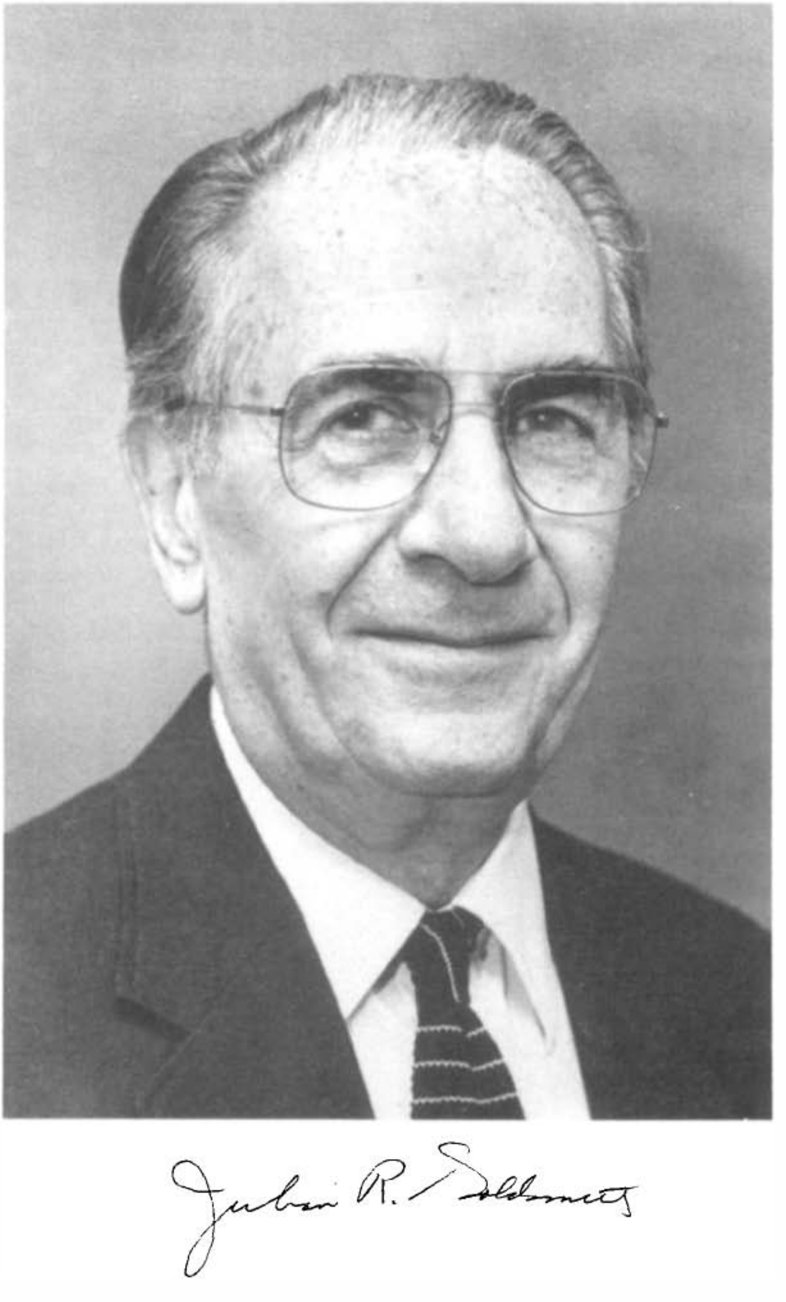

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SOME CHICAGO GEORECOLLECTIONS

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All thoughtless people who have lived long enough probably feel that their life span embraced a period of great change, of progress, and perhaps even of revolution. As one fortunate enough at more than three score and ten to be still personally active in the laboratory, I can look back at what I used to be paid for doing and observe how my day-to-day work conjoins with the views of those probing the Earth since the 1930s. I shall attempt to look back without too much reinventing or tampering with history, and shall also attempt to give my views of things then and now, limiting myself to that area of our science best known to me.

I arrived as an undergraduate at the University of Chicago in 1936, one year behind my age group because of an overly protective mother who delayed my entrance into first grade, ensuring an unfortunate midyear schedule throughout school. I came to the University of Chicago not so much because of any reputation for quality, but rather for its convenience to my suburban home in Oak Park. Oak Park and River Forest Township High School was a good school (we were all told), but, sadly, I was able to neglect certain aspects of my education, particularly in science. I came to the university with too little mathematics and with no chemistry or physics. Having grown up on the prairie, I had an interest in nature, but more toward the biological than the physical sciences. My most eye-opening subject in high school was a course called "Social Problems"; not only was the subject of sociology unknown in most secondary schools, but in the then stuffy village of Oak Park, who had ever heard of a social problem? I was tempted to major in it, but then again I might also have

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become a professional photographer, for photography was an ardent hobby since fourth grade. My freshman advisor at Chicago was Bill Krumbein, who coincidentally was an instructor in the Department of Geology, and when I was seeking an elective fourth subject in my first year, he suggested the introductory course in geology. Never having heard of geology, I took it, perhaps in part because of the lack of prerequisites. If I had taken chemistry first, as I should have done to be better prepared for geology, would I have ever heard of geology?

