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ADVENTURES IN LIFELONG LEARNING

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In earth science, as in other branches of natural science, opportunities for significant research evolve. A new field is opened up either through a liberating concept or by creation of powerful instrumentation. If the field is sufficiently important, many scientists enter it. In due course the cream is skimmed. Unless a new approach is taken, or more powerful instrumentation becomes available, or new and searching questions are posed, or important applications arise, the field becomes mature, and creative people leave it. Most of the recent advances that have occurred in earth science have been associated with increased instrumental capabilities coupled with improved data handling.

I have been an observer of developments in earth and planetary sciences since 1946. During that interval, what were basically observational sciences have evolved into what are predominantly laboratory activities. Some instrumental capabilities have improved by orders of magnitude. During the past 45 years approaches to earth science have also become far more interdisciplinary. Many scientists trained in physics, chemistry, or biology have addressed interesting problems. Throughout this period, part of the activities were devoted to purely academic or basic research problems. Concurrently much of earth science was highly applied. In future, an increasing share of employment opportunities is likely to be in applied areas, with environmental efforts encompassing a larger factor.

In this first chapter of the current *Annual Review of Earth and Planetary Sciences*, I shall follow precedents established by other such contributors. These authors were given free rein to write what they pleased. Most chose to provide autobiographical material, mention some of their associates,

and describe aspects of the key developments in earth science that they had been part of or had witnessed. In addition to these topics, a little space will be devoted to an estimate of some future trends in earth science.

For me, life has been an interesting series of adventures. I have pursued a policy of lifelong learning. But at times the adventure has been physical in nature. My first view of the great copper ore body at Bingham Canyon came in 1918 when I was five years old. That was a sequel to physical combat. My father was a civil engineer with a smelting company, and we were gentiles living in Mormon country. One day when I had ventured away from home, I was surrounded by a group of young Mormons. They designated one of their members to pick a fight with me. He was bigger than I, but I fought so fiercely that he ran home crying. I was promptly accorded the status of an honorary Mormon and later accompanied the boys on a hike to gaze on the canyon.

After the conclusion of World War I, the family returned to my birthplace on the outskirts of Tacoma, Washington. When I was 14, my father got me a summer job with a professional surveyor. We surveyed city lots, accident scenes, and mining claims. This experience, together with a good training in trigonometry, enabled me (following graduation from high school in January) to be employed as a chainman on a hydroelectric construction project located about 70 miles from Tacoma. When I arrived at the construction camp in February 1930, I was assigned to assist a surveyor (transit man). Our assignment was on a steep hill where huge segments of steel pipe (penstocks) were being installed. These were destined to carry a large stream of water to the powerhouse below. We provided the steelworkers with the information essential to place the pipe correctly. The task had only begun and I had been at the camp about three weeks when on a weekend the transit man went to Seattle, got drunk, and was thrown in jail. The next Monday morning, the engineer in overall charge said to me, "Abelson, go up on the hill and line in those penstocks." I had no assistant, and it was necessary to recruit a steelworker to perform what I had previously done with the rod or plumb bob. The steelworkers tended to cheat. I found it expedient to address them in a language they would respond to. My vocabulary of profanity soon equaled or exceeded theirs. We got along well after that.

The seven months spent at the construction camp as a surveyor was a maturing experience. It made me appreciate the potential value of a college education.

In September 1930 I enrolled as a chemical engineer at the then Washington State College. Marvelous years of learning followed. I was especially fond of the chemistry laboratory. By taking a large load of courses I was able to obtain my bachelor's degree in three years. The

depression year of 1933 was not a good time to obtain employment. Thus it was pleasing to be offered a teaching assistantship in physics by the departmental chairman, Paul Anderson. During the next two years, I took further courses in physics and did research leading to a master's thesis. A course in Modern Physics taught by S. T. Stephenson included an assignment to read an article by Ernest Lawrence describing his cyclotron. It was immediately clear that Berkeley was going to be an exciting place. Professors at Washington State were helpful in obtaining for me a teaching assistantship and entree to the Radiation Laboratory. I was the only out-of-state student to receive a teaching assistantship in physics at Berkeley that year.

In the era of 1935–1939, the Radiation Laboratory was the world center of nuclear physics. Visitors included leading scientists of the day and other notables such as Leopold Stokowski. Most of the members of the laboratory were post-docs. As a lowly graduate student I was one of the first to experience the effects of big science on education. Ernest Lawrence was a great scientist. From about 1931 and for the remainder of his life, he was a successful promoter of larger and larger accelerators—big science. But during the prewar era, a graduate student in the Radiation Laboratory spent a large fraction of his time operating or repairing the cyclotron.

