

Creation Research Society Quarterly

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Haec credimus:

For in six days the Lord made heaven and earth, the sea, and
all that in them is, and rested on the seventh. — Exodus 20:11

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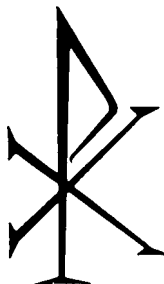
MARCH 1989

NUMBER 4

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Instructions to authors can be found in June Quarterly.

Send all manuscripts to Dr. D. B. DeYoung, Grace College, 200 Seminary Drive, Winona Lake, IN 46590.

Cover Illustration

A scrub pine tree root system exposed by the receding water level of Lake Allatoona in Georgia (Autumn 1987). The tap root of the pine is seen exposed below the trunk of the tree. Fibrous-like roots spread out laterally stabilizing the tree and providing water for plant functions. Judging from the photograph, the tap root bears the main support for the tree.

If the soil is appropriate, certain species of plants will produce one main "tap root" that grows more than any of its branch roots. If the same plant lives in a shallow soil where tap root formation is not possible, it will produce a branched root system in which there is no tap root but many branch roots grow laterally to considerable distance. Some plants are genetically programmed to produce tap roots in nearly all circumstances, e.g., carrots. Likewise many grasses (e.g., corn) never produce tap roots but form a branching system that botanists refer to as a "fibrous" root system. Thus by both genetic and physiological means, the Creator has preadapted plant roots for optimum survival under substrate conditions.

Photograph by Emmett L. Williams

Caption by George F. Howe and Emmett L. Williams

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Errata

CRSQ 25:85

Equation (2) should be $v^{\infty} c(T)$

CRSQ 25:145, column two, quote citation should read
Anon. e, 1977, pp. 170-2.

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CREATION RESEARCH SOCIETY

History The Creation Research Society was first organized in 1963, with Dr. Walter E. Lammerts as first president and editor of a quarterly publication. Initially started as an informal committee of 10 scientists, it has grown rapidly, evidently filling a real need for an association devoted to research and publication in the field of scientific creation, with a current membership of over 600 voting members (with graduate degrees in science) and over 1100 non-voting members. The *Creation Research Society Quarterly* has been gradually enlarged and improved and now is recognized as the outstanding publication in the field.

Activities The society is solely a research and publication society. It does not hold meetings or engage in other promotional activities, and has no affiliation with any other scientific or religious organizations. Its members conduct research on problems related to its purposes, and a research fund is maintained to assist in such projects. Contributions to the research fund for these purposes are tax deductible. The Society operates two Experiment Stations, the Grand Canyon Experiment Station in Paulden, Arizona and the Grasslands Experiment Station in Weatherford, Oklahoma.

Membership Voting membership is limited to scientists having at least an earned graduate degree in a natural or applied science. Dues are \$17.00 (\$21.00 foreign) per year and may be sent to Glen W. Wolfrom, Membership Secretary, P.O. Box 14016, Terre Haute, IN 47803. Sustaining membership for those who do not meet the criteria for voting membership, and yet who subscribe to the statement of belief, is available at \$17.00 (\$21.00 foreign) per year and includes a subscription to the Quarterlies. All others interested in receiving copies of all these publications may do so at the rate of the subscription price for all issues for one year: \$20.00 (\$24.00 foreign).

Statement of Belief Members of the Creation Research Society, which include research scientists representing various fields of successful scientific accomplishment, are committed to full belief in the Biblical record of creation and early history, and thus to a concept of dynamic special creation (as opposed to evolution), both of the universe and the earth with its complexity of living forms. We propose to re-evaluate science from this viewpoint, and since 1964 have published a quarterly of research articles in this field. In 1970 the Society published a textbook, *Biology: A Search for Order in Complexity*, through Zondervan Publishing House, Grand Rapids, Michigan 49506. All members of the Society subscribe to the following statement of belief:

1. The Bible is the written Word of God, and because it is inspired throughout, all its assertions are historically and scientifically true in all the original autographs. To the student of nature this means that the account of origins in Genesis is a factual presentation of simple historical truths.

2. All basic types of living things, including man, were made by direct creative acts of God during the Creation Week described in Genesis. Whatever biological changes have occurred since Creation Week have accomplished only changes within the original created kinds.

3. The Great Flood described in Genesis, commonly referred to as the Noachian Flood, was an historic event worldwide in its extent and effect.

4. We are an organization of Christian men and women of science who accept Jesus Christ as our Lord and Saviour. The account of the special creation of Adam and Eve as one man and woman and their subsequent fall into sin is the basis for our belief in the necessity of a Saviour for all mankind. Therefore, salvation can come only through accepting Jesus Christ as our Saviour.

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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**

Received 6 August 1988 Revised 25 September 1988

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.

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

*See Gish (1975).

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^aAvailable from CRS Books, 5093 Williamsport Drive, Norcross, GA 30092 for \$2.00 prepaid and postpaid.

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 presumably older formation came to rest on top of the presumably 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. Doherty, 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 Herdtklotz, 1977; Helmick, Rohde and Ross, 1977; Amer, 1978; Cannell, 1978; Williams and Herdtklotz, 1978; Williams, House and Herdtklotz, 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 $10,000 \text{ in}^3$ 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, Herdtklotz, 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. Herdtklotz (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



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 Herdtklotz, 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 Herdtklotz (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 Herdtklotz, 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 Herdtklotz (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 deterrent 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 ($\text{CaSO}_4 \cdot 2\text{H}_2\text{O}$) 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 MgCl₂ solution, or comparable amounts). They postulate that when the two solutions are mixed, the MgCl₂ 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 Sevkert 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.

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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 earth's 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.

A careful reading of his paper will show that he does not actually claim that the total dipole magnet reversed. But what he does claim needs to be challenged. His “evidence” is difference-in-direction of magnetization in rocks on the earth. There is much difference in having reversed magnetization in rocks here and proving a “flip” in the dipole magnet, or even a part of it. That magnet is more than a thousand miles away.

In his argument he refers to *magnetic field*. That is not synonymous with *dipole magnetic field*. Magnetic field includes the “noise” from extraneous sources. Whereas the dipole magnetic field is the ideal field with the “noise” removed, by Gauss’ procedure. To have a reversal, the axis of the dipole magnet must reverse.

There is no conclusive evidence that the axis of the dipole magnet has ever reversed. There is evidence, from the historic evaluations of the dipole moment, that the axis of this dipole magnet has been precessing westward.

Stanley F. Stanulonis, Jr. (1974) has shown this precession to be due to solar wind—dipole magnetic field interaction. He carried his research further to evaluate the charge density in the earth’s core. Here is an illustration of a physical cause and effect based on good physics and magnetic moment data. Nothing of this caliber has been advanced to prove reversals of the axis of the dipole magnet.

In support of his reversal theory, Humphreys refers to a figure, in my book *Origin and Destiny of the Earth’s Magnetic Field*, which I used to discredit the “rock” magnetization data. It is reprinted here as Figure 1 for reference. This figure has only the most recent 160 years of archeomagnetic data (the bottom curve) from a Russian paper’s 8,000 years of data. This figure also contains, for comparison, a plot of the earth’s magnetic moment data, of the Gaussian type (the upper curve), which is the scientifically valid one. Let the reader decide where the credibility lies.

*Available from CRS Books—see inside back cover.

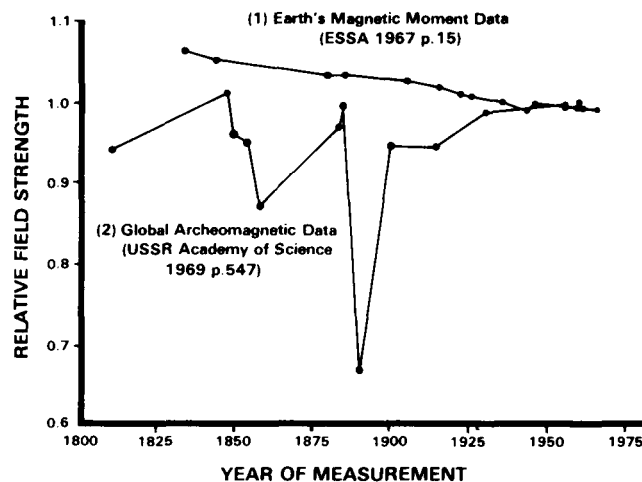


Figure 1. The most recent 160 years of archeomagnetic data (bottom curve). Also see CRSQ 21:109-13.

It may be commendable in Humphreys to associate reversed directions in rock magnetization with the Biblical Flood, a catastrophic event that certainly changed the face of this earth. But like so many other problems that have never been solved, it would be more convincing if there were a physical model from which one could derive a physics solution.

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RADIOACTIVE HALOS: GEOLOGICAL CONCERNS

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Abstract

The geology involved in the polonium halo research is examined. Since there is a lack of locality and specimen information, the geology associated with the presence of polonium halos is incompletely understood. A preliminary examination of this geology casts doubt on the explanatory power of Gentry’s model. Further research into the geology of halos is necessary.

Introduction

Robert Gentry has presented polonium halos as evidence for the rapid formation of the earth’s crystalline rocks—at a rate that is too great to allow for the operation of currently understood natural laws and processes. Gentry (1984, 1986, 1987a) has also produced an explanatory model—not only for the origin of the polonium halos, but also for the granites in which they were supposedly found, and even for the earth itself.

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According to Gentry’s theory for the origin of polonium halos, God created the earth’s primordial rocks, including granites and contained polonium, sometime during a singularity in the creation week about 6,000 years ago (Gentry, 1987a, pp. 97-8, 104). The theory also posits that God created granites and large biotite crystals almost instantaneously (Gentry, 1987a, p. 97) in forms that cannot be reproduced by man or natural process (Gentry, 1987a, p. 104). I call this theory Gentry’s “special creation-week theory” for polonium halos. It is a “special” theory in that it requires

that created rocks *cannot* be reproduced, and a "creation-week" theory because the origin of the rocks is thought to have occurred during the creation week. Many variations of this theory can be imagined. A "general singularity theory" for polonium halos might simply posit that God created the rocks containing polonium halos in less than 1/100 of a second. This theory neither requires that it happened during the creation week, nor that the created rocks are non-reproducible. It is thus a singularity theory rather than a creation-week theory, and a "general" rather than a special theory. Another variation is what might be called the "general creation-week theory" which would require that the creation of the rocks occurred during the creation week, but not necessarily in non-reproducible forms. Gentry's "special creation-week theory" is one type of "general creation-week theory," which in turn is one type of "general singularity theory" for the origin of the polonium halos (Figure 1). The purpose of this paper is to examine Gentry's model and each of the above-mentioned variations to determine if any such non-naturalistic theory for the origin of the polonium halos is sufficiently justified by the physical data.

Geological Problems

Gentry's model depends heavily upon a proper understanding of the geologic setting of his polonium halos. However, Gentry has made the understanding of the geology very difficult. As Wilkerson (1987) points out, Gentry has provided a very incomplete and imprecise list of localities from which his polonium halos were taken. This neither allows us to fully understand the geology of the sites, nor to reproduce his data by collecting more samples. Furthermore, he has neither provided a single museum catalogue number, nor even the name of the museum(s) into which he deposited his slides and specimens. According to the acknowledgements on several papers (Gentry, 1967, 1968, 1970, 1971; Gentry, *et al.*, 1976; Gentry, *et al.*, 1974) many of his specimens were borrowed from museums. Without a list of museum specimens, the geology of the radioactive halos cannot be investigated.

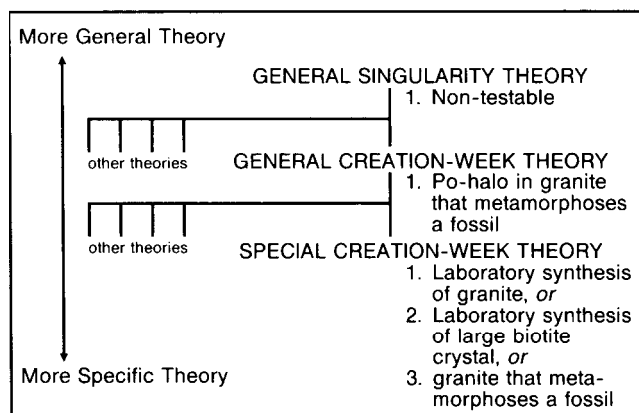


Figure 1. The relationships among the Three Different Non-naturalistic Theories for the Origin of Gentry's Polonium Halos.

The more general theories are displayed higher and the more specific lower. Listed beneath each of the three theories are those physical observations that would falsify that particular theory and any of those beneath it, but *not* above it.

Attempting to improve our picture of the geology of Gentry's polonium halos, I have searched his papers for locality information. Table I lists the result, along with the references. Table I also includes references to the geology of some of the areas. These references are the result of a preliminary investigation. A better understanding of the geology of the sites must await a complete locality and specimen list.

Table II summarizes some of the geology of the localities listed in Table I. Although the mineral species in which the halos were found is always given (column 2, Table II), the radiohalo papers do not always identify the rock type from which the minerals were taken. As a result the identity of many of these rocks (column 3, Table II) remains uncertain. In some cases the geologic reports mention that only a single class of rocks is found in the area which could provide the mineral species. In such cases the designation is reasonably certain. In other cases the reports mention that more than one likely rock type is found in the area. In these cases, the identity of the rock is very uncertain. It must be noted that all such uncertainty could be eliminated by a petrographic examination of the rocks from which Gentry made his slides. Once museum specimen numbers are supplied such a study of Gentry's specimens should be performed.

A study of the geology of the polonium halos raises some disturbing issues. Firstly, Gentry (1987a, p. 97) claims that the polonium halos were always found in granites. This is not true (column 3, Table II). Most of the rocks from which polonium halos have been taken are actually granitic pegmatites. Granitic pegmatites have the mineral composition of granite, but not the texture of granite. Rhyolite is another rock with a granitic composition but not granitic texture. Yet, Gentry feels that rhyolites are not granites (Gentry 1986, p. 130). Rhyolites *have* been reproduced in the laboratory, so to accept rhyolites as granite would falsify his "special theory" of polonium halos. If the rocks identified as pegmatites in Table II are truly found to be pegmatites, Gentry's "special theory" will have to be modified to include granitic pegmatites as well as granites (and somehow *not* include rhyolites at the same time).

