), who was born at Bunbury, Western Australia and trained as a nurse in 1918. Contrary to the common stereotype, I got along very well with her during her frequent visits. Noel's paternal grandparents were John Samuel White, born in Richmond, Victoria, and Frances Mary Browning, born in England.

Our children are Susanna Margaret, born in Canberra on February 11, 1962; Judith Caroline, born on January 26, 1964; and Helen Rosalind, born on February 26, 1966. I have one grandchild, Angelo Liangis, born to Helen on December 10, 2005, in Canberra.

CAPE TOWN AND TEKTITES

Louis Ahrens left Oxford for the chemistry chair at the University of Cape Town, where I followed him after a couple of years. South Africa was superficially attractive after England, which was still recovering from the war. At Cape Town I set up a subdepartment in geochemistry within the Geology Department. Although the Moon was not accessible then, tektites were, and there were few trace element data on them, so I got some tektites from the South African Museum. I worked on tektite chemistry with Robin Cherry in the Physics Department, an enjoyable collaboration. My assistant, Maureen Sachs (later Kaye), was so good at spectroscopic analysis that she appears as a coauthor on several tektite papers. I worked on the problem of whether tektites were derived from the Moon or Earth, concluding as early as 1962 that they came from Earth, a determination later confirmed by lunar data.

I started off gradually enough on the problem, looking at the trace element compositions; reading the literature, large even then, on the topic; and comparing the data with such terrestrial analyses as were available. The real breakthrough came from working with Robin Cherry, when we discovered that all the major elements, including potassium, showed negative correlations with SiO_2—in contrast to terrestrial igneous rocks, which show positive correlations—so indicating a terrestrial sedimentary origin for the tektites' parent material. I did not then realize that I was entering into one of the great scientific controversies of the time. At the tektite conference at the University of Pittsburgh in 1963, I was pitchforked into the debate, which lasted until the arrival of lunar samples in 1969—although as late as 1994, Chris Koeberl and I had to write a rebuttal of a paper by John O'Keefe espousing a lunar origin.

I have always regarded myself as mild mannered, anxious for a consensus, and disliking disputes and controversy. However, I seem to have been involved in scientific arguments for most of my career.

AUSTRALIAN NATIONAL UNIVERSITY, CANBERRA

Not liking the political situation in South Africa, and being aware of the formation of the very attractive ANU, I wrote to John Jaeger in Canberra and in 1961 was appointed as a senior fellow in geophysics at the Research School of Physics at the ANU, where I spent much of my subsequent career. In 1974 it became the Research School of Earth Sciences (RSES). There were considerable advantages in working at the ANU due mainly to the wisdom of its founders. But in contrast to my previous collegial academic experience, I found myself in an environment of small competitive groups with scarcely any interaction between them.

My collaboration with Maureen on tektites continued for several years after I came to the ANU, where she finally joined me around 1967, but she left to work in the Geology Department around 1970, having found the atmosphere in geophysics too unfriendly. I soon realized that optical emission spectroscopy had reached its limits and acquired a spark source mass spectrometer in 1964. After about a year in Canberra, I received an offer of a readership in Oxford but decided to remain in Canberra, mainly for family reasons.

SPARK SOURCE MASS SPECTROMETER

There were many problems with the spark source mass spectrometer (AEI MS 7), which used a high-frequency spark (25 kV) to volatilize ions that were then swept into the mass spectrometer with the entire spectrum from hydrogen to uranium recorded on photoplates. The problem was that the instrument had no accurate way of measuring the intensity of the ion beam. This was a considerable dilemma that might forever consign the technique to a semiquantitative status.

Some other users thought that the problems lay in the heterogeneous mineralogy of geological samples, so that fusion of samples was the answer. However, analyses of glassy tektites gave the same problems as more mineralogically complex rocks did, and so it was clear that sample heterogeneity was not the main issue. The standard method of obtaining the photoplate response by a graded series of exposures, recommended by the manufacturers and so used in spectrochemical analyses, was not applicable because it was not possible to get accurate measurements of the ion beam for the different exposures.