I had never thought about the origin of the things that are "there"—the mountains, the rocks, the minerals (what were they?), even the soil—although I avidly read the family set of the Book of Knowledge, which linked shrinking wrinkled apples with the origin of mountains. I became interested, and my involvement was fostered and promoted by the inspirational teaching of J Harlen Bretz, to whom I was exposed in my second quarter of the first year. I was hooked, but it became immediately obvious that I had real deficiencies to make up, so I girded my loins and sailed into mathematics and science. My third year in college almost did me in, and at one point I had run myself down to the point that I just got sick and went home to bed for a week, but wonder of wonders, I *liked* the subjects!

Sometime during this period I became aware of a quiet guy who had a title that differed from the others: Norman Levi Bowen, Charles L. Hutchinson Distinguished Service Professor. NLB had come to the university shortly after I did, in 1937, as a replacement for the retiring Albert Johannsen. Johannsen was an outstanding petrographer, and in addition to having a love affair with igneous rocks, he also was an authority on dime novels, postage stamps, first editions of nineteenth-century illustrated books, coins, genealogy and autographs, to mention a few of his interests. Words of erudition, wisdom, and humor stand out in and at the beginning of each of the chapters of his four-volume compendium, *A Descriptive Petrography of the Igneous Rocks*. The difference in interests, however, between Joh and Bowen was striking—petrography versus petrogenesis. I had become aware of the research going on in Bowen's "hot lab" (before "hot" meant radioactive), in which the students, mostly Canadians, were doing things not done by the other students. By this time I had decided that I was interested in the more quantitative aspects of Earth science, and experimental petrology filled the bill.

In the last half of the 1930s, the Department of Geology at Chicago was highly respected and, if one uses attractiveness to students as a criterion of quality, had produced more PhD degrees in geology than any other institution. This lead continued until the early 1960s, when it was probably lost to Columbia. Reputations of institutions and departments, however, persist *at least* a generation after the fact, and although in the 1930s the geology department at Chicago was considered one of the elite, and received a large boost when Bowen arrived, in hindsight I feel that it had benefited from that lag and was overrated. Chicago *was* a world leader when T. C. Chamberlin left the presidency of the University of Wisconsin to build a department at the newly created University of Chicago in 1892. William Rainey Harper, the first president of the university, was an enormously persuasive man, who convinced Chamberlin not only to leave the presidency of a major university, but to come as chairman in a non-existent department at an embryonic university. Remarkable how one so far removed from science (a Hebrew scholar and nominal theologian) was insightful enough to *select,* in addition to *acquire,* outstanding men of science such as Chamberlin. Chamberlin became deeply involved in inter-departmental cooperation with F. R. Moulton of astronomy, and the two developed the long-lived and important planetesimal hypothesis of the origin of the solar system.

Even if the aura of the "early days" of the Chicago department persisted into the 1930s, it had become, I feel, quite conservative and, in spite of the firebrand Bretz and the shot in the arm by Bowen, rather stodgy. Although we were thoroughly drilled in Chamberlin's concepts of multiple working hypotheses, little time was spent in courses in the discussion of alternate theories, at least with respect to the older, or more grandiose, aspects of geology. Perhaps this was just an expression of the general American conservatism as exemplified by the ruling concept of permanence of continents and ocean basins and, unlike European teachings, rejection of any consideration of continental drift. In sharp contrast to this attitude, however, was the Bretzian catastrophism of the channeled scablands, which was ridiculed by a large part of the geologic community. The story of the ultimate exoneration and appreciation of the renegade Bretz, who used observations and perceptions that violated the sacred cow of uniformitarianism, need not be told here, but the importance of his work wasn't fully felt until Martian surface features indicated the validity of his views on other planets! Bretz was a forceful, incisive person, whose total effort as a teacher was to make students think and to look at all sides of a problem, and he taught me that little in science is cut-and-dried.

My interests, however, drew me closer to Bowen, and by the time my graduate work began, we had developed something of a teacher-pupil bond, fostered by my respect for him and aided by his appreciation of my willingness to approach him directly and discuss things on a one-to-one basis; he was bothered by the fact that most students held back in awe. A more reasonable picture of Bowen can be illustrated by an action that took place when the war interrupted my graduate research work in the "hot lab." An enlightened Selective Service Board had called me in and told me to bend every effort in the next month or two to find employment in a situation in which my background would be used to aid the war effort, other than as a soldier. I was hired by the Corning Glass Works and prepared to leave Chicago. One day, while working in the lab before leaving, I suddenly felt my arms pinned to my sides from behind as I was lifted and rotated 90°—and released. Looking up from the floor, I saw a grinning Bowen, holding a card from the Office of the Registrar and reading "Drop Goldsmith."

