Editorial Comments

This is my last issue as editor of the Quarterly and I express appreciation to many people who have helped me. To my wife, Mary, who typed and did proofreading, to Mrs. Carol B. Armstrong for galley proofreading and to the many peer reviewers who have kindly criticized manuscripts and offered valuable suggestions. Also I thank the authors, article and book reviewers for their contributions to the Quarterly. It is remarkable that the Quarterly is able to maintain such high standards when it is realized that the editor, proofreaders, peer reviewers and authors receive no remuneration for their contributions and that the effort expended is done on a person's "free time." I hope each of you will support Dr. Donald B. DeYoung as he assumes the responsibility of editor.

Duane Gish reviews the past 14 years of creationist research (ending March 1988) in this issue with Part I of his series on geology. Part II will appear in the June Quarterly. There are pro and con discussions of Robert Gentry's work on radioactive halos. Tom Barnes answers some criticism on his model of the earth's decaying magnetic field. Ronald Calais answers some criticism on a previous "Protoavis" note and offers some new information on the topic. The last part of the minisymposium on the speed of light is presented with Setterfield's answer to the criticisms of a recent decay in the speed of light. This symposium included a discussion of the special theory of relativity and its relation to the origins debate, a proposed small-scale model of the universe and a criticism of the big bang model. There are several shorter selections that may interest you in the Quarterly.

Prospective authors, please send your manuscripts to Dr. Donald DeYoung, Grace College, 200 Seminary Drive, Winona Lake, IN 46590.

Emmett L. Williams

MORE CREATIONIST RESEARCH (14 YEARS)*—PART I: GEOLOGICAL RESEARCH

DUANE T. GISH**

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Abstract

Creationist research in the past 14 years is reviewed as it was in the first decade of the Creation Research Society (Gish, 1975). Part I is a review of geological research and Part II (to appear in the next Quarterly) is a review of biological research.

Introduction

A review published in the *Creation Research Society Quarterly* 12:34-46[a] described the research that had been published in the *Quarterly* through Volume 10. The present review describes the research published in the *Quarterly* through March 1988, completing nearly 25 years of publication. This review has been restricted to those articles which describe bench-type or field-type research. This restriction has caused the omission of some extremely significant and interesting theoretical research by Dr. Thomas Barnes, Emeritus Professor of Physics, University of Texas, El Paso (Barnes, 1975; Barnes and Upham, 1976; Barnes, Pemper and Armstrong, 1977; Barnes, 1980; Barnes and Ramirez, 1982a; Barnes, *et al.*, 1982b; Barnes, 1983) and some interesting and productive mathematical research by Dr. David Rodabaugh (1975a, 1975b, 1975c, 1976) among others.

One of the many false charges leveled at creation scientists is that there are few biologists among them, that the creation science movement is dominated by engineers. This has been the consistent claim, for instance, of John Patterson, a bitter anti-creationist engineering professor at Iowa State University (Patterson, 1982, pp. 55-8). Actually, the largest category of membership of the Creation Research Society with advanced degrees in science are biologists and biological research papers constitute one of the largest groups described in this review, followed closely by geological research papers.

*See Gish (1975).

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[a]Available from CRS Books, 5093 Williamsport Drive, Norcross, GA 30092 for $2.00 prepaid and postpaid.

Overthrusts

While in Europe in the spring of 1975, Clifford Burdick, a consulting geologist, visited thrust formations near Loch Assynt, in Northern Scotland, and the Glarus formation, near Schwanden, Switzerland. Burdick (1975, pp. 155-6) confirmed that the Scottish formations, the Moine, Ben More, and Glen Coul, which extend typically over a little more than five miles, are indeed thrusts. In contrast to these findings, his investigation of the Glarus formation confirmed Walter Lammerts' assessment (1972, pp. 251-5) of this formation that it reveals no evidence of overthrusting. The Glarus formation had been designated as an overthrust, not on the basis of any physical evidence for thrusting, but solely on the order of the fossils. Burdick quotes Billings (1955, p. 131) as saying:

> Parts of some of the great overthrusts in the Alps are so devoid of slickensides, gouge, and mylonite, that they passed unnoticed and were for a time mapped as sedimentary contacts. It was only after paleontological evidence was obtained . . . that the existence of the great faults was recognized.

An overthrust is claimed because the Verrucano, a coarse-grained arkosic schist, said to be Permian, overlies slate, described as Eocene, which evolutionists believe to be about 200 million years younger than the overlying Permian.

Although both the Verrucano and the slate are metamorphic, indicating heating, there is no evidence of sliding or other such motion. Just above the slate are two rather thin layers of limestone with a thin layer of water-deposited clay showing no signs of disturbance. As Billings has pointed out, and as confirmed by Lammerts and Burdick, there is none of the usual

telltale signs of overthrusting at the sharp contact line. Burdick concludes his assessment by stating:

> I suggest that the Glarus formation shows a complex petrologic history of depositions, metamorphism, folding, erosion, fresh deposition, and more regional metamorphism. There are marked unconformities, but no signs of thrusting; only signs of readjustment after the folding.

Burdick (1974, pp. 56-60) also has reported his reinvestigation of the so-called Lewis Overthrust. His original report had appeared in *CRSQ* 6:96-106. On the basis of his research, Burdick maintained that the usual evidences for overthrusting—mylonite, breccia, and slickensides—were missing. He concluded, therefore, that this may not be a true thrust.

In the summer of 1973, geologists Malcolm Fargher and Walter Peters accompanied a field trip sponsored by the Bible-Science Association. Fargher had reported the existence of slickensides and other physical criteria in the vicinity of the thrust contact, suggesting that the Lewis Overthrust might be a true thrust. In October of 1973, Malcolm Fargher accompanied Burdick on a reinvestigation of a number of points of contact. Most geologists assume that the Lewis Overthrust is a true thrust, because Cretaceous shale is overlaid with Precambrian Altyn dolomite, which is assumed to be more than 500 million years older than the Cretaceous. To illustrate the fact that in all such cases evolutionary geologists would assume that an overthrust must have occurred, Burdick quotes Nicholson (1897, p. 40):

> It may be said that in any case where there should appear to be a clear and decisive discordance between the physical and paleontological (fossil) evidence as to the age of a given series of beds, it is the former that is to be distrusted rather than the latter.