As Wakefield (1988a) and Wilkerson (in press) have shown, both the Fission and Silver Crater Mines are dug into calcium vein-dikes—the former primarily of calcium and fluorite, and the latter of calcium and biotite. These rocks are neither granitic in composition, nor granitic in texture. If this identification is verified, Gentry's theory should be modified to include some calcite vein-dikes. Finally, the South India halos were supposedly found in gneiss, a rock considered to have been altered by heat and/or pressure since formation. Since some gneisses are thought to have been formed from granite, one might ask if it is not possible that the polonium was created in granite and the granite metamorphosed into gneiss. However, such metamorphism would most likely destroy the halos, as Gentry thinks it destroyed the halos on the moon (Gentry, 1987b, p. 104). Furthermore, the halos themselves are found in the mineral cordierite. Cordierite is not produced in conventionally-defined granite, but rather is produced under the conditions that caused the metamorphism itself. There is then little doubt that the

Table I: A List of Localities from Which Polonium Halos Have Been Reported and References to the Geology of the Areas.

- 1) India (Gentry, 1971, p. 729)—actually Southern India, Mahadevan (1927, p. 445 [emanation halo]) locality. Geology: Krishnan, 1968.
- 2) Ireland (Gentry, 1968, p. 1229) Gentry's reference does not mention any Ireland localities. Possibly locality #3.
- 3) Co. Carlow, Leinster Province, Ireland (Joly, 1917, p. 458 [emanation halo])—probably at Ballyellen (Joly, 1923, p. 682). Geology: Stilhnan and Holland: 1981.
- 4) Japan (Gentry, 1973, pp. 358-9)—actually Ishigure District, Japan, Iimori and Yoshimura (1926, p.19 [Z₁ halo]) locality.
- 5) On river, near Woelsendorf. Bavaria, West Germany (Schilling, 1926, p. 241; Gentry, 1973, p. 355). Geology: Wakefield, 1988b.
- 6) Scandinavia (Gentry, 1971, Figure 1A; Gentry, *et al.*, 1973, p. 283). Possibly one or more of localities 8 thru 12.
- 7) Norway (Gentry, 1968, pp. 1228, 1230, Figure 1 left). Possibly one or more localities 8 thru 11.
- 8) Iveland District, Aust-Agder Co., Norway (Gentry, 1971, note 9; Gentry, *et al.*, 1974, p. 566). Geology: Bjørlykke, 1935.
- 9) Eroland, Aust-Agder Co., Norway (Meier and Hecker, 1976, p. 186). Geology: Barth and Dons, 1960.
- 10) Moss, Ostfold Co., Norway (Meier and Hecker, 1976, p. 186). Geology: Barth and Dons, 1960.
- 11) Kragero, Telemarck Co., Norway (Meier and Hecker, 1976, p. 186, figures 4, 8, 10). Geology: Kofseth, 1942.
- 12) Ytterby, Sweden (Gentry, 1966, p. 453 [Type G halo]; 1973, p. 358 [may be ²¹⁰Po halo]).
- 13) Mount Apatite, Androscoggin Co., Maine (Henderson and Sparks, 1939, p. 240 [Type A halo]). Geology: Fisher and Barrell, 1934.
- 14) Portland, Middlesex Co., Connecticut (Henderson and Sparks, 1939, p. 241 [Type C halo]). Geology: Stugard, 1958.
- 15) Cottman Street Quarry, Philadelphia, Philadelphia Co., Pennsylvania (Henderson and Sparks, 1939, p. 241 [Type B and Type C halos]). Geology: Rose, 1970.
- 16) La Malbaie (Murray Bay), Gaspé Co., Quebec, Canada (Henderson and Sparks, 1939, pp. 240, 241 [Type A and Type C halos]). Geology: Alcock, 1935.
- 17) Dingwall, Victoria Co., Cape Breton Island, Nova Scotia, Canada (Henderson and Sparks, 1939, p. 240 [Type A halo]). Geology: Neale, 1955.
- 18) Huron Claim, Manitoba, Canada (Henderson and Sparks, 1939, p. 240 [Type A halo]). Geology: DeLury and Ellsworth, 1931.
- 19) Star Lake, Manitoba, Canada (Henderson and Sparks, 1939, pp. 240, 241 [Type C halo]). Geology: Davies, 1952; Lang, 1952.
- 20) Wilberforce, Haliburton Co., Ontario, Canada (Henderson and Sparks, 1939, pp. 240, 241 [Type A and Type B halos]). Probably the Fission Mine, since Fission bought out Wilberforce in 1947. Geology: Wilkerson, in press; Rowe, 1952.
- 21) Weissman Mine, Dill Township, Sudbury District, Ontario, Canada (Henderson and Sparks, 1939, pp. 240, 241 [Type A and Type C halos]). Geology: Ellesworth, 1932.
- 22) Cheddar, Hastings Co., Ontario, Canada (Henderson and Sparks, 1939, p. 241 [Type C halo]). Geology: Hewitt, 1957; Slack, 1949.
- 23) Faraday Mine, Faraday Township, Hastings Co., Ontario, Canada (Gentry, 1971, p. 728, figures 1B, 1C). Geology: Lang, Griffith and Steacy, 1962; Wakefield, 1988a; Wilkerson, in press.
- 24) Silver Crater Mine, Faraday Township, Hastings Co., Ontario, Canada (Gentry, *et al.*, 1974:564). Satterly and Hewitt, 1955; Wakefield, 1988a; Wilkerson, in press.
- 25) Conway Granite, New Hampshire (Gentry, personal communication, August 1986).

The underlined name in each is the abbreviated form of the locality used in the text and Table II. General localities (e.g. Scandinavia) are not otherwise discussed if polonium halos are known from more specific localities within that region (e.g. Kragero Norway, within Scandinavia). References in parentheses are those pages and figures that indicate polonium halos were found in that locality. If the reference uses an abandoned name, that name is indicated in brackets.

South India polonium halos were *not* formed in granite, but rather in gneiss. This would indicate that in addition to granitic pegmatite and possibly some calcite vein-dikes, at least one gneiss possibly would have to be included among the created rocks of Gentry's theory. With further examination however, the geologic picture causes even more difficulty for Gentry's model.

In nearly every case the rock in which the polonium halos were found can be interpreted to be *younger* than some other rock (column 5, Table II). The pegmatites and granites are sometimes observed to have halos of metamorphism about them, seemingly indicating that when they were hot, they were hotter than their surroundings. This implies the surrounding rock had cooled earlier, and thus is older. In other cases the polonium-containing rocks are seen to include partially melted pieces of the surrounding rock. These "xenoliths," "roof pendants," etc., also seem to indicate that the surrounding rock was already cool or at least solid when the polonium-containing rock was still fluid enough to allow the xenolith to be incorporated. In still other cases, the polonium-containing rock is seen to separate two pieces of the surrounding rock and even in some cases to distort it. In other cases the polonium-containing rock is finer-grained near the contact with the surrounding rock. Collectively these evidences indicate that at one time the surrounding rock was more solid and cooler than the polonium-containing rock. This, in turn, is interpreted to mean that the polonium-containing rocks are actually *younger* than the rocks that surround them. Now it may well be that God created these "older" rocks as well. After all, God must have created other things with the appearance of history.

However, Gentry tells us of none of this in any of his papers. He claims simply that ". . . Precambrian granites . . . are the basement rocks underlying the continents . . ." (Gentry, 1987c, p. 235). This claim is incorrect as rocks at great depth are actually much more mafic. In addition, the polonium-containing rocks have much apparent history inscribed within them indicating they are not simply "basement" rocks of the continents. In some cases, not only are the polonium-containing rocks younger than granites, pegmatites, and gneisses, but also volcanics and even sediments. If the rocks in Table II are correctly assigned and the geology of the area actually shows the age relations of the last column, and polonium halos could only be formed as a result of God's creation, then it must be admitted that God created the primordial rocks with a tremendous amount of apparent history.

Gentry not only claims that the polonium-containing rocks are granite and primordial, but also that they are Precambrian. This may also be incorrect (column 4, Table II). At least six of the polonium-containing rocks—the Carlow and Conway granites, the Woelsendorf fluorite and the rocks of Malbaie, Mount Apatite, and Portland—are thought to be Phanerozoic (post-Precambrian). If this is verified, then not only is Gentry's understanding of their geologic position incorrect, but also his "special theories" as well as the "general creation theory" are falsified as well (see discussion of the "general theories" below).

Problems with the "Special Theories"

One of the most serious problems with Gentry's model is that his "special theory" has been virtually, if not completely falsified. Gentry's "special theory" (1987b, p. 104) maintains that God created granite and that it cannot be simulated by man. A necessary deduction from Gentry's "special theory" is that man cannot create granite in the laboratory (Figure 1). And truly, an artificial granite has not yet been produced.

Table II: Geology of Localities from Which Polonium Halos Have Been Reported.

Locality	Mineral	Rock	Accepted Age	Older Rocks
S. India	Co	Gn	PrC: Arch**	----
Carlow	B@	Gr*	Phz: Dev*	Phz Sed
Ishiguro	B@	----	----	----
Woelsendorf	F@	vein*	Phz*	Gr/Gn
Iceland	B@	P	PrC: Prot*	Gr/Gn
Froland	B@	P** (Gr, Gn)	PrC: Prot*	Gr/Gn
Moss	B@	P** (Gr, Gn)	PrC: Prot*	Gr/Gn
Kragero	B@	P** (Gr, Gn)	PrC: Prot*	Gr/Gn
Ytterby	B@	P	PrC: Arch*	----
Mt. Apatite	B@	P** (Sh, Gn)	Phz: Dev/Carb**	Gn & Sch
Portland	B	P*** (Gr, Gn, Sh)	Phz: Dev/Carb***	Gn/Gr/Gn/Sch
Cottman	B	P*** (Gr, Gn, Sh)	PrC:	Sch, Gr & Gn
Malbaie	B	Gr****	Phz*	Phz Sed
Dingwall	B	P*** (Gn, Gr)	PrC**	Gr/Gn
Huron Claim	B@	P* (Gr, Sh)	PrC: Arch	Sch
Star Lake	B@	P*** (Gr, Gn, Sh)	PrC:	Sch & Gn
Fission M.	B@	Ca-F vein*	PrC: Prot*	Gn
Weissman M.	B@	P* (Gn)	PrC: Prot*	Gn
Cheddar	B@	GR*	PrC: Prot*	Gn
Faraday M.	B@	P* (Gr)	PrC: Prot*	PrC Sed
Silver Crater M.	B@	Ca-B vein*	PrC: Prot*	----
Conway	---	Gr	Phz:	Sch, Gn

Abbreviations: Arch: Archaean (Early Precambrian); B: Biotite; Ca: calcium; Carb: Carboniferous; Co: cordierite; Dev: Devonian; F: fluorite; Gn: gneiss; Gr: granite; P: granitic pegmatite; Phz: Phanerozoic; PrC: Precambrian; Prot: Proterozoic (Late Precambrian); Sch: schist; Sed: sedimentary rock. Symbols: @: uranium is reported from the locality, the rock, or the mineral; *: very likely that designation is correct; **: possible, though somewhat uncertain, that designation is correct; ***: very uncertain that designation is correct; ----: no data found; /: means "which is older than." Rocks in parentheses are other possibilities based on the geology of the area, with the most likely rock listed first, the next most likely listed second, etc.

However, the physical process of cooling of granitic plutons has long been thought to require thousands to millions of years of time. Consequently, up until recently, most researchers have felt that long periods of time (many human generations) were *required* to produce granitic texture. So strong has been this conviction that before the mid-1970's no real attempts were made at simulating granitic textures. The classical study of Jahns and Burnham showed that the cooling of a molten pegmatite or granite produced "typical assemblages of major minerals, along with many textural features characteristic of natural pegmatites . . . duplicated in miniature . . ." (Jahns and Burnham, 1958, p. 1592). Large crystal size was thought possible only if one waited lifetimes. It was when silicate crystal growth studies produced large crystals in short times (e.g. Mustart, 1969; Swanson, Whitney and Luth, 1972), that granitic texture simulation began to be considered. Swanson's (1977, p. 977) study of cooling molten synthetic granite led him to conclude that ". . . long periods of time are not necessary for the development of plutonic textures" [Swanson's emphasis]. In sum, though a true granite has not yet been produced in the laboratory, many granitic features have been. The most common granitic minerals—plagioclase, orthoclase, and quartz—have each been grown in crystal forms and sizes characteristic of granites (e.g. Mustart, 1969; Swanson, Whitney and Luth, 1972). In addition, plagioclase zoning (Lofgren, 1974), comb-layering (Lofgren and Donaldson, 1975), and mafic-before-felsic crystallization (Nany and Swanson, 1980)—all observed in field granites—have been simulated in the laboratory. Though a true granite has not been simulated, the evidence indicates that it soon may be. Even if simulation of a granite were unsuccessful, the experiments so far conducted have shown that if God created granites, He did so with many features now reproducible by man.

Since Gentry (1986) feels that fossils in sedimentary rocks from at least the Eocene and below were buried in the Flood, he then believes that all fossils are post-creation in age. Thus if it can be shown that there is *any* granite in contact with a fossiliferous rock which has a halo of metamorphism about it, then Gentry's "special theory" and his "special creation-week theory" are both falsified (Figure 1). Any granite that so metamorphoses a fossil, must postdate it, and thus is not primordial, but actually post-Flood. There are many claims in the literature of Phanerozoic granites, and a number of them are in contact with metamorphosed, fossiliferous sediments. If it can be shown that any of these granites' halo of metamorphism has actually metamorphosed a fossil, then Gentry's theories are falsified. One example of a claimed Phanerozoic granite is a quartz monzonite (granite variety) pluton in contact with metamorphosed limestones containing fossil corals, brachiopods, crinoids, forams, etc., in Inyo County, California (Hall and MacKevett, 1962). Another example is the Galloway granite complex of Scotland, which is claimed to actually metamorphose graptolites (Peach and Horne, 1899, p. 644). These examples and others (Wilkerson, in press) should be examined to test Gentry's special theory.