Nature, however, had kindly provided a way around the problem of obtaining the photoplate response curve by providing a number of elements (e.g., Ba, Yb) with several isotopes that gave a graded intensity scale that could be measured on one exposure, independently of the beam current. Thus it was possible to step around the problem and to ratio measurements of isotopes in individual exposures directly to the internal standard isotope on the same exposure. This was the secret that made the spark source mass spectrometer a fully quantitative instrument, able to compete with other trace element techniques such as X-ray fluorescence, instrumental neutron activation analysis, and radiochemical neutron activation analysis. Once these problems were understood, a computerized technique to handle data processing was developed. I spent a few years getting the technique working, thereby extending my analytical capabilities to elements unreachable by emission spectroscopy, such as the rare earth elements (REE), U, Th, and Hf.

The MS 7 produced several notable results. Apart from the lunar work, it established the REE patterns essential for resolving the composition and evolution of the continental crust. Then, as part of a collaboration with Brian Mason, it established the various groups of the calcium-aluminum inclusions (CAIs) in the Allende meteorite, notably the famous Group 2 inclusions, with their exotic REE patterns. As an aside, it took some time to establish that their thulium enrichment on mass 169 was not due to interference from $C_{14}H$.

Not being a fan of big science, I mostly worked with one or two assistants and one graduate student, although there were usually two or three visitors. Many visitors appeared from other universities, long before the days when this became "politically correct." The rules were simple: a viable scientific project, a minimum stay of three months, and the visitors had to operate the machine, so that they could understand how the numbers were obtained.

We never undertook service work; in my opinion, it is not the business of universities, using public money, to compete with industrial laboratories. The tail soon starts to wag the dog, when the people who are paying for analyses demand their data.

CONTINENTAL CRUST

During the 1960s I began to think about the problems of the continental crust, culminating in my 1967 paper in *Tectonophysics*, "The Origin and Growth of Continents" (Volume 4, pp. 17–34). Needing a good petrologist, I worked with Alan White on orogenic andesites and their relation to continents, and we published some definitive papers. The very first problem was how to find a bulk composition for the continental crust that was heterogeneous sometimes on a scale of meters. I soon realized that the key to this problem was to use common sediments, such as shales, that through the processes of erosion and sedimentation had homogenized the disparate compositions of the crust. However, Australian surface rocks were deeply weathered. Fortunately the Bureau of Mineral Resources (now Geoscience Australia) had retained drill cores from the major sedimentary basins, to which I was allowed access.

I initially worked on sedimentary samples with Weldon Nance, whom I had recruited from the lunar lab at NASA in Houston. Using REE patterns, readily measurable on the spark source mass spectrometer, we established that the upper continental crust was uniform across Australia, North America, and Europe back to the Archean–Proterozoic boundary about 2.5 billion years ago.

As the work with sedimentary rocks progressed, I found an ideal collaborator in Scott McLennan, who had come from the University of Western Ontario and knew a lot about both sedimentary rocks and geochemistry. We were compatible from the first meeting, and our remarkable collaboration has extended to over 30 years, 40 papers, and two books with never a cross word. Our 1985 book on the continental crust, *The Continental Crust: Its Composition and Evolution*, remains a classic still often referred to. In 2009 we published *Planetary Crusts*, in which we fully developed the concepts of primary, secondary, and tertiary crusts of planets, which I had first introduced some years before. After Scott finished his PhD thesis in 1981, he was appointed to a research position in Canberra that led to the writing of our 1985 book and much else. Scott has since gone on to become a leading authority on the crust and sedimentary rocks of Mars.