Burdick reports his reinvestigation of the contacts at Marias Pass, Dry Creek, at Two Medicine, Roes Creek, Cut Bank, and Crowsnest Pass. At Marias Pass, there is a definite unconformity, where the underlying Cretaceous shale has been faulted, with a westward dip of about 45°. Burdick could find no evidence, however, of differential movement at the point of contact between the underlying Cretaceous shale and the overlying Altyn dolomite, such as gouge, breccia or slickensides. At the Dry Creek, at Two Medicine contact, the overlying Altyn dolomite had been deformed to the point of brecciation. The crushing and brecciation was local, however, with no gouge layer or other evidences of a giant thrust between the Precambrian and Cretaceous formations. Apparently the dolomite had been crushed and brecciated before it could be moved, the force required to break it up being far less than the force required to move this mammoth block of rock.

At Roes Creek, there appeared to be no evidence of thrusting between the underlying black Cretaceous shale and the overlying buff-colored rock. Burdick agreed with Fargher, however, that the overlying rock was neither Belt Series Altyn dolomite nor Cretaceous shale. At the Cut Bank contact, the contact line is razor sharp, with no gouge or brecciation in the overlying Altyn dolomite, as had been the case at Dry Creek. At Cut Bank, the underlying Cretaceous shale had been severely broken and brecciated, thus providing precisely the reverse of the situation at Dry Creek, where the overlying Altyn dolomite had been broken and brecciated. Burdick points out that if the severe breakage and brecciation of the underlying Cretaceous shale had been caused by overthrusting of the overlying Precambrian Altyn dolomite, both surfaces would have been brecciated, with gouge and slickensides. In the absence of such physical evidences, Burdick concluded that this contact provided no evidence for thrusting.

Crowsnest Pass is located along the highway at the Continental Divide at the British Columbia-Alberta boundary. Burdick reports that here the overlying Precambrian Belt Series, Altyn dolomite, lies conformably over the Cretaceous shale with a sharp contact, although the beds lie at about a 40° angle. There is a slight brecciated seam of an inch or two between the beds, as would be expected from differential movement due to folding, but there is no evidence of a thrust-fault, according to Burdick.

Burdick closes his report with the following statement by R. C. Emmons, geologist at the University of Wisconsin:

> Under-thrusting and upwelling appear to have bypassed the usual period of scrutiny, into one of intransigent acceptance, and are widely invoked, though unestablished in the geologic literature, as for example is overthrusting. Both vagrant concepts have assumed a sacrosanct status under geophysical husbandry that denies communion to opposition..

Another alleged thrust fault is the Heart Mountain formation in Wyoming. This area had been studied by Burdick (1977, pp. 207-10), and he reports that he could find no evidence for overthrusting. With the so-called Heart Mountain Thrust, Madison (Mississippian) limestone is found overlying Tertiary limestone, or dolomite. Since the Mississippian is supposedly more than 200 million years older than the Tertiary, according to uniformitarian geologists, they must assume that the presumedly older formation came to rest on top of the presumedly younger formation by overthrusting. Burdick cites reports of other geologists who failed to find the usual evidences expected for overthrusting for Heart Mountain, but who nevertheless persist in believing in an overthrust on account of the fossil evidence.

Burdick reports that the whole area has been severely folded and deformed. Southwest of and adjacent to Heart Mountain, the strata have been so severely deformed that in places they rest on the edge. The contact of the Madison on Heart Mountain with the underlying bed is covered with limestone rubble. Some geologists have interpreted this rubble as tectonic breccia caused by thrust movement. Burdick noted, however, that this rubble is no different from the erosional rubble all around the steep sides of the mountain. Where the underlying sandstone was exposed, it did not show brecciation or mylonite. Burdick found definite evidence of fault action at Heart Mountain, but it involved normal, or vertical fault action rather than overthrusting. He thus discounted the

generally accepted idea of thrust-faulting at Heart Mountain and interpreted the formation as a normal fault at the apex of an anticline.

Charred Branch from the Paluxy

Frederick P. Beierle (1979, p. 87) reported the finding of a charred tree branch in the Cretaceous limestone of the Paluxy River bed near Glenrose, Texas. This is the site, of course, of the controversial reports of the discovery of alleged human tracks near those of dinosaurs. In the company of Wilbur Fields, Rex Hess, and others, Beierle and his co-workers found the charred branch embedded in the Cretaceous rock about 200 yards downstream from dinosaur tracks in the same formation. The branch was seven feet long and about two inches in diameter. They are of the opinion that the limb had fallen into soft limestone, perhaps from a tree that had been hit by lightning, the limestone hardening after completely encasing the smoldering limb, leaving the limb half-burned.

The Thornton Quarry Deposits

The Thornton Quarry, located in a massive limestone deposit containing a high concentration of fossil corals, crinoids, brachiopods, and other shallow marine organisms, is the world's largest commercial limestone quarry. The limestone is believed by evolutionists to be about 400 million years in age (Silurian). Based upon a limited literature search and a few field trips to the site, geologist David B. D'Armond (1980, pp. 88-105) has published a preliminary report in which he presents evidence contradicting the generally accepted notion that the limestone deposit of the Thornton Quarry is an ancient fossil coral reef. D'Armond suggests that this deposit was formed due to catastrophic wave-action generated during the Genesis Flood.

D'Armond's interest in this alleged fossil reef was aroused during the several fossil-hunting field trips to the site sponsored by then-existent Institute for Creation Research Midwest Center. The Thornton Quarry is located just to the south of the Chicago metropolitan area and the Material Services Corporation (a division of General Dynamics) regularly allows field trips and fossil hunts into the area under company supervision. The Thornton deposit is one of dozens of similar deposits in Illinois, Indiana, Ohio, Michigan, Wisconsin, and Iowa, as well as in several locations in eastern Canada. Geologists describe these deposits as part of a wedge-shaped fossil coral reef archipelago positioned offshore from an ancient shoreline. D'Armond is severely critical of the description of the Thornton deposit by Jerome Ingels (1963, p. 405), who believed this to be a fossil reef.