Graduate students were expected to make their own experimental equipment. I was a competent machinist and glassblower and was able to build the necessary instruments, including a bent-crystal X-ray spectrograph, to study products of neutron irradiation of uranium. I missed the discovery of fission, but during a few months thereafter was able to identify 15 products of uranium fission (Abelson 1939) and obtain my PhD degree.

In September 1939 I became a staff member at the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, where I was assigned to participate in construction of a cyclotron. The intended purpose of the machine was to supply radioactive tracers to Washington area scientists. Events in Europe modified priorities at DTM. Merle Tuve, Lawrence Hafstad, and Richard Roberts chose to develop a proximity fuze. My interest and efforts moved toward uranium. In May 1940, I carried out chemical procedures which provided unique identification of element 93 (McMillan & Abelson 1940). Then I turned to developing the liquid thermal diffusion method for separating uranium isotopes.

I duplicated some of the earlier work that involved aqueous solutions of a target element. However, it was soon evident that separation of isotopes of uranium would require use of UF_6 as the working fluid. The compound was not available, and only a few grams of it had ever been made. I proceeded to build my own fluorine generator and invented a method for making UF_6 . I personally synthesized the first 100 kg of the chemical.

Much of the synthetic work was conducted in a laboratory at the old National Bureau of Standards. Successful partial separation of the isotopes was achieved at the Naval Research Laboratory. A pilot plant was built at the Philadelphia Navy Yard. Ultimately 2100 48-foot-long columns were installed at Oak Ridge, Tennessee (Abelson et al 1946).

Following the end of World War II in August 1945, I stayed on with the Navy for another year. During that time, I participated in testing some experimental nuclear reactors (critical masses of enriched uranium) and was in charge of preparing a feasibility report on building a nuclear submarine. In the meantime I was considering what to do next. Ernest Lawrence asked me to go back to Berkeley. Merle Tuve sketched a more intriguing future in biophysics.

Tuve was slated to succeed John Fleming as Director of the Department of Terrestrial Magnetism. He wished to broaden the scope of the department to emphasize experimental aspects of physics over the older observational tradition of the department. The program was to include geophysics as well as other programs. Tuve suggested that physicists might be capable of making unique contributions to such major outstanding problems as the origin of life. He also noted opportunities created by the availability of radioactive isotopes. In September 1946, I returned to DTM as Chairman of the Section on Biophysics.

Soon I became acutely aware that biological processes are extremely complex and that a person trained in chemistry and physics could not expect to make a quick, important discovery in biology. During the next three years I participated in tracer experiments which led to publications in respectable biological journals. During this period I spent two summers at the Marine Biological Laboratory at Woods Hole. There I made the first observations on the changes in ^{32}P metabolism that occur when sea urchin eggs are fertilized.

Richard Roberts, a member of the Biophysics Section, made a crucial contribution to the group after he returned from taking a phage course at Cold Spring Harbor. He brought back microbiological techniques. Soon the group, consisting of myself, Ellis Bolton, Roy Britten, Dean Cowie, and Roberts, was heavily engaged in studying biosynthesis in *Escherichia coli*. We taught ourselves comparative biochemistry. The only carbon tracer then available was ^{14}C in the form of CaCO_3 . Using plants, I produced ^{14}C -tagged sugars which were useful in probing mechanisms of synthesis of the various amino acids (Abelson 1954a). Ultimately we worked out most of the synthetic steps in the bacterium and especially the amino acids. We treated *E. coli* as a chemical engineer and accounted for the detailed fate of input carbon, sulfur, and phosphorus. We prepared a book that came to be a bible for the many microbiologists working with

E. coli (Roberts et al 1955). The volume had additional value when *E. coli* was chosen as a key biomaterial for carrying out genetic engineering. The achievements of the group demonstrated that it is possible for five people collaborating well to do world-class science in a new field.

Merle Tuve was an innovative scientist who was broadly aware of potential initiatives. He sensed that a great opportunity existed in the dating of the earth's crust. Accordingly, around 1949 a seminar entitled "Milestones in the Pre-Cambrian" was organized, which I attended. Tuve also brought Thomas Aldrich to DTM in 1950 to initiate a dating program. Aldrich had been a graduate student at Minnesota with Alfred Nier. He quickly organized a rock dating program that in its day was the best. Many of the scientists who became leaders in geochronology spent some time with him at DTM. I was able to be helpful to Aldrich early by suggesting use of ion exchange resins in the processing of rubidium and strontium.

