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LESSONS OF A RENEGADE RESEARCHER ABOUT SUPPRESSION IN SCIENCE

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Orthodoxy or dogmatic belief systems can prevent new inspirations and can stifle the force of wonder in science which leads to the experience of new discoveries.

If success is measured by holding permanent positions in institutions, corporations, government services, or universities, then those like myself have been less than successful. Nevertheless, it is likely that many of my colleagues have found my scientific contributions to be quite useful. According to forensic anthropologist Clyde Snow, the most successful scientists are dependent on becoming experts, not advocates. When scientists become advocates for a particular theory or idea, they often become trapped by their own belief system, and their full scientific growth cannot occur. Science can be as dogmatic as religious orthodoxy, and the scientific community also can be overly protective of its own “holy relic”, the status quo. Ironically, science is supposed to dispel dogma, but examples of persecution by the scientific “Inquisition” abound. Although the threat of actually being burned at the state has passed, an iconoclastic scientist, like myself (or Galileo), now can suffer a kind of career "death". One would think that the scientific establishment, having been proven wrong so many times, would adopt a bit more humility: we keep on learning that we don't know as much as we think we do. The following recollections in my 50-plus year quest as a research scientist clearly demonstrate the widespread suppression by the scientific

community of ideas which might be a threat to its own entrenchment.

PLATE TECTONICS

In high school, I was impressed with the work of Alfred Wegener and his theory of continental drift (the precursor of modern plate tectonics). As early as 1620 Francis Bacon was stricken with the match between the opposing shores of the Atlantic. In 1858 Snider [1] illustrated how South America and Africa were united in late Carboniferous times, over 250 million years ago (see figure 1). By observing similarities of the Atlantic coast lines of Africa and South America in their geography textbooks, elementary school children could easily see how continents might have drifted apart. However, most geologists took a very dim view of continental drift until approximately 1960.

As a geology student at Texas Tech University (TTU) in 1957, I chose continental drift as the subject of my stratigraphy class term report. But my professor was displeased that I would be so enthusiastic about such a "questionable" theory, and my grade of C- was much lower than I probably deserved. The professor placed all the graded reports on a table outside his office, so that they could easily be retrieved by the students. By the time I arrived to get my report, it was gone, but it's fancy and expensive binder remained. In the 1950's, very few readily accessible copying machines existed, and theft, camera, or copying by hand often was used, if anyone wanted a copy of a document. Evidently, some fellow student did the most convenient thing and simply took my report. It was the only one known to be missing. Bizarre! Within ten years of my term report, striking new evidence for continental drift was presented in the modern concept of plate tectonics. This caused an abrupt paradigm shift in geology. By the 1960's, new evidence had convinced most of the original critics of continental drift that those few remaining geologists with skepticism about plate tectonics now were worthy of ridicule. By 1970 the geological establishment already was belittling the provincialism and dogmatism which had plagued the geological sciences in the nostalgic, quaint "pre-plate tectonic era".

ORIGIN OF OIL AND GAS

Although the idea was thoroughly ridiculed in the 1950's, a few scientists back then believed in the possibility of natural production of oil and gas from non-living sources. Such a subject appeared to be ideal for an assigned term report in my petroleum geology class at TTU. However, my class professor wouldn't permit a report on this controversial topic. He admonished that anyone who wished to be employed as a petroleum geologist must abandon ideas about inorganic oil and gas or be laughed out of the business.

Today the likelihood of naturally occurring "inorganic oil and gas" is considered doubtful by many. In spite of this, such noted scientists as Carl Sagan [2] indicated that some of the earth's commercially extracted natural gas may be primordial and not of biological origin. If some natural gas could be of primordial origin, couldn't this also be true of oil? Thomas Gold, a respected astronomer and professor emeritus at Cornell University, maintained for many years that oil was renewable, primordial, and continually being produced under tremendous pressure

in the depths of the Earth. As this oily primordial “syrup” migrated to the surface, it was attacked by bacteria, giving it the appearance of an organic origin dating back millions of years [3]. Gold [4] indicated that other planetary bodies (Jupiter, Uranus, Neptune, Saturn, and Titan), which were constructed of solids similar to those of Earth, also had petroleum on them. This peculiar statement is not so shocking in view of the hydrocarbon rain and atmospheres with hydrocarbon molecules associated with these bodies. Even more remarkable is Gold’s assertion that most of the chemists who have analyzed natural petroleum in detail considered a biological origin unlikely. Nobel Laureate Sir Robert Robinson [5] claimed that, "....all of the arguments from the constituents of ancient oils fit equally well or better with the conception of a primordial hydrocarbon mixture to which bioproducts have been added."