As D'Armond describes, a modern reef exhibits the following characteristics:

1) distinct and highly ordered growth structure in the core: no bedding present
2) shallow dipping, bedded talus slopes on flanks of the core
3) a high percentage of reef-forming algae (50-80%)
4) solid, anchored, hard bedrock as initial foothold and growth points
5) a V-shaped growth profile of the reef as a whole

D'Armond rejects Ingels' claim that the Thornton deposit is a fossil reef, for the following reasons:

1) there is no solid footing for reef growth;
2) the "reef core" does not exhibit growth structures;
3) red calcareous algae, the main reef-builder, is totally missing;
4) flank deposits are too steep for the underwater angle of repose, and the "core" could not be the source of deposits that are stratigraphically above the core, nor the dome-like structure;
5) actual exposed reef shape does not correspond to modern reefs;
6) similar massive shallow-water limestone deposits are not occurring in today's reefs;
7) the ubiquitous fossil "tar" depicts rapid and complete burial of animals before they could decay—unlike the slow processes of a reef.

In a very extensive and detailed analysis of postulated catastrophic effects that would have been produced by the Genesis Flood due to the deluge, tsunamis, storm surges, tidal waves, and earth movements and convulsions, D'Armond synthesizes a working hypothesis for the formation of the Thornton "reef" deposit and the underlying strata. According to D'Armond's synthesis,

> . . . the massive Cambrian and Ordovician strata underlying Thornton, including the St. Peter Sandstone formation, is primarily a mid-Flood deposit. Plant and animal materials, along with sediments broken loose by earthquakes, volcanoes, tsunamis, and other forces, were deposited in final form by the massive tidal action of the Flood during the months of deepest inundation. Judging from the direction of transport and deposition ascribed by modern geologists to the sediments underlying the Thornton "reef" deposits, it appears that this direction may be interpreted as being the result of tidal wave fronts driven by the Coriolis effect, which created massive tidal currents that swept over shoal areas and Canadian Shield area into deeper basins. Thus the deep underlying massive sediments may be relegated to the action of tidal waves and currents in the Catastrophic-Wave Action Model. Tidal waves and currents would also be primarily responsible for massive limestone deposits derived from chemical precipitation occurring with rapidly changing temperature, pH and chemical precipitation, while longer periods of deposition would greatly increase the chances of contamination by other materials.
>
> Following the mid-Flood deposition attributed to tidal effects, a rapid emergence of continental land masses started to occur, triggering additional violent crustal movements which, in turn, caused large numbers of tsunamis to sweep over newly emergent shorelines. The newly formed Silurian deposits, being uplifted, became a shoreline area capable of receiving coral reef fragments torn loose and transported by tsunami-type waves. The source area for these reef materials could have been actual antediluvian reefs growing on Precambrian basement rocks in the general area of present-day Hudson Bay. It could also be that the

very large area of reef deposits embedded in the Paleozoic sediments on the southern edge of Hudson Bay are actual reefs, with footholds on the underlying Precambrian, that were in place before the Flood. At any rate, the allochthonous reef deposits at Thornton and elsewhere along the archipelago, are most probably the result of tsunami wave deposition.

It is also during this time that the great wind of Genesis 8 would have been in operation, and thus storm surge deposits also could have been expected in the area. Such deposits might be represented by the material piled on top of the Thornton reef blocks, causing moderate anti-clinal dips throughout the area. The continuing emergence of the continental land masses would have uplifted these deposits, and probably would have caused steepening of the angles of dip to their present attitudes. And finally, a thin layer of Pleistocene sediments covered the area as a result of a brief glacial period.

I believe that this extensive coverage of D'Armond's working hypothesis is desirable, in order not only to present adequately D'Armond's interpretation of the Thornton deposit, but also to call attention to his Catastrophic-Wave Action Model. Readers are urged to read D'Armond's paper and to critique his model. Although D'Armond's study on the possible origins of the Thornton Quarry deposits is only preliminary, it does establish firmly that these deposits do not constitute a fossil reef, and that, while the conclusions of his study are hypothetical, it should be noted that a satisfying synthesis of the Thornton deposits can be made from the known facts.

STUDIES IN THE GRAND CANYON

Palynology

Dr. George Howe published the initial paper (1986, pp. 99-104) in a series on Precambrian pollen in Grand Canyon formations. Interest in this subject was stimulated by a report by C. L. Burdick that he had isolated pollen grains of various land plants from rocks of late Precambrian Hakatai shale while doing graduate work towards a Ph.D. (subsequently denied) at the University of Arizona under Gerhard Kremp, beginning in 1964. Burdick (1966, pp. 38-50) finally published the results of his work without support of Kremp. In his paper, Howe presents a thorough review of Burdick's work and subsequent work by others at the University of Arizona and by Arthur V. Chadwick of Loma Linda University, Loma Linda, California. The latter workers claimed they could not repeat Burdick's results and attributed Burdick's results to contamination. Howe's review includes excerpts from extensive correspondence and the comments of those involved. It provides an insight into what happens when results are obtained which are incompatible with current evolutionary thinking.

It was decided at a Creation Research Society meeting in April, 1983 that fresh samples from the Grand Canyon should be collected and analyzed by both the Burdick and the Chadwick-Doher methods, every possible effort being made to exclude contamination. A chemist, G. T. Matzko, agreed to carry out the pollen extractions according to both procedures.

The second paper in this series was published by W. E. Lammerts and G. F. Howe (1987, pp. 151-3). As noted in the first article in this series, claims have been made that Burdick's recovery of pollen grains from the Precambrian Hakatai shale was due to contamination of present-day pollen picked up either during collection and transportation of the samples or by infiltration into the samples prior to collection. Lammerts and Howe therefore undertook to assess the rate at which pollen grains will contaminate exposed slides in order to determine whether or not pollen contamination might occur routinely in the laboratory or in the field.