Problems with the "General Theories"

There is at least one observation which would falsify both Gentry's "special theories" *as well as* the "general creation theory. If a granite can be found which has metamorphosed fossiliferous sediments *and* contains an apparently primary Po halo, the halo must be acknowledged to be post-creation-week in age (Figure 1). To test this a search should be made for polonium halos in granites that metamorphose fossils (Wilkerson, 1987). It would be necessary to find polonium halos in only one such granite. If any of the six Phanerozoic-aged rocks in Table II are found to metamorphose fossils, the falsification would also be complete. It is most likely that at least one of these rocks is truly post-creation in age. If halos were found in any Phanerozoic granite, then both "special theories" as well as the "general creation theory" would be falsified (assuming God did not create the fossils at the creation event as well).

Although it is most probable that both special theories as well as the "general creation theory" of radiohalos may soon be falsified, the "general singularity theory" will prove to be more elusive. There is no observational data that can be compared with the theory's deductions to either falsify or confirm. As a result, the "general singularity theory" of polonium halos may be true, but since empirical evidence cannot be brought to bear upon it, it is not a scientific theory.

Naturalistic Origin Likely

No satisfactory, naturalistic theory has yet been proposed for the origin of the polonium halos. There is some circumstantial evidence, though, that suggests a naturalistic explanation is possible. Radiohalos are found in over 40 different minerals (Gentry, 1973), all of which can be produced hydrothermally in the laboratory (Brown, 1987a). Although the lack of radiohalos in moon and meteorite rocks could be due to post-creation metamorphism (Gentry, 1987b, p. 104), it

may be more than coincidence that these rocks also formed in environments considered to have lacked water (Brown, 1987a). Furthermore, most of the polonium halos are found along conduits, cleavage planes, fractures, and dislocations in crystal structure (Joly, 1917, p. 458; Schilling, 1926; Meier and Hecker, 1976, p. 188). These would be expected to be sites where water could deposit radioactive uranium and/or daughter products. All Irish halos (Joly, 1917, p. 458) and Woelsendorf halos (Schilling, 1926) are claimed by the original investigators to have occurred *only* along cracks or conduits. Gentry (1968, p. 1229; 1973, p. 355) claims to have found halos not associated with conduits, verifying Henderson's similar claim but he neither identifies the locations nor the specimens. Meier and Hecker (1976, p. 188) also point out that the polonium halos they found had a distinctly different origin than the uranium and thorium halos. Although the uranium and thorium was found within the lattice of the biotite, the polonium was not. This seems to indicate that the emplacement of the polonium *post*-dated the creation of the biotite. Perhaps there was a post-creation dissolution/precipitation event that allowed for the transport of the polonium and/or radioactive precursors. None of these observations, either singly or collectively, produce an alternative explanation. Gentry's work on the halos still stands as a challenge to anyone trying to explain them by a naturalistic explanation. However, they *suggest* the possibility that water may be related to the origin of the polonium halos.

Additionally, most, if not all, the rocks which contain polonium halos also contain uranium (column 2, Table II). Each of the polonium-containing rocks should be examined to determine if this relationship is always valid. Furthermore, polonium halos should be sought in granites that do *not* contain uranium, in case the search for polonium halos has thus far been biased toward those units where uranium halos are found. Meier and Hecker (1976, pp. 188-9) also report that polonium halos are found in abundance in those minerals where uranium is also found to be abundant. So perhaps relative abundance of uranium and polonium halos should also be determined for each of the polonium-containing rocks. If there is a strong correlation between polonium halo abundance and uranium concentration, perhaps there is also a causal relationship.

Brown (1987b) also elaborates on an argument of Stephen Dutch (1983; 1987). Why is it, that not all the polonium isotopes were created in the earth's first rocks? Of the 26 known isotopes of polonium only nine are in the decay series of other elements (U-238, U-235, Th-232, or Pu-241). Polonium radiohalos that appear to be primordial are identified *only* with isotopes from among this set of nine that can be produced by natural decay. Yet, 15 of the remaining 17 independent polonium isotopes produce daughter products which would allow them to be distinguished from the others. If any of these 15 other isotopes had been created in abundances similar to the abundances of Po-218, Po-214, and Po-210 then their halos should be identifiable. However, no evidence has been produced to date for any of these other 15 isotopes of polonium. The only "primordial" isotopes of polonium found thus far just

"happen" to be those isotopes which are found in the decay series of other elements. If God created this polonium, it seems that He only chose to create the isotopes that can be formed naturally.

Their association with uranium in abundances that may be correlated with uranium concentration, as well as their identity as polonium isotopes only found in the decay series of other elements, seem to suggest that the origin of polonium halos is related to the decay of other radioactive substances. This fact, combined with the circumstantial evidence for water in the origin of polonium halos, suggests a naturalistic explanation is possible.

Conclusion

There are serious geological problems that should be considered in the evaluation of Gentry's research. These problems leave our understanding of the geological setting of the polonium halos uncertain. A better understanding of the geology of Gentry's localities is needed. With a complete list of localities and specimens, many important questions can be resolved.

Gentry's particular "special theory" of polonium halos encounters problems on the mode of God's creative activity which can be avoided with "general theories." His special theory has also been virtually falsified—namely with laboratory studies in granitic texture, the limitation of polonium isotopes to those in radioactive decay series, and the possibility of Phanerozoic plutons which metamorphose fossiliferous sediments. Additionally, it seems that some of the polonium-containing rocks are actually post-creation-week in age. If this is true, then even the "general creation-week theory" is falsified. The remaining theory, the "general singularity theory" cannot be tested empirically, so is not a scientific theory. All non-naturalistic theories for the origin of polonium halos proposed to date seem unsatisfactory in the light of geologic data.

There exists some circumstantial evidence that the origin of polonium halos is related to the presence of water. Further circumstantial evidence indicates that polonium halos are somehow dependent upon the presence of uranium and possibly thorium. The collective consideration of all such evidence leads to the possibility that the so-called "primordial" polonium *may* be uranium- (and possibly thorium-) derived and hydrothermally transported.

Acknowledgements

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RESPONSE TO WISE

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Abstract

I appreciate the opportunity to respond to Wise's comments. I respond on a paragraph by paragraph basis to the criticism.

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Pars. 1 and 2.—In these two paragraphs Wise mixes some of his own views with mine. To clarify the issue, I

have made a clear statement of my creation model in A Tentative Creation Model in the appendix.

Par. 3, 4, and 5.—There is no difficulty in studying polonium halos for anyone who wishes to do so. Joly saw polonium halos and he had no museum specimens to study. Henderson studied polonium halos at length and he had no museum specimens. I have studied them at even greater length without museum numbers with which to refer. The reason that polonium halos have been studied without museum numbers is that they are of worldwide occurrence; they are easy to find. I have reported polonium halos in granites and pegmatites from several continents. Their occurrence is as widespread and pervasive as is the occurrence of those rocks all over the world. University geological museums contain countless thousands of rocks from such locations; so there is no dearth of material to study polonium halos. Moreover, polonium halos do not change their characteristics from one continent to the other so that their study is not confined to a single site or location. If Wise needs material to study polonium halos all he has to do is order biotite specimens from Ward's Natural Science Establishment in Rochester, New York. At any time during the past several years Wise could have availed himself of this material and made as many petrographic sections as he wished to study the rocks.

Par. 6 and 7.—There is no question that polonium halos in rocks raise some very disturbing issues for conventional uniformitarian geology. However, Wise's assertion that I claim polonium halos are "always found in granites," is patently untrue. In fact, if Wise had carefully read my scientific reports, he would have seen that I specifically note the existence of polonium halos in Precambrian pegmatites (Gentry *et al.*, 1974), fluorite (Gentry, 1973, 1974) and cordierite (Gentry, 1973). What Wise apparently has not understood is that the existence of polonium halos in crystalline rocks served to identify these rocks as the created rocks of this world and that further research will identify even other varieties of rocks as being in this category. In particular, the existence of polonium halos in the biotite at the Fission and Silver Crater Mines serves to identify the host "vein dikes" as also being created rocks, and as already noted, 15 years ago (Gentry, 1973) I published information on the existence of polonium halos in cordierite; so there is no question that at that time I considered the cordierite and its host rock to be among the created rocks. Contrary to Wise's evaluation, this information does not present a difficulty to my creation model. Neither does the inclusion of gneiss as a type of created rock cause a problem as Wise seems to imply. The best that can be said is that it presents a problem for his understanding of my creation model.

In addition, I must note that rhyolite is not granite. Rhyolite and granite have only one thing in common and that is elemental composition. However, granite and rhyolite differ somewhat in mineral composition, quite considerably in mineral grain size, and especially in the presence of polonium halos in one and absence of them in the other.

Par. 8.—In this paragraph Wise first comments on the age sequence of polonium-halo-containing rocks but, interestingly, he does not discuss either the model or the dating method used to arrive at his age sequence.

Rather, the entire basis for his conclusions on age sequences is the information in his Table II. Without any disclaimer or discussion of any alternative interpretation of the geological terms in that table, the "accepted age" referred to there seems to be just the conventional geological age determined by uniformitarian geology. In other words, Wise is implicitly using the results of uniformitarian radiometric dating to establish an age sequence of rocks containing polonium halos. However, as I show several times in my book (Gentry, 1988), there is no scientific basis for accepting the crucial assumption of decay rate constancy and without that assumption the conventional ages determined by radioactive methods are meaningless.

Much of the rest of the paragraph is given to various claims about the nature of polonium-halo-bearing rocks but no references are provided to substantiate the interpretation given. Do such references even exist? If so, why were they not provided? I would be happy to respond in print to Wise's claims about polonium-halo-containing rocks if and when he can provide valid documentation for them.

Par. 9.—I have referred to Precambrian granites as basement rocks of the continents to convey the widespread occurrence of polonium halos and also as an illustration of the vast amount of rock which must be identified with the rocks that were created. To say, as Wise does, that some rocks below the earth are of more mafic composition than granites in no way detracts from the evidence pointing to such granites being among Earth's genesis rocks. In this paragraph Wise again makes claims about polonium-halo-containing rocks being younger than "volcanics and even sediments." But I find no documentation for such claims. I would gladly have responded to them if references had been supplied.

Par. 10.—Again Wise erroneously asserts that I associate polonium halos only with granites. And to clarify terminology, I used the term "Precambrian granites" to avoid any possible confusion with a variety of rocks that sometimes are associated with crystalline granite. However, an integral part of my creation model is that granites with polonium halos, of whatever presumed geological age, are created granites. Thus, contrary to Wise's opinion, polonium halos in these other granites do not at all invalidate my creation model.

Par. 11.—Wise recognizes that granite synthesis has not occurred because he states, "And truly, an artificial granite has not yet been produced." Yet he attempts to leave the impression that synthesis is soon to come by quoting various geological reports relating to the synthesis of various single crystals of minerals. The fact is, however, that single crystals of minerals are not pieces of granite; granite, as I have used the term, is a coarse-grained mixture mainly composed of feldspar, quartz, and biotite, and this has not been reproduced in a hand-sized specimen. Thus Wise begs the question when he admits, "though a true granite has not yet been produced in the laboratory, many granitic features have been." At the very best, this is an overstatement because, first, biotite, one of the primary mineral components of granite, has not been synthesized in macroscopic-sized crystals. Secondly whatever minerals have been synthesized in the laboratory do

not contain the polonium halos which are in natural granite, and Wise ignores this tremendous disparity between laboratory synthesis of single minerals and the actual occurrence of granite in nature. Wise is entitled to his belief that a true granite may soon be synthesized in the laboratory, though such a belief is inconsistent with the fact that, when a granite melt cools in the earth, it recrystallizes to rhyolite, not granite.

Wise apparently feels that granite studies over the past few decades bear on the crucial issue concerning polonium radiohalo evidence for creation. Do they? For many years (Gentry, 1979) I have proposed that there is a test whereby it is possible to determine whether the creation or evolutionary view of earth history is correct. This falsification test enables the nonscientist to distinguish real facts from what are simply deductions based on unproven uniformitarian assumptions. Evolution's basic premise is that the earth geologically evolved to its present state over billions of years by the action of known physical laws. A consequence of this premise—technically known as the *uniformitarian principle*—is that all the rocks now on or within the earth formed by natural processes. The evolutionary scenario views granites—a widely distributed rock type that contains polonium halos—as having formed countless thousands of times during the course of earth history. If this is true, then it certainly should be possible to synthesize a small, hand-sized piece of granite or a 10-cm-wide crystal of biotite in a scientific laboratory. Thus, I have invited (Gentry, 1979, 1984, 1986) my scientific colleagues who believe these rocks formed naturally to confirm their view by experimental demonstration. But my nine-year-old invitation (Gentry, 1979) for them to produce such specimens has produced only silence. This is not surprising. *The parentless polonium halos in these rocks provide unique evidence that they did not form by natural processes.*

Par. 12.—There are places where granites (or granodiorites) are surrounded by metamorphosed, fossiliferous-bearing, sedimentary rock. But contrary to Wise's view, such occurrences do not falsify my creation model. Conventional uniformitarian geology teaches that granitic melts have intruded into fossiliferous sedimentary rocks, thereby producing a metamorphic zone. But my explanation of such metamorphism is quite different from that scenario and is based on the previously mentioned fact that, when granite is melted in the earth and subsequently cooled, it recrystallizes to form rhyolite, not granite. My model for explaining metamorphosed, sedimentary rocks adjacent to granites—such as those that occur in the Santa Rita mining district in southeastern New Mexico—is as follows: Sometime during the Flood, movements within the earth could have broken open an underground aqueous reservoir which then contacted an intensely hot magma at considerable depth. That contact could then have produced a superheated fluid loaded with volatile components extracted from the magma. (In this scenario these volatile components would subsequently become the mineralizing agents in producing the ore bodies.) This superheated fluid would in turn have generated tremendous subterranean pressures. Movements within the earth also would have fractured the heretofore unbroken granodiorite basement rock.

