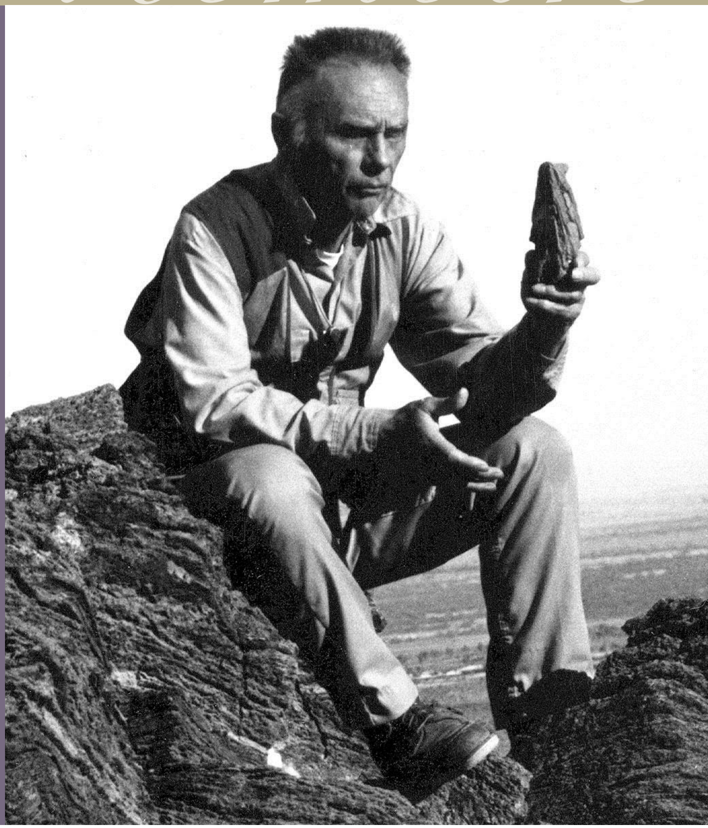
Albert E. J. Engel

1916–1995

BIOGRAPHICAL *Memoirs*



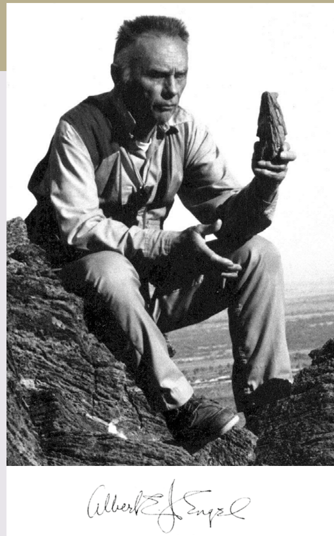
A Biographical Memoir by
James H. Natland

NATIONAL ACADEMY OF SCIENCES

ALBERT E. J. **ENGEL**

June 17, 1916–March 31, 1995

Elected to the NAS, 1970



Photography by R. J. Stern. 1975

By James H. Natland

Albert Edward John Engel, hereafter referred to simply as Al, was born on June 17, 1916 in St. Louis, Missouri. He died on March 31, 1995, at his home in Victor, Montana. Al attended the University of Missouri, graduating with an A.B. in 1938 and an M.A. in 1939; his master's thesis was titled The Geology of the House Springs Area, Missouri (Engel, 1939). Al continued as a part-time instructor at Missouri until 1940. In that year, he entered a doctoral program at Princeton University, where he took courses in chemical geology under A. F. Buddington, igneous and metamorphic petrography and sedimentary petrography under H. H. Hess, regional physiography under Patrick MacClintock, paleobotany under Erling Dorf, and structural geology under W. Taylor Thom. In Al's file at Princeton, there is a notation by Hess about Al and three of his classmates: "These four men unquestionably represent the best class I have had at Princeton."

Because of World War II, Princeton adopted an accelerated study program, allowing its students to complete their coursework within three years or fewer. Al withdrew from the university, signed on as a "Junior Geologist" for the U.S. Geological Survey (USGS), and went to Arkansas in search of quartz crystals for use in radios needed for the war effort. This work led to his Ph.D. dissertation, *The Quartz Crystal Deposits of Central Arkansas*, which he defended at Princeton in 1945. His thesis supervisor was Buddington and his examining committee consisted of Hess, Dorf, MacClintock, and Thom, as well as Richard M. Field. Field and Thom were founders of a celebrated summer field-training program in Montana that was in its heyday during Al's years at Princeton. After the examination, Buddington commented on Al's excellent preparation and field skills.