At Corning I became involved in the surface chemistry of silicates, especially 96.5% SiO_2 glass (Vycor). We were using it in a ceramic process to make insulators and other parts for a mysterious factory in Oak Ridge, Tennessee. It turns out that these were used in the Calutron for isotopic separation of uranium, from which atomic bombs were made, but at that time what went on was a very well kept secret. I became involved with the role of water in silicates and in OH-bonding to unsatisfied silicon atoms, all of which has carried over to current interests in silicate behavior. In 1946 I returned to Chicago to complete my PhD, which was granted in June 1947. The war had created much more than just a hiatus in the workings of the university, the faculty, and in the lives of most students. During the war Bowen had gone back to the Geophysical Laboratory, Carnegie Institution of Washington (1942–44), for war work, from which he returned to Chicago as departmental chairman. Bowen broke the long-lasting financial drought in the department: The expense and equipment budget went up by a factor of 30, and staff changes took place. Carey Croneis had gone to Beloit College as president; Krumbein formally resigned; R. T. Chamberlin retired, with Bretz also retiring shortly afterward; Marvin Weller came to Chicago, as did Tom Barth, Walter Newhouse, Lee Horberg, N. A. Riley, and Robert Balk; and in 1947 I was taken on as a Research Associate, as were Hans Ramberg and Kalervo Rankama. Barth then was the prime mover in recruiting several other "Scandahoovians," as they were called: Frans Wickman, who left after one year to become the director of the Riksmuseets (Swedish National Museum), Brunjoff Bruun, and a few others in the area of analytical chemistry and spectroscopy, but they were essentially transients. Bowen shook the place up a great deal, and the university administration respected him and demonstrated that respect by increasing the departmental budget. After two years as chairman he resigned from that position and in 1946 returned once again to the Geophysical Laboratory, where he retired in 1952. Unfortunately (or fortunately?) for me, he left Chicago before I finished my thesis research, and although Tom Barth became my titular advisor, I was really on my own.

I inherited Bowen's laboratory and shortly afterward was able to install hydrothermal equipment, based on the designs of Frank Tuttle. My first graduate student, Irving Friedman, had been associated with Frank at the Naval Research Laboratory during the war. The laboratory, as set up by NLB, consisted of one platinum-wound quenching furnace and one Pt-wound melting or preparation furnace, plus a commercially available Tagliabue temperature controller, a White single potentiometer, and a reflecting wall galvanometer. Only one temperature at a time could be measured. This, plus an old analytical balance, which had been Johannsen's, was *it*. Yet in the late 1930s, it was the only working high-temperature petrological lab in the USA outside of the Geophysical Laboratory! Experimental petrology was pretty well confined to Washington, DC, and then Chicago; the University of Michigan had also obtained a furnace or two, but with few exceptions it never seemed to catch on there.

But this is all prehistory—setting the stage, as it were. The changes that took place right after the war were enormous. The University of Chicago was a truly inspiring place to be—the scientists here, including those brought here during the war for the "Metallurgical Project" [better known as the Manhattan Project (the self-sustaining nuclear reaction)], chiefly in physics and chemistry, were outstanding. I suppose I still mourn the premature death of Enrico Fermi, a man to whom *any* scientist could talk on *any* subject and come away enriched. I shall limit myself to a quick rundown on the postwar revolution in geochemistry and associated geophysics that took place, the seeds of which were planted by the presence of rather few people—Harold Urey, Harrison Brown, Bill Libby, Mark Inghram, Subrahmanyan Chandrasekhar, Andrew Lawson—and fostered by the presence of many others, including Joe and Maria Mayer, Sam Allison, Gerard Kuiper, Cyril Smith, Charlie Barrett, Tony Turkevich, Hans Suess, Clyde Hutchison, George Reed, and others. Curiously, Willie Zachariasen, who became one of my dearest friends, although potent in crystallography and an outstanding mineralogist, interacted little with the other Earth scientists. Urey fathered isotopic geochemistry, and Libby carbon-14 dating; Mark Inghram developed a mass spectrometer that made it possible to work with small samples, opening the field up to the Earth scientists; double beta decay was pioneered by Inghram and John Reynolds, and also a search for extinct short-lived nuclides was carried out by Jerry Wasserburg and Richard Hayden. The cross-disciplinary interactions were the guide to future developments. No one was afraid to talk to anyone else in a different field, and this was particularly important for the young people. It was at this time that new and innovative techniques were introduced by Inghram and others into other fields such as geochemistry, astronomy, and cosmology. Harrison Brown, along with Harold Urey, gave a major boost to the work in meteorites and cosmic abundances begun by V. M. Goldschmidt in Norway and W. D. Harkins at Chicago prior to 1920. G. N. Lewis stated, presumably referring to the odd-even rule, "It was Harkins who first called attention to the striking connection between the atomic weights of the elements and their abundance, not only in the earth's crust, but in the meteors."