Doomsayers to the contrary, the world contains much more recoverable oil than was believed 20 years ago. Many petroleum engineers and geologists find it difficult to explain how the Middle East has more than doubled its oil reserves in the past 20 years, despite few new discoveries and a half century of intense pumping. Some geologists have suggested that the estimated 660 billion barrels of oil in the region could not be derived entirely from dead plants and other organismic sources. [6] Pennz-Energy Company’s Eugene Island 330 oil field, deep in the Gulf of Mexico, might be compatible with Gold’s ideas. Upon its discovery in 1973, Eugene Island behaved like a “normal” oil field. After production peaked at about 15,000 barrels per day (bpd), it slowed to about 4,000 bpd in 1989. Suddenly Eugene Island began to rapidly refill, perhaps from some continuous source miles below the surface. In 1990 the United States Department of Energy granted $10 million to investigate the Eugene Island phenomenon with its anomalous geological formation and production history. The grant funds have been exhausted and many questions are left unanswered, but good indirect evidence indicates a link to a very deep-seated system of migrating oil, possibly a primordial and non-biological source. In 1999 Eugene Island was producing about 13,000 bpd and reserves had increased from 60 million to 400 million barrels. Some would view Eugene Island as simply an anomaly of nature, but it is likely that this oil field has a deeper meaning in more ways than one.

WOES OF A WAYWARD WRITER

A few weeks after successfully completing work on my BS Degree in Geology at TTU in 1958, I took the university’s test for admission to graduate studies in geology. The results indicated that I had flunked the essay part of the test. Although admission had been granted to me on a provisional basis, it was judged that I would “not be able to do the research writing required for a thesis in science and should enroll in a remedial composition or technical writing class”. A strange situation, since I already had received fairly good grades in four courses in English composition at TTU. Moreover, my first scientific manuscript recently had been published in a respected journal, and several of my other manuscripts had been accepted for future publication. This was the first of well over 100 of my scientific articles and books, totaling over 7,000 pages in print, in my long career.

Upon leaving TTU without attending Graduate School, I already had more

scientific work in print than some of my former professors. What was my writing "problem" at TTU? Could it be that I received better than deserved grades from incompetent English teachers and possessed some strange gift for writing which was not recognized by the examiners? More likely, the essay for admission to Graduate School contained some of my iconoclastic ideas with which the examiners (like my geology professors) did not agree.

Ridicule about inorganic oil, continental drift, and other issues had been intense under some of my professors and classmates at TTU. I endured similar derision for over a year as a graduate student at the University of New Mexico (UNM) before leaving the study of geology forever in 1959.

QUASICRYSTALS AND THE PENTAGONAL SYSTEM

After only a few weeks of graduate study at UNM, it was clear that my unusual notions about some aspects of science were not going to be well received by the professors who mostly followed the strict lines of conventional geology. A publication [7] of mine describing a theoretical system of pentagonal symmetry had come to the attention of several geology graduate students and professors. They were shocked that I had made such ostensibly outrageous and untenable statements. I was openly mocked in a crystallography class by a UNM professor and was told in no uncertain terms that five-fold symmetry absolutely cannot be repeated, i.e., the rules of geometry and three dimensional space prohibited any crystalline substance from having five-fold symmetry. The lesson seemed to be obvious. For example, a wall could be completely tiled with squares or it could be completely covered with hexagons, but gaps occurred when attempting to cover a wall completely with pentagons. Early in 1958, the manuscript of my publication was rejected by the editors of *American Mineralogist* who explained that, "Pentagonal symmetry cannot be repeated crystallographically, and such hypothetical crystals cannot be expected to occur in nature." They suggested that I omit any mention of the possibility of synthesis of a substance (*e.g.,* an alloy) with five-fold symmetry which could be developed under the hypothetical scheme described in my manuscript. After following their suggestion, the issue of potential pentagonal symmetry in minerals was avoided, and the manuscript was finally published in another journal. In spite of this, many subtle clues indicated that five-fold symmetry could occur in nature, although not in the pure crystalline form, *e. g.*, five radiating appendages or ambulacral areas in starfishes, echinoderms, etc.

The Oxford mathematician, Roger Penrose (of black hole and space-time singularity fame), showed how it is possible to tile (with no gaps) a plane by means of five-fold symmetry, using a fat rhombus and a thin rhombus. In 1984 electron microscope studies at the United States Bureau of Standards discovered large-scale five-fold symmetry in an alloy of aluminum and manganese. According to Davies [8] this strange material had, "... symmetries that violate a fundamental theorem of crystallography: its atoms are arranged in a pattern that is physically impossible for any crystalline substance". It was termed a quasicrystal. The Penrose tiling pattern (see figure 2) serves as a model for quasicrystals and possesses orientational order but not translational order, i.e., it bypasses the theoretical preclusion of pentagonal symmetry because it is not periodic: no matter

what the extent of the tiling, no local pattern will ever have cyclic repetition. In recent years, such scientists as Davies have resolved the whole perplexing situation of quasicrystals and pentagonal symmetry development. Nevertheless, mystics and esoterists for centuries have placed significance on the attributes of the pentagon and its mysterious developmental derivatives, such as the ennegram. Inspired by the mystics, I was able to intuitively perceive the need for the development of an artificial plan in my publication to accommodate some of the puzzling problems which would later arise, such as quasicrystals.