In 1944, Al married the woman who became, and who would remain, his closest personal colleague throughout his career. He and Celeste had two children: Bob, who became a biology professor; and Tom, an attorney. In 1945, Al and Celeste began

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an investigation of metamorphic and associated igneous rocks in upstate New York near the Adirondack Mountains in the Grenville Subprovince, which was to carry them through more than 20 years of significant publications. These papers were marked by rigorous scholarship, assiduous attention to field relations, a felicitous prose style, and impeccable chemical analyses of rocks and minerals, the latter obtained using wet-chemical techniques by Celeste. It is fair to say that in terms of both accuracy and precision, the analyses reached the epitome of the classical geological art, which now is virtually lost. They also took weeks apiece to accomplish, at least the way Celeste did them. Al was always extremely proud of Celeste's work. In later years, during a dissertation qualifying examination, he was shocked to hear that the student expected to obtain some 150 analyses of Samoan lavas by X-ray fluorescence procedures. After disparaging XRF as a technique, Al said, "That's more than my wife ever managed to obtain in all our years of work together!"

Al stayed with the USGS until 1948 and then assumed an associate professorship at CalTech, where he was promoted to full professor in 1953. In 1959, Al and Celeste both moved to the Scripps Institution of Oceanography—she with the USGS office there—where the Engels obtained and carried out seminal studies on basalts of the ocean floor, which they termed "oceanic tholeiites." Some aspects of the chemistry of these rocks, notably their low concentrations of K_2O, Rb, U, and Th, provided an unusual analytical challenge, and Celeste Engel was the first person to obtain reliable analyses. While at Scripps, Al and Celeste also investigated lunar samples, among which the ones with extraordinarily high TiO_2 were the analytical challenge.

During these years, Al carried out field studies in South Africa, the Anza Borrego desert of southern California, and the Rocky Mountains. He was a classically trained field geologist and petrologist, and was passionately interested in Earth's crust, whether oceanic or continental, or whether it formed in the Archaean Eon, the Proterozoic Eon, or yesterday at a spreading ridge. Al supervised or co-supervised some fine students in his years at Scripps, including in sequence Allan Divis, Rodey Batiza, Tim Dixon, Bob Stern, and Patti Schultijean. His last papers while at Scripps dealt with extraordinarily broad problems in crustal evolution and continental dynamics.

Al retired to the Bitterroot Valley of Montana in 1986, where he continued to work on problems in hydrology for the USGS and write in local newspapers about the impact of development on aquifers. He said to me that hydrology is an area in which a well-trained geologist can make a significant impression even if he doesn't begin to work on it until he is in his sixties.

Al was a member of numerous scientific societies, a fellow of the Geological and Mineralogical Societies of America, and a fellow of the American Association for the Advancement of Science. He was a distinguished lecturer at several universities in the United States, as well as in Canada and South Africa. He was awarded an honorary D.Sc. degree by his alma mater, the University of Missouri, in 1978, and was elected to the National Academy of Sciences in 1970. Al's Academy citation notes his work in the Grenville and on oceanic tholeiites, but then goes on to say: "Engel's advice on problems of science is valuable because of his wide and fundamental interests, his interaction with foreign scientists, and his forthrightness of expression." In much of my personal reminiscence, which now follows, the tenor of this forthrightness should be apparent.

In 1968 I was about to leave MIT, where I had majored in earth and planetary science, for a graduate education at Scripps. I was summoned, as individually were all other seniors, to the office of department chairman Frank Press, for (as it turned out) his one-on-one comments on my prospects. Frank was sitting at his desk in rolled-up shirtsleeves and with loosened tie, examining my file. He pointed to a chair. I sat. After a time, he said, "Al Engel is at Scripps. For what you want to do, that's a good place to go. Looking at your grades here, you'll do fine." I wasn't quite sure what to make of the last remark, but that was it, and he waved me out; and odd as it seems now, at that point I knew nothing about Al Engel. Because my petrologic education at MIT covered granites and experimental petrology, oceanic tholeiites were not quite yet in the syllabi of my residually oriented instructors.

The first order of business at Scripps was to become an oceanographer. I didn't see a rock. We did, however, have a wild Institution-wide required seminar titled "On the Origin and Evolution of Practically Everything." Al was a regular and somewhat boisterous attendee who sat in the middle of the auditorium—about six rows back, that is—well positioned to fire potshots. Among others, we had Harold Urey speaking on the origin of the Moon, Geoffrey Burbidge on the origin of the elements, Stanley Miller on the origin of life, Jim Arnold on the origin of the Solar System, Bill Menard on the origin of the Pacific Basin from the perspective of plate tectonics, and Al himself on the origin of continental crust. At one point, when one of the speakers decried our ignorance of the poorly known Archaean, Al—who had mapped ultramafic pillow lavas, later called komatiites, with Morris and Richard Viljoen—countered that some of the best known geology on the face of the Earth is Archaean in age. That really meant that he and his South African colleagues had mapped it. That was also the first time I ever heard anyone describe plate tectonics as "yipes, stripes, and bullshit!" (One reviewer of this manuscript insisted that I not leave out Al's favorite word.)

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At this time, Al had kind of a notorious reputation among students. He sometimes was rough on them, as most of them were in love with plate tectonics and he was not. "Yipes, stripes, and bullshit!" cut deep. So warning fingers about Al were raised at me from the moment I arrived at Scripps.