Once that happened, extremely high pressure from both underground magma and the geothermal fluid would cause the uplift of a huge section of the fractured granodiorite into the overlying sediments. The magma referred to here would cool to form rhyolite and other secondary rocks in the area, whereas the hot geothermal fluid is envisioned as the mineralizing agent for both the uplifted granodiorite as well as the surrounding sedimentary rocks. In such cases the metamorphic zone in the surrounding sedimentary rocks would be produced by heat from the geothermal fluid rather than from a cooling granitic melt.

Par. 13.—Again Wise maintains that granites surrounded by metamorphosed, fossiliferous deposits would invalidate my view that granites are created rocks. And he tacitly assumes the causative agent in producing such metamorphism is heat from a cooling granitic melt, which is contrary to the experimental evidence, namely, for the third time, that a granitic melt cools to form rhyolite, not granite. In response to the previous paragraph I have outlined a scenario whereby metamorphosed, sedimentary rocks can be produced around granites by hot geothermal fluids at the time of granite uplift. Thus, such occurrences are within the framework of my creation model.

Par. 14.—Wise makes a clear, unequivocal statement of fact when he says, "No satisfactory, naturalistic theory has yet been proposed for the origin of the polonium halos." He then rehashes a number of plausibility arguments, all of which I have rebutted in the open scientific literature (Gentry, 1968, 1971, 1973, 1974, 1984, 1986; Gentry *et al.*, 1973, 1974, 1976), in an attempt to deny the validity of his own statement. For example, the insinuation that polonium halos occur only along cracks or conduits is denied by the photographic evidence even in Henderson's reports as well as in my own reports (Gentry, 1967, 1968, 1971, 1973, 1974, 1984; Gentry *et al.*, 1974) and especially in the color photographs in my recent book, *Creation's Tiny Mystery* (Gentry, 1988). In an effort to promote a water-related origin of polonium halos, Wise cites someone else's opinion to the effect that all of the minerals containing polonium halos can be produced hydrothermally in the laboratory. This idea is, of course, a widely held belief of uniformitarian geology. But it lacks experimental confirmation as far as reproducing macroscopic-sized actual crystals are concerned. For example, for many years I have challenged geologists to produce a hand-sized specimen of biotite—one of the more prominent halo-containing minerals that is presumed to be of hydrothermal origin—as a means of verifying that biotite can be produced hydrothermally according to the conventional evolutionary view (Gentry, 1979). Almost a decade has passed, and no evidence exists to indicate such a synthesis has been accomplished. So there is no scientific basis for claiming that natural crystals of biotite are of hydrothermal origin, or more specifically, that all polonium-halo-containing minerals are of hydrothermal origin.

Par. 15.—On another matter, Wise's contention that the search for polonium halos has been biased toward areas where uranium halos are found is untrue. To be sure, Wise heard me describe the occurrence of polonium halos in the distinctly uranium-poor White Moun-

tain (New Hampshire) granites during my technical presentation at the 1986 International Conference on Creationism. For some reason it appears he has overlooked that information in his present evaluation.

Par. 16.—Here it is most important to understand that Wise raises a question about what *does not exist*. On the other hand, my experimental work on radioactive halos deals with what *does exist* and the problems that one encounters in trying to explain these halos on a uniformitarian basis. Wise faults me for not explaining the halos that do not exist. In this case, I see no reason to attempt to explain something that does not exist. Moreover, Wise's association of the other polonium isotopes with primordial polonium is something that is based on uniformitarian views of earth history and in no way discounts the creation of primordial polonium in primordial rocks. In particular, each chemical element in the chart of the nuclides lists both naturally occurring isotopes as well as those which have been identified in nuclear accelerator experiments. Modern astrophysics attributes both the naturally occurring stable and long-lived radioactive isotopes—such as U-238 and Th-232—in this chart with *primordial nuclides* produced in *stellar nucleosynthesis*. Doubtless some chemical elements in stars are produced by nucleosynthetic reactions, but I have yet to see the scientific evidence which justifies assuming that the origin of Earth's chemical elements can be traced to stellar nucleosynthesis. Thus, I find no rational basis for accepting the modern astrophysical concept of primordial isotopes.

Par. 17.—Ion microprobe analyses of polonium halo centers have revealed scientific evidence supporting an independent origin for the polonium responsible for halos (Gentry, 1971; Gentry *et al.*, 1974). By way of further explanation, the isotopic composition of lead derived from uranium decay—meaning the Pb-206/Pb-207 ratio—must always be considerably less than the activity ratio for U-238/U-235, which at the present time is 21.8. Since Po-210 halos in coalified wood originated from uranium decay, it was expected that their centers would exhibit Pb-206/Pb-207 ratios consistent with uranium decay, and ion microprobe analyses confirmed this was the case (Gentry *et al.*, 1976). But when the same technique was applied to polonium halos in minerals, I found ratios greater than 22, which is too high to associate with uranium decay (Gentry, 1971). Such isotope ratios identify a new type of lead, which is distinct from the isotopic composition of any type of common or radiogenic lead known heretofore. This is the scientific evidence which uniquely identifies polonium halos in rocks as having originated with “parentless” polonium—polonium that originated independent of uranium daughter products.

These extraordinary lead isotope ratios, when combined with the absence of evidence for secondary transport of uranium daughters (Gentry, 1967; 1968) as well as the evidence for geometric design in the spectacle halo (Gentry *et al.*, 1974), provide a valid scientific basis for associating polonium halos in granites and other rocks with primordial radioactivity.

Conclusion.—A close examination of Wise's paper fails to reveal the “serious geological problems” relative to my creation model and granites being created rocks. Specifically, polonium halos can easily be stud-

ied by anyone who has the desire to obtain the minerals containing them. Their widespread and pervasive occurrence in granitic rocks and pegmatites assures scientists all over the world easy access to study the geology in whatever country they are found. The claim that laboratory studies in granitic texture have virtually falsified my theory of created granites is contradicted by experimental evidence from the laboratory of nature which shows that a granite melt cools to form rhyolite, not granite. Hence the idea that granites “metamorphose fossiliferous sediments” is nothing more than a deduction based on the erroneous view that granites formed from a cooling melt. Finally, it is one thing to conclude, as Wise does, that polonium halos “may be uranium- (and possibly thorium-) derived and hydrothermally transported,” but it is another thing to virtually ignore, as Wise also does, the published scientific evidence to the contrary.

Readers genuinely desiring pertinent information about my creation model—and not what others speculate about my model—should carefully study my position as stated in the appendix and the discussion of the supporting scientific evidence in my book *Creation's Tiny Mystery* (Gentry, 1988).

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*The references not cited in this section may be found in Wise's reference list.

APPENDIX

A TENTATIVE CREATION MODEL**

Introduction

This model is based on my interpretation of the Biblical account and scientific evidence; it does not represent the official position of any organization. It elucidates only my views as a creation scientist. Scientists who differ with the assumptions inherent in this model need to publish their own model of earth history. In this way all ideas can be critically tested by the scientific method. I respect the right of any individual to choose that model which fulfills his philosophical and/or scientific view(s) of origins.

Scientific Basis

If the earth was created, then it is axiomatic that created (primordial) rocks must now exist in abundance on the earth; and if there was worldwide Flood, there must now also exist a vast amount of sedimentary rocks resulting from that event. The Precambrian, and by extension the similarly appearing non-Precambrian granites, are identified as part of the primordial rocks of the earth. Evidence which supports the above axiom includes: (i) widespread occurrence of Po halos in Precambrian granites (Gentry, 1967; 1968; 1971; 1974; 1984; Gentry *et al.*, 1973; 1974); (ii) U/Pb ratios in coalified wood (Gentry *et al.*, 1976); (iii) Po-210 halos in coalified wood (Gentry *et al.*, 1976); and (iv) Pb and He retention in zircons (Gentry, 1982a, b).

Postulates and Singularities

This model allows for considerable latitude in the operation of physical laws because I believe the

**References noted in this appendix can be found in Wise's reference list.

Biblical record strongly suggests that the earth came to its present condition through both natural and supernatural processes. The special time periods when supernatural processes were operating on a global scale are called singularities. The Genesis account appears to encompass at least three major singularities: (i) the first six days of creation, consisting of literal 24-hour periods, when the earth was called into existence, (ii) the Fall of man, a time characterized by degenerative changes to the entire earth and life thereon, and (iii) the time of the Flood, characterized by global changes both on the surface and the interior of the earth. A possible fourth singularity would be the division of the earth in the days of Peleg (Gentry, 1984). Accelerated radioactive transformation rates are postulated as being possible within each of these singularities and, in addition, are considered to be a factor in the generation of intense heat within the earth at the time of the Flood. In this model, creation week is considered to have occurred about 6000 years ago and the Flood about 4300 years ago. The uniform action of physical laws *between* singularities is an integral part of this creation model. However, the period of a singularity does not imply complete abandonment of natural laws but rather that there was an added factor at work.

Creation Week Events

A continual series of creative events is envisioned to have occurred throughout the period of Day 1 and quite possibly more throughout Day 3. This scenario includes rapid, sequential creation/formation of different rock types in close proximity and this certainly could have resulted in cross-cutting relationships of either the same or different types of primordial rocks. These different rock types and textures result from rapid crystallization of separate primordial (created) liquids. The appearance of dry land out of a watery environment on Day 3 may have been accompanied by the rapid formation of certain sedimentary rocks, in particular those that geologists classify as Precambrian, which initially would have been free of fossils. Creation week may have included vulcanism and the formation/creation of some rocks which geologists classify as intrusive. Conceivably, there may also have been mixing of different created-rock types.

Flood Events

The global Flood is assumed to have produced tremendous upheavals of the earth's crust. Excepting only newly created matter (or rocks), the period of the Flood was characterized by numerous occurrences of both natural and supernatural formation, cooling, deposition, intrusion, uplift, mixing, erosion, and vulcanism. This model postulates that the bulk of fossil-bearing and sedimentary rocks probably formed during the opening and closing stages of the Flood, with lesser amounts being formed during the long period of subsidence, readjustment, and run-off after the Flood. Since the long-term geological effects of the Flood may have lasted for centuries, we should expect to find evidence of numerous combinations of created rocks, the Flood-related rocks, and the postFlood rocks.

To illustrate, extensive vulcanism during the Flood and postFlood periods could have precipitated the intrusion of volcanic magma into sedimentary forma-

tions. This would provide a mechanism whereby the primordial and other rock types, formed during creation week, could have mixed or interacted with Flood-related volcanic and sedimentary material. Consider that, as magma moved upward toward the earth's surface, it could have passed through and melted, or alternatively encapsulated, a variety of rocks, beginning with those created on Day 1 or Day 3, and extending through those formed by volcanic and sedimentary activity during the Flood. When that magma finally solidified, it would have been a composite of all the rocks just mentioned. If the magma temperature was not too high, then the composite rock would have been expected to contain unmelted fragments of many rocks through which the magma had passed.

Summary

It is proposed that most coarse-grained rocks, such as the Precambrian granites and pegmatites, were created via rapid crystallization from primordial liquids of differing compositions. But for reasons stated above, not all rocks with coarse-grained characteristics are necessarily composed of only created rock material. Neither do I consider it impossible to duplicate every type of created rock. The types of created rocks are not restricted except that they did not contain fossils at the time of creation. Granites with Po halos, regardless of their "geological age," are primordial rocks and this tentative model assumes they were created in such a way that they cannot be duplicated by natural process. In contrast, the evolutionary model assumes that the entire earth originated solely by natural processes and, in particular, that granites crystallized from a slowly cooling melt with Po halos forming much later from uranium daughter activity.

Testing the Models

If the evolutionary version of earth history is correct, it should be possible to duplicate natural granite by first synthesizing a granite rock *without* halos, and then demonstrating that *Po halos can be formed within it using uranium daughters*. For years scientists have been challenged to perform this crucial experiment (Gentry, 1979). Could it be the continued failure to duplicate natural granite is because this rock required supernatural power to form?

Materialism

The following quote from a novel published in 1910 illustrates the influence of atheistic materialism on our culture. A medical doctor having been congratulated by a mother for curing her child, offers the following commentary:

A great surgeon of France centuries ago was accustomed to say of a convalescent patient: 'God cured him; I dressed him.' I do not know whether, if I dared speak for the science of medicine near the close of the nineteenth century, I could say that. That is not the language of science now. If science thanks anything, it thanks other sciences and respects itself. (Allen, 1910, p. 34)

Reference

Allen, James Lane. 1910. *The Doctor's Christmas Eve*. Macmillan, New York.

PANORAMA OF SCIENCE

Examples of Rapid Burial Necessary for Fossilization

The four illustrations (Figures 1-4) shown in this discussion are drawings of actual fossil specimens carefully copied by Ronald C. Calais. The drawings contain precise details but are not to scale. The larger fossilized fish appear to have been victims of a sudden catastrophe with resultant rapid burial while in the process of devouring smaller fish. It is likely that the larger fish were preparing to "enjoy a meal" when they and their prey were suddenly covered with sediment that later hardened with both the predator and prey becoming fossilized. Did this burial process occur as a result of the Flood? Generally most creationists would assume that such was possible during the Flood or afterwards when the waters receded and considerable tectonic action occurred. Rapid burial necessary for

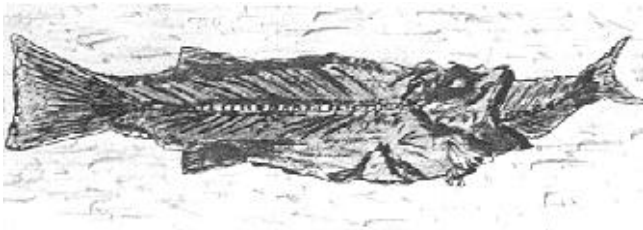


Figure 1. A larger fish (perch . . . *Mioplosus*) in the act of swallowing a smaller herring (*Knightia*) on a slab from the Green River Formation, Wyoming. This is a specimen illustrated in countless volumes. Other early Eocene fish from this locality are found in this same pose.