I have always been careful to enlist expert help, such as from Alan White on andesites and Scott on sediments. So when Scott and I began to work on Archean sediments we enlisted Ken Ericksson and Vic McGregor as coauthors, while Roberta Rudnick, my last PhD student, worked with me on the lower crust. Roberta is now the world's leading authority on that subject and is a member of the National Academy of Sciences.

I worked with many other scientists, including Mike Perfit, Ted Bence, Angelo Peccerillo, Al Levinson, Marc Norman, Dick Price, Mike Gorton, Amir Khan, Horton Newsom, Christian Koeberl, Peter Kolbe, Jim Gill, Tezer Esat, Charles Vitaliano, Mike McElhinny, Tony Erlank, Andrew Duncan, Dave Whitford, and Ian Campbell. With Campbell, I wrote the now-famous 1983 paper in *Geophysical Research Letters* entitled "No Water, No Granites—No Oceans, No Continents" (Volume 10, pp. 1061–64). Essentially through the studies of common sedimentary rocks, we resolved many of the problems with the continental crust. It had clearly changed fundamentally in composition around the Archean–Proterozoic boundary.

The Archean sediments presented many diverse REE patterns. Initially, their sedimentary REE patterns resembled those of andesites, but I realized that, in contrast to later periods, andesites were scarce in the Archean and so were not a suitable source for the crustal composition. We soon realized that the REE patterns in the sediments were a combination of two patterns: one a steep pattern originating from the sodium-rich granites of the tonalite-trondhjemite-granodiorite (TTG) suite, and the other a flat pattern derived from the common Archean basalts, the other major component of the Archean crust. When mixed, these gave an REE pattern mimicking those of andesites, another one of nature's tricks.

We eventually produced models for the origin and growth of the continental crust (**Figure 1**), one for the Archean and another for post-Archean time. During the Archean, before the operation of plate tectonics and subduction zones, the crust was basaltic, with flat REE patterns. The basalts occasionally thickened and melted at their base to produce the sodium-rich granites of the TTG suite, with their steep REE patterns that showed no depletion in Eu. The REE patterns in the sediments represented all variations of the two distinct patterns, with 1:1 mixes resembling andesitic patterns. This pattern persisted though most of the Archean, with a major spurt in crustal growth during the Late Archean. During post-Archean time, the bulk crustal composition was supplied by andesitic volcanoes via the operation of plate tectonics, which began in the Late Archean. Internal melting within the crust, well described by petrologists, produced the potassium-rich granitic upper crust, with its marked depletion in Eu, that is so well represented in shales. This left a depleted, more basic lower crust.

There appears to be little evidence of an ancient granitic crust, often claimed by isotope geochemists who have misinterpreted the zircon data. Such crust as existed in the Hadean was probably basaltic, with occasional islands of TTG granites, from which a handful of zircons have survived, formed from remelting of thicker sections of the crust.