In early 1953 I was beginning to have long thoughts about new learning experiences and a possible change in professional activities. Almost by magic, with no initiative on my part, a major possibility arose. The post of Director of the Geophysical Laboratory of the Carnegie Institution of Washington had become vacant and had been open for about a year. I was asked to consider taking the position. I did not respond immediately. I was aware that the laboratory had established an excellent worldwide reputation. About one fourth of the literature cited in advanced books on petrology included publications of the lab. A multidisciplinary group of scientists were principally engaged in physicochemical studies of oxide systems giving rise to minerals found in the igneous and metamorphic rocks. Experiments under precisely controlled conditions were conducted at high temperatures. The experiments, though important and highly significant to field geologists, had become rather routine. Later, when high pressure equipment was developed, the results were more exciting. But at the time, I had reservations about taking the position, and I knew I did not wish to participate personally in phase equilibrium research. However, I discovered that I could establish a different research agenda for myself, and in the end the attraction of a new intellectual adventure prevailed. The decision was reached in the spring of 1953, with the directorship to begin in September of the same year.

Already in early 1953 I was interacting with some of the leading geologists, including W. W. Rubey. He and colleagues organized a "Conference on Biochemistry, Paleoecology and Evolution" held June 9–11, 1953 (Woodring 1954). It involved 23 of the most creative scientists of the time, among them Sterling Hendricks, George Evelyn Hutchinson, and George Gaylord Simpson. The disciplines represented included biochemistry, bio-

physics, biology, paleontology, geology, and geochemistry. I attended the symposium and benefited greatly from it. One of the items that caught my attention was a discussion of the role of geochemistry in imposing limits on speculations concerning the origin of life and in evaluating paleoecology. In addition there was optimism that biochemistry could make contributions to geochemistry. I came away from the symposium with the resolve of conducting personal research in organic geochemistry.

The policy of the Carnegie Institution of Washington is to identify creative scientists as staff members and then to give them freedom to make their own decisions concerning details of their research. In the course of a year the director of a department must make decisions regarding personnel and budgets and also supervise preparation of an annual report. However, unless a director creates unnecessary chores, time is available to conduct a personal research program, and a director is expected to do so. In addition, a director makes speeches, serves on visiting committees, engages in activities of the relevant scientific societies, and takes on other tasks.

On the day I took office as director in September 1953, I noted that various scientists were proceeding with their research. I found a few administrative functions dealing with the support staff that needed to be improved, but that was soon taken care of. My attention turned to research with a goal of determining if amino acids might be preserved in fossils. In today's world the cost of initiating a new program in geochemistry is likely to amount to \$100,000 or much more, and delays in obtaining funds are likely. The funds necessary to begin my program in organic geochemistry amounted to less than \$100. The jars for paper chromatography and the necessary chemicals were procured within a week. As a starter I chose to examine the present-day abundant common clam *Mercenaria mercenaria*, of which there were ample fossil shells in exposures nearby. The present-day shells contained about 0.2% of proteins that included the usual amino acids. Miocene shells contained a lesser number and amount of amino acids. Further studies showed that some of the amino acids were much more stable than others, but all were subject to degradation at elevated temperatures. Time of degradation versus temperatures were run on a number of the amino acids, especially alanine. The degradation was found to follow a first-order Arrhenius equation over a wide range of temperatures. At low ambient temperatures some protected amino acids can endure for hundreds of millions of years. The results of the amino acid studies appeared in our annual report (Abelson 1954b). They were also described in an article published in *Scientific American* (Abelson 1956a). This had the effect of stirring interest in paleobiochemistry and fossil biochemicals. Many copies of reprints of the article were sold by Freeman. At that time I was not the only person doing organic geochemistry. A few chemists

were active in petroleum company laboratories. However, most of them were secretive about their activities.