LIMITS OF LONGEVITY

In 1960 the topic of discussion during a coffee break in the Botany Department at Kansas University (KU) was a report by Paul Tasch, a geology professor at Wichita State University. Tasch [9] had revived some bacteria which evidently had been preserved remarkably well for over 250 million years in dense Permian salt deposits. Nearly everyone present either openly laughed or scoffed at such a seemingly outrageous claim of rejuvenation of apparently dead or "fossilized" bacteria. Nearly everyone tried to discredit Tasch's work by saying that his samples were contaminated by modern bacteria. I had some doubts about the antiquity of the bacteria but did not laugh. Instead, I countered by asking for the evidence that all of Tasch's bacteria were modern and contended that cytoplasmic material of insects had been found intact after millions of years in amber and that ancient microenvironments, other than salt and amber, could be well suited for superb preservation, such as Carboniferous coal balls (over 280 million years old). Many of the coffee drinkers thought it best to leave, in view of this surprising defense of Tasch and the outspokenness of a student who had the audacity to question the opinions of learned professors. Upon leaving, one of the professors, Robert Lichtwardt, said that in the absence of any first hand knowledge of Tasch's work, he would withhold any judgment until some more prudent time. Apparently, Lichtwardt was as appreciative of my forthrightness as I was of his.

It was noteworthy that the courage and veracity of Tasch in regard to his own careful observations in the face of scientific orthodoxy was matched by the caution and reservation of Lichtwardt in the face of that same orthodoxy. Subsequent to the work of Tasch, many well researched investigations of revived ancient bacteria have been reported, some over 25 million years old, *e. g.*, Monastersky [10]. Most of us could learn important lessons from the model of caution offered by Lichtwardt, in lieu of not knowing which of these opposing views about the time limits on the duration of life might prevail.

WHAT IS LIFE?

My studies at KU introduced me to the works of Charles Fort [11] who questioned our knowledge of organisms and life. He stated that Darwin nor anyone else was able to tell what was meant by a "species" and that the concept is not possible to define because there is nothing final to find out - it's like looking for a needle that no one ever lost in a haystack that never existed. Since the definition of life was unclear (even the concrete in a sidewalk displayed some of the traits of life), my inclination was to view the boundary between living and non-living as so

fuzzy that all existence should be viewed as a continuum.

At KU early in 1963, I had completed the writing of my thesis for the MA Degree in Botany but failed the oral examination for that degree later in the year. Some of my answers were shocking to the examiners. One professor's mouth dropped in astonishment at my reply to the question, "What is life?" I had followed a very simple and ancient theme from Greek Milesian or Hylozist Philosophers (6th Century, BC), oriental mystics, ancient sages, etc.: boundaries between living and non-living are transitional. We are either somewhat more alive than rocks and molecules from which we originated or we are somewhat less dead than the rest of the universe from which we will surely return upon our "alleged" death. The condition of life and lifelessness, existing as a continuum of opposite extremes united in wholeness, was later explained in modern terms by such noted scientists and modern physicists as Fritjof Capra [12]. But these ideas were far too extreme for those in the mainstream of American biology in the early 1960's, and further explanation was required. I said that conceivably everything is alive, but some things are more alive than others, or, on the other hand, it is possible that everything is dead, but some things are less dead than others, and this "less dead" state is usually referred to as "being alive." This interesting situation compelled me to speak of my experience with the origin of oil controversy. If something as obviously organic as crude oil had a structure which spontaneously (or naturally) originated from the combining of materials from the inorganic environment and if organisms which were based upon organic compounds in turn depended upon the inorganic environment for their continued existence, then the distinction between "alive" and "dead" was not precise. Having seen the sustained discomfort at my explanations, I tried a different approach, by asking, "Are viruses alive or dead?" No one would commit to a definite answer, because consensus of opinion at that time was that viruses were transitional between living and nonliving. Now the examiners seemed even more uncomfortable, because a maverick student was asking questions of them, instead of giving the expected traditional answers to their questions.

After having been told of my failure of the oral examination, I also was informed that the Botany Department Chairman said that my performance was the worst that he had seen in his entire time at KU. Jobs were scarce in botany and geology at that time, and I considered returning to my first work, manual labor in the oil fields. A professor, R. C. Jackson, who was one of my examiners, must have been impressed with my plight, because he suddenly gave me moral support. With his encouragement, a few months later I was able to pass (just barely) the oral exam and complete my MA Degree. However, I was not allowed to continue my graduate studies for the PhD at KU, because my unconventional ideas were unwelcome to the department chairman.

EXTRATERRESTRIAL LIFE

My views on potential fossils in organic meteorites had received some disdain from professors and graduate students at the Botany Department at KU. The controversial George Claus, a collaborator with Bartholomew Nagy in the sensational work on potential extraterrestrial fossils in carbonaceous meteorites,

had visited me more than once at KU. Sadly, Claus was hounded and persecuted (like Velikovsky) by the scientific establishment for his views on controversial subjects in astronomy, and my problems were similar to those of Claus.