When Bowen left Chicago, W. H. Newhouse became chairman of the Department of Geology. Walter Newhouse was an idealist and a reformer, whose mission was to eliminate the trivial that he felt cluttered up much of geology. He was a completely dedicated, totally honest and forthright man whose directness and attitudes angered some elements of the department. In addition to his fierce determination to modernize and eliminate traditionalism, personal problems became intertwined with his science and administration, and unpleasant conflicts developed within the department, ultimately leading to the departure of at least three faculty members. Perhaps unrelated to this was the fact that Harold Urey, needing more space than available to him in the chemistry department, wanted to move his laboratory to the basement of Rosenwald Hall, the principal home of the Department of Geology. For reasons not clear, possibly because of Newhouse's concern with domination from an "outside" source, he was turned down and ended up in the Research Institutes building, several blocks away. Urey was resented by some as an outsider. At the time I certainly had no basis to fault Newhouse's decision, for Harold (he *insisted* that I call him that, though the fact that he was the same age as my father and his Nobel Laureate status made it *most* difficult at first) wanted me to work with him on diamonds in meteorites. I declined, feeling I had to do "my own thing." By that time many, if not most, of Urey's laboratory colleagues were students, or recent PhDs, from the Department of Geology. Heinz Lowenstam, associate professor of geology, brought his knowledge of carbonate-depositing organisms to the laboratory and has consistently applied mineralogy and geochemistry to organisms and fossils. Sol Silverman was the first student in the department to work with Urey on oxygen in silicates; Harmon Craig, Jerry Wasserburg, Cesare Emiliani, and Irving Friedman all played important and varied roles.

Things were moving fast. In rather few years, new ideas, tools, and outlooks were developed that produced a quiet revolution in the chemistry of the Earth and planets, the implications of which remained for the most part obscure to classical geologists. Even George Kennedy, *far* from a "classical geologist," asked me (sometime later) if I thought isotopic analysis of Earth materials would really prove valuable. Although students in the Department of Geology at Chicago participated and contributed significantly to the revolution, as I have indicated, it extended beyond the department into other disciplinary areas, and the sharpness of focus provided by hindsight gives me a better perspective not only of its importance but of the resistance provided by most formal departments of Earth science. Harold Urey was a remarkable man, and with this same hindsight I would go so far as to say that his importance to Earth science has been matched by very few. His interest in the Earth and planets began with extension of his earlier Nobel Prize work on hydrogen isotopes to oxygen isotopic fractionation as a function of temperature and thus to paleotemperatures. His first published work involving isotopic fractionation in nature and leading to paleotemperatures was in 1932 (with G. M. Murphy) on N and O isotopes in air, Chilean nitrates, coal, and magnetite. In 1934, with S. H. Manian and W. Bleakney, he measured $^{18}O/^{16}O$ ratios in meteorites and igneous rocks. As an outgrowth of discussions with Harrison Brown on compositions of meteorites, he then became interested in the Earth and its origin, and rather soon thereafter he became fascinated with the Moon. He had a large picture of it in his office and would waylay anyone, especially visitors, and expound on it. His concept of the origin of the Moon, as a primitive object, turned out to be wrong, yet the energy (and the authority) with which his case was pursued greatly stimulated research and, as incorrect ideas at times do, proved beneficial. He rather soon expanded his interests to the planetary bodies, and disagreements with Gerard Kuiper, of the Department of Astronomy, led to a (apparently unidirectional) degree of ill feeling that to my knowledge was unique with Urey. His excitement and enthusiasm produced an ongoing series of seminars in the Department of Geology, in which one could witness the workings of his mind, for he thought out loud and did his calculating at the blackboard from the top of his head. He "learned" about geology during this period and in fact reinvented important concepts that were not part of his formal background! The series was ongoing because he would continuously change or modify his views and reschedule something for the next day! It was a wonderful sight to behold. And now? Has any MBA made a study of the number of jobs created in the areas of isotopic analyses and interpretation?