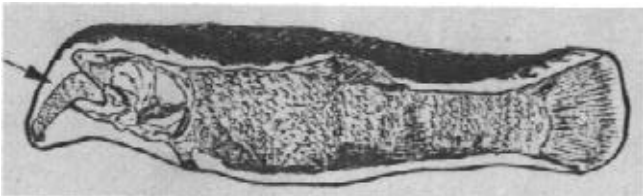


Figure 2. *Cearana* Fossilized while swallowing a smaller member (indicated by arrow) of the same genus. The nodular concretion enclosing both specimens is from the Santana Formation (Cretaceous), Chapada do Ararife, Regiao do Ceara, Brazil. The fossil fish from this region are remarkable because they are preserved with scales intact. Sometimes even color patterns are discernible!



Figure 3. A large *Caturus* sp. with the head of an unidentified species of fish in its mouth. This fossil came from the Solnhofen limestones, near Eichstatt, W. Germany, home of the controversial *Archaeopteryx*. This formation is dated as Upper Jurassic.

fossilization has been discussed in prior creationist writings (Morris, 1967, p. 95; Hedtkke, 1971, p. 214; Williams, 1986, p. 63; Snelling, 1988, pp. 32-6).

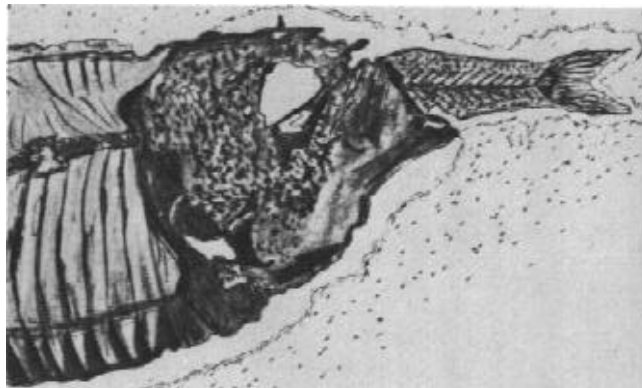


Figure 4. Another specimen from the famous Green River deposits, but of a different genus. The large fish is in the act of devouring a juvenile of the same species . . . *Gosiutichthys parvus*.

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- CRSQ—*Creation Research Society Quarterly*
 Hedtkke, R. 1971. A geo-ecological explanation of the fossil record based upon divine Creation. *CRSQ* 7:214-21.
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 Contributed by R. C. Calais and Emmett L. Williams

Rapid Burial and Fossilization—Australian Style

Dr. Andrew A. Snelling, Creation Science Foundation, Ltd., Australia recently discussed (1988, pp. 32-6) the implications of a fossilized fish discovery in his country. The site of the excavated fossil layers was in the Somersby Falls area approximately 50 miles north of Sidney. The fossils were found in a grey silty shale within Hawkesbury Sandstone, a sedimentary unit in the Sidney Basin. The shale with the fossil material formed a lens (2-3m) within the sandstone supposedly deposited in the Triassic Period—225 million years ago (p. 33). Interestingly the fossilized fish were not scattered throughout the shale lens "but were confined to certain narrowly defined zones" (p. 33).

There are three such zones in the western part of the shale lens. Each zone is separated by approximately a metre of barren material. Each zone is up to 10 centimetres thick and consists of two or more sub-levels with fossilised fish on each respectively. Concentration of fossilised fish within each zone is so great as to suggest that each is a true mortality zone. The occurrence of sand and pebbles in association with the mortality zones suggests that the death and burial of the fish was rapid and catastrophic. The good state of preservation and lack of damage confirms the view that these fish rapidly died in place and were equally rapidly buried. (pp. 33-4)

The shale layer yielded as many as seven fish fossils per square meter including “at least eight extinct species” (p. 34). Examples of particular fish fossils found were identified as *Cleithrolepis*, *Hydropessums*, *Promecosomina*, *Palaeoniscids*, *Saurichthys*, *Gosfordia*, *Xenacanthus* and two species of small fresh water sharks.

Snelling concludes his article with the following remarks (p. 36).

The only thing we can say about the discovery of these fossilised fish with any certainty is that they died catastrophically and were preserved very quickly in a manner which is not inconsistent with the conditions described for Noah's Flood in the Scriptures. Furthermore, the massive nature and thickness of the Hawkesbury Sandstone does not necessarily imply a river delta flood plain system (that is just one interpretation). Observational evidence in the sandstone is equally consistent with rapid flood deposition under massive amounts of water. But this spectacular fossil find does confirm that for fish to be fossilised they need to be overwhelmed suddenly and catastrophically and that such events were on a large scale.

It is highly debatable whether the Somersby fossil deposit can be interpreted as a buried lake ecology with schooling patterns and predator-prey relationships. Quite the contrary The fossilised fish are found in three zones at three different levels within the shale and this is totally inconsistent with a lake ecology being preserved during burial by a single sudden catastrophe. If such were the case the fossilised fish should be found in a single zone or level that represented their ecological positions at the time of burial by the incoming catastrophic sediment-laden current.

But such a violent current would pick up and carry the fish along to a subsequent place of burial removed some distance from where they originally lived. The observational evidence of three levels of fossilised fish accompanied by sand and pebbles in the shale is totally consistent with such a catastrophic sediment-laden current picking up the fish from where they lived, sorting them during turbulent flow and then burying them once the sediment load was dumped elsewhere. Thus the Somersby shale with its fossilised fish cannot have been the lake where the fish lived, only their ultimate burial site.

Furthermore, one is at a loss to understand how Dr. Ritchie and his colleagues expect to learn ‘more about the evolution of fish’ by studying these dead fish preserved in hardened mud. Any comment about evolutionary ancestors or descendants is mere speculation, not based on any observation of one lifeform changing into another.

In point of fact, the actual observations, as reported above, are quite definite—none of these fish discovered remain in existence today. Yet they are clearly recognisable as fish—‘to the untrained eye they look just like the fish you’d see on any fishing trawler.’ And one, the lungfish *Gosfordia*, is similar to lungfish alive in Queensland today—so they can’t be evolutionary intermediates to today’s fish.

Furthermore, none of these fish have any so-called evolutionary ancestors in the fossil record. They just appear fully formed and functional as fish in fossil deposits like this one at Somersby. These fish were thus created to reproduce ‘after their kind’ as Genesis states, and then were overwhelmed by a sudden catastrophe that entombed them and their kin in stone with no progeny to go on inhabiting the waters of this earth.

Another discussion of the necessity of rapid burial for fossilization related to the Flood or its aftereffects is found in this issue (Calais and Williams, 1988, p. 181).

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- Calais, R. C. and E. L. Williams. 1988. Examples of rapid burial necessary for fossilization. *Creation Research Society Quarterly* 25:181.
 Snelling, A. A. 1988. An exciting Australian fossil fish discovery. *Creation Ex Nihilo* 10(3):32-6.

Contributed by Emmett L. Williams

The Solar System May Be Young After All

Newton thought that the solar system could not have maintained its order for very long and that God had to intervene now and then to keep the solar system stable. Laplace, on the other hand, disagreed and “proved” to the satisfaction of his contemporaries that the solar system was stable over long periods of time. However Laplace’s proof was not rigorous and all attempts to make it so have failed (Rothman, 1988). Recently, more sophisticated numerical methods have shown that indeed the solar system likely is unstable, implying that the solar system is young.

Sussman and Wisdom (1988) extrapolated the orbits of the five outer planets 845 million years into the future specifically to analyze the stability of Pluto’s orbit. Pluto is of interest in this regard because it has an eccentricity of 0.25 and has inclined 16° to the plane of the ecliptic. Pluto also crosses the orbit of Neptune. Numerical models all have simplifying assumptions and unmodeled variables that can affect the results. However, a good model can still give qualitative information. Sussman and Wisdom ignore the four inner planets and the masses of the five outer planets which they say should not affect their results. The main conclusion is that the orbit of Pluto is unstable: “They find that the orbit of Pluto becomes unpredictable on a time scale of about 20 million years. Such unpredictability is the hallmark of a chaotic system” (Rothman, 1988). Not only that, Pluto’s chaotic orbit affects the rest of the solar system, resulting in the whole solar system becoming unstable with time. “Like all models, Sussman and Wisdom’s model involves simplifications. But if it represents the real solar system, Pluto’s chaotic motion must eventually be transferred to the other planets, thereby making the behavior of the entire solar system chaotic. Unless of course, God decides to intervene” (Rothman, 1988).

The implication of this research is that the solar system must be relatively young compared to the assumed age of 4.6 billion years. Sussman and Wisdom (1988, p. 437) state: “Thus, considering the age of the solar system, 20 million years is a remarkably short time scale for exponential divergence.” Maybe the solar system is not as old as they think.

References

- Rothman, T. 1988. God takes a nap. *Scientific American* 259:20
 Sussman G. J. and J. Wisdom. 1988. Numerical evidence that the motion of Pluto is chaotic. *Science* 241:433-7.

Contributed by Michael J. Oard

Seed Pods: Creation or Evolution Philosophy?

Fresh seed pods from bush balsam (one of the plants of the balsaminaceous genus *Impatiens*), a garden annual, are shown in Figure 1. When pressure is applied to the pods as noted in Figure 2a, they curl and split in the manner illustrated in Figure 2b dropping the enclosed seeds over a limited area. Some curled pods and their expelled seeds are shown in Figure 3.

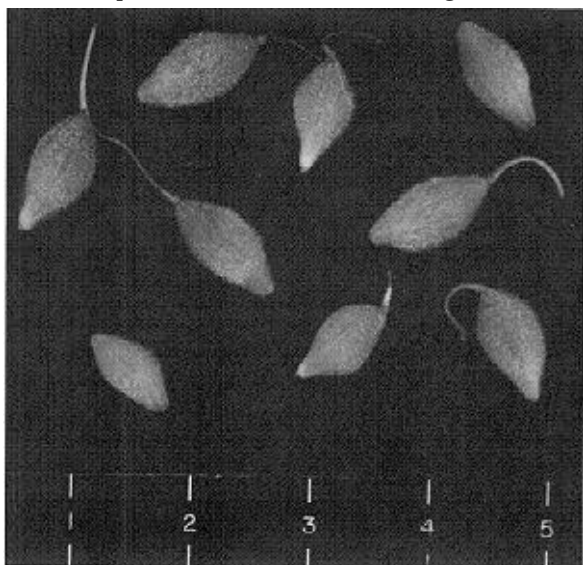


Figure 1. Fresh seeds pods of hush balsam (Dimensions of ruler in inches)

A creationist would claim that God designed this system to cause the type of seed dispersion that is suited for this particular plant to survive and multiply. An evolutionist would claim that the plant gradually acquired this seed dispersal method by mutations and natural selection which solved its particular survival problem. Whether one is either a creationist or an evolutionist it could be imagined that the pressure illustrated in Figure 2a could be generated by a mammal stepping on the pod thereby ejecting the seeds. Also pressure from the drying process alone can curl and split the pods releasing the seeds. Many interesting "scientific stories or fables" could be authored that would weave compelling tales of the "whys" of seed dispersion.

The observation or scientific fact in this case is simply that this species of *Impatiens* has a particular seed dispersion system. The "why" depends upon the scientist making the observation. The creationist prefers a supernaturalistic "why" whereas an atheistic evolutionist prefers a naturalistic one. The "why" is an interpretation of an observation and is a position of FAITH. One individual gives glory to the Creator, another to brute matter (materialism or pantheism). These positions depend upon the philosophical pre-

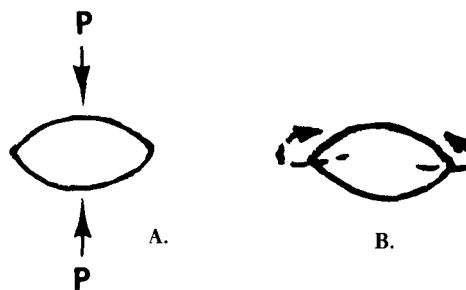


Figure 2. (a) Forces **P** applied over the area of the pod resulting in a pressure
 (b) Pod curls after application of pressure

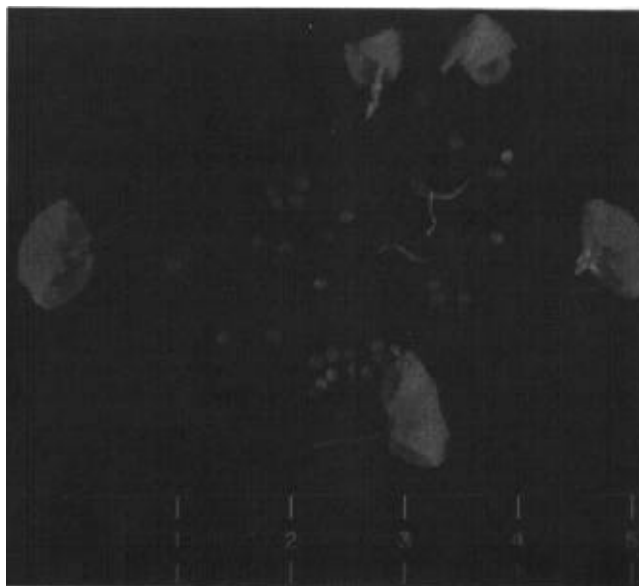


Figure 3. Curled pods and ejected seed that resulted from the application of a pressure illustrated in Figure 2a (Dimensions of ruler in inches)

suppositions of the scientist "interpreting" the observation. Both explanations are equally mystical in the sense that they cannot be repeated for man's observation.

Man simply replaces one philosophical belief (supernaturalism) with an equally philosophical belief (naturalism). It is fashionable in our time to weave many naturalistic myths around known scientific facts, then amazingly the modern scientist (the weaver) often declares himself liberated from the shackles of religion and mysticism!!! Man is indeed incurably religious.