Lammerts and Howe used various conditions in their experimental procedures, using clean ordinary slides, and slides coated with Vaseline, scotch tape, and water; collecting points near pollinating pine trees on breezy and quiet days, near sunflower and pollinating corn plants, and under an oak tree; overnight exposure to an office near pine trees; and on a laboratory table with 86 hours of exposure. The effect of wind, as should be expected, was very important. In 10 minutes, on a breezy day, 27 pollen grains were collected near a pine tree on two slides, but no grains were collected on two slides at the same location a quiet day. Lammerts and Howe concluded that the chance of contamination by airborne pollen during slide preparation and during field work is extremely low. Whatever pollen might blow into a laboratory on a windy day quickly settles to a desk top or the floor, where it sticks; therefore, they state, it would seem that reasonable care in cleaning the table, slides, and cover slips, would render unnecessary positive pressure and filtered air supplies.

The third publication in this series was authored by G. F. Howe, E. L. Williams, G. T. Matzko, and W. E. Lammerts (1988, pp. 173-82). They reported their efforts to determine whether fossil pollen grains could be recovered from the Precambrian Hakatai Shale, as Burdick had reported, or whether negative results would be obtained as reported by University of Arizona workers and Chadwick. Samples were collected from rock exposures of the Hakatai Shale, Hermit Shale,* and Supai Group.** Hermit Shale was examined in addition to Hakatai Shale to see if pollen grains could be recovered from other shales and the Supai Formation was included as a control.

Rock samples were taken after chipping back several inches from the exposed surfaces of rocks which had no obvious seams or cracks, and were collected directly into plastic bags, every precaution being taken to prevent contamination. Samples were sent to G. T. Matzko for processing and pollen extraction in the chemistry laboratory at Bob Jones University. Some samples were given washes with dilute hydrochloric acid followed by water washes; some were given only water washes, and two samples of the Hakatai Shale were given dilute hydrochloric acid washes followed by digestion in dilute hydrofluoric acid, and then were given two water washes. From samples of loose material of Hakatai Shale recovered from two different locations, given either the hydrochloric acid and water washes or just water washes, fossil pollen grains were recovered on 8 of 15 slides, and from loose material from two locations of Hakatai

*The Hermit Shale is Permian.

**The Supai Group includes Pennsylvanian and Permian rocks.

Shale given the acid wash, hydrofluoric acid digestion, and water wash, fossil pollen grains were found on 8 of 10 slides. Photographs of the slides, taken by George Howe, and scanning electron photomicrographs, taken by E. L. Williams, were sent to an experienced palynologist for examination. This palynologist was not aware of the source from which the samples had been obtained. This was done so that objective analyses could be obtained from an expert in the field not associated with the CRS group. According to his best judgment, pine pollen, Ephedra-like pollen, angiosperm-type pollen, fungal spores, and possible algal cells were present on some of the slides. Howe and his co-workers concluded that these results support Burdick's claims of having discovered fossil pollen grains of gymnosperms and angiosperms in the Precambrian Hakatai Shale. This would, of course, refute the belief of evolutionists that the Precambrian sedimentary material was laid down hundreds of millions of years before pine trees (gymnosperms) and flowering plants (angiosperms) existed on the earth.

Howe and co-workers suggest the possibility that the reason Chadwick and the University of Arizona workers failed to confirm Burdick's findings was because these workers may have exposed their samples to prolonged digestion with hydrofluoric acid. L. I. Doher, who had developed the method used by Chadwick and the other workers, had pointed out that hydrofluoric acid has a corrosive effect on pollen and spores, and recommended that the sample not be exposed to the acid any longer than necessary, and should be washed with water immediately after disaggregation with hydrofluoric acid. The CRS group plans to pursue further research which will involve additional extracts from their rock samples, with a more extensive examination of the Supai and Hermit materials.

Formation of Dripstone Deposits— Stalagmites and Stalactites

Uniformitarian geologists assume that dripstone deposits, such as stalagmites and stalactites, form very slowly, and therefore, the existence of large stalagmites and stalactites in natural limestone caves would have required tens of thousands of years or more to form. Creationists challenge this assumption and have therefore exhibited considerable interest in present-day examples of rapid natural dripstone formation and have conducted laboratory experiments designed to measure rates of dripstone formation under various conditions. As a result, many articles and research reports on the subject have been published in the *Quarterly* (Anon., 1971; Keithley, 1971; Harris, 1971; Armstrong, 1972; Brady 1973; Williams, 1975; Williams, et al., 1976; Williams and Herdklotz, 1977; Helmick, Rohde and Ross, 1977; Amer, 1978; Cannell, 1978; Williams and Herdklotz, 1978; Williams, House and Herdklotz, 1981; Williams, 1987).

Most recently, a spirited exchange on the subject has been published (Wise, 1988; Williams, 1988). Helmick, Rohde and Ross, in April 1976, discovered numerous stalactites which had formed under a concrete bridge near Cedarville, Ohio (1977, pp. 13-7). The bridge had been built in 1941, and thus the stalactites had formed in no more than 35 years. From the size of the stalactites, they calculated that the minimum rate of growth was 0.53 cm^3 per year, considerably in excess of 0.164 cm^3 per year sometimes mentioned in the geological literature. They actually observed growth rates several times the minimum rate during some of the year. They refer to reports of growth rates of stalactites on the concrete roof of the Experimental Mine of the United States Bureau of Mines near Bruceton, Pennsylvania, up to 40 times the minimum average rate observed under the concrete bridge. They also relate the fact that the large stalagmite known as Crystal Spring Dome, in Carlsbad Cavern, has been reported to be growing at the rate of 2.5 in^3 (41.0 cm^3) per year, in spite of the present, dry New Mexico desert above. They calculate that at this rate, a 10,000 in^3 stalagmite, which would require 1,000,000 years to form at an average deposition rate of one in^3 per hundred years, could actually be formed in only 4000 years. Taking into account the possibility of even higher growth rates, they declare it is apparent that even the largest known dripstone could have formed in only a few thousand years. The observation of the relatively rapid rate of growth of the stalagmite in Carlsbad Cavern is especially important, since this involves growth rates under a natural cave environment from calcium carbonate, rather than from concrete, which contains a considerable amount of calcium hydroxide in addition to calcium carbonate. Calcium hydroxide is about 100 times more soluble in water than is calcium carbonate (but see below the discussion of this factor in the exchange between Wise and Williams).