The phrase "revolution in Earth science" is generally thought of, and rightfully so, as the geodynamic or plate tectonic revolution, which related diverse aspects of Earth science to a major theme. How long is the time constant that distinguishes revolution from evolution? In my opinion another revolution had been taking place, but with a longer time constant, in geochemistry, beginning with V. M. Goldschmidt and relating Earth science with the physical chemistry of the cosmos. When I was a student, Bowen (arguably the world's leading petrologist) taught petrology without really considering pressure, except in terms of gas or fluid pressures. He *knew* about the role of pressure as a variable, but it did not enter into his petrogenetic scheme, in magmatic differentiation. These were the days when crustal concerns were dominant, and little or no thought was given to the mantle. The major issue in petrology was the granite controversy. Arguments flew on the matter of granitization vs magmatism, on the issue of solid diffusion in crustal genesis, and other esoterica. I think it fair to say that the first real scientific thinking about rocks developed at that time, largely through the efforts of Bowen. Percy Bridgman at Harvard and J. Johnston and L. H. Adams at the Geophysical Laboratory were the only ones doing or who had done high-pressure research, but this was unusual. Bowen did, however, develop the concept of a petrogenetic grid, which of course involved pressure, particularly as applied to metamorphism: This came when he developed a graduate course in metamorphic petrology at Chicago. Metamorphic petrology was more or less primitive, and my notes in the course are quite thin compared with those in igneous petrology. Granulites, for example, have really prospered—they were uncommon rocks then and known to but a few Scandinavian geologists. They have since somehow undergone an enormous volumetric increase in the Earth. The enormous budding of interest and development in metamorphic processes and rocks didn't really begin until after the war, promoted by laboratory studies at elevated pressures. The development of hydrothermal apparatuses and of higher pressure piston-cylinder and other devices took place, at first with George Morey and then O. F. Tuttle of the Geophysical Laboratory. Mention should be made also of the pioneering use of X-ray diffraction in opposed-anvil devices, including diamond cell designs pioneered by John Jamieson and Andrew Lawson. Lawson was (a very young) chairman of the Department of Physics, doing solid-state physics at high pressures. John did his PhD research with Lawson, although his degree was granted in geology. As a young faculty member, I too used Lawson's high-pressure laboratory and benefited from the insight of a physicist (perhaps tempered by being the grandson of the geologist A. C. Lawson of Berkeley). Jamieson was the first to determine the equilibrium relations of polymorphs (calcite-aragonite) by measuring relative solubilities in a high-pressure cell.

Little use had been made of thermodynamic calculations, in large part because few thermochemical data were available. A. L. Day and E. T. Allen's derivation of the melting curves of the plagioclase feldspars from ideal solution theory, and Bowen's elaboration of this, plus his location of the solidus curve prior to World War I, were about it. To my knowledge, Hans Ramberg was the first, using a combined field and theoretical approach, to seriously apply thermodynamics to rock-forming processes, and his 1951 paper with George Devore on element partitioning of Fe^{++} and Mg^{++} between phases as a function of temperature (still referenced in 1990) is a major conceptual advance in geothermometry. This was added to the pioneering work at the Geophysical Laboratory, including that of Bowen, Leason Adams, George Morey, Roy Goranson, George Tunell, and others on homogeneous and heterogeneous equilibria and the creation of the acid-solution calorimetric laboratory by F. C. Kracek, T. G. Sahama, and K. J. Neuvonen in the late 1940s, based on the pioneering work of K. K. Kelley. The calorimetry of silicates was first carried out in the Geophysical Laboratory by W. P. White earlier in the century, and solution calorimetry of geologically important substances carried out by K. K. Kelley at the Berkeley, California, laboratory of the US Bureau of Mines. Hans Ramberg also set up an acid-solution calorimetric laboratory at Chicago, and Dick Robie, who did his PhD work in calorimetry with Ramberg and in low-temperature calorimetry with J. W. Stout in chemistry at the Research Institutes, went to the US Geological Survey and there developed a world-renowned laboratory. These and other developments at the Geophysical Laboratory, including high-pressure apparatuses so important to experimental mineralogy and petrology, have been well covered by Yoder[1]; I am attempting to recount here, for the most part, the happenings that took place essentially on my home turf. Incidentally, and while mentioning names, Hat Yoder received his SB degree at Chicago in 1941, was tutored in meteorology by Carl-Gustaf Rossby, commissioned in the Navy, returned in 1946 for one quarter in the "hot lab" with Bowen, and then (after being advised by Bowen that high-pressure facilities would not be available at Chicago) missed all the excitement by going off to MIT. He ultimately became director of the Geophysical Laboratory.

Shortly after the war our department became alerted to the possible availability of German scientists; Tom Barth, in particular, knew Fritz Laves. In 1947 I had been granted one of the early Office of Naval Research (ONR) contracts, for the study of order-disorder phenomena in silicates, and arrangements were made with the US Navy to bring Laves to Chicago as a "Paperclip Specialist," the code name for German scientists who were willingly brought here by the navy without the knowledge of immigration and customs. Laves was put under my care (he would now be called an "illegal"), under the aegis of my ONR contract. I felt foolish, for he was a mature, internationally known and respected crystallographer and mineralogist, a student of V. M. Goldschmidt, yet he was my ward and was paid by my contract. Ushering him, after the fact, through immigration and customs was quite an experience: The officials didn't like it one bit!