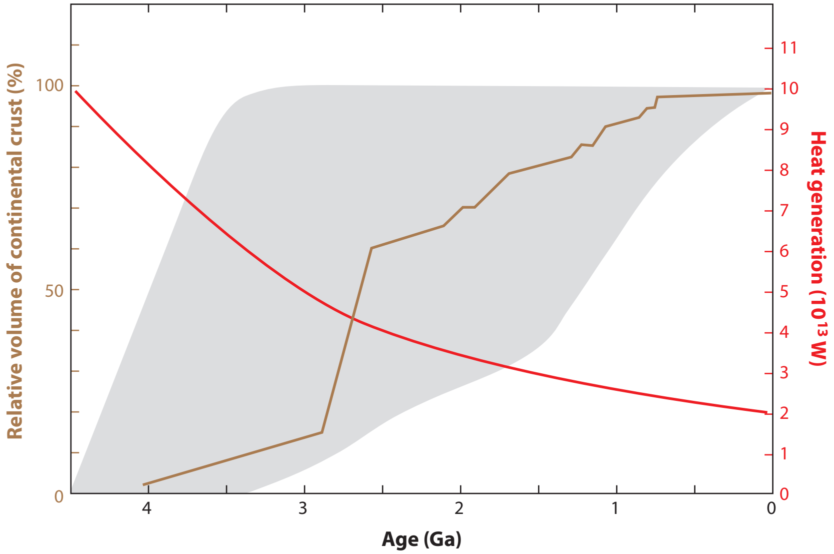

Figure 1

The growth of the continental crust though time (*brown line*), according to Taylor & McLennan (Taylor SR, McLennan SM. 1985. *The Continental Crust: Its Composition and Evolution.* Oxford, UK: Blackwell). The shaded area includes other estimates. Also shown is the decline in heat generation in Earth with time (*red line*). Adapted from figure 5 in Ben-Avraham & Stein (Ben-Avraham Z, Stein M. 2007. Origin and evolution of continents. In *Treatise on Geophysics*, Vol. 9: *Evolution of the Earth*, ed. D Stevenson. Amsterdam: Elsevier).

NASA AND THE MOON

While I was engaged in studying the continental crust I pursued a parallel investigation into the Moon.

My political future at the ANU was bleak late in 1968, and I owe much of my subsequent academic career to Robin Brett, then geochemistry chief at the NASA Manned Spacecraft Center (now the Johnson Space Center) in Houston (seconded from the United States Geological Survey), who invited me to spend a few weeks in Houston at the newly established Lunar Science Institute (now the Lunar and Planetary Institute). My entitlement to study leave from the ANU amounted to three months per year, which enabled me to escape to the Lunar Science Institute, where I was always made very welcome. I eventually spent a total of several years in Houston, including visiting for several months in 2005 as a Heritage Fellow, and I wrote three books during the time I spent there.

After a few weeks in Houston in early 1969, I was asked by Bill Hess (then at NASA) to run the spectroscopic laboratory in the Lunar Receiving Laboratory at the NASA Manned Spacecraft Center, where the initial chemical analysis of the first lunar sample to be returned to Earth was to be carried out. I ultimately spent several months in Houston. My wife, left in Canberra with a half-built house, duly finished it.

As a member of the Lunar Sample Preliminary Examination Team (LSPET), I reported my data each day to a senior group of scientists who constituted the Lunar Sample Analysis Planning Team (LSAPT). Its most formidable members were the "Four Horsemen": Jerry Wasserburg, Paul Gast, Bob Walker, and Jim Arnold. It was a difficult time when I needed every item of knowledge that I had acquired during the previous 15 years working in spectroscopic analysis, and

I eventually succeeded in obtaining correct analyses. After it was over, Jim Arnold told me that "the whole scientific community is in your debt," and Jerry Wasserburg, a more outspoken member of LSAPT, told me that "without you, we would have been up shit creek without a paddle." One of the great advantages of being involved in the lunar program was that one encountered scientists from many different disciplines, so that one had to explain the relevance of one's work and make it understandable to others in remote fields. A byproduct was that I made many friends outside my narrow specialty in what is a wonderful community of scientists.

John Wood's retelling of the famous quarantine spill during Apollo 12 in his Leonard Medal citation in *Meteoritics and Planetary Sciences* (Volume 34, pp. 315–16) provides me with an opportunity to reassure readers that I did not put the world at risk from lunar germs. This episode occurred during the preliminary examination of the Apollo 12 samples in December 1969. Someone managed to puncture a rubber glove in one of the biologically secure cabinets, needle-nosed tweezers having been thoughtfully provided. When Robin Brett alerted me to this incident, I retreated to a small room where there was a calculator and so missed the resulting sweep-up of many of my fellow workers, including Robin Brett. But the first lunar samples had been returned by the Apollo 11 mission in July 1969 and had already been widely distributed to the worldwide lunar community, so that hundreds of workers had been exposed to lunar samples with no ill effects.