In due course, oceanic tholeiites became part of my stock in trade. I wound up gravitating to Jim Hawkins for thesis supervision, but I wanted Al on my committee. To know him better, I decided to take his field course of mapping in the high-grade metamorphic rocks on the fringes of the Peninsular Ranges batholith at Anza Borrego. We had to show up at 5 a.m. on Saturday for the two-hour ride, and so once a week we mapped all day throughout the term. While someone else drove, Al entertained. The stories were sometimes hard to believe. He once said that, when he arrived as a graduate student at Princeton, he was assigned some office space. One evening, he went looking for cleaning gear down in the basement, where he found a man whom he took for the janitor. He asked for a mop. The man turned out to be Harry Hess.

At Princeton, Al had gotten to see Norman Levi Bowen reprise some of the lectures used for *Evolution of the Igneous Rocks*. He also found a mentor in Buddington, who got him into Adirondack studies. And Al would lament during our rides, would say, "Nobody does that kind of work any more." He kept comparing the metamorphic rocks there with the ones we were mapping, then spin off a yarn, like the time he recalled the largesse of his landlady during his and Celeste's summer field sessions in upstate New York. Al and

Celeste were prone to weighting their upstairs rooms with tons of rocks, which this lady allowed, for a time. The accumulation of rocks eventually reached the point of damaging the supporting rafters and cracking the plaster on the ceiling underneath. The woman finally insisted, as Al put it, that the rocks, if not the owners, had to move to other quarters.

The wrap-up for the field course was a three-day excursion all over the batholith, camping as we went. The first night out, Al turned in early. At one point, though, nature called. The night was clear, with a fair light from the moon, dazzling stars, and a gusting wind. At one point, probably around 2 a.m., several of us were awakened by something thrashing around in the sagebrush. Probably a cow, we thought. It stopped, so we went back to sleep. With the cold dawn, we stiffly stumbled out of our bags to get the day going. But then we found Al, still in his bag but sitting up and shivering violently, holding a bloody sock to a wound on his head.

"What happened, Al?"

"Well, I took some new medicine for my bursitis last night, and it must have made me dizzy when I had to get up. I hit a rock going down."

Judging from all the noise, he didn't go down without a fight. We got some coffee and breakfast into him and took him to a doctor, even though he insisted on stopping to explain some outcrops along the way. We in turn insisted that we could complete the trip the next weekend.

As it turned out, Al had a concussion. He didn't appear at Scripps the following Monday. On Tuesday, he showed up with his head bandaged, and evidently still looking like a wraith. When Jim Hawkins saw him in the corridor, he reacted as we had.

"Good God, Al! What happened?"

"Well, I fell and split my head open on a piece of granite."

Then, Jim told me, Al corrected himself. "Actually," he specified, "it was quartz diorite."

The time came for my qualifying exam. I went to the various members of my committee to outline what I intended to say about a research project. All were encouraging except Al. I had proposed a topic involving Hawaiian alkalic lavas and mantle xenoliths. Al's eyes could turn kind of steely when he was disapproving, and his voice got very measured. He first of all confessed that he did not have a very good attitude about this particular subject. Nevertheless, none of the experimental work I intended to summarize meant anything. And the field studies on xenoliths that I mentioned were simply wrong. This was not to say that Al had examined the xenoliths himself. He hadn't. He just didn't believe the conclusions of the study.

So I went away and changed my topic—to island-arc calc-alkalic lavas, which I had begun to sample on Catalina Island. My qualifying exam, however, was ferocious. It was a dialog between Al and me, with peripheral questions from the rest of the committee, which included two other members of the National Academy. Al actually stood up to ask me questions, and he repeatedly interrupted me, no matter who asked the questions. If someone else asked a question about thermodynamics, for example, Al would ask me to say what I thought it had to do with geological processes. The occasion really was a matter of who was going to talk down whom. Naturally, an hour and a half later, Al won. Someone taking pity on me finally said, "His brain is gone, let's let him go." But because Al had left virtually no time for me to get in trouble with anyone else, I wound up passing, with some provisos. With Al, there were always provisos.

After my exam, I took Al's course in crustal evolution. We all had to pick a topic to talk about. I chose the origin of atmospheres and oceans, keying on the work of Rubey (1952) and Holland (1962). Al appeared to become more and more exercised as I proceeded. After about five minutes, Al asked me why I was wasting everyone's time? I protested. "But, Al, the paper I'm talking about was in a volume you edited!" (This was the Buddington volume of the Geological Society of America.) However, what he really wanted me to do all along was talk about oceanic tholeiites and alkali basalts—from his point of view, of course. So, off the top of my head, I did a chalk talk on the subject that Al had rejected for my qualifying exam, and among other things I deliberately noted what was wrong with the experimental work. After the class was dismissed, Al was actually somewhat apologetic, because a senior geologist, John Hunt, who was sitting in on the class, told him that he had been kind of hard on me. But Al said that he wanted the other students to hear about this. I just wish that he had given me some advance notice.