[1]See Yoder, H. S. Jr. 1989. Scientific highlights of the Geophysical Laboratory, 1905–1989. In *Annual Report of the Director, Geophysical Laboratory*, pp. 143–97. Washington, DC: Carnegie Inst. Washington.

Although I held the title of research associate, I was in fact a faculty member, embedded in the university budget, and had students and gave classes. These were the days before research associates were equated with "postdocs," before the concept of "soft money" was part of the system, and I don't recall ever hearing of "summer salaries." My contract paid all of the stipends of Irving Friedman, Gunnar Kullerud (who did his research in my laboratory, but got his ScD at Oslo!), and Ursula Chaisson (now Ursula Marvin); Fritz Laves and Tom Barth were also involved. Kalervo Rankama and Hans Ramberg were research associates at that time, and Frans-Erik Wickman's status was that of a fellow for one year, because he had accepted the position of director of the Riksmuseets in Stockholm. My collaboration with Laves, which lasted until 1954, when he took Paul Niggli's post at the ETH in Zurich, was a fruitful and highly rewarding one. I cannot say too many kind things about Laves and how he treated this tenderfoot as an equal. Incidentally, the ONR experience itself was rewarding, for their support of areas of science not directly related to the navy's mission was generally recognized as a model for the way government money should be used to support research, with no strings attached. The ONR became the organization that showed the way for the National Science Foundation, first funded in 1951.

In the "early days" of ONR support, the naval officers in charge didn't quite know how to rank civilians. The admiral in charge of the Chicago branch office apparently considered me (with a PhD) to be the equivalent of a moderately high-ranking officer, but it was amusing to watch the way these contacts were (softly) handled. My contract officer was a metallurgist and quite interested in what happened to my metal (mostly stellite) pressure vessels in the experimental work, and much less so about what happened *inside* the "bombs."

A second personal fruitful collaboration was with Donald Graf, of the Illinois Geological Survey, who for some years came to Chicago one day a week. Oiva Joensuu, our wonderful optical spectroscopist, who did first-rate chemical analyses with now obsolete apparatuses, called him "Mr. Friday." We were both interested in carbonates, and got together after Keith Chave, then a student of Heinz Lowenstam's, introduced me to the problems of Mg in calcite. Keith had no intention of looking into the laboratory determination of equilibrium phase relations. I get the impression that much of this work in the 1950s has held up, and the concept of protodolomite, or poorly ordered Ca-rich dolomites, is ensconced in the lore of sedimentary petrology. My venture in carbonates was made more entertaining by the willingness of carbonates to undergo reactions, including order-disorder equilibria, that were kinetically difficult or impossible in the laboratory with the more recalcitrant feldspars.

At that time the research of chemists, physicists, and astrophysicists was certainly of more concern to those in the Earth sciences than vice versa. At some point between the days of the Chamberlin-Moulton association and circa 1940, geology seems to have lost its dignity. Urey's interest in and major contributions to Earth science were part of a process that helped make "geology" an active component of modern science. The issue of the "softer" nature of the geological sciences has been around for a long time, and the perception of geology as an easier way to satisfy science requirements than the more rigorous physics or chemistry has been long known, and remains so today. Although when I was a student this matter did not seem to be openly used as a discriminatory factor within the faculty of the Division of the Physical Sciences (Mathematics, Statistics, Chemistry, Physics, Geology, Astronomy), it *was* there, however, although Bowen did his part to reduce it. After the war several factors came into play to help alleviate the situation: (*a*) the presence of a group of outstanding faculty of different disciplines who had been involved with the "Project" and thus had *lived* with close interaction and cooperation for some time; (*b*) the formation of interdisciplinary research institutes incorporating scientists from several departments in a new building; (*c*) the presence of a group of mature, serious students whose graduate careers had been delayed by the war; and (*d*) the intermingling of students from geology, chemistry, physics, and astrophysics in the laboratories of Harold Urey, Harrison Brown, Mark Inghram, and others.

In addition to those people from this period whose names have already been mentioned, to give an idea of the products of what I could call this golden age, I shall list, at random, the following people who were at Chicago as faculty, students, or research associates. The cutoff in time that separates the names listed from those who followed them is both arbitrary and ill defined, but it does not get too deep into the 1950s. Omissions can thus be blamed on their youth and/or my weakness of memory. To keep the length of this piece down, I do not identify their fields or accomplishments, but many may be known to you:

Sam Epstein	Robert Nanz
Clair Patterson	Stanley Miller
Toshiko Kuki Mayeda	Jack S. Kahn
Edward Goldberg	James Arnold
George Tilton	Johannes Geiss
Peter Eberhardt	Anthony Turkevich
Fred Begemann	John A. S. Adams
Truman Kohman	Thomas Sugihara
Giovanni Boato	Edward Martell