In support of a publication of mine in 1965 [13] on the acid resistant microfossils from the Alais and Orgueil organic meteorites, all of my evidence (microscopical preparations on glass slides) of potential extraterrestrial life from those meteorites was sent to the Rijksmuseum (Stockholm, Sweden) for repository deposition. The reply from that museum implied that I had a lot of nerve to send unsolicited materials for deposit there. However, they were accepted with all of the enthusiasm that might have accompanied a donated bag of vacuum cleaner sweepings.

In 1966 a manuscript of mine on the evidence for microfossils in the Orgueil carbonaceous meteorite was rejected by the journal, *Science*, on the basis that "such caustic cleaning treatment of samples from the meteorite would have destroyed any living matter." The reviewer and editor both were unaware that this treatment with Schulze's Solution (saturated potassium chlorate with concentrated nitric acid) was standard procedure for the cleaning and analysis of fossils (including spores, pollen, etc.) in coal beds. Spores, pollen, and plant fragments are naturally coated with a waxy material (which is essentially unaffected by any acids or caustic solutions) and can be effectively cleaned with Schulze's Solution without damage. Soon thereafter, another of my similar manuscripts was submitted to *Science* and was renounced by the reviewers on the basis that, " ... all fossils and potential living organic material in a meteorite would be incinerated before it hit the earth." Apparently the reviewers were unaware that several cold meteorites (such as the Bokkevold which was accompanied by ice) have landed on earth. Moreover, vast temperature differences have been recorded in various areas within some meteorites. After combining the two manuscripts into one, it was published in *Nature* in 1966 [14].

Much laughter and learning has resulted from these and similar behaviors attributable to the many erratic defenders of the gates of conformity and other critics of evidence for extraterrestrial life. Having learned much from my previous bouts with the scientific hierarchy, I tried to be less controversial and was lucky to have completed my PhD in Biology in 1966 at the University of Louisville (where my unusual views were more favorably received than in any of the other five universities where I was a student or employee).

TEACHING AND ACADEMIC PROVINCIALISM

From 1967 to 1970 I was Assistant Professor of Biology at Northeast Louisiana University (NLU). I incorrectly believed that it would be possible for me to adapt to such an institution as NLU (a former teacher's college) where advances in the frontiers of science were viewed as proper pronunciation of scientific terms, choosing the student of the year, debating the nature of teleological statements, etc. In 1970 the dean and other administrators informed me that I would not be recommended for tenure because my performance was not in accordance with NLU's administrative policy and motto that, "people are more important than things." This was an indictment that my teaching style spent too much time on

research and publications with too little on lectures and their preparation. My current National Science Foundation (NSF) Academic Year Extension Grant for research at NLU could not be completed, because I had just lost my job there. In spite of having my academic sponsor default, I was allowed by NSF to complete my research at the Tropical Atlantic Biological Laboratory in Miami, Florida.

INSOLENCE IN MEDICAL SCIENCE

In 1976 I was in Providence Hospital in Cincinnati, Ohio, pending the passage of a large kidney stone. The treatment of my attending physician (hereafter referred to as “Dr. Quack”) was inappropriate. I had a reaction to his medications and could hardly breathe. After only two days in his care, my left lung collapsed. Like so many people who previously had only minor medical problems and who acquired some new affliction because of exposure to so much disease and mismedication in a hospital, I almost died before being released. In 1998 it was reported that over 100,000 patients in hospitals in the United States die each year from adverse drug reactions [15]. Doctors, drug corporations, and hospitals all share in the blame for this persistent problem which often has been ignored. The day after my lung collapsed, my wife was visiting. My bed was next to the hall, and I could hear the resident doctor (hereafter designated as “Dr. Duck”) outside my room discussing my case with my wife. He told her that I was going to die! By the time she returned to my room, my recovery from the collapse already had begun. My proclamation was, "I am going to live, if but for no other reason than to give a tongue lashing to the pathetic, overbearing, unprofessional excuse for a practitioner of medical science!" The next day my kidney stone passed, and I felt well enough to try to release myself as quickly as possible from the hospital before Dr. Quack really caused my death. Unfortunately, my insurance company probably would not pay my bill, if I dismissed myself from the hospital. Being a devout miser, I decided to relax and take my chances until officially dismissed. But I also decided to follow only those instructions of Dr. Quack which met with my approval - I had taken my life into my own hands. How many of us would have been willing to stake our own lives to prove that overbearing behavior, suppression of alternative remedies, and standard procedure in medicine could be inappropriate? My studies of alternative medicine indicated that therapeutic doses of vitamin E and pranayama yoga (deep breathing exercises) would probably be beneficial. As a strict follower of medical convention and the American Medical Association (AMA), Dr. Quack refused to condone either. My wife was able to smuggle vitamin E into the hospital and I was able to practice pranayama yoga in secret during the remaining two weeks I was held under "house arrest" by Dr. Quack. During that time, Quack informed me that I “would soon die”, if his recommended treatment was not followed. After leaving the hospital, my self-prescribed therapy was followed, and it continues even to the present. I filled none of Quack’s prescriptions and ignored his instructions (except to be examined in his office, a week after my release). By 2002 I had not been in a hospital or a doctor’s office (except twice for general check-ups), since the death defying episode with Duck and Quack over 26 years before.