A year or so later, when Rodey Batiza was taking the same course, he said that Al showed up for one class with the latest issue of *Earth and Planetary Sciences* in his hand, and he was laughing so hard that he was nearly in tears. He told the class, "Some damn fool claims he has found a piece of the core in a stream bed in Oregon!" This was, of course, the Josephinite episode, in which some chunks of iron-nickel ore indeed were found in a stream bed. Henry Dick, who mapped the ophiolite, already had determined that these chunks formed under extremely reducing conditions during serpentinization in the ultramafic portion of the Josephine ophiolite. Al didn't yet know of that work, but he knew enough to know that those rocks could not fantastically have come from the core.

Al also led a spring field trip, which he partially subsidized from his own pocket, to the Grand Canyon. He got us down to the bottom, where we spent the night among the Vishnu schists, with visions of John Wesley Powell and company shooting the rapids. He asked me to talk about differences between katazonal and metazonal processes. At dusk, following our meal, he shared a bottle of Scotch with us. Next day we toiled out. Al was amazingly fit for someone with his medical history. During our drives over the Southern California batholith, he had alleged that he was infected with what must have been undulant fever while looking for quartz crystals in Arkansas during World War II (he still found some beauties), and I know that he was seriously ill at one point with bilharzia, the African fluke disease, following a field season in South Africa. A year or two before our Grand Canyon venture, returning from a medical leave Al told me, "They tried to cut the meanness out!" and was obviously pleased they hadn't. In any event, climbing out of the Grand Canyon didn't faze him at all.

That afternoon, as we were barreling down the road toward California in university vans, we all got pulled over by the Arizona Highway Patrol, one by one. Al was in the lead van and emerged with a look of serious determination on his face while stuffing the tail of his shirt into his pants and pulling up on his belt. I knew the look. That patrolman didn't have a chance.

I only saw one person—a student—ever get the best of Al, and that was Sharon Stonecipher, who did a thesis on zeolites in marine volcanogenic sediments, which included some experimental work. Years before, Al had worried about zeolites in the protolith of the Grenville paragneiss. It was really hard to find something about rocks that he knew nothing about. At Sharon's thesis defense, he asked one of his typical Engelian questions. He used to say, "Nature is a triumph of kinetics over equilibrium." In this vein, he was concerned that Sharon's experimental studies did not duplicate the extremely low rates of reaction during diagenesis in the real world. So he asked, "If you had all the money in the world, how would you get around this problem of reaction rates?" Sharon looked upward momentarily, as if in thought, then she leaned forward on the dais and, smiling conspiratorially, replied, "Well, with all the money in the world, I think I'd try to get

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someone going on solving the problem of immortality." Al knew he'd been had, as the audience laughed around him.

Al loved to do field work, and in his last years at Scripps, he did extensive mapping in the metamorphic rocks east of the batholith. A similar mapping project was under way as well by a group at San Diego State. Al gave a talk at a Geological Society of America Cordilleran Section meeting in Anaheim in which he repeatedly jabbed at his competition. During his sequence of slides, one appeared of a skillet on a stove, with a wrapped chocolate bar in the skillet. He said, "That's a mistake. Go on to the next one." The next slide of course was of the melted candy bar spread out over the bottom of the skillet. Al said, lest anyone forget, "This is just to show that deformation is triaxial!" Another of his slides was of a ridge covered with fairly tall Ocotillo cacti. Al said, "All these are students chasing me with their rock picks!" At one point of refutation of the competition's mapping, he said, "They can take this conclusion and stick it where the sun never shines!" This got such good audience response that he used it again a couple more times. The poor session chair tried to pull Al off the stage when his time was up, but Al refused to budge. He said, "I'm older than you! Leave me alone!" Some audience members thought Al was not being even-handed, and I overheard some of their irritated comments as the session broke. However, I think they were most upset at the fact that Al was enjoying himself so thoroughly.

Al's curmudgeonly style was what might be called "equal opportunity," as his skepticism and irreverence were not aimed only at colleagues in his field. When he was elected to the National Academy, I bumped into him in the third-floor corridor of Ritter Hall and offered my congratulations. Al said, "Well, I don't know about that, considering who else is in it!" But he was laughing and obviously pleased.

One time Al contributed a thoughtful article, "Time and the Earth," to a Princeton University symposium volume in which he gave as close to a full statement of his philosophy of life as I think he ever wrote down (Engel 1967). The serious appeal he made was for any well-educated person to understand how long it has taken geologically to produce the environment in which we live, and how quickly we are messing it up.