George Reed	Ernest Nickel
Ernest Anderson	D. C. Hess
A. D. Suttle	Sherry Rowland
Peter Baertschi	Ernst Schumacher
Jacob Bigeleisen	H. B. Wiik
George Wetherill	Denis Shaw
Edward Olsen	Robert Ginsburg
H. Hamaguchi	Bertram Donn
H. Kigoshi	William Chupka
Haro Von Butlar	Charles McKinney
John McCrea	Edward Chao
Ray Siever	Leon Atlas
Meyer Rubin	Ernest Ehlers

This list and the preceding names include seven Day Medalists of the Geological Society of America, fourteen members of the National Academy of Sciences, three Nobel Prize winners, one Vetlesen Prize winner, and one recipient of the Crafoord Prize. Out of this environment, the following people personally influenced my scientific career the most:

1. J Harlen Bretz, whose full name was really Harley Bretz. The J (no period!) was added, and the spelling of Harley changed, for the sake of dignity—and that from the least dignified person I had met until that time!
2. Norman L. Bowen, my mentor and role model. His humor seemed not apparent to those who didn't know him well. His nickname, "Ham," did not originate as a play on words (ham bone), but rather was the result of neighborhood kids hearing and misinterpreting his Welsh father pronounce, with amusement, the shortened form of "Harmon," as "Hahm," after NLB's Sunday school report card came back as Harmon Bowen.
3. Tom F. W. Barth, who got me concerned with order-disorder phenomena and other crystal-structure matters. His work, with E. Posnjak, on "variate atom equipoints" helped open my eyes. Barth really didn't like V. M. Goldschmidt, perhaps out of jealousy, and once told me that Goldschmidt had never really done anything important. For a time, to avoid invidious comparison, he would introduce me and spell g-o-l-d-S-M-I-T-H.
4. Frans-Erik Wickman, who in one short year inspired me with his clear thinking in crystal chemistry and other structural matters.
5. Hans Ramberg, who arrived on the scene at the same time that Wickman and I did. Considered unconventional (to say the least!) by some, and certainly controversial, his originality and insight shone through,

and he was the first to show me the value, in mineralogy and petrology, of practical thermodynamics. In my opinion Ramberg stands as a major figure in the modernization of petrology, and hopefully his contributions will not be forgotten before being fully appreciated.
6. Fritz Henning Emil Paul Berndt Laves, a cultured, kind friend, who unselfishly shared a great deal of science with me and also showed me a view of life and history and its interaction with science in Europe through the eyes of one who had come from Nazi Germany.

The postwar developments directly influenced at least *two* other institutions over and above any induced by the normal dispersion of students and research associates. The first blooming of Caltech resulted when Robert A. Millikan was brought from Chicago in 1921 as president. Prior to his arrival, it was a small local institution, of little reputation. In his autobiography, Millikan says, "The institution was indeed a very weak institution, with practically no endowment, but with three buildings on campus. . . ." The Earth sciences at Caltech got another great shot in the arm after World War II when Harrison Brown, Heinz Lowenstam, Clair Patterson, Sam Epstein, Charles McKinney, and (shortly afterward) G. J. Wasserburg all went from Chicago to Pasadena. I was personally most strongly affected by Sam's departure, for we had planned a joint study of oxygen isotopic equilibrium at elevated temperatures and pressures, a field of research that thus lost the opportunity to get started ahead of its time. Thirty years elapsed before I took up isotopic work, and then not with Sam, but with his first graduate student, Bob Clayton, and with Tosh Mayeda, who *didn't* leave for Caltech and is still energetically here. We have, with colleagues, been working on oxygen isotopic fractionation equilibria at high pressures between a variety of phases, first with water as the exchange medium, and now with $CaCO_3$ and directly with CO_2. Reactions are made possible in the dry systems by a large rate enhancement at pressures greater than those used (1–2 kbars) in earlier experimental work. In addition to fractionation factors useful for geothermometry, we are looking at diffusion and reaction mechanisms.

The second California migration was to San Diego, when in 1955 Harmon Craig and Hans Suess joined the faculty there along with Walter Elsasser from Johns Hopkins to become the first three people brought to the University of California at San Diego by Roger Revelle to build a new way of life. Harold Urey joined them after retiring from Chicago in 1958, and afterward Harmon and company brought Stanley Miller, Jim Arnold, Joe and Maria Mayer, Walter Kohn, and others from Chicago.