Contributed by Emmett L. Williams

QUOTE

The biblical creation-based historian begins with the personal Triune God of the Bible as Creator and thus the absolute and ultimate sovereign over history; he must hence reject process historiography as apostasy. Note that biblical creation-based historiography establishes absolute historical meaning for individual human beings who are responsible to God their Creator for their historical acts.

Myers, Ellen. 1987. Towards a Biblical Creation-based historiography. *Creation Social Science and Humanities Quarterly*. X(1):3-4.

THE LITOPTERNA—A LESSON IN TAXONOMY: THE STRANGE STORY OF THE SOUTH AMERICA 'FALSE' HORSES

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Abstract

The supposed evolution of the horse was confronted by contradictory fossil evidence from South America. The solution in 1910 was to banish the contradictions into an obscure separate order of mammals. This paper illustrates the strange and wonderful contortions of taxonomists to remain mindlessly loyal when the theory of evolution fails them.

Editor's note—(The reader may wish to consult previous Quarterly articles on the subject of horse evolution: Cousins, F. W. 1971. A note on the unsatisfactory nature of the horse series of fossils as evidence for evolution. CRSQ 8:99-108; Davidheiser, B. 1975. Horse brain, cow brain. CRSQ 12:88-9.

A Familiar Story

In the fall of 1985 the Government of Canada opened the new world-class Tyrrell Museum of Palaeontology, said to be the largest fossil museum in the world, in the heart of one of the great dinosaur fossil beds of the world at Drumheller, Alberta. And there in the great museum, surrounded by a fabulous dinosaur collection, is the centerpiece of the 'proof' for evolution—the horse series from little *Eohippus* to modern *Equus* in five easy stages—the century-old illustration endlessly copied and recopied billions of times in four generations of textbooks, and just as endlessly in other media for the education and edification of the masses.

The horse series is the claim to fame of Yale palaeontologist Othniel C. Marsh who discovered fossil bones of the horse and of the *Hyracotherium* (incorrectly and commonly called *Eohippus*) in Wyoming and Nebraska. His reconstruction and arrangement of these fossils are still on display at Yale University and have been copied with some variation in the sequence in museums everywhere (Taylor, 1984, p. 152). It has become axiomatic that the horse evolved in the midwest of the United States, and if fossil *Equus* is found in Africa, or anywhere else, it migrated there from Nebraska or thereabouts.

Fix (1984, p. 164), who wrote a devastating critique of so-called human fossils, echoes countless writers in the field when he says that you can throw out any 'evidence' you like about evolution, but "we still have the evidence pertaining to the horse."

This paper focuses on some very interesting fossil discoveries in South America which both Darwin and the great Argentinian palaeontologist, F. Ameghino, placed among the horses (Scott, 1910, p. 551). In 1910, however, Scott (Cifelli, 1983, p. 28) removed the fossils from any connection with the horses and placed them in a far-removed and separate order, the Litopterna. This decision on classification, supposedly highly controversial and very much open to debate, has remained a closed question up to the present, as we shall see in the following discussion. Storer (1979, p. 804-5), places the Litopterna in Order 22 and the Perissodactyla, which include the horses, in Order 33. With creatures such as the aardvark, the elephant, and sea cow placed in the intervening orders, one gets the message that the Litopterna (odd-toed, hoofed mammals) and the Perissodactyla (odd-toed, hoofed mammals) are about

as remote in structure from one another as one can get.

In the process of examining the decision to remove the Litopterna from the order containing the horses, we shall gain a rather illuminating lesson into the basics of taxonomy—the elements that go into the sometimes agonizing decision about placing a woefully incomplete or puzzling fossil into this or that category. Although the creationist may sharply and categorically disagree with the evolutionary assumptions of a palaeontologist, this paper in no way reflects on the fascinating but often excruciatingly difficult work of that field of study. Collecting and restoring fossils is not evolution. The interpretation of the evidence, as we see especially in the case of horse and human fossils, often is.

For the benefit of the reader with little or no background on taxonomy in zoology, the following somewhat oversimplified points should be noted as we examine some aspects of the horse story. The animal kingdom is divided into 22 phyla of which the last one, Chordata (notochord), includes the horses. Of the four great classes of Chordata (amphibians, reptiles, birds, and mammals), the latter includes the horses. Mammals then are divided into above 30 orders which classify about 4,400 living species and numerous fossil forms. Orders are quite broad, for example, the Primate order includes every kind in all the world of lemur, monkey, ape, gorilla, and even man, following evolutionary thinking. In this paper I focus on two of the orders: the extinct Litopterna and the Perissodactyla. The latter is the more familiar one, and includes a remarkable span of creatures, such as the horses, asses, zebras, tapirs, and rhinoceroses. Bizarre extinct creatures are also included. By definition, the horse is more closely related to any other creature within this order, such as the rhinoceros than to any other animal in any other order (Storer, 1979, *passim*).

The key to classification is phylogeny or the supposed evolutionary history of a group, such as small to large, more primitive to more modern structure, and the like. Some of the beliefs about evolutionary development are examined below. For a long time the five-toed *Phenacodus* from the Order of Condylarthra was thought to be the ancestor of the horse. The bases on which it was disqualified are interesting and vulnerable: It was too large for the supposed sequence including *Eohippus*; it was too well developed, that is, it was not 'primitive' enough; its existence in time did

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not allow for evolving into *Eohippus*. *Phenacodus* was replaced by *Hyracotherium* (*Eohippus*) as the ancestral horse. This creature was removed from still another order, the Hyracoidea, or the hyraxes and coneys. For more than a century, this animal has been placed at the beginning of horse evolution (Scheele, 1955, p. 27).

Litopterna Placement into a Separate Order

That the litopterns present a problem in classification is unquestioned. Made up of two very different kinds of creatures, one group seemed very camel-like while the other is sometimes described as out-horsing the horse. The Argentinian palaeontologist, F. Ameghino with his brother, Carlos, are described by Scott as providing services to palaeontology that are quite inestimable. They discovered the formations, the unique South American fossils, and arranged them in chronological order. Ameghino, who first distinguished and named the order of Litopterna, concluded that the Litopterna belonged with the Perissodactyla, that is, with the horses. Even Charles Darwin, when he discovered the camel-like, least horse-like *Macrauchenia* in the Pampas, declared unequivocally that it belonged to the order now called the Perissodactyla. What more would he have said if he had found the horse-like kind (Scott, 1910, p. 114-5)?

It is small wonder, then, that Scott (1910, p. 551) observes that this order has been the subject of much debate because of supposed convergence.

Convergence, we note, is a device in taxonomy which declares that though two animals may closely resemble one another (for example, the horse and the litopterns), their ancestors did not. Therefore, the likenesses are merely coincidental and may safely be ignored. This device, of course, innocent of any supporting evidence, has gotten many a taxonomist out of many a sticky wicket. As we shall see, little interest is shown today in taking a second look at how the litopterns are classified. Simpson (1961, p. 87) confronts the striking similarity between horse and litoptern in this manner:

In spite of the various collocations of the typologists, horses are not so convergent toward any other living group that a modern taxonomist would be likely to mistake their homologies. There is, however, an extinct South American ungulate, *Thoatherium*, between which and horses there is strong convergence, notably in the fully one-toed feet. The discoverer of *Thoatherium*, Florentino Ameghino, an evolutionary taxonomist capable in his time (1845-1911), concluded that the resemblances are homologous and that litopterns and horses had a common ancestry closer than that, for instance, between horses and rhinoceroses.

How Are Litopterns Described?

They are very like a horse!

It is interesting and instructive to see how palaeontologists describe the litopterns. The statements below are edited so as to focus only on the expressions of various authors on how the horse and the litopterns were alike.

An important scholar and authority, Scott (1910, p. 551-68, 738), makes these statements:

- a. The teeth and skeleton of the modern horses are extremely characteristic and unlike those of any other family, except for one group of the south American Litopterna.
- b. The upper molars have cusps as in several families of perissodactyls; the incisors are arranged as in the horses.
- c. The orbits, shifted behind the teeth, as in the later and more advanced horses.
- d. The feet tridactyl as in the perissodactyls.
- e. These extraordinary animals, remarkable for the many resemblances to the horses, are well proportioned agile and graceful creatures and resembled in appearance their very distant relatives of the northern hemisphere. It is in just this likeness to relatives which are both structurally and geographically so distant, that the wonderful features consist.
- f. The shape of the hoofs and the whole appearance of the feet are most surprisingly like that of the three-toed horses.
- g. The extraordinarily interesting (Litoptern) with completely monodactyl feet.
- h. Limb bones, especially the femur, have a decided resemblance to those of *Mesohippus*, a three-toed horse. The most remarkable feature of the skeleton is the structure of the single-toed feet, which are more completely monodactyl than in any other known mammal.
- i. Limb-bones, approximating much more closely to the proportions seen in the horses.
- j. The head would seem to have had some resemblance to that of a small horse.
- k. Most extreme of all is the much debated case of one family of the Litopterna, the genera of which resemble the three-toed and one-toed horses of the North American Tertiary in marvelous fashion.

Thoatherium from Argentina is the most completely monodactyl mammal known, surpassing even the modern horses in the complete reduction of the splint-bones.

Earlier in this century (*Encyclopedia Britannica*, 1910, 11th edition, 16:791; 17:526), the litopterns are described thus:

- a. The Litopterna, exclusively in South America, are perhaps more nearly akin to the Perissodactyla (that is, to the horse).
- b. The Litopterna show a curious parallelism to the equine line; the feet are very like those of *Hipparion* (a horse).

Current authors also expressed themselves thus about the litopterna:

- a. The litopterna have notably horse-like fore and hind limbs (Peyer, 1968, p. 317).
- b. The *Macrauchenids* of the Litopterna, whose record with regard to high crowned teeth, almost exactly parallels that of the horses (Keast, 1972, p. 283).
- c. A three-toed form of the litopterns, *Diadiaphorus* (Figure 1), lived alongside *Thoatherium*, which seems more horselike than any true horse, for it was single-toed with splints more reduced than those of modern equids (Romer, 1966, p. 260).

Finally, Colbert made these observations (1967, p. 343, 549):

- a. The *proterotheres* were the "horses" among the South American ungulates. They never became very

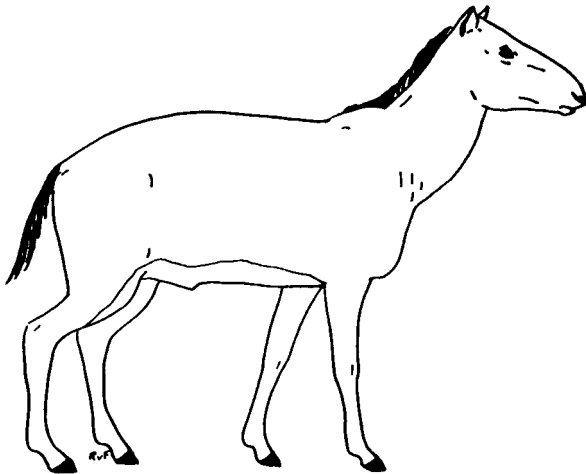


Figure 1. A three-toed South American "false horse," *Diadiaphorus*. In the restoration the tail of the animal was left relatively hairless to make it look less horse-like. Here the tail hair is partially "restored"—a possibility just as likely. (Redrawn by Ruth von Fange from Scott, 1962).

large, but some of them evolved in ways that were remarkably similar to horses, especially in the adaptations of the feet for running.

b. In a way the litopterns are easy for us to comprehend for they are more directly comparable to the hoofed mammals with which we are familiar. To put it another way, there were close parallelisms that make the litopterns seem to us like reasonably orthodox hoofed mammals.

c. In these Litopterna the skull is just as in the horses; the incisor teeth, the cheek teeth parallel to some degree the horses of the same age, and a molarization of premolars as in the horses.

d. The hind feet were especially horse-like; the ankle bone was similar to that of horses.

e. It seems reasonable to suppose, therefore, that the habits and the mode of life of these litopterna were similar to those of middle Tertiary horses in North America.

How Are Litopterns Described? They are very unlike a horse!

After reading all of the above, we are quite non-plussed to find that things are not at all as they seem. The same palaeontologists show another side of the coin:

a. It is totally different from them (the horses) in dentition. The dentition and the rest (except limbs) of the skeleton show, however, that instead of kinship, parallel evolution took place (Peyer, 1968, pp. 274-5).
b. The feet of three-toed forms were strikingly (if only superficially) similar to those of horses (Keast, 1972, p. 270).

c. *Thoatherium* is said to be more horselike than true horses. This pseudohorse was, however, comparatively unprogressive in other respects, for the cheek teeth were low crowned, and the carpus was poorly adapted for monodactyl running (Romer, 1966, p. 260).

We leave the final comments to Scott (1962), the authority who "removed" the Litopterna from the Perissodactyls and placed them into a separate order:
a. The horse-like Litopterna are a remarkable instance of convergence, for with all their resemblances, they

were not remotely related to the true horses, for they were not even perissodactyls (p. 257).

b. But as already suggested, the resemblances to horses are superficial, while the differences are fundamental (p. 562).

c. It must again be emphasized, however, that such likenesses to horses are superficial; the fundamental and significant characteristics are more primitive than in the most ancient of known perissodactyls (p. 566).

d. The members of this family (the Litopterna) are remarkable for the many and deceptive resemblances to the horses which they display (p. 561).

The Literature on the Litopterna

If indeed the litopterns are a most remarkable case of convergence, that is, the animals are very similar, but their ancestors were not, one would suppose that such a case would attract widespread attention in the scholarly world and a large number of studies. Such is far from the case, however. An extensive computer search of a number of scientific data bases brought forth very little—three journal titles in English and four in Spanish—but none of these were of any substance in dealing with theoretical issues supposedly involved. There does not appear to be any book in English or Spanish devoted entirely to the litopterns—a major surprise, nor does there seem to be a book about the horse and its apparent twin, the litopterns.