Here he produced an aphorism that I later heard him use many times: "An optimist is someone who believes that this is the best of all possible worlds. I'm a pessimist. I believe the optimists are right." Being a uniformitarian, he foresaw a series of small disasters rather than one big one, sort of like the revelation of most of the geological record. In the face of this, he believed that the human race is adaptable. Thus we would survive as a species because we would overcome, one by one, the various catastrophes we might produce. And however traumatic they might seem at the time, they still probably would not noticeably dent the geological record. Of course, there is always the possibility that we will leave behind Jerry Winterer's "Post-Industrial Stratigraphy"—a widespread plastic layer followed by a universal radioactive layer, rather like the widespread shocked quartz and universal iridium at the K-T boundary.

When Al got the draft of my thesis for review, which ironically wound up being about alkalic lavas of Samoa, he was actually fairly gentle—disbelieving, but gentle. He did have a very pointed comment, however, about one sentence—in fact the first line of R. A. Daly's *Igneous Rocks and the Depths of the Earth* (1933), which I guilelessly chose to use as an epigram. It stated, "A final theory of Earth history must be largely founded upon the unshakable facts known about igneous rocks." Al's comment was that there could be no such final theory. He meant never, not under any circumstances, not in his generation nor mine. To his classes he would say, "We stand in the foothills of the mountains of ignorance!" He felt we should be humbled by the Earth, and then perhaps we might learn something from it. I wonder if he ever said anything to Daly.

Anecdotes aside, I cannot describe Al without saying something about his science. Al reached scientific maturity at a time when the grand petrological debate was about granites. His hero Buddington wrote a review regarding granites (Buddington 1960) that used field evidence to neatly balance the claims of pontiffs (magmatists, who argue that granite is igneous in origin) and soaks (metasomatists, who argue that granite is produced by reaction with alkalai-rich hydrous fluids) (Bowen, 1947). Decades earlier, Bud had written his memoir on the Adirondacks (Buddington 1939), describing the rocks surrounding the massif as evidence for granitic/syenitic intrusion and associated migmatization concordant with original bedding. In his later review, these were the hallmarks of deep crustal, or "katazona," plutons. Now Al, with Celeste, and mainly in the 1950s and '60s, proceeded to document the effects of very high-grade amphibolite-facies metamorphism adjacent to the Adirondacks massif; they did so by carefully field mapping and geochemically assessing the transformations of the surrounding rocks.

As a result, they wrote a remarkable series of papers (Engel 1949, 1955, 1956; Engel and Engel 1953a, 1953b, 1958b, 1960a, 1960b, 1962a, 1962b, 1963a; Engel et al. 1961, 1964). The laboratory work for these papers, done by wet chemistry for major oxides and spectrographic methods for trace elements, must have taken Celeste forever. In the end, however, the Engels were able to show just how much K, Rb, Ba, and related elements in two of the original facies, the major paragneiss and the amphibolite, had fled from the immediate vicinity of the intrusion across a metamorphic isograd into more distant rocks. This was documented not just rock by rock, but also mineral by mineral. The vehicle of the escape, of course, was water, in the form of some high-temperature metamorphic fluid. Ever after, Al called K, Rb, Ba, U, and Th the "chemical congenors" of water, and when he came to Scripps and started comparing oceanic tholeiites with alkalic basalts from seamounts, this clearly was what he had in mind.

Al did an unusual and difficult thing rather late in his career. He repotted himself. He found that he needed to know more about the constitution of the modern ocean crust, and at least partly for that reason he left CalTech. He came to Scripps and began to dredge. Neither he nor Celeste expected quite what they found. Whatever one might say about oceanic tholeiites, now awkwardly termed MORB, the surprise was that they were so "dead," as he termed it, in the heat-producing radioactive elements, and this in rocks from the most dynamic portion of the Earth's surface. Celeste's account, in her autobiographical *Rocks in My Head*, is that no one at first believed her analyses. Skeptical colleagues asked for samples to replicate her results, which they did, but Celeste says that none of them ever apologized.

The discovery of oceanic tholeiite meant that previous estimates of the bulk composition of the Earth's crust, which were based solely on continental facies, had to be wrong. Models of crustal evolution derived from geochemical mass balances of only continental rocks were thrown into a cocked hat. Dynamic models relating radioactive sources of heat to convection also had a problem.

The short summary paper by Engel, Engel, and Havens (1965) proposed an average chemical composition for oceanic tholeiites based on 17 of Celeste's chemical analyses from the Atlantic, Indian, and Pacific Basins, and this has barely been budged by acquisition of thousands of new analyses in the more than 40 years since. The paper pointed to the Archaean as a period of time when similar rocks (but also including the ultramafic eruptives later termed komatiites) erupted on the sea floor, to become incorporated in greenstone belts. It posed a geochemical relationship, between oceanic tholeiites and seamount-capping alkalic basalts, that has been controversial ever since. And it provided a graphical estimate of the abundance of alkalic basalts through the ages—more of them have erupted recently, and there are virtually none in the Archaean. Not bad for a short paper.