Moreover, the quarantine had already been breached during the splashdown of the returning Apollo 11 lunar capsule. The safe recovery of the astronauts was naturally of overriding concern. Immediately following splashdown, the command module was opened and the astronauts clambered into a raft before a helicopter took them to the deck of an aircraft carrier. Because the fine-grained lunar soils were exceedingly dry, they stuck to everything due to static charge so that they coated the inside of the capsule and adhered to the astronauts' suits. The South Pacific Ocean was thus well exposed to lunar material, making the subsequent burdensome quarantine procedures in the Lunar Receiving Laboratory irrelevant. I wonder how many geochemists have carried out analyses while wearing a gas mask. Henry S.F. Cooper, then a *New Yorker* reporter, interviewed me often during this time and gave an account in his 1970 book *Moon Rocks*.

A year or so later in Houston I collaborated with Petr Jakes from Prague, when we were both at the Lunar Science Institute during 1973–1974. This resulted in the famous Taylor-Jakes model for lunar evolution. Petr Jakes and I proposed that the interior of the Moon was differentiated and that the mare basalts were derived from cumulate zones of minerals developed during the crystallization of a molten Moon that had produced the thick highland crust of feldspar. Thus the REE Eu was enriched in the feldspathic highlands but depleted in the source regions of the mare basalts. This view became the accepted model, although it disagreed with a model from my colleague at the ANU, Ted Ringwood.

There is an ironic twist in this story. The key to the correct model of lunar evolution lay in the great enrichment of Eu in the lunar highlands (formed by flotation of feldspar) so that the basalt samples derived later by partial melting from the interior show a depletion in this element. This information was only revealed at the First Lunar Science Conference, in January 1970, after Ringwood had developed his model. My spectroscopic analyses in LSPET at Houston in July 1969 could not detect Eu but only Y and Yb among the REE. Curiously, La, normally detectable, was below the detection limit, pointing to a depletion in light rare earth elements (LREE) in the mare basalts. However, my spark source spectroscope in Canberra had the capability to determine all the REE. But during the crucial period of September to December 1969, I had no access to the lunar samples in Canberra, having been excluded from the local team despite my involvement in Houston. If Ringwood had come to my lab (one floor up from his office) to ask for an analysis on the mass spectrometer (which could determine Eu), he would have known about the Eu depletion in mare basalts and might have come up with a correct model.

PLANETS

Finally, with those fields exhausted by about 1985, I began to work on the problem of Solar System evolution. In the late 1980s, I began to work on a book on the Solar System, following a suggestion by a Houston colleague, Fred Hörz, that I write one based on chemistry. This eventually resulted in *Solar System Evolution*, published in 1992; a substantially rewritten second edition followed in 2001.

As I contemplated the array of planets, satellites, and assorted debris in our Solar System, and the role that impacts had played, I slowly came to realize that the planets and other members of the system were the end product of a long series of chance events in circumstellar disks that was unlikely to be repeated in detail elsewhere. So I concluded that whereas stars are relatively uniform in composition and differ mostly in mass, planets are formed by much more random processes, so resembling "the products of a junkyard." In an essay in *Nature* in 2004 (Volume 430, p. 509), I commented that "it seems likely that we may eventually find that planets forming from disks rotating around young stars will occupy all available niches within the limits imposed by the cosmochemical abundances of the elements and the laws of physics and chemistry."

I have also become involved in the question of the existence of extraterrestrial intelligence, the so-called search for extraterrestrial intelligence (SETI) matter. My interest arose by chance. About 1994 I decided to write a more popular account that eventually became *Destiny or Chance: Our Solar System and Its Place in the Cosmos*. Then I was awarded the Leonard Medal from the Meteoritical Society in 1998, which involved giving an invited lecture. So I chose to talk about the difficulties of forming Earth-like planets; my lecture, "On the Difficulties of Making Earth-like Planets," was published in *Meteoritics and Planetary Science* in 1999 (Volume 34, pp. 317–29). By this time, I was coming to the conclusion that although there were many rocky planets of about the same mass as Earth, true clones of Earth, or Earth-like planets (sensu stricto), were unlikely to be found. But I did not start with this in mind. All I had begun to do was to write a book about the chemistry of the Solar System, but I soon came to be labeled in the "rare Earth camp," just as I had been placed in the "terrestrial camp" over the origin of tektites when I was only trying to understand their chemical composition.