The litopterns are touched on in some expected places. Martin (1967) treats Pleistocene extinctions. This Yale University publication promised slight to moderate coverage of South America. Litopterna is misspelled as Liptoterna several times in the text, and only the more camel-like varieties are briefly noted. The author failed to list the order in the index. Similarly, Peyer's (1968) authoritative reference on animal dentition carries the same misspelling of Litopterna, and at least one of the brief references to this order is omitted from the index.

It seems more than passing strange that there is a great curtain of silence around this problem described to be of great significance for the philosophy of evolution, full of far-reaching consequences, and supposedly the subject of much debate.

A Lesson in Taxonomy

On the surface it would appear that a good deal of soul-searching by leading experts went into the decision to remove the litopterns from among the horses. It is important to let palaeontologists describe how decisions are made about difficult classification problems. Certainly the average reader at this point would not have a clear picture on why the separation took place.

Just how does the taxonomist do the extraordinarily difficult task of classifying animals when part of the animal resembles one kind of creature while other features are much like a completely different kind? Our first insight comes from Simpson (1961) in the introduction to his pioneering book on animal taxonomy, where he quotes A. J. Cain:

Is it not extraordinary that young taxonomists are trained like performing monkeys, almost wholly by imitation, and that in only the rarest cases are they given any instruction in taxonomic theory?

The following material illustrates the state of such theory as it applies to the horse. It will quickly become apparent that we are dealing with quite fragile beliefs used in an *ad hoc* manner with respect to the horse at least, rather than demonstrable principles, and that one belief may contradict another, thus forcing an arbitrary choice in the direction chosen by the taxonomist. Here are some of the beliefs of the taxonomist:

a. The splitters versus lumpers beliefs. The first problem that should be mentioned is the almost legendary one of the lumpers versus the splitters. Lumpers focus on the remarkable degree of variety found within species and are sparing about identifying "new" species. Splitters tend to magnify minor differences among specimens and clutter the field with an inordinate number of new species. Most of the outraged comments on the subject appear to come from the lumpers, and no satisfactory solution is in sight, especially with the fossil record which often must deal with fragmentary remains. One of the best examples of the work of the splitters is the human fossil record where every new fragment is hailed before the media as a sensational new breakthrough, and of course a new species. Fix (1984, p. xxv) has observed that such news conferences tend to be held just before new funding is requested from foundations and other donors.

b. The embryo belief. It is believed that embryos represent or repeat ancestral adult structure, thus showing the evolutionary stages through which a species went (Simpson, 1961, p. 87). It is well known that this strange belief, known as recapitulation theory*, still persists in high places among evolutionists, however, even though leading evolutionists have discarded the idea long ago.

c. The subjectivity belief. Simpson (1961, p. 119) states that it is virtually impossible to be completely nonarbitrary for any taxonomy other than species, and frequently also for species, but he declares that there is nothing wrong with being arbitrary in the practice of an art, including the art of classification.

d. The evolutionary taxonomy belief. How does one decide which resemblances among animals count and which ones do not? According to Simpson (1961, p. 85), the only way is by the use of evolutionary taxonomy. The taxonomist creates a canvas of who the ancestor was, and the changes that must have occurred through the ages for the modern form to evolve. Whatever fits this totally speculative blueprint is taken as a homology—a resemblance that counts. For reasons not made clear, some creatures apparently suffer from arrested development. Thus the rhinoceros, and still more the tapir, are more 'primitive' with respect to their toe development, or closer to the supposed ancestor than the horse. If little or no change is evident between modern forms and ancient fossils, such occurrences are tagged as living fossils—an undefined evasion of the issue.

e. The anatomy belief. In proportions and general appearance, the three families have little in common (tapirs, rhinoceroses, horses); but so like in anatomy are

they that placing all in the same order is obviously the only course to take (Scott, 1962, p. 395).

f. The teeth versus skeleton belief. The classification adopted will depend upon the relative importance given to the teeth, on the one hand, and the skeleton, on the other. For example, we are told that the dentition of the Litopterna shows that these extinct mammals of the South American Tertiary cannot be related to the horses, despite notably horse-like fore and hind limbs (Scott, 1962, p. 397).

g. The tooth belief. Taxonomists believe mammals originally had 44 teeth but through specialization over a long period of time, the number was reduced. Along that line of thinking Branson (1952, p. 451) suggests that man also fits this pattern when he says that man has 32 teeth and is somewhat specialized in that respect.

h. The toe beliefs. The number of digits on each foot of those belonging to the order of Perissodactyla is usually odd, one or three, but may be four, as in the front foot of the tapir. See Figure 2. No five-toed perissodactyl has yet been found (Scott, 1962, p. 395). There is the belief in a principle that many animals originally possessed five digits—a primitive stage—and that through evolutionary processes the number was reduced, for example, to a perfect one in the case of the horses. Thus we have Branson (1952, p. 451) reflecting this belief when he states that man is primitive in respect to number of toes. He does not discuss man's fingers. With respect to the horses, the first toe is already 'missing' when the story begins with *Eohippus*, and the fourth toe of the front feet was only a temporary stage between five and three (Branson, 1952, p. 456). Thus the tapir with four toes on the front feet today and the other orders of animals with the same pattern may be expected to drop off the fourth toes momentarily. Simpson (1961, p. 86) extends this belief by stating that the common ancestor of horses, tapirs, and rhinoceroses must have had four toes on front feet and three on hind feet, and he uses *Eohippus* and tapirs as his proof.

i. Time belief. There is a belief that *Hipparion* (three-toed horse) became extinct three million years ago and was replaced about two million years ago by *Equus*, a true horse. Johanson (1981, pp. 176-7) relates with some relish that his rivals, the Leakeys, are in trouble trying to establish a date of three million years for a 'human' fossil find when it was found with two-million year old *Equus* teeth. "You can't turn a three-million year old *Hipparion* into a two-million year old *Equus* just because you want to." Curiously, this belief and the famous pig studies of Basil Cooke supersede any K-Ar dates in Africa in establishing dates for the so-called human fossils found there.

j. Uniformitarian belief. Wright (1980, p. 827) comments on a gradualistic view of evolutionary change, for example, that in the lineage of horses from *Eohippus* to a modern form, the average change in size was .0001 inch per thousand years.

k. Environment beliefs. South American ungulates (the litopterns) illustrate very nicely the close correlation between animals and their environments, and indicates how similar environmental conditions will lead, by genetic processes, to the evolution of remarkably similar animals, quite unrelated except through their very remote ancestors (Colbert, 1967, p. 338).

*Editor's note: Previous Quarterly articles on the recapitulation theory that may be of interest to readers—Rusch, Sr., W. H. 1969. Ontogeny recapitulates phylogeny. *CRSQ* 6:27-34; Wolfrom, G. W. 1975. Perpetuation of the recapitulation myth. *CRSQ* 11:198-201; Lammerts W. E. 1988. Article review. *CRSQ* 25:147

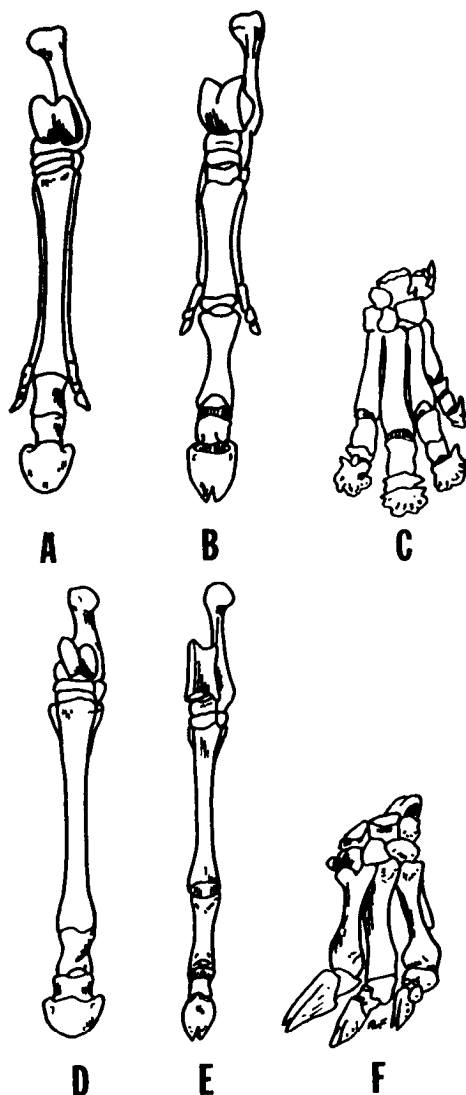


Figure 2. Which two feet do not belong to the Order of Perissodactyla that includes the horses? Answer: B and E are litopterns, supposedly not remotely related to the horses. All the rest are from the same Order of Perissodactyla: A and D are horses; C is a tapir; and F is a "clawed" horse, the chalicotherium. (Redrawn by Ruth von Fange from Colbert, 1967; and Scott, 1962).

1. Geography beliefs. Environment beliefs are offset by beliefs related to geography. With or without evidence, taxonomists invoke land bridges, mountain or other barriers, drowned lands forming an island, such as South America at times, in order to attempt to explain the fossil record. Palaeontologists believe they have located the point of origin where a type of animal evolved, and that similar creatures found in other parts of the world migrated from the place of origin. There is considerable aversion to suggesting two or more evolutions of the same species, despite the environment belief noted above. We are familiar with such declarations, for example, that man evolved in East Africa, the horse originated in Nebraska and/or Wyoming, and the litopterns originated in South America. Such statements are of course based on where the fossils have been found along with estimates of their age. Scott (1962, p. 255, 406) observed that geography is difficult

to apply because of the uncertainty concerning the manner in which the evolutionary process operates. While it is easy to announce that many genera and families of mammals travelled in both directions across the Bering land bridge, Scott speaks of unexplained facts and the difficulty to understand absence of crucial fossils to support such beliefs. The above by no means exhausts the list of beliefs which taxonomists use for explanation and classification. What is missing from these principles is any logical, scientific method of choosing among them. Taxonomists sometimes acknowledge the hazards of their profession, although such uncertainty is seldom alluded to in texts and popular literature on the subject. Scott (1962, p. 254) rightly says that ancestor charting:

is the most inexact of the sciences, because it has such a large subjective element and depends so much upon the judgment of the individual naturalist. It is for this reason that palaeontologists differ so often and so radically in the answers which they give to phylogenetic questions, for their fundamental preconceptions are so irreconcilable, that one regards as quite impossible what another believes to be usual and normal.

It seems appropriate to give several examples of the kinds of difficulties which taxonomists have identified in their work.

a. Simpson (1961, p. 84): Horses and rhinoceroses are evidently related in some degree. On the other hand horses differ from rhinoceroses quite strikingly in having only one toe and in numerous other respects, such as thinner and more hairy skins. In those respects and some other characteristics rhinoceroses resemble hippopotamuses more than they do horses.

b. Scott (1962, p. 431): There is a problem of expected but missing fossils. Tapirs lived in the Pleistocene along the Pacific and in the eastern forests, but apparently not on the Great Plains. There is not a half-complete skeleton between the Eocene and Pleistocene, yet they must have been plentiful to persist to the present.

c. Scott (1962, p. 114) points up the difficulty of relating fossils from South America to other regions. For example, he notes that no Paleocene deposits, marine or continental, have been found in any part of South America, and that most of the tertiary formations in South America are little more than names. Only one formation, the Patagonian, is thought to relate directly with a formation in the northern hemisphere.

d. Dentition—tooth structure—has received much comment. Peyer (1968, p. 317-8) observed that the significance of tooth characteristics in determining relations among animals has been differently interpreted in the course of time. Many assignments of groupings based on dentitional features have proved to be untenable. Peyer then gives examples where animals with similar teeth are placed in different orders, while some with different dentition go into the same order. Limbs too may follow the same pattern in classification. He cautions that the obviously great significance of odontological features should not lead to their one-sided over-evaluation. It should always be remembered that dentitional characteristics represent only one aspect of an animal's structure. And Scott (1962, p. 397) chimes in that too exclusive a depend-

ence upon the dentition has more than once led to unfortunate error.

How Does Scott Dispose of the Unwanted *Litopterna*?

As we observed earlier, Scott (1910) describes the classification of the *Litopterna* as a problem of great significance for the philosophy of evolution and that his solution has far-reaching consequences. Thus it comes as a surprise to find his pivotal monograph in storage in one of the great research libraries of the world, the University of Michigan, and to note that apparently no one had ever opened it or checked it out during the 77-year period it lay in the library after it was received as a gift from Princeton University. Further, the writer assigned to write the monograph died, and the actual author, Scott, devoted all his time in the La Plata Museum in Argentina studying other groups of Santa Cruz fossils. Finally, he states that his analysis was done without any detailed stratigraphical knowledge of the formations or any record of where the specimens were located in the strata. Such comments are hardly the foundation for such a pivotal work.

Scott (1910, pp. 5-8) devotes only several pages explaining why he concluded that the *Litopterna* should be placed into a separate order, and he lists some differences of dentition and skeletal remains. One could ask why he does not do the same with the tapir, rhinoceros, and other forms in the *Perissodactyla* order which differ far more radically. He acknowledges that other taxonomists differ:

It is not surprising that students of the *Litopterna* should have reached opposite conclusions regarding the systematic position of the group, for this is merely another case of the oft recurring problem, as to how far certain resemblances are offset by differences of structure. The answer to this question is largely conditioned by the opinion which is held concerning the manner in which the evolutionary process acts, and how generally similar structures are acquired in unrelated, or distantly related groups.

As we shall see, it is no secret why Scott's conclusion was quickly and generally accepted without objection or even discussion at least in print.

There Are Problems and Questions

From the foregoing discussion, it seems reasonable to offer some comment about the horse and the *litoptern*—some of which has already been said by others in other contexts:

(1) The horse story is still based on old uniformitarian principles now repudiated by leading evolutionists, but if this outworn concept is replaced by punctuated equilibrium, which operates in such a way as to leave no fossils, then in terms of paleontology the new thinking rests on negative or missing evidence, which Darwin and many other evolutionists declared worthless (Fix, 1984, p. 169). The *litopterns* were removed from the order because they upset a now repudiated view of horse evolution.