RADIOMETRIC “PRECISION” DATING

By 1990 my long experience had taught me that unusual research frequently would be met with scorn and suspicion by the traditional scientists, and this inspired me to work on less controversial projects. Since their inception, all high-technology methods of dating had become a type of sanctification, and appeared to be beyond reproach, not only in geology, but in archaeology, astrophysics, etc. Nevertheless, inherent precision limitations of K-Ar (Potassium-Argon) radiometric dating of volcanic rocks in the Columbia Plateau region of Washington state were known [16]. Moreover, Holmgren [17] had stated that, “K-Ar dates lack the resolving power to delineate specific Columbia River flows or groups of flows.” Later investigators of the lava flows in this region had become so cautious that they disregarded certain questionable K-Ar dates of these rocks in their studies (*e. g.*, [18]). I resolved to make some sustained efforts at traditional stratigraphic studies to determine if they might be more useful in addressing these problems.

My investigation [19] of diatom fossils in sedimentary beds encased by these volcanic rocks supported the proposition that, under certain conditions, traditional micropaleontological techniques (*e. g.,* extinct species with restricted ranges in the fossil record, dominant species within assemblages, etc.) could be used as successfully (or even more successfully) to determine the age of a given fossiliferous zone than a series of samples older and younger than the same zone from the general region which have been “precision” dated by K-Ar or other radiometric methods. Because such micropaleontological techniques previously had been used for correlations to provide forensic evidence for use in court, they should have been sufficiently diagnostic to use in the correlation of paleoenvironments and dating of sedimentary rocks like those in the Columbia Plateau. It has been many years since my investigation was published, and I have yet to learn of any criticism of it. That my provocative premise could escape the wrath of the high-tech scientific crowd probably is because it used well established (but time-consuming) traditional methodologies which have not yet been discarded.

HUEYATLACO, CAULAPAN, AND ARCHEOLOGICAL SUPPRESSION

Numerous mainstream archaeologists seem to behave more like historians or politicians than scientists. Some archaeologists, instead of welcoming new ideas, theories, and discoveries, seem to shun them as a challenge to their existing belief systems. But this is not unexpected in such politicized "social" sciences as anthropology and archaeology. Many archaeologists are stodgy and have a tendency to ignore information from other disciplines (such as geology, physics, etc.) which might contradict their favored ideas about artifacts and ages of sites. It is this persistent disregard and/or denial of abundant evidence to the contrary by the entrenched archeological hierarchy which attracts the scrutiny of many scientists from other disciplines (such as myself). In any field of endeavor, the perennial ignoring of anything which does not fit the current paradigm is not scientific behavior. In respect to this problem, Will Hart [22] commented, “We have seen this same ‘unscientific’ approach applied to archaeology and anthropology, where 'scientists' simply refuse to prove their theories yet appoint themselves as the final arbiters of 'the facts'". Many archaeologists (and scientists) seem to think

that consensus of thought is more important than objectivity and through investigation.

The Hueyatlaco artifacts from the Valsequillo region, south of Puebla, Mexico, often have been used to demonstrate cover-ups by the archaeological community, for example, [20, 21]. The controversy with the Hueyatlaco artifacts is simple: bona fide artifacts have been found in situ in deposits which, by various means, have been demonstrated to be older than the Last Ice Age, but most American archaeologists disagree. It is unlikely that any competent biostratigrapher, paleontologist, geologist, or geochronologist would disagree. Archaeologists often try to discredit the artifacts themselves or their in situ emplacement as they go off on their fanciful speculations. Their defenses include such phenomena as "redeposition", which archaeologists are so fond of invoking whenever artifacts have an age that does not fit their current ideas. Redeposition is very rare in areas like Valsequillo. It was known in Europe over a century ago that these artifacts (including the Dorenberg Skull, an early human) from the Puebla region were older than the Last Ice Age [23]. A substantial body of evidence for the great antiquity of these artifacts comes from the sciences of geology, geomorphology, paleoecology, soil studies, radiometric dating, tephrochronology, and micropaleontology. Contributions of Hugo Reichelt, F. Hustedt [24], and myself [25, 26] to our knowledge of many important marker fossil diatoms of the Puebla region are adequate to make a good case for an age assignment of Last Interglacial or Sangamonian (sensu lato = 80,000 to about 220,000 years BP), without considering such radiometric and high-tech dates as those mentioned by Szabo et al. [27] which also denote a Last Interglacial (or older) age for the Hueyatlaco artifacts.