For most of my stay (1953–1971) at the Geophysical Laboratory, I was fortunate in having for colleagues in research Thomas Hoering and Ed Hare. Both are skilled scientists with excellent capabilities in instrumentation. Hoering is a talented experimentalist in many fields including mass spectrometry and gas chromatography. Soon after Hoering arrived at the Geophysical Laboratory he made operational a state-of-the-art mass spectrometer. As new equipment for gas and liquid chromatography became available, they were procured and exploited. Ed Hare developed fast, sensitive amino acid analyzers. The capabilities of his instruments were a great improvement over earlier techniques. They made feasible many detailed studies that I participated in. With his equipment he could measure the amounts of various amino acids in a hydrolyzate in about 20 minutes. The device could detect a picomole of an amino acid. The original Stein and Moore Dowex–50 resin column separation method required about a week. It could detect 50 nanomoles of most of the amino acids. Paper chromatography was performed in two days with a convenient level of amino acids of 0.1 micromoles.

Both Hoering and Hare were very productive in research. Their activities were supplemented by fellows, including Patrick Parker, Richard Mitterer, and Jack Valentine. During the 1960s we had one of the liveliest centers of organic geochemistry. Most of the experiments were designed to illuminate major questions. These included:

- Did ancient forms of life employ biochemicals and biosynthetic processes similar to those of today?
- How stable is organic matter under natural conditions?
- What are the mechanisms in the formation of kerogen, the major form of organic matter in nature?
- What are the conditions and mechanisms by which kerogen gives rise to petroleum?
- What was the nature of the primitive atmosphere of the Earth and of some chemicals formed abiogenically?
- Does life exist elsewhere in the solar system?

Each year during my stay at the Geophysical Laboratory I conducted some research. The activities described in annual reports of the laboratory tended to be scouting expeditions. Brief mention of some of the more significant ventures follows.

Together with Ed Hare I examined the amino acid content of 100 different mollusks, including clams, oysters, snails, and nautilus (Hare & Abelson 1964). Some of the species had evolved relatively recently. Some

were closely related to species present during Paleozoic times. The total protein plus chitin content of shells ranged from 5% in ancient forms to as little as 0.01% protein in highly evolved species. The fractions of specific amino acids varied greatly among the recently evolved forms, especially the neogastropods. However, the amino acid distribution and content of the various ancient molluscan forms were strikingly similar, pointing toward a single common ancestor.

The proteins of all living forms consist principally of L-amino acids. Ed Hare and I asked the question, "What is the behavior of L-amino acids in the environment as a function of time and temperature?" The answer is that they become a mixture of D and L isomers. The amino acids in nearby upper Pleistocene shells of *Mercenaria mercenaria* were found to contain substantial amounts of the D-amino acids. The characteristic time for racemization ranged from 10,000 to 1,000,000 years, depending on temperature and the particular amino acid. Amino acids in aqueous solutions held at 160°C were racemized in a week. Any primordial soup probably contained a racemic mixture of amino acids. Subsequently, Ed Hare employed an amino acid analyzer to follow the conversion of isoleucine to alloisoleucine as a function of time and temperature. This research led to a geochronometer that was helpful in correlating Pleistocene sedimentation. Today petroleum geochemists often develop useful information for exploration by measuring the degree of isomerization of other chemicals such as the steranes.

Together with Tom Hoering I made detailed studies of the fractionation of carbon isotopes that occurs in photosynthetic microorganisms (Abelson & Hoering 1960, 1961). Some of the organisms were thought to be closely related to Pre-Cambrian species. The organisms were grown under controlled conditions. After harvesting, their amino acids were separated and carbon isotope ratios of parts of each of the various molecules were determined. We found that CO₂ is fixed by at least three different pathways and that isotope fractionation is similar in the various species.

Much of my efforts was focused on kerogen. This complex, insoluble substance of variable composition constitutes most of the earth's organic carbon. Ancient kerogen was the source of the world's petroleum. Kerogen is formed from the constituents of living matter which include complex carbohydrates and proteins as major components. In an aerobic environment these substances are usually consumed as food. However, scavengers are far less efficient in an anaerobic environment. Following death and lysis, constituent simpler sugars and amino acids or peptides are released by hydrolysis or enzyme actions. Thus reactive aldehyde groups of sugars become available to react with amine groups of amino acids. In the laboratory a similar situation to nature can be studied when solutions of

glucose and amino acids are brought together at pH 8. The components react noticeably within a day at 4°C. With time and somewhat higher temperatures they combine to form substances that have properties quite similar to natural humic acids and kerogen. Together with Ed Hare I synthesized a number of these artificial kerogens and compared their behavior to that of natural kerogens (Abelson & Hare 1969, 1970). The two types of kerogen were exposed to solutions of a mixture of amino acids. Results with the two types were closely similar. Cysteine disappeared very rapidly; dibasic amino acids, rapidly; hydrophobics at an intermediate pace; and hydrophilics slowly. With both types of kerogen the disappearance of amino acids was accompanied by the appearance of substantial amounts of NH_3 . Both types of kerogen exhibited electron spin resonance which was enhanced by the presence of oxygen.