(2) There is a troubling dishonesty in the way the showpiece of evolution is displayed in museums and in the texts. Many prominent authorities in evolution have declared the horse series to be fictional. No attempt is

made to sort fact from speculation, and contradictory evidence is ignored (Fix, 1984, p. 189).

(3) Hapgood (1970, p. 280-91) is one of numerous authors who has documented the obvious fact that the great fossil beds of North and South America, Siberia, Africa, India, and elsewhere, are the result of catastrophic events, much more recent than conventional thinking permits, yet interpretation invariably follows the old uniformitarian path of slow gradual change over many millions of years. Darwin observed in South America that extinct animal bones, when heated in the flame of a spirit-lamp, exhaled a very strong animal odor and even burned. Yet he could not draw the obvious conclusion, because in his thinking there would be no time for present-day species to evolve. Note how catastrophism is treated in Alaska by Hopkins (1967, pp. 266-7). The author states that unfortunately most of the Alaskan Pleistocene vertebrate material now in museums have no known stratigraphic context. Speculations about age are therefore based entirely upon the morphology of the skeletal material and upon assumptions of probable trends with time of changes in such critical dimensions as horn width and tooth size. We may add that such guesses are built on now discarded uniformitarian notions.

(4) While granting the great difficulties inherent in developing a taxonomy for animals, the arbitrary and subjective path taken in orders such as those including the horse and the *litopterns* should be fully confessed. It is easy to document such excesses where the grossest differences are shrugged off as irrelevant, while the most minute differences are taken as crucial. A *perissodactyl* may have claws—the *Chalicotheres*—but the *litoptern* described as more horselike than the horse “is not even a *perissodactyl*” when among other things it has a smooth spot on the heel.

(5) One kiss of death for the *litopterns* in South America was that the three-toed form lived after the one-toed form became extinct, according to interpretations of the time zones in which the fossils were found. This would have by itself made the assumed horse sequence untenable.

(6) Contradictory evidence is routinely discarded to make life possible for speculative animal family trees. As Scheele observes (1955, pp. 62, 80), it takes an expert to decide if horse bones are of recent vintage or 10,000 years old. In Africa, bones are collected which can scarcely be distinguished from fossil forms that are known from late Pliocene times, that is, up to at least several million years old according to the conventional time scale. Skeletal material has always been endangered, that is, destroyed, if it looks too modern. In many ways we can see that evolution is a theory that prohibits thinking rather than stimulates it.

(7) An isolated island is invoked for South America at times in an attempt to explain horse distribution or lack of it. Yet during the same periods other animals such as the giant sloth and the armored *glyptodont*—both extremely slow-moving creatures—apparently travelled freely between the two continents. The horse people do not talk with the sloth people. The possibility of land bridges between South America and Africa, much more recent than the conventional geological time table would allow, ought to be re-examined. This concept was argued by F. Ameghino who was the

discoverer and namer of the litopterns among his many other achievements in palaeontology (Cifelli, 1983, p. 42).

(8) It seems reasonable to conclude that if the horse story were done again from scratch by evolutionists today with no preconceptions other than their commitment to evolution, it would be very different than the fraudulent illustration with which we are confronted *ad nauseam*. Despite the tapir, which no one wants to deal with seriously, little *Hyracotherium* would not be misnamed *Eohippus* and it would be placed with the hyraxes and conies where it has always belonged. The litopterns would be placed with the horses where they belong, but of course another equally speculative horse ancestry would have been created by imaginative evolutionists, innocent of any substance. The creationist can see relationship among creatures, but the evolutionist can see relationship only in terms of supposed ancestry.

(9) The gross subjectivity of taxonomy is well illustrated by the horse/litoptern story and the many contradictory statements of taxonomists about the skeletal parts, ancestral forms, and the like. The statements given on why the litopterns are not horses are vague and unconvincing. It borders on the incredible that an authority would accuse the litopterns of practicing deception for looking so horselike. If the reasons are so obvious that litopterns are not horses, it ought to be possible to communicate this clearly in the professional literature. A careful examination of reconstructions and illustrations of the fossils is interesting. *Eohippus*, closely related to the hyrax or coney, and which bears no resemblance at all to the horse, is made to appear precisely like a miniature horse, while the litoptern branches particularly horselike are made to appear more like antelopes. Tails are made to look quite hairless, when structurally they are no different from that of the horse.

In this paper I have dealt with only a tiny aspect of the horse story. Much more remains to be sifted. But when Ruse (1982, p. 311) says, "It would be nice to see the Creationist take on the question of the horse, which is one of the best documented cases of evolutionary change," I suggest that Ruse ought to begin reading the evaluations of the horse written by his own colleagues. In this all too brief examination of the Litopterna and the horse, we have found nothing to contradict Cain. See Simpson's quote of A. J. Cain mentioned previously (p. 186-7).

In her book, *Out of Africa*, Dineson (1963, p. 121) gives a remarkable description of the behavior of natives in her area.

... I learned that the effect of a piece of news was many times magnified when it was imparted in writing ... but if a mistake was made in writing, which was often the case, as the Scribes were ignorant people, they would insist on construing it into some sense, they might wonder over it and discuss it, but they would believe the most absurd things rather than find fault with the written word.

Despite the fictional nature of horse evolution, believers cannot let go of the idea because every text and countless other materials endlessly and reassuringly repeat the fiction.

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MINISYMPOSIUM ON THE SPEED OF LIGHT—PART IV

THE ATOMIC CONSTANTS IN LIGHT OF CRITICISM

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Abstract

Criticisms to the arguments offered to support a recent decay in the speed of light are answered. The hypothesis is still a viable model for any young-earth discussion.

Introduction

The proposal that the speed of light, *c*, has undergone a decay with time has drawn a variety of

comments. After the critiques were formulated, a significant development has occurred. Apparently independently of our work, a Soviet scientist, V. S. Troitskii of the Radiophysical Research Institute in Gorky, U.S.S.R., has published an article (1987) in *Astrophysics and Space Science* 139:389-411 entitled

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“Physical Constants and Evolution of the Universe” coming to essentially the same conclusions as we did in our Report.** Among them was that c decay (hereafter designated CDK), and the associated variation in a number of atomic constants, explains such cosmic phenomena as the red-shift, the isotropy of the microwave background, the superluminal jets, and other superluminal observations. His treatment also set the CDK postulate on a sound footing with regard to General and Special Relativity.

One conclusion by Troitskii was that the initial value of c was of the order of 10,000 million times its current value. This was based on observations of the microwave background. Depending on which of the three most favored decay curves was followed, our Report suggested initial c values ranging from 10 million up to 100,000 million times c now if red-shift, microwave and radiometric data were to accord with theory. We are in substantial agreement with Troitskii on this point.

Troitskii indicated that it may take 10 years of observation of the constants to finally prove the point. However, he seemed unaware of the wealth of data accumulated over the last 300 years, that is included in our Report, which substantially confirms the CDK thesis. These data have been criticized by Gerald Aardsma, D. Russell Humphreys, Roy Holt and Robert H. Brown in CRSQ. This reply addresses the major issues they have raised point by point.

AARDSMA'S CRITIQUE (CRSQ 25:36-40)

Unusual Behavior of c

In criticizing the Report, Aardsma suggested that it was remarkable that experimental proof of the CDK theory has eluded us by just 30 years. However, as de Bray (1931) stated:

If the velocity of light is constant, how is it that, INVARIABLY, (his emphasis) new determinations give values which are lower than the last one obtained. . . There are twenty-two coincidences in favour of a decrease of the velocity of light, while there is not a single one against it . . .

This comment reveals that the very idea of CDK came from the experimental data itself, not any theoretical source.

Today, measurements of c by laser techniques have been locked into the atomic time-frame and so, as any changes in c move lock-step with the atomic system of measurement, no variation will be noted. However, the measurements of some associated constants are not so constrained, and since about 1975 have shown a three standard deviation change in the direction of increasing c (Cohen and Taylor, 1986; Petley *et al.*, 1987). If this trend is sustained, it would appear that CDK reached a minimum around 1975-1980 and is slowly increasing again.

Bevington Quote

The use of the Bevington (1969, p. 1) quote is misleading unless it is read beside his clarification. As it stands in the critique, it implies a gradual, one-sided asymptotic approach of the measured values of a physical quantity to their ‘correct’ value. However,

**The Report refers to ‘*The Atomic Constants, Light and Time*’ by Trevor Norman and Barry Setterfield.

Bevington (p. 10) makes it plain that some measurements of the quantity will be too large and others too small. They will approximate a normal distribution, with some measured values above, and others below, the true datum. That datum is then approached asymptotically from both above and below. The suggestion that the one-sided decay in c is merely the result of the normal behavior of physical measurement does not conform to Bevington or statistical theory.

CDK Registered by 16 Methods

Aardsma states that “. . . it is highly unlikely that 16 different experimental methods would all accidentally and independently conform to the same mathematical equation describing c decay, if c was constant.” I agree! That was the purpose of the analysis in the Report. Each of the 16 methods used to measure c did in fact register a statistical decay. Furthermore, in the many instances where the same equipment was used later, a lower value for c resulted. Generally, aberration measurements obtained at the Pulkova and Flower Observatories, as well as those conducted by the International Latitude Service each individually registered a decay, as did the whole suite of aberration measurements. As Aardsma states, this would be highly unlikely if c were constant. Coincidence fades as a possibility when confirmatory trends appear in 475 measurements of 11 other atomic quantities by 25 methods, as tabulated in the Report.

Uncertainties in Decay Rates

Aardsma points out that for all 163 c data, an uncertainty in the decay rate of ± 100 km/s per year would not be very ‘convincing,’ whereas ± 1 km/s per year would. He then gives the decay for all 163 c data as 38 ± 8 km/s per year. However, if the least squares procedure is followed (Bevington, 1969, pp. 104-5) the uncertainty is even smaller than that. I have corrected the Roemer point to read 292000 km/s for reasons outlined below. When this is done, all 163 c data yield a decay rate of 28.59 ± 0.0016 km/s per year. Analyses of data from each of the 16 methods of c measurement give similarly ‘convincing’ results.

The Data and Subjective Bias

It was suggested that subjective bias and misapplied analysis may yield unwarranted conclusions. He prefers the data to be analyzed as a whole. Yet Aardsma felt some difficulty with obviously aberrant values such as Cassini’s. One way of overcoming that difficulty is illustrated by the following procedure.

- a. 163 data, with uncertainties up to $\pm 23,000$ km/s:
Decay = 28.59 ± 0.0016 km/s per year, $r = -0.26$
- b. 134 data, with uncertainties less than 1000 km/s:
Decay = 8.46 ± 0.0018 km/s per year, $r = -0.60$
- c. 120 data, with uncertainties less than 300 km/s:
Decay = 5.06 ± 0.0023 km/s per year, $r = -0.47$
- d. 64 data, with uncertainties less than 100 km/s:
Decay = 7.34 ± 0.0032 km/s per year, $r = -0.61$

The low values of the correlation coefficient, r , reflect the large numbers of points involved. When the confidence in the decay trend is calculated for those r values, they are at 99.97% or higher. In each case the mean value of the data is significantly above c with

good confidence intervals. These results indicate the general nature of the decay trend, whether or not the aberrant values and various error margins are included. This surely overcomes any objection on the basis of subjective bias.

Seven Statistical Tests

The center-piece of Aardsma's critiques is his weighted analysis of the data. This was claimed to be necessary because of the wide range of uncertainties in the measurements. However, as shown above, it is possible to treat the data with respect to error bars without weighting.

I have used standard statistical procedures to analyze the c and other data. Firstly, analysis of data means broadly indicated CDK. Data means were usually significantly above c now for each method and for all 163 points. Newcomb in 1886 reported that the 'best' c value in 1740 was about 1% higher than that pertaining in 1880. Birge in 1941 conceded that the average c value obtained in the 1880's was in turn 100 km/s higher than that in 1940. These statements of observational fact confirm our data means analysis. Secondly, median analysis of all 163 points indicated that the hypothesis that the median value was equal to c now could be rejected at the 97% confidence level. Regardless of measurement error or date of observation the distribution of c values was significantly skewed. Thirdly, the Spearman-Rank test indicated that there was strong correlation with the date of observation for all 163 data as well as for data analyzed according to the 16 methods of observation.

Confident that these three non-parametric tests indicate CDK, we then applied the parametric tests. The least-squares linear fit and Students t -distribution were used as outlined in the Report with concordant CDK results. Analysis of residuals indicated a non-linear decay. Residuals reduced from 22,000 for an assumption of a constant c , to under 2000 for a curve fit (Malcolm, 1982). A final parametric test of the mean square successive difference performed on successive data or each third datum, produced high confidence intervals for CDK with time. Therefore, although the conclusions from Aardsma's weighted analysis differs, it can be unequivocally stated that seven major statistical tests all favor CDK. Trace the reason why the weighted analysis used by Aardsma deviates from the united testimony of the other tests.

Weighting Procedures

Weighting may be done by a variety of criteria. One method is to weight data according to the number of observations in determining each point. When this is done for all 163 points, a decay of 11.26 ± 0.000095 km/s per year results with $r = -0.276$ and a confidence in the decay correlation of 99.97%. Another method combines the number of observations with the inverse of the uncertainty (error bars). On this basis, all 163 data yield a decay of 1.12 ± 0.00026 km/s per year with $r = -0.284$ and a confidence in the decay correlation above 99%.

Inverse Square Error Weighting and r

The criterion used by Aardsma for weighting is the inverse square of the uncertainty (error bars). For all 163 data, this gives a value for the correlation coefficient $r = -0.00375$ with a low confidence interval. At

best this is about 1/73 of the value for r by the above methods. All the above methods give reasonably concordant values for r . However, weighting by the inverse square of the error produces a straight line that plainly does not fit the data. If it did fit, r would have been significantly higher and the confidence interval would indicate a better correlation as they are readily obtainable by other methods. On that basis alone, this form of weighting is inappropriate for the data set.

This method will show a decay trend if it is linear. Data with small error bars, that count highly in the analysis, would still follow a line with the same