Although J. L. Lorenzo made important contributions to archeology, some of his antics with the Hueyatlaco site were shameful and not in the best interests of science or archaeology. He was a bastion of doctrinaire archaeology. Evidently he made a special effort to discredit the plentiful evidence for the great antiquity of important artifacts at Hueyatlaco [28]. He cast doubt on the careful and sustained work of Juan Armenta Camacho and Cynthia Irwin-Williams by claiming that the archaeological materials of great age at Hueyatlaco were planted as a hoax [29]. Some professional archaeologists in Mexico still believe Lorenzo's misrepresentations.

Irwin-Williams was a respected archaeologist who tragically died in 1990. She was made to suffer because her work on the Hueyatlaco artifacts did not fit the "late arrival" (postglacial) or "Clovis First" concept of the American archaeological establishment. She contributed greatly to our knowledge of the artifacts of the Valsequillo region. However, she failed to appreciate the full importance of geochronological and geological principles in interpreting the relationships of older archaeological sites (*e. g.*, Hueyatlacto), especially those which are beyond the range of efficacy of C^{14} date determinations. The claim of ca. 21,850 years BP for the Hueyatlaco artifacts by Irwin-Williams [30] is linked to the much younger Caulapan artifact site (which is over 5 km away), and this claim evidently resulted from misunderstanding geological principles, deception, or wishful thinking (or perhaps all three). Apparently she made a special attempt to justify equating the

age of the Caulapan site with the Hueyatlaco site, by ignoring considerable geological evidence to the contrary. This rather bold (even irresponsible) superimposing of this date onto the Hueyatlaco artifacts oddly has become accepted by archaeologists (*e. g.,* Dincauze [31]) even though it might discredit the traditional late (or postglacial) arrival of humans in North America. This alleged "correlation" with the Caulapan site has been discredited by Virginia Steen-McIntyre and myself [32].

At Hueyatlaco such deceptions as those of Lorenzo and Irwin-Williams readily can be exposed, *e. g.*.: (1) certain fossil diatoms (see figure 3), which were scraped from inside the Dorenberg Skull, became extinct before the Last Ice Age and correlate with some of the same extinct diatoms in beds which enclose bifacial tools over 8 meters below the modern surface [33]; and, (2) these and lower beds contain tool marked bones of animals which became extinct before the Last Ice Age. Eventually the evidence for the great antiquity of these artifacts will accumulate to the point where it cannot be ignored, even by the most reluctant ultraconservative archaeologists.

CLOSING COMMENTS

Could scientists, who are guided by closed belief systems, be afraid of the enthusiasm of upstarts (like myself) because we might encourage others to overthrow the sacred orthodoxy of established ideas in which there is so much vested interest?

A large part of my life has been spent communicating with students and young scientists. It is important to catch the imagination of young aspirants in any field of work. Those like Einstein, Edison, Tesla, Galileo, Copernicus, and da Vinci all would agree. Science, like life in general depends on trial and error (experimentation) to answer objectives of wonder. When wonder no longer is active, science, religion and other pursuits ultimately will become meaningless in a cosmos which is constantly changing. Failure of theories and ideas is not a disgrace to science, but closed mindedness is. It is delightful to observe many young children of today who view science as a big wonder factory and to remember inspiring lines of verse from my own youth: “Seeing wonder always to prevail, one can bear the future, even if to fail.”

REFERENCES

[1] Snider, A. (1858). La Creation et ses Mysteres Devoiles. Paris. F4

[2] Sagan, C. (1997). Billions and Billions. Ballantine Books, New York, p. 117 (296 pp.)

[3] Cooper, C. It’s No Crude Joke: This Oil Field Grows even as It’s Tapped. Wall Street Journal, April 16, 1999, pp. A1, A6

[4] Gold, T. (1997). Reasons for Expecting Sub-surface Life on many Planetary Bodies. Proceedings, International Society for Optical Engineering, vol. 3111, pp. 7-14.

[5] Robinson, R. (1966). Origins of Petroleum. Nature, vol. 212, pp. 1291-1295.

[6] ibid., Gold, (1997).

[7] VanLandingham, S. L. (1958). The Pentagonal System. Compass, 1958, vol. 35, pp. 105-110.

[8] Davies, P. (1988). Cosmic Blueprint. Simon and Schuster, New York, pp. 78-80 (224 pp.)

[9] Tasch, P. (1960). Paleoecological Observations on the Welling Salt (Hutchinson Member). Transactions of the Kansas Academy of Science, vol. 63, pp. 24-30.

[10] Monastersky, R. (1995). Ancient Bacteria Brought back to Life. Science News, vol. 147, p. 308.

[11] Fort, C. (1941). The Complete Books of Charles Fort. Henry Holt and Company, New York, pp. 13-14 (1125 pp.)

[12] Capra, F. (1975). The Tao of Physics. Shambala, Berkeley, CA, pp. 20-25 (330 p.)

[13] VanLandingham, S. L. (1965). Acid Resistant Microfossils from the Alais and Orgueil Meteorites. Nova Hedwigia, vol. 10, no. 1-2, pp. 162-176.

[14] VanLandingham, S. L. (1967). Origin of Round-body Structures in the Orgueil Meteorite. Nature, vol. 216, no. 5112, pp. 252-253.