The disappearance of amino acids and other small organic molecules in organic-rich anaerobic sediments is only partially due to microorganisms. Once kerogen is formed, it reacts to a large degree irreversibly with amino acids and proteins. A substantial body of data on such reactions was presented in the 1968–1969 annual report.

In 1962 knowledge concerning the formation of petroleum was primitive. It was known that there are source rocks containing organic matter and porous reservoir rocks that trap petroleum after it is formed. It was also well known that by heating oil shale to 550°C one could obtain petroleum-like fluids. Hoering and I decided to conduct studies at mild temperatures on the kerogen of the Green River shale and other formations to come closer to duplicating petroleum-forming processes occurring in nature (Hoering & Abelson 1963). The research yielded strong evidence that mild thermal degradation of kerogen is the principal mechanism by which hydrocarbons in natural gas and petroleum are produced. We observed the liberation of a series of saturated straight-chain hydrocarbons which are abundant in natural petroleum, ranging from methane (C_1) through n-dodecane (C_{12}). By conducting our experiments at a series of different temperatures ranging from 25°C to 400°C, we were able to monitor maturation processes. For example, emission of CO_2 was completed early. By incubating kerogen in the presence of D_2O , Hoering was able to demonstrate that the D was incorporated in the formed hydrocarbons. Thermal tests on kerogens ranging in age from recent to Pre-Cambrian times were also performed. The Pre-Cambrian carbonaceous matter yielded mainly small amounts of methane.

A paper I presented on June 25, 1963 at the Sixth World Petroleum Congress (Abelson 1963) included the following:

- (a) The processes of biosynthesis have remained fundamentally unchanged during the past 500 million years.

- (b) Thus the study of recent sediments is cogent to the problem of petroleum genesis.
- (c) Important chemical steps leading from living matter toward petroleum occur rather rapidly following death of the algae. Examination of recent sediments discloses that most of the organic matter is in the form of kerogen—an insoluble complex. Although some nitrogen is present, almost none of the nitrogen can be liberated as free amino acids. Recent work by Abelson and Parker shows that even the most recent sediments contain almost no unsaturated acids. This is in striking contrast to the algae, the fatty acids of which are mostly unsaturated. Another puzzle is the relatively low content of saturated fatty acids in recent sediments. Although the algae contain 5–25% fatty acids, the organic matter in sediments is less than 0.1% fatty acid.
- (d) The organic contents of recent sediments are already much altered and resemble petroleum more closely than biochemicals.
- (e) In a note added in proof to the paper, the Hoering-Abelson work on pyrolysis of kerogen was mentioned: “Green River shale was incubated at temperatures ranging from 185°C to 407°C. Products were qualitatively similar to those found in natural gas and petroleum. Only minor amounts of unsaturates were formed. Temperature dependence of rate of evolution of methane was consistent with second order reaction kinetics ($A = 10^9$, $E = 40,250$ cal/mol).”

One of my ventures was to conduct experiments on the abiogenic synthesis of chemicals on the primitive earth. The procedures were similar to those employed earlier by Stanley Miller, who had worked with a methane-ammonia atmosphere. I tested a number of different compositions including one with CO, N₂, and H₂. These seemed more likely to approach that which actually existed on the early Earth (Abelson 1956b, 1966). I obtained amino acids. A crucial intermediate is HCN. I also noted that irradiation of a mixture of CO₂, N₂, and H₂O did not give rise to any amino acid. These observations were relevant to the question of whether life had originated or existed on Mars. The planet possesses a trivial amount of H₂O, and its atmosphere consists mainly of a limited amount of CO₂. Nevertheless, at the annual meeting of the National Academy of Sciences in 1965, a committee of the Space Science Board took the position: “The biological exploration of Mars is a scientific undertaking of the greatest validity and significance.” I reacted to that statement some months later with an article in the *Proceedings of the National Academy of Sciences* (Abelson 1965) with data and an analysis which conveyed the message that existence of earth-like life on Mars was extremely unlikely. The mes-

sage was disregarded, and a huge sum was expended in an effort to find life there. No life was found. In future, those people considering the possibility of life elsewhere in the solar system should also note the following.