E. B. Cannell (1978, pp. 9-11) reported rapid stalactite growth in two cement tunnels in a water treatment plant located on the Ottawa River in Quebec. The minimum growth rate, calculated on the basis of the date of construction of the tunnels and the date of discovery of the stalactites, and the volume of the largest stalactite, was 4.61 cm^3 per year, 28 times greater than the average of 0.164 cm^3 per year cited in geological literature. Although temperature ranges in the tunnels were approximately those in natural caves, Cannell did cite a number of conditions that are unlike those that are encountered under natural conditions that might affect rates of formations.

Amer (1978, pp. 9-11) reports on the discovery of stalactites in an abandoned tunnel that was formerly part of the London subway system. Some of the stalactites were two feet in length. London's underground railway system was completed in 1890. This would yield a growth rate of about 70 mm per year, which is considerably greater than that reported by Cannell for his stalactites.

Williams, Herdklotz, Mulfinger, Jonsonbaugh, and Pierce (1976, pp. 211-2) published the first in a series of four papers placed in the *Quarterly* concerning laboratory experiments on the rate of deposition of calcium carbonate from an aqueous solution. Their experimental apparatus was designed to simulate the solution of calcium carbonate as ground water seeps through limestone formations and then redeposits the calcium carbonate as stalactites, as the mineralized water drips from the roof of limestone caves.

In their experiments, they employed tap water plus carbon dioxide; tap water plus carbon dioxide plus 5% sodium chloride; and tap water plus carbon dioxide

plus 1% acetic acid. Normal surface water percolating through soil picks up carbon dioxide present in soil. The solution containing added sodium chloride is postulated to be similar to waters of the Flood that would have receded from the earth through recently consolidated limestone. The solution containing added acetic acid simulates a type of Flood water containing humic acid from the decay of organisms. The solutions containing sodium chloride and acetic acid dissolved four to five times as much calcium carbonate as did the water containing only carbon dioxide. The solution containing carbon dioxide and sodium chloride deposited almost twice as much dissolved calcium carbonate as did the solution containing only carbon dioxide. These investigators claimed their experimental results indicated that massive precipitation of calcium carbonate is possible under laboratory conditions. If their laboratory conditions approximate natural conditions that may have existed after the Flood, their results would also indicate, of course, that the formation of stalactites and stalagmites would have occurred much more rapidly than under present conditions.

E. L. Williams and R. L. Herdklotz (1977, pp. 192-9) published the second paper in the series. They cite reports by several investigators that establish the fact that water percolating through soil picks up relatively large quantities of carbon dioxide. They used apparatus similar to that described in the first paper and also a simpler apparatus. Their test solutions were similar to those in earlier experiments, and they also tested for the effect of temperature. For the experiment testing the effect of temperature, they employed water containing only carbon dioxide. The carbon dioxide-enriched water, warmed to about 45°C, dissolved the limestone, and redeposited the limestone as it dripped from the apparatus. The deposition is not due to evaporation, but is due to the loss from solution of carbon dioxide. The solubility of calcium carbonate is regulated by the relationship

$$CaCO_3 + H_2O + CO_2 \rightleftharpoons Ca^{++} + 2HCO_3^-$$

Addition of carbon dioxide shifts the reaction to the right, dissolving calcium carbonate and forming the much more soluble calcium bicarbonate, while decomposition of calcium bicarbonate with the loss of carbon dioxide from solution shifts the reaction to the left, with formation of the much less soluble calcium carbonate, resulting in its deposition. The higher temperature drives off carbon dioxide from solution, and shifts the reaction to the left, with deposition of calcium carbonate. The experiment was very successful, with large amounts of calcium carbonate being deposited on the strings employed in their apparatus, similar to what is found in natural stalactites.

Based on the rates of deposition of calcium carbonate they obtained under various conditions—5% sodium chloride solution, plus carbon dioxide at 25°C; water, plus carbon dioxide at 45°C; water, plus carbon dioxide, with the temperature raising from 8°C to 25°C—a very rapid rate of calcium carbonate deposition was indicated.

Williams and Herdklotz, postulating conditions that could reasonably be assumed to have existed at the time the Flood waters would have been receding, made an attempt to calculate the rate at which caves could form in limestone deposits. Under ordinary conditions, if 15% of 40 inches of rain per year were available for limestone solution, their calculations indicated that in one year, a cave of 3 ft. x 6 ft. cross section x 120 ft. long would be formed per square mile of surface. Of course, during the waning stages of the Flood, quantities of water vastly in excess of that would have been available for dissolution of calcium carbonate and consequent cave formation.

The third paper in the series was also published by Williams and Herdklotz (1978, pp. 88-91) who attempted to produce calcium carbonate dripstone under laboratory conditions which included water charged with carbon dioxide dripping in an atmosphere of 100% humidity. No dripstone formed. It has been suggested that decomposition of proteins and other nitrogen-containing substances would produce ammonia and other amines. To test this effect, an experiment was conducted with carbon dioxide-charged water in which ammonia was admitted into the apparatus. Even under excessively humid conditions, some calcium carbonate did precipitate. Thus it appears that even under very humid conditions, with ammonia present in the atmosphere the precipitation and subsequent slow growth of dripstone is possible.

In order to determine whether some of the dripstone which was produced from dolomite (which contains both calcium and magnesium) was formed by evaporation as well as by precipitation due to loss of carbon dioxide (as happens when true dripstone forms), a sample of the dripstone produced in the laboratory at 45°C was titrated in solution with EDTA (ethylene diamine tetraacetate). This revealed that all of the deposit was calcium carbonate, indicating that none had formed by evaporation. If some of the dripstone had formed by evaporation, the deposit would contain both calcium and magnesium carbonates.