[15] Anonymous (1998). Study Finds Drug-Reaction Toll is High. Wall Street Journal, April 15, p. B9.

[16] Watkins, N. P. and Baksi, A. K. (1974). Magnetostratigraphy and Oroclinal Folding of the Columbia River, Steens, and Owyhee Basalts in Oregon, Washington, and Idaho. American Journal of Science, vol. 274, pp. 148-189.

[17] Holmgren, D. A. (1970). K/Ar Dates and Paleomagnetics of the Type Yakima Basalt, Central Washington. *In* E. H. Gilmour and D. Stradling (eds.), Proceedings of the 2nd Columbia River Basalt Symposium, Cheney, Washington, 1967, pp. 189-199.

[18] Beeson, M. H. and M. R. Moran (1979). Columbia River Basalt Group Stratigraphy in Western Oregon. Oregon Geology, vol. 41, pp. 11-14.

[19] VanLandingham, S. L. (1991). Precision Dating by means of Traditional Biostatigraphic Methods for the Middle Diatomaceous Interbeds within the Middle Yakima (Wanapum) Basalt of South Central Washington (U. S. A.). Nova Hedwigia, vol. 53, no. 3-4, pp. 349-368.

[20] Cremo, M. A. and Thompson, R. L. (1993). Forbidden Archeology. Govardhan Hill Publishing Company, Badger, CA, 914 pp.

[21] Steen-McIntyre, V. (1998). Suppressed Evidence for Ancient Man in Mexico. Nexus, vol. 5, pp. 47-51.

[22] Hart, W. (2002). Archeological Cover-ups - A Plot to Control History? Nexus, vol. 9, no. 3, pp. 53-56, 74.

[23] Reichelt, H. (1900). Fossile Bacillariaceen eines Kalktuffes aus Mexico. Zeitschrift fur angewandte Mikroscopie, Bd. 5, H. 1, S. 3-10.

[24] Hustedt, F. (1913-1934). Atlas der Diatomaceen-Kunde. R. Reisland, Leipzig, Germany, Tafeln 295 & 389.

[25] VanLandingham, S. L. (2000). Sangamonian Interglacial (Middle Pleistocene) Environments of Deposition of Artifacts at the Valsequillo Archeological Site, Puebla, Mexico.≈ Transactions 35th Regional Symposium for Southeastern New Mexico and Western Texas, Southwest Federation of Archeological Societies Annual Meeting, 1999, Midland, TX, pp. 81-98.

[26] VanLandingham, S. L. (2002). Correlation of Sangamonian Interglacial Age (sensu lato = 80,000-ca. 220,000 y BP) of Artifacts at the Valsequillo Archeological Area, Puebla, Mexico, by means of Diatom Biostratigraphy. 17th International Diatom Symposium, August 25-31, 2002, Ottawa, Canada, Book of Abstracts, p. 151.
[27] Szabo, B. T., Malde, H. E., and Irwin-Williams, C. (1969). Dilemma posed by Uranium-Series dates on Archaeologically Significant Bones from Valsequillo, Puebla, Mexico. Earth and Planetary Science Letters, vol. 6, pp. 237-244.

[28] Lorenzo, J. D. (1967). Sobre Metodo Arqueologico. Boletin de Instituto Nacional de Antropologia e Historia, Junio 1967, pp. 48-51.

[29] Irwin-Williams, C. (1967). Comments on Allegations by J. L. Lorenzo concerning Archaeological Rese≈arch at Valsequillo, Puebla. Paleoindian Institution, Eastern New Mexico University Miscellaneous Publications, 7 pp.

[30] Irwin-Williams, C. (1973). Summary of Archaeological Evidence from the Valsequillo Region, Puebla, Mexico. 9th International Congress of Anthropological & Ethnological Sciences, 1973, Chicago, IL, pp. 1-22.

[31] Dincauze, D. (1984). An Archaeo-Logical Evaluation of the Case for Pre-Clovis Occupations. Advances in World Archaeology, vol. 3, pp. 275-323.

[32] VanLandingham, S. L. (2003). Correlation of Artifact Horizons at the Hueyatlaco Archaeological Site with Sangamonian (sensu lato=80,000 to ca. 220,000 yr BP) Age Diatomaceous Samples, Cores, and Measured Sections from the Valsequillo Region south of Puebla, Mexico. Transactions 38th Regional Archeological Symposium for Southeastern New Mexico and Western Texas, Southwestern Federation of Archeological Societies Annual Meeting, April 6, 2002, Lubbock, TX, p. 67.

[33] Ibid.