Virtually all organic chemicals are unstable, degrading at rates that are dependent on temperature. Under favorable conditions, some chemicals can persist for hundreds of millions of years. But in natural environments substances are usually altered by mutual reactions. An example of the consequences of such reactions was provided in experiments in which *Chlorella pyrenoidosa* pellets were incubated at 170°C and 143°C. In 10 days at 170°C, half of the amino acid residues disappeared (Abelson 1961). It was estimated that in a 20-minute exposure at 170°C, on the average, each typical protein molecule would experience degradation of about one constituent residue. A similar degree of degradation is estimated to occur in 35,000 years at 15°C. Thus if organisms are in an environment in which they cannot repair damage they lose viability. These estimates are crude, but they indicate limitations on potential survival time for nonmetabolizing organisms.

Before billions of dollars are spent on the assumption that life may exist on Mars, careful experiments should be made on the long-term survivability of nonmetabolizing living matter. A variety of organisms and spores and a broad range of temperatures should be included. There should also be experiments on exposure at various levels to the forms of radiation that penetrate the surface and subsurface of the planet.

During the days of my tenure as director, the style of operations and research at the Geophysical Laboratory was unique by today's standards. Each staff member was expected to personally conduct his or her own research. There was a good machine shop and good procurement but there were practically no technicians. When a graduate student, or post-doc, or other visitor was accepted to work at the laboratory, a staff mentor was chosen. The duty of the mentor was to hasten the day when the new person could be conducting independent research. Often only a few days were required. The laboratory was supported on endowment funds and contingency money was quickly available to foster unexpected initiatives. Our only chore, and that was a major one for me, was to produce an annual report. Each year the lab produced what amounted to a book, and I edited every manuscript. The discipline of assembling material for the annual report was extremely stimulating to the staff. In the month or so before the deadline the lights in the laboratories burned late.

One of the initiatives I took as director was to increase substantially the number of fellows and guest investigators. There were both pre-doctoral and post-doctoral fellows; their tenure was two to four years. My 1966–1967 report stated, “During the past 15 years, 56 fellows and 25 guest

investigators have been in residence. They have represented 15 universities of the United States and 15 foreign countries. Most of our alumni are now associated with major universities.”

The American Geophysical Union was the focus of one of my adventures. To a degree I was associated with the Union beginning in 1939 when I came to the Department of Terrestrial Magnetism of the Carnegie Institution as a staff member. Most of the members of the department were geophysicists. I was sufficiently interested in geophysics that in 1947 I joined the AGU. I became active in AGU immediately after I was made Director of the Geophysical Laboratory. A few years later I was appointed Chairman of the AGU Meeting Committee. This brought me in frequent contact with leaders of American geophysics. I especially enjoyed conversations with Harry Hess, Maurice Ewing, and Lloyd Berkner. They were great enthusiasts and fun to be with.

In 1958 a crisis loomed in the matter of adequacy of publications in geophysics. Research activity was increasing and programs related to the International Geophysical Year (I.G.Y.) were projected to lead to many articles. Merle Tuve was editor of the *Journal of Geophysical Research*, which published about 800 pages a year and was subsidized by the Carnegie Institution. He was weary of being editor and wished to dispense with the chore. The AGU published *Transactions of the American Geophysical Union*, but the reputation of the *Transactions* was only fair.

Maurice Ewing, President of AGU, and Lloyd Berkner, President-elect, decided that AGU should accept JGR from Merle Tuve and embark on an expanded publication program.

I was invited to a meeting involving those two, together with Waldo Smith and James Peoples, a geophysicist at the University of Kansas. During the meeting I made a few suggestions about the AGU publications program. These were meant only to be constructive. To my surprise when the meeting ended, James Peoples and I had been asked to serve as co-editors of the *Journal of Geophysical Research*.

When I considered the invitation to become co-editor, I could visualize a process whereby with minimal effort on my part the journal could be expanded while quality was maintained. There were excellent capabilities available for style editing, proofreading, manuscript custody, and reviewing. My principal activities were to identify emerging frontiers of geophysics such as space research and to recruit manuscripts. James Peoples capably handled the half of the journal devoted to solid earth. To cover costs of an expanding flood of manuscripts, I saw to it that page charges were established. We maintained a four-month publication time. In a few years the *Journal of Geophysical Research* was publishing 6000 pages annually, and AGU was prospering.