Williams and Herdklotz, in this report, cited statements by uniformitarian geologists, cautioning against claims that the time span required to form stalactites and stalagmites can be estimated using rates of formation under present conditions. They quoted James H. Gardner (1935, p. 1270):

> The rate at which dripstone forms is a variable factor, due to changing circumstances; it depends on the amount of seepage water, the quantity of carbonate in solution, and the rate of precipitation. It is a common practice to attempt to fix the age of dripstone by the rate at which it forms, but this is plainly a valueless calculation. It invariably results in the fixing of the age of a stalactite or stalagmite in proportion to its size; the largest will be the oldest and the smallest the youngest. For example, in Carlsbad Cavern at the present time, the management maintains a large sign on an immense stalagmite, stating that it is estimated to have an age of 60 million years. Guides give the information that the calculation is based on the rate of so many cubic inches per year at which such dripstone formed. The writer believes that such signs should be removed by the National Park Service as being misleading to the public.

In quoting Gardner and others, creationists do not imply that they necessarily agree with creationists that

these stalactites and stalagmites did form in just a few thousand years, and, of course, creationists acknowledge that neither laboratory nor field work should be used to make claims concerning the age of these dripstones. Laboratory experiments and investigations in the field by creationists may be used, however, to indicate that it is possible that these dripstones could have formed much more rapidly than is usually acknowledged.

The fourth paper in this series was published by Williams, House, and Herdklotz (1981, pp. 205-8, 226). In these experiments, they found that there was a lag time of about 400 hours before dripstone began to form. They suggested that this lag may be due to the time necessary to allow the removal of carbon dioxide from solution, or it may be due to the time necessary to supersaturate the solution with calcium carbonate before solid nuclei of the precipitating compound will become stable. They also tested for the effect of drip time. They found that a time between drops (in seconds) of 43 and 90 yielded a bit over 0.05 grams per string, a time of 125 gave 0.132 grams, and a time of 215 gave 0.108 grams per string. They postulate that fast drop formation is a deterrant to precipitation, because the "dwell time" of the drop on the string is not sufficient to allow the release of carbon dioxide so that calcium carbonate can precipitate, while excessive "dwell time" may cause slow monocrystalline growth rather than rapid polycrystalline growth that occurs with somewhat faster moving drops.

They concluded that their results indicate that pressure loss in dripping water, in which calcium carbonate and carbon dioxide are dissolved, can produce rapid precipitation of calcium carbonate under laboratory conditions. The rate of precipitation is dependent on a number of factors, including pressure drop, chemical composition differences in solution and atmosphere, drip rate, and temperature differences. These experiments lead to the conclusion that large masses of calcium carbonate can be deposited rapidly, under proper conditions.

In an appendix Williams and his co-workers quote a report from a newsletter of a caving club (Trout, 1975).

> The trip really became interesting when we came to the area just above the rubble slope which leads to the "Rattlesnake Room." The new growth was simply unbelievable. All who were familiar with the cave were engaged in a "come over here and see what is new" contest.
>
> The real shock came when someone pointed out the new growth behind the "Bat Burial" formation. Three new stalactites had grown and the longest was some longer than 12". The time since the last photo was taken of this wall was just over 3 months ago so the growth rate of the largest stalactite would be approximately 4" per month or 1 inch every 7.5 days. Unbelievable? Yes! In fact, if any caver believes this without seeing for himself it would surprise me. Luckily though we have been photographing the same spot for 15 years and have all the photos with dates.

Depositional Interbedding and Time Frames in the Grand Canyon

The Grand Canyon and theories concerning its formation have long inspired interest by geologists, evolutionists and creationists alike. Evolutionary geologists have expressed increasing frustration at attempts to explain its formation. Evolutionary geologists believe that the area encompassing much of the Canyon was uplifted 65 million years ago, but that the Colorado River which flows through it did not originate until about four million years ago. It is obvious that if these assumptions are correct, the Colorado River could not have cut the Grand Canyon. If a newly flowing river encountered an uplifted area, it would never climb up over it and subsequently cut a canyon—it would simply flow around it. In the museum on the south rim of the Canyon is a description of several geological theories on the formation of the Canyon, followed by an admission that all of these theories have serious faults. The Havasupai Indian account of the formation of the Grand Canyon is then given. According to these Indians who live in one of the offshoots of the Canyon, the Grand Canyon formed during a great world-wide flood. Much physical evidence supports this belief.

William Waisgerber, a consulting geologist and President of William Waisgerber and Associates, Consulting Geologists; George Howe, Director of the CRS Grand Canyon Experiment Station and Chairman and Professor, Division of Natural Science and Mathematics, The Master's College; and Dr. Emmett Williams (1987, pp.160-7) reported on two field trips to the Grand Canyon to study the alleged unconformity between the Mississippian Redwall Limestone and the Cambrian Muav Limestone along the North Kaibab Trail. Evolutionary and other uniformitarian geologists believe that there exists a 200 million-year time gap between the top of the Cambrian Muav Limestone and the base of the Mississippian Redwall Limestone, since intervening Ordovician, Silurian, and Devonian rocks are absent. Clifford Burdick, a consulting geologist who had made an earlier study of the contact between the Cambrian Muav and the Mississippian Redwall, reported that he had found evidence of intertonguing between these two formations, contradicting the notion that 200 million years had intervened between the deposition of the Cambrian Muav and the Mississippian Redwall. Waisgerber and his colleagues, with support from the CRS Research Committee, formed a field team to reinvestigate the area studied by Burdick.