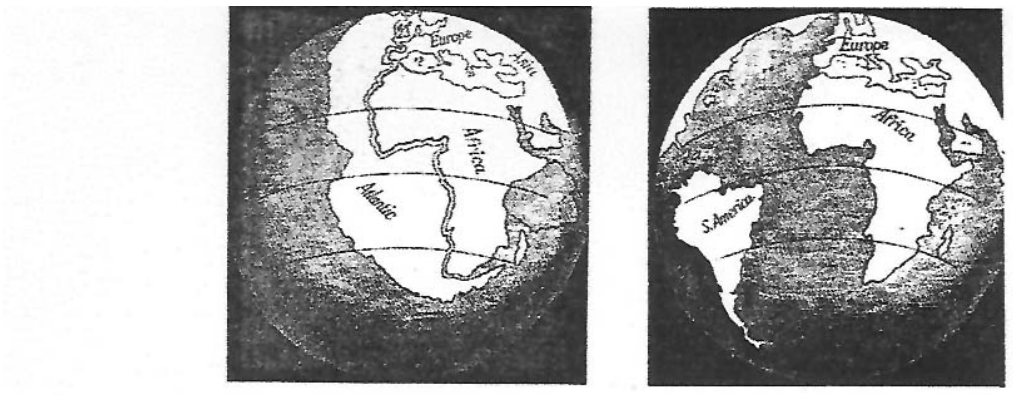

Figure 1: Maps published by A. Snider in 1858 to illustrate continental drift.

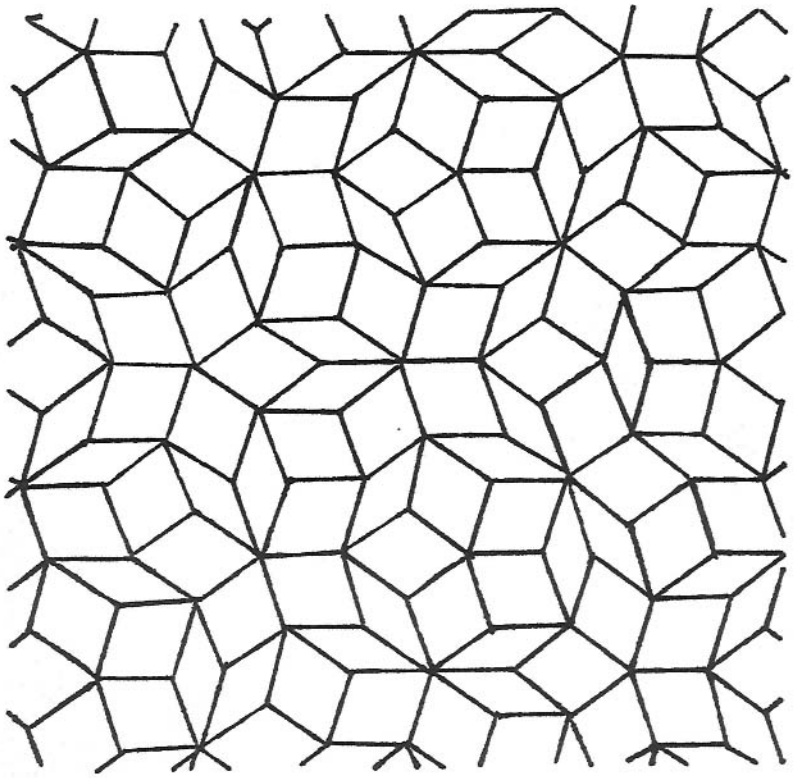

Figure 2: Penrose's tiling pattern composed of two types of units, wide and narrow diamonds with edges of equal length. Using only the two shapes the entire plane can be covered without gaps to produce a remarkable design that has five-fold symmetry.

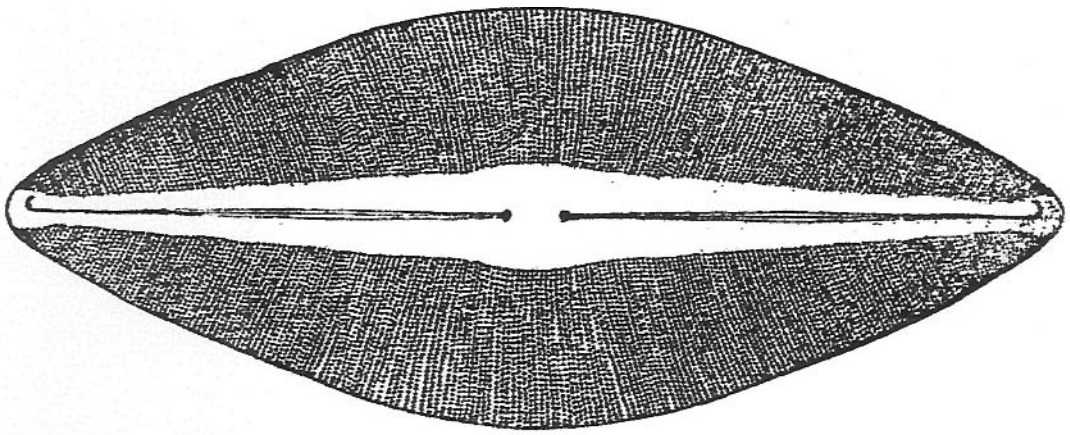

Figure 3: Navicula dorenbergii, a diatom which became extinct before the Last Ice Age; magnified 1000 times.