The cultural difference that I sensed as a student (and later) between the different sciences was perhaps as real as C. P. Snow's "two cultural" distinction between scientists and humanists. Urey and his colleagues in several departments helped, in my opinion, in reducing the cultural differences not only between Earth scientists and those in the "harder" sciences, but also between the various types of Earth scientists. I hasten to add that the concepts of plate tectonics also played an important and even pivotal role in producing convergence of the subcultures (and I learned that although apples and the Earth are approximately spherical, they have little else in common, wrinkles notwithstanding). As a student and young faculty member, I was made aware of the fact that I was not a "field geologist," or that I did not do field work. A certain amount of disdain existed between the experimentalists and the khaki-pants-and-scuffed-boots group, although perhaps neither side was free of a concealed touch of envy and even respect. There are obviously things that cannot be resolved by field work alone, just as there are matters that would never be considered by an experimentalist or theoretician isolated from the outside world. Today, field work and theoretical/experimental work, both of which are essential, are not the exclusive domains of disparate groups, but a good geologist may be competent in or at least conversant with a variety of fields; not only is there a greater understanding between disciplines, but there is also increased mutual respect. Pioneering work on the "geochemistry" of the solar system and beyond by Robert Clayton, Edward Anders, Jerry Wasserburg, and Lawrence Grossman—work that bears on the origin of meteorites and planetary bodies—has had an impact far beyond the Earth science community. New frontiers have continued to develop. The intrusion of fluid dynamics into geology was first applied to postglacial rebound, and then to mantle convection and convection in magma chambers. One might say it has helped stir up geology.

Allow me to recount a history of internal unification that, although local, I feel to be of some significance to the Earth science community. Meteorology was a latecomer to Chicago, beginning in 1940 with the arrival of Horace Byers, who was instrumental in setting up an institute within the Department of Physics. In 1941 Carl Gustaf Rossby came to Chicago as its first director; he had started a Department of Meteorology at MIT in 1928 (the first in the US) and in 1939 went to Washington as assistant chief of the US Weather Bureau. During World War II meteorological cadets were trained for the Air Force, and in 1944 the institute became the Department of Meteorology. In 1960–61 I was the associate dean of the Division of the Physical Sciences, and Willie Zachariasen was the dean. At that time the Department of Geology remained relatively isolated on a campus containing a variety of people in other departments also concerned with some aspect of the Earth and planets. This seemed more and more to be an artificial fragmentation of talent. At this time, fluid dynamics, applicable to the atmosphere, hydrosphere, and the solid Earth, was traditionally treated independently in separate departments or even institutions, with little or no interchange of ideas. Attempts to shape cooperative science usually fail, but inducing the intermingling of independent scientists with the attendant interchange of thoughts and inspirations can be very fruitful. Willie Zach wholeheartedly agreed with me that a condensation of sorts would be worthwhile, perhaps in part because of a dean's desire to reduce the number of areas of responsibility under his wing and thus simplify the administration of the division. He appointed me chairman of a committee to investigate the matter, and our 1960 recommendation, with the unanimous consent of the faculties of the Departments of Geology and of Meteorology, resulted in a merger of the two in 1961; at the same time joint appointees from the Department of Chemistry and the Research Institutes (Clayton, Anders) and the Department of Astronomy (Joe Chamberlain) joined the new department. Bill Reid became a joint appointee with the Department of Mathematics shortly thereafter. Why shouldn't fluid dynamicists talk to paleontologists? William McNeil, historian, expressed amazement, saying that it was probably the first time in recorded history that two departments had, by choice, given up their individual autonomy. The institute concept, so successful during and after the war in bringing and keeping people together, was extended to a unified department, an educational as well as research unit.

The formalization of the new department took much less effort than the choice of a name! Name selection became tangled with distinction. The term Earth Sciences, which embraced more of the faculty than most other designations (exclusive of meteoritic and planetary types), was looked down upon by several, perhaps self-consciously for reasons already mentioned (Earth is a dirty word). The least troublesome name turned out to be "The Department of the Geophysical Sciences," soon abbreviated to DoGS. This name was even approved of by paleontologists and geochemists, perhaps seasoned by the name of the Geophysical Laboratory, long respected, even if it might have been more aptly named "The Geochemical Laboratory." The "new" department, now 30 years old, has prospered and grown. Times have indeed changed, for the (unwarranted) complaints we received 30 years ago from former students (for the most part geologists) that we had deserted field studies and the traditional subjects are no longer heard. It seems to us that the study of the Earth and its environment is best handled by all concerned with it, and that rigid, old-fashioned subdivisions served no purpose other than to stifle knowledge and obstruct free interchange of ideas and information. A larger view has evolved during my lifetime, one in which I feel my university has played an important role. Not only is there a kinship between various types who deal in the workings and history of the Earth, but with those who look to the planets and to the stars beyond. A more recent interest in Earth science by a host of physicists, chemists, and computer modelers appears to have been promoted by fear—a belated concern with the health of the planet. Many outside of the traditional Earth science community are now involved in the physics and chemistry of environmental changes. As one who sees little cause for optimism in the behavior of people in the world at large, including their diminishing interest in science at a time when its importance to the world is growing, it is heartening to note that great progress has been made in the extension of interests and the broadening of vistas of geology into areas formerly inhabited for the most part by scientists in other disciplines, as well as by an increasing interest of physicists, chemists, and applied mathematicians in the Earth sciences.

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