In early 1962 the position of Editor of *Science* became open. I was Chairman of the AAAS Meetings Committee, but not interested in becoming editor. When first approached by Dael Wolfe, Executive Officer of AAAS, I told him that I did not wish to stop my research in organic geochemistry. Ultimately an arrangement was made in which I stayed on at the Geophysical Laboratory but devoted a part of my time to *Science*. A combination of circumstances led to a favorable outcome. Dael Wolfe was both Executive Officer and Publisher of *Science* and was an excellent manager. The staff of *Science* was competent and helpful. The trends in advertising revenues and membership were positive. Techniques of recruitment of articles that were successful with JGR were also applicable to *Science*. The telephone was a useful device. My principal activities were to recruit and select articles, to write editorials, and to seek to make the magazine more interesting and useful to the readers. The staff took care of the large work load associated with the magazine. I enjoyed the stimulus of seeking to be aware of the important developments affecting the scientific enterprise. For a person who loves to learn, it is a great challenge. In addition, the readership of *Science* is alert and critical. Serving it is a privilege.

In 1971 I became President of the Carnegie Institution of Washington and continued as Editor of *Science*, but it was no longer feasible for me to maintain a research program. However, I continued to have learning experiences. I served on visiting committees to universities and on the Advisory Council of the Jet Propulsion Laboratory. I had a glimpse of the corporate world as a member of the Board of Directors of the Washington Gas Light Company and as a member of the Research Coordination Council of the Gas Research Institute. A fairly comprehensive view of U.S. industrial R&D was obtained during two-day visits to more than 20 major industrial research and development laboratories. Another venture was guided tours of automobile production facilities in the U.S. and Japan. I also visited a number of Japanese and Chinese research laboratories. One of my fun travels took me to the Alaska pipeline during its final construction phases. In early March 1977 I was at Valdez, the Brookes Range, and the production facilities at Prudhoe Bay. Earlier I had been underground in a number of mines. In these various circumstances the differing and often contrasting value systems and modes of operation were particularly interesting. For example, in the industrial world research is closely coupled to development, and industrial R&D is usually conducted by interdisciplinary teams. In general the morale of the team members I encountered was at least as high as that in academia.

One of the characteristics of scientific research is that it is on the forefront of societal change. In turn, the frontiers of research are continually evolving.

ing. This is particularly true of earth and planetary sciences, and it will continue to be. Prior to World War II, advances in earth science were largely derived from observation, together with use of the pick and hammer and hand lens. Geophysics was in its beginning phases; geochemistry was largely analytical chemistry of rocks. The most sophisticated aspect of earth science was experimental petrology of the kind conducted at the Geophysical Laboratory. The total number of earth scientists who spent most of their time in the laboratory was probably less than 100.

In the late thirties and following World War II, several men whose basic training was in physics or chemistry made seminal contributions to earth science. One was Alfred Nier, a physicist who with a mass spectrometer he had built made the first dating of Pre-Cambrian rocks. Another was Maurice Ewing, whose background was in physics. He was a great enthusiast who personally explored the oceans and invented many pieces of equipment to facilitate his work and that of colleagues. I remember well an inspiring lecture Ewing gave on exploration of the mid-Atlantic ridge. Merle Tuve organized a series of experiments in explosion seismology which discredited the early picture of the onion-like structure of the interior of the earth. Julian Goldsmith (1991) has written of the stimulus to isotope geology and planetary science provided by Harold Urey, a chemist at the University of Chicago. Students who were there at the time have had a profound effect on earth and planetary sciences—one that continues today.

The I.G.Y. and Sputnik were followed by space exploration and expanded funds for research. Opportunities created by return of the moon rocks were a great stimulus. The Allende and other meteorites provided materials for study of early events in the formation of the solar system. In recent years 8000 meteoric fragments collected in Antarctica have provided additional targets for measurements. Objects originating outside the solar system have been identified. Meteorites produced by impacts of other bodies on the Moon and Mars have also been studied. More of such objects will doubtless become available.

An important factor in the vitality of earth science has been isotope studies made feasible by great improvements in mass spectrometry. These have involved improvement of at least an order of magnitude in the precision of measurements, greatly enhanced rates at which determinations can be made, and a decrease of several orders of magnitude in the weight of samples required for the mass spectrometer. Today the mass spectrometer is probably the most ubiquitous and powerful tool in earth and planetary sciences. Its applications include dating of events throughout the history of the Earth and the solar system, studies of metamorphism, studies of determination of prehistoric ocean temperatures, studies of

formation of sulfide ore deposits, identification of plumes originating in the mantle, and applications to organic geochemistry.