Nowadays, the impact of these findings may be difficult to understand. Before Al began to dredge, no one had any serious idea about the composition of the ocean crust, or of the thousands of seamounts that dot the sea floor. Gordon Macdonald didn't actually prove that alkalic olivine basalts overlie tholeiites on Hawaiian volcanoes until about the time when Al began to examine the sea floor (Macdonald and Katsura 1962, 1964). Tilley had only come up with that idea slightly more than a decade earlier (Tilley 1950). The ocean floor was a petrological terra incognita, and Al and Celeste were pioneering explorers. They, more than anyone else, opened the ocean floor up for petrologic study.

Still, Al never held with the various high-pressure hypotheses on the origin of alkalic basalts. These theories, and Yoder and Tilley's (1962) argument for low-pressure "thermal barriers" (which was published when Al and Celeste's own results were first reaching print), were no barriers at all to a geologist. A thermal hump found in a three-component closed system during an experiment need not be a barrier to multicomponent magmas operating in nature. To Al, the elements K, Rb, Ba, U, and Th were still the "chemical congenors" of water, and the path from the mantle to the top of a seamount was long and complicated. Take the Adirondacks, eliminate quartzose continental crust and its castoff sediments as potential contaminants, put the differentiating column on its end, and the same geochemical processes will occur. Alkalis and related elements will stream into the late basalts capping a seamount. Today, we call it metasomatism.

Despite all the isotopic work and the various forms of geochemical modeling that Al came to know about before his retirement, he never really admitted that it cannot be quite this way. I think that, from Al's perspective, his posture was largely genuine, though also partly calculated. He claimed no priority for his hypothesis other than its basis sound field relations and good geochemical insight. By contrast, he viewed proponents of other hypotheses as being far more doctrinaire than he, as they held to geochemical models that were tied to a number of questionable assumptions, such as extrapolation from experiments in tiny iron crucibles that reacted with their silicate charges; and these proponents did not really care to scale up the experiments to tests in the field. In his opinion, most of these other scientists were not competent to do this anyway, and they certainly were not as competent as he in judging field relations.

In "Time and the Earth," Al described a geologist as someone who, with a good pair of feet and some common sense, could reach sound conclusions about the Earth largely by examining the rocks in their settings. Most geochemists, he argued, were proprietors of machines who simply waited for samples. Similarly, geophysicists merely designed simplified problems to stick into computers and wait for results. Geology, to Al Engel, was not a humble task. It was both ennobling and essential, the proper way to the truth about the Earth.

Now after long consideration, I believe that some of the evidence against Al's idea about alkali basalts is pretty strong, but his field relations are impeccable, as work at Scripps on Jasper Seamount (Gee et al. 1992) demonstrated. The problem actually has not been properly reviewed since the Engel and Engel papers were published, either by critique of the data or reexamination of the seamount provinces that Al dredged. I sometimes give thought to doing it myself, but know that if I got it wrong, Al would kill me.

Al looked to the sea floor, in which pillowed terrains in greenstone belts are common, in part to understand the Archaean. He viewed the Earth as a long-term engine of differentiation. The continental crust is "penultimate froth," churned out by this engine; the hydrosphere, atmosphere, and biosphere are "ultimate." In Engel et al. (1974), he and colleagues finally traced the gross geochemical progress of formation of the continental crust, tying it to what he believed to be a simple geodynamic regime that acted from the Archaean into the Paleozoic (Engel and Engel 1973; Engel and Kelm 1972), through the bulk of the history of the Earth and of the accretion of the continents. Then it changed. The breakup of Pangaea in some measure restored the earliest geochemical patterns of crustal differentiation, so that we find the greatest similarities between arc lavas and batholiths of the last 100 million years and some of the oldest igneous rocks on this planet. The rocks in between are ...well ... more potassic (of course).

Al did two other things with his dredging program. He went to a deep chasm in the Pacific sea floor called the Nova Trough, discovered during the Nova Expedition. (Scientists at the University of Hawaii also claimed discovery, and they named it after the nearest island, thus the place now combines the two names and is called the Nova-Canton Trough.) Al went there to do serial dredging from its base to the crest of a seamount on its lip. He found a substantial exposure through most of the ocean crust, with gabbros at the bottom. Second, he found alkalic basalts on the summit of the seamount. I believe that most of this got published in Russian (Engel and Engel 1966), although I have seen an English version in manuscript from.

In any case, based on this work, when the Mohole project foundered and was succeeded by the Deep Sea Drilling Project, Al served on the original Panel for Igneous and Metamorphic Petrology. There he proposed what he called the "poor man's Mohole," an attempt to understand the ocean crust by drilling a series of holes down the wall he had dredged at the Nova-Canton Trough, each hole sampling successively deeper layers of the ocean crust. A hole at the bottom (in some 8 km of water!) would not have far to go to reach the mantle.

The Ocean Drilling Program (ODP) in the 1980s and '90s endorsed a crustal drilling program called "Offset-Section Ocean Crust Drilling" that actually used Al's "poor-man" principle. Exposures of gabbro and peridotite are now being targeted to understand the lower ocean crust and upper mantle. Several legs of drilling have been carried out under the banner of Offset-Section Ocean Crust Drilling, and serious consideration is once again being given to drilling a Mohole. Al should be named the patron saint of this program.