It is ironic that by the time I was halfway through writing *Destiny or Chance*, in 1995, the first true discovery of extrasolar planets was announced. Amid the hype, it was difficult to establish facts, and by the time the book was published, only a handful had been confirmed. Then there was also some confusion about the role of the recently discovered brown dwarfs. I eventually wrote, in 2012, a sequel called *Destiny or Chance Revisited* in which I surveyed both the extrasolar planets and the development of Earth and of *Homo sapiens*. It became clear as I wrote that *Homo sapiens* was present on this planet as a result of not one but two unrelated chance events. The first was the removal of the dinosaurs and much else at the end of the Cretaceous, allowing the mammals to fill the vacant ecological niches. The second was the unrelated development of the mantle plume under East Africa that resulted in the origin of the Rift Valley environment that eventually led to the development and evolution of *Homo sapiens*.

But many do not accept the findings of geology, biology, and evolution, preferring instead the delusions of superstition and the belief systems of ancient and very ignorant desert tribes. This no doubt accounts for the egocentric development of the countless varieties of religions, with their emphasis on the past few thousand years and on the existence of *Homo sapiens*.

Returning to the problem of intelligent life elsewhere, the question of the exquisite timing needed to establish communication with civilization on another planet soon became apparent. So I became convinced that the existence of intelligent life elsewhere at present was unlikely, despite much popular support.

As a final comment, the words of Margot Fonteyn, the celebrated ballerina, seem appropriate: "The one important thing I have learned over the years is the difference between taking one's work seriously and taking one's self seriously. The first is imperative and the second is disastrous."

In summary, I was very fortunate in my mentors (Allan, Mason, Wager, Ahrens, and Urey) and in acquiring some expertise in analysis. This allowed me to solve the question of whether tektites came from Earth or the Moon and led on to considerations about the origin and composition of the continental crust. Then I was fortunate to become involved in the Apollo lunar program, which led me to developing a model for the geochemical evolution of the Moon. Next I developed an interest about the rest of the Solar System that led to further thinking about planets and the place of *Homo sapiens* in the universe. Altogether, I have published 10 books and 240 papers in scientific journals.

RETIREMENT

In 1990, I reached the mandatory retirement age of 65. No suggestion was ever made at that time that I might remain in RSES, but I was kindly given shelter in the Department of Nuclear Physics in the Research School of Physical Sciences, where I spent a useful and productive decade. During this time, I made several visits to the University of Vienna, where I gave a series of invited lectures. Next I moved to the Geology Department, where I taught planetary science for some years before the department was amalgamated with RSES. I have now returned to RSES, where I have continued working under more favorable conditions. Since "retirement" I have published 58 papers and five books.

My other interests include, not surprisingly, history as well as baroque and classical music; I find little of interest composed after about 1820, although there are occasional later spikes in the Romantic period up to about 1900.

AWARDS

In addition to many minor honors, my list of major awards includes the V.M. Goldschmidt Award, the premier award of the Geochemical Society; the Leonard Medal, the premier award of the Meteoritical Society; and the Bucher Medal of the American Geophysical Union for my work on the continental crust. I was also elected as a Foreign Associate of the US National Academy of Sciences in 1994 and was appointed a Companion in the Order of Australia (AC) in 2008, at that time the highest civilian award "for outstanding service to science, particularly in the fields of geochemistry and cosmochemistry as a researcher, writer and educator." Nevertheless, I still retain my New Zealand passport.

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