Waisgerber and his colleagues confirmed Burdick's observations concerning interbedding of the Cambrian Muav and the Mississippian Redwall. Along the North Kaibab Trail is a sign erected by the National Park Service identifying the contact between the Redwall Limestone and the Muav Limestone. The CRS team reports that commencing from an area about 100 yards north of the sign to about 100 yards south of the sign, all beds apparently interfinger with one another. They determined that yellowish appearing micaceous shales were the uppermost Cambrian Muav Limestone. Immediately above these shales were typically reddish-colored Mississippian Redwall Limestone beds. Any attempt to trace individual beds laterally, southerly or northerly along the North Kaibab Trail, however, resulted in a reverse stratigraphic relationship. Suppo-

sedly, older Muav Formation yellowish beds rested on allegedly younger reddish-stained Redwall limestone beds. Lateral and vertical facies changes within both formations indicate the absence of unconformable relationships between the Redwall Limestone and the Muav Limestone. In other words, where allegedly older Cambrian Muav Limestone rests on allegedly younger Mississippian Redwall Limestone, the contact is a true sedimentary contact and thus the Muav Limestone was deposited on top of the Redwall Limestone. The evidence contradicts the notion that here, where "older" strata (older by 200 million years!) rests on "younger" strata, the inversion was caused by overthrusting or other geologic events.

Waisgerber and colleagues searched an area 50 feet above and below the contact line between the Muav Limestone and Redwall Limestone for physical evidences of the supposed 200 million-year hiatus between these two formations. They point out that such evidences would include 1) obvious, pronounced erosional features incised into the highest of Muav Limestone beds; 2) basal Redwall Limestone beds exhibiting boulders and cobbles of eroded Muav Limestone beds; 3) Muav Limestone beds dipping somewhat more steeply than overlying Redwall Limestone beds; 4) Muav Limestone beds being somewhat more folded than Redwall Limestone beds; 5) more complex joint systems in the Muav than in the Redwall; 6) more faulting in the Muav than in the Redwall, and particularly, 7) a decidedly different lithology within each of the formations, due to supposed changing regional environments. None of these features was seen. All of the beds were seen to be homoclinal, each bed resting directly on another bed with no known structural deviation. Joint planes commencing in alleged Muav Limestone beds seemingly intersected Redwall Limestone similarly. There were no notches and grooves (which would be evidence of a time gap, the time required for the underlying strata to be incised by erosion) in the underlying Cambrian Muav Limestone filled in by material from the Mississippian Redwall Formation, as should be the case if there were a huge time gap between the laying down of these two formations. The evidence clearly indicates that the Mississippian Redwall Limestone was laid down conformably on the Cambrian Muav Limestone with no time gap in between.

The authors of the paper cite the publications of several uniformitarian geologists which also indicate the difficulty in identifying evidences for an unconformity between the Muav and Redwall Limestones. Their paper also contains citations from the geological literature in which the authors admit the difficulty in documenting other alleged unconformities in the Grand Canyon. Waisgerber, Howe and Williams close their paper with the following conclusions:

1. The unconformity supposedly separating the Redwall Limestone from the underlying Muav Limestone does not exist. Consequently there cannot be any 200 million-year hiatus.
2. Since the 200 million-year hiatus cannot exist, the dating of Redwall Limestone and Muav Limestone as Mississippian and Cambrian with their supposed ages, respectively, cannot be valid.
3. Because the Paleozoic time periods cannot be valid, then the longer time unit known as the Paleozoic Era cannot be real.
4. Since the Paleozoic Era cannot be a real geologic time unit, historical geologic time must be suspect.
5. Because historical geology is suspect, the mega-evolutionary model cannot be confirmed by historical geology because there is no true definition of geologic time.
6. Since the evolution model cannot be sustained historically, it behooves all scientists to search for alternative models as regards the origin of the earth, the origin of life on earth, and the time necessary to effect such origins.
7. The various formations within the Grand Canyon area could have been deposited one formation on another, without the need for millions of years of depositional time and millions of years of unaccountable time (hiatuses).

Precipitation Brought About by Mixing Brines

The existence of extensive beds of rock salt (sodium chloride), gypsum ($CaSO_4.2H_2O$) and anhydrite ($CaSO_4$) has long been considered by uniformitarian geologists to be evidence for evaporation, over tens of thousands or millions of years, of shallow seas on inland lakes. Thus, these beds are commonly referred to as evaporites. Many of these deposits are massive. Some salt domes are described as having salt cores that have a roughly circular or oval horizontal section 1,000 feet to two miles or so in diameter. The core may extend downward for several thousand feet. It is believed that there are plugs in Europe extending downward 15,000-20,000 feet. Since it requires evaporation of 8,000 feet of sea water to produce 100 feet of salt, it would require an unbelievable amount of evaporation to produce several thousand feet of salt and of course the sea floor would have to continually subside at just the right rate to maintain the existence of the sea.

In recent times, geologists have recognized the many difficulties in the evaporite scenario and have sought other explanations for the formation of these extensive salt beds. One of the more recent suggestions has been that these salt beds formed when brines were intruded into the ocean from openings in the sea floor (Nutting, 1984). Thus, vast time spans would not be required for the formation for these so-called evaporites, or salt formations. It has been suggested that the mixing of different kinds of brines, say of sodium chloride and magnesium chloride, each originally saturated, might cause precipitation of one or both of the salts. Omer B. Raup has conducted some experiments that have shown that much salt is precipitated when brines are mixed. The precipitation took place without any evaporation of water or change of temperature.

F. L. Wilcox and S. T. Davidson (1976, pp. 87-9) thought it worthwhile to repeat some of Raup's work and to carry the work further and they have reported the results of their experiments sponsored by the CRS Research Committee. They used saturated solutions of sodium chloride (NaCl) and of magnesium chloride ($MgCl_2$). Mixing of the brines caused precipitation of NaCl. They found that the greatest amount of NaCl precipitated, expressed as percent of the total NaCl initially present in the mixed brines, was obtained when the volume percent of the NaCl brine was about

20% (that is, when the brines mixed consisted of 20 ml of the saturated NaCl solution and 80 ml of the saturated $MgCl_2$ solution, or comparable amounts). They postulate that when the two solutions are mixed, the $MgCl_2$ tends to attract water molecules from NaCl. As the number of water molecules available to NaCl decreases, the NaCl begins to precipitate from solution. They suggested future experiments employing subsaturated solutions and about 25 volume percentage NaCl solution and testing the effect of temperature.