After I participated in ODP Leg 118 in 1987, when the first serious drilling in and recovery of abyssal gabbros took place—in the Indian Ocean—I went to see Al, who was at that time beginning to clear out his office and labs following his retirement. He was very interested in my descriptions of the highly deformed and strongly differentiated gabbros we had recovered, some of them extremely rich in the oxide minerals ilmenite and magnetite. He noted that rocks like this are very uncommon on land but did mention some occurrences among intrusions in the deep high-grade metamorphic belts on which he had built his career. I think he understood the implications of finding such complicated rocks in the ocean crust.

He did mention his poor-man's Mohole, and I described our efforts to get JOIDES Resolution to stop trying to hammer through basalts all the time. We talked about the technology of offset-section crustal drilling. He was pleased that we were finally getting off the dime, and understanding of the difficulty of starting a hole on bare rock and on a steep slope. The deep waters at Nova-Canton are an additional difficulty there. As usual with such problems, Al agreed, all it takes is money.

Of all the people I ever met at Scripps, Al Engel was the one whose critical standard made the deepest impression on me. Some of it, no doubt, was what he called "authoritarian bluff," and was put on for students and colleagues. But the game was for you to figure out when he was doing it, so that you would recognize it when someone else was doing it. So Al the contrarian sticks in my mind, and sometimes weighs on my conscience. The scientific questions are difficult. Look at what Al Engel did in his time with some of the most difficult ones in his profession, and try to measure up.

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REFERENCES

Bowen, N. L. 1947. Magmas. *Bull. Geol. Soc. Amer.* 58:263-280

Buddington, Arthur. 1939. Adirondack igneous rocks and their metamorphism. *Geol. Soc. Am. Mem. no. 7*.

Buddington, Arthur and J. R. Balsley. 1960. Magnetic susceptibility, anisotropy, and fabric of some Adirondack granites and orthogneisses. *Am. J. Sri*. 258-A (Bradley Volume):6-20.

Daly, R. A. 1933. *Igneous rocks and the depths of the Earth*. New York: McGraw Hill

Engel, A. J. 1939. Geology of the House Springs area. Unpublished M.S. thesis, University of Missouri.

Engel, A. J. 1949. Studies of cleavage in the metasedimentary rocks of the Northwest Adirondack Mountains, New York. *Am. Geophys. Union Trans.* 30:757-764.

Engel, A. J. 1955. The Greenville Problem, in Symposium on the Precambrian: Geological Society of Canada Proceedings. In *The Greenville Problem*, 54-68. Royal Society of Canada Special Publication #1.

Engel, A. J., C. G. Engel, and R. G Havens. 1961.Variations in properties of hornblendes formed during progressive metamorphism of amphibolites. USGS Prof. Paper 424-C. Washington, DC: US Geological Survey

Engel, A. J. and C. G. Engel. 1966. The rocks of the ocean floor. In *Proceedings of the plenary session, Second International Oceanographic Congress,* 53-86. Moscow: Navka.

Engel, A. J. 1967. Time and the Earth. In *Vanuxem Symposium on Time.* 121-156. Princeton, NJ: Princeton University Press.

Engel, A. J. and D. L. Kelm. 1972. Pre-Permian global tectonics: A tectonic test. *Geol. Soc. America Bull.* 83:2325-2340.

Engel, A. J. and C. G. Engel. 1973. *Pre- and post-Permian global tectonics and crustal evolution*. Milano, Italy: Mondadari.

Gee, J., H. Staudigel, and J. H. Natland. 1991. Geology and petrology of Jasper Seamount. *Journal of Geophysical Research-Solid Earth and Planets*. 96:4083-4105.

Holland, H. D. 1962. Model for the evolution of the Earth's atmosphere. In *Petrologic Studies: A volume to honor A. F. Buddington*, 447-477 New York: Geological Society of America.

MacDonald, G. A. and T. Katsura. 1962. Relationship of petrographic suites in Hawaii, 187-195. American Geophysical Union Monograph 6.

MacDonald, G. A. and T. Katsura. 1964. Chemical composition of Hawaiian lavas. *J. Petrol* 5:82-133.

Rubey W. W. 1952. Geologic history of sea water: An attempt to state the problem. *Geol. Soc. Am. Bull.*, 62:1111-47. Reprinted in: *The Origin and Evolution of Atmosphere and Oceans*, pp. 1-63. Wiley, 1964.

Tilley, C. E. 1950. Some aspects of magmatic evolution. *Q. J. Geol. Soc. London*, 106:37-61.

Yoder, H. S. and C. E. Tilley. 1962. Origin of basalt magmas; an experimental study of natural and synthetic rock systems. *Journal of Petrology* 3(3):342-529.