Activity in organic geochemistry has expanded greatly since the 1960s. By the early 1970s many fossil biochemicals (now called biomarkers) had been identified. These substances were originally components of living matter—for example sterols, which had been modified to become steranes. Modifications of the original structures are often found to be time and temperature dependent. Thus measurement of biomarkers can provide evidence of the thermal history of kerogen and the oil produced from it. Detailed information about the thousands of components of petroleum can be readily obtained by use of gas chromatography coupled with mass spectrometry and computer analysis.

Information obtained by organic geochemists is incorporated into computer models that are important to successful petroleum exploration. The organic geochemists are now members of the exploration team. Their activities and numbers are now reflected in a number of publications, including journals devoted to their field.

I turn now to speculation about future efforts in earth science. Priorities and opportunities will be heavily affected by societal concerns about the environment and subsequently about energy supplies. The level of research activities will, of course, be affected by availability of funds. Prospects for the funding of the kind of academic research currently in vogue are only fair. During the past several years the number of grant proposals have increased faster than funds. In 1990 only 32% of proposals in earth sciences were funded by the National Science Foundation. That rate is down from 50% several years earlier. There will still be a continuation of exciting results as even more powerful equipment becomes available. However, the important findings will largely be made by those with access to the best equipment.

Some of the students currently on the path to a PhD degree may obtain faculty appointments, but a majority will probably be unable to obtain grant support to carry on research similar to that of their thesis. For many years, the federal government was a relatively reliable source of support for basic research. The overall budget squeeze could lead to erratic effects on funds available to granting agencies. At the same time, there is more congressional interest in achieving faster practical effects from research support. We are now in a different era than that of 1955–1985.

One of the questions that the nation faces is what to do about global change and the greenhouse effect. Other countries are proposing reductions in emissions of CO₂, and they are pressuring the U.S. to comply. Trillions of dollars could be involved, and the knowledge base for decisive action is weak. Hence the federal government will be supporting a broad program to measure global change, present and past.

A much larger potential for employment is in the private sector, where huge amounts of money will be involved in remediation of waste dumps and treatment of their leachates. Efforts to produce some of the more than 300 billion barrels of oil left in place after secondary recovery will merit attention. Improvements in knowledge about methods of influencing the movement of fluids in porous rocks and soils would have tremendous economic payoffs.

The remediation of waste dumps and treatment of their leachates will require efforts over a period of 30 years or more and expenditures of perhaps \$500 billion. The waste sites differ in volume, geometry, and content. Some have only inorganic contaminants and soil; others have soil and complex mixtures of many different organic chemicals of varying toxicity located helter-skelter in the waste site. The various chemicals have different solubilities in water and degrees of adsorption to soil. Some of the chemicals are readily leached and are already in ground-water plumes some distance from the waste site. At the other extreme are chemicals that are virtually immobile. To date the science and technology employed in assessing the situation at sites and remediating them has been costly and primitive. Achievement of effective and efficient remediation will require an interdisciplinary approach that includes earth scientists as key members of the team.

Other problems of contamination of ground water arise from pesticides used in agriculture and by leaky underground tanks containing chemicals or petroleum products. Solution to these problems will require the efforts of hydrologists knowledgeable about the behavior of chemicals in soils.

Ultimately it may be found that some of the phenomena encountered at waste sites have counterparts in the movement of petroleum in soils and rocks. The hydrophobic nature of some of the chemicals is comparable to that of components of petroleum. Indeed, benzene, toluene, and the xylenes are present in petroleum, at many waste sites, and in some leachates.

The habitat of oil is, of course, greatly different from that of a waste site. Nevertheless, one can question whether the technology of petroleum production is more advanced than that of the remediation of waste sites. After all, the technology has left about two thirds of the oil in place. Ultimately a combination of bright ideas, computer modeling, and choice of the best drive mechanisms will extract much of that oil at nominal costs.

The past 50 years have witnessed enormous changes in the conduct of earth science. The next 50 years are likely to bring changes presently unknowable but of comparable magnitude. Professors who have a crucial role in shaping the capabilities of their students should seek to prepare them for an uncertain future. One way is to emphasize acquiring knowledge

of the fundamentals in mathematics, physics, chemistry, earth science, and biology as undergraduates. Then as new opportunities arise their students can master the special knowledge needed to tackle the new problems.

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