Investigation of an Elliptical Formation in the Tendurek Mountains of Turkey

William H. Shea (1976, pp. 91-5) described an elliptical, boat-shaped object in the Tendurek Mountains about 30 miles southwest of Mount Ararat in Turkey. This object was brought to public attention in 1959. Captain Sevket Kurtis had taken photos in the vicinity of the Tendurek mountains and he brought the photos with him when he came to Ohio State University to do advanced work in connection with aerial surveying. It was reported that Captain Ilhan Duripinar had discovered the object on one of the photos while using a stereoplanograph in preparing maps. The picture was published in several newspapers and magazines, along with speculations about the Ark. Shea did not visit the site but his discussion was based on an examination of the photo and a report by a group that visited the site in 1960. They found no archeological evidence of the Ark and no human artifacts. Shea speculates that possibly this is the site where the Ark landed (the site is at an elevation of 6,000 feet) but that the Ark itself was destroyed by fire due to hot lava which contacted the boat.

Clifford L. Burdick, (1976, pp. 96-8) visited the site of this object in 1973. He reports that the object is only a geological and tectonic phenomenon. That year Burdick was a member of a team that was on Mount Ararat searching for the Ark. In the course of events, he met the commanding general at Dogubayaset, a city near Mount Ararat. The general claimed he could take Burdick to the site of the object for which they were searching, the Ark of Noah. Burdick was escorted to the Tendurek Mountains and to the site of the boat-shaped object reported in 1959.

According to Burdick's observation, a small fault or fracture of about 500 feet occurred along a stream bed. Apparently a granitic or rhyolitic type of intrusive lava had pushed up through clay along the center of the formation, making an elevated ridge along the center. Possibly as the molten or plastic rock rose through the clay bed of the wash, it raised the hardened clay with it. Burdick reports that the hardened clay did resemble the sides of a ship, and from a distance might be interpreted as such. Burdick's observations convinced him that this object could not possibly have any relevance to the Ark.

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INVITED PAPER

DWINDLING RESOURCE EVIDENCE OF A YOUNG EARTH

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Abstract

The best evidence for a young earth is the dwindling energy field of the earth's dipole magnet. Humphreys (1988) has no physical model for the reversed directions in rock magnetization he proposes. The state of the earth's dipole magnet cannot be evaluated from rock magnetization data.

Introduction

This article is a reply to D. Russell Humphreys' article (1988) "Has the Earth's Magnetic Field Ever Flipped?". My concern with his article is that it may lead one to believe that there is not much difference in the scientific approach available to the creationist and evolutionist on the earth-age problem.

This article makes a sharper distinction between the two approaches and chooses a battle ground where that distinction is more evident. A particular case is given where its superiority is illustrated by evaluating the electrical conductivity and heating in the earth's core.

The Dwindling Resource

The best physical evidence that the earth is young is a dwindling resource that evolutionists refuse to admit is dwindling. To admit that it is dwindling is tantamount to admitting that the earth is young. To deny that it is a dwindling resource is phony physics. The physics of this dwindling phenomenon is vastly superior to anything the evolutionists, theistic or secular, have to offer. The dwindling phenomenon can only have a young-age solution for the earth. This physics of the young earth is sufficient to refute the whole gamut of evolution. Whether they know it or not, evolutionists are trapped between the horns of a dilemma: a young earth or faith in phony physics.

The dwindling resource is the *magnetic energy* in the field of the earth's dipole magnet. This magnet is a huge electromagnet in the core of the earth. Its present value of current is about six billion amperes. The sole source of energy to drive its electric current is the magnetic energy in its field. The present value of that magnetic energy is 2.5×10^{19} joules, that is all that remains of the magnetic energy it had at the time of creation (Barnes, 1983).

Physics of the Earth's Magnet

In accordance with the laws of electricity and magnetism and the first and second laws of thermodynamics, the flow of energy is as follows: Magnetic energy in the field is being transformed into electric current energy; the electric current energy in turn is being transformed into heat which is an ohmic loss of energy that is nonrecoverable.

A rigorous theoretical physics solution for this dwindling phenomenon is given in the author's paper (1973) "Electromagnetics of the Earth's Field and Evaluation of Electrical Conductivity, Current, and Joule Heating in the Earth's Core." The evolutionists have no such definitive means of evaluating these core properties, without accepting this decay property.

Historic Evaluations of the Earth's Magnet

The earth's magnet is a *dipole magnet*, having a north pole and a south pole. To evaluate the state of this magnet one must evaluate its *magnet moment*, a vector from which one can compute the strength and direction of its field at every point in its field.

Due to the fact that there are billions upon billions of other magnets and magnetic disturbances, it is extremely difficult to evaluate the magnetic moment of the earth's magnet. The first scientist to do it was Carl Friedrich Gauss. He invented the instrument needed, the magnetometer, obtained field measurements over an extensive portion of the globe and developed the mathematics to reduce these data. It took five years to collect and reduce the data for this one evaluation of the earths dipole magnetic moment for the year of 1835.

It has been similarly evaluated many times since then, confirming the theoretical physics solution which predicts this dwindling phenomenon. When the phenomenon is extrapolated backward in time, it confirms a young earth age, because of the physical bounds on its original value.

What About Reversed Directions of Rock Magnetization?

It is erroneous to imply that the state of the earth's dipole magnet, the value and direction of its magnetic moment, can be evaluated from rock magnetization data. Humphreys may leave the impression that it can, when he states: "Barnes' criteria are more stringent than what is needed." Since Gauss used those criteria, I prefer to give credit to Gauss. No one has ever made a valid evaluation of the magnetic moment of the earth's dipole magnet from rock magnetization.

Humphreys has no valid physical theory for reversal of the earth's dipole magnet, whether it be associated with rock magnetization or any other data. If he did, the evolutionists would surely be interested in it, because they have yet to find one.

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