SELECTED BIBLIOGRAPHY

1946 The quartz crystal deposits of western Arkansas. *Econ. Geol.* 41:598–618.

1953 a) With C. G. Engel. Grenville Series in the northwest Adirondack Mountains, New York, Part 1: General features of the Grenville Series. *Bull. Geol. Soc. Amer.* 64:1013–1047.

b) With C. G. Engel. Grenville Series in the northwest Adirondack Mountains, New York, Part 2: Origin and metamorphism of the major paragneiss. *Bull. Geol. Soc. Amer.* 64:1049–1097.

1956 a) With J. S. Brown. Revision of Grenville stratigraphy and structure in the Balmat-Edwards District, northwest Adirondacks, New York. *Bull. Geol. Soc. Amer.* 67:1599–1622.

b) Apropos the Grenville. *Roy. Soc. Canada Special Pub.* 1:74–96.

1958 a) With R. N. Clayton and S. Epstein. Variations in isotopic compositions of oxygen in Leadville Limestone (Mississippian, Colorado) and its hydrothermal and metamorphic phases. *J. Geol.* 66:374–393.

b) With C. G. Engel, A. A. Chodos, and E. Godijn. Progressive metamorphism and granitization of the major paragneiss, northwest Adirondack Mountains, New York, Part I: Total Rock. *Bull. Geol. Soc. Amer.* 69:1369–1414.

1960 a) With L. A. Wright. Talk and soapstone. In *Industrial minerals and rocks,* 3rd ed. pp. 835–850. Englewood, CO: American Institute of Mining, Metallurgical, and Petroleum Engineers.

b) With C. G. Engel. Migration of elements during metamorphism in the Northwest Adirondack Mountains, New York. In *U.S. Geological Survey Professional Paper 400-B, B465–B470.*

c) With C. G. Engel. Progressive metamorphism and granitization of the major paragneiss, northwest Adirondack Mountains, New York, Part II: Mineralogy. *Bull. Geol. Soc. Amer.* 71:1–58.

1962 a) With C. G. Engel. Hornblendes formed during progressive metamorphism of amphibolites, northwest Adirondack Mountains, New York. *Bull. Geol. Soc. Amer.* 73:1499–1514.

b) With C. G. Engel. Progressive metamorphism of amphibolite, northwest Adirondack Mountains, New York. In *Petrologic studies: A volume in honor of A. F. Buddington,* edited by A. E. J. Engel, H. L. James, and B. F. Leonard. Boulder, CO: Geological Society of America, 37–82.

1963 a) With C. G. Engel. Metasomatic origin of large parts of the Adirondack phacoliths. *Bull. Geol. Soc. Amer.* 74:349–352.

b) With C. G. Engel. Basalts dredged from the northeastern Pacific Ocean. *Science* 140:1321–1324.

1964 a) With C. G. Engel. Mineralogy of amphibolite interlayers in the gneiss complex, northwest Adirondack Mountains, *New York. J. Geol*. 72:131–156.

b) With C. G. Engel. Composition of basalts from the Mid-Atlantic Ridge. *Science* 144:1330–1333.

1965 With C. G. Engel and R. G. Havens. Chemical characteristics of oceanic basalts and the upper mantle. *Bull. Geol. Soc. Amer*. 76:719–734.

1968 With B. Nagy, L. A. Nagy, C. G. Engel, G. O. Kremp, and C. M. Drew. Alga-like forms in onverwacht series, South Africa: Oldest recognized lifelike forms on Earth. *Science* 161:1005–1008.

1969 Time and the Earth. *American Scientist* 57:458–483.

With C. G. Engel. The rocks of the ocean floor. *Proceedings of the 2nd International Oceanographic Congress, Morning Review Lectures*. New York: United Nations Educational Scientific and Cultural Organization, 161–187.

1970 With C. G. Engel. Lunar rock compositions and some interpretations. *Science* 167: 527–528.

With C. G. Engel. Continental accretion and the evolution of North America. In *Adventures in Earth history.* Edited by P. Cloud. pp. 293–312. San Francisco: W. H. Freeman and Co., ; reprinted from *Advancing frontiers in geology and geophysics: A volume in honour of M. S. Krishnan* (1964). pp. 17-3. Hyderabad: Indian Geophysical Union.

1971 With C. G. Engel. Mafic and ultramafic rocks. In *New concepts of sea floor evolution: General observations*. Edited by A. E. Maxwell, E. Bullard, and J. L. Worzel. pp. 465-519. New York: Wiley-Interscience.

1972 With D. L. Kelm. Pre-Permian global tectonics: A tectonic test. *Bull Geol. Soc. Amer.* 83:2325–2340.

1974 With S. P. Itson, C. G. Engel, D. M. Stickney, and E. M. Cray, Jr. Crustal evolution and global tectonics: A petrogenic view. *Bull. Geol. Soc. Amer.* 85:843–858.

1980 With T. H. Dixon and R. J. Stern. Late Precambrian evolution of Afro-Arabian crust from ocean arc to craton. *Bull. Geol. Soc. Amer.* 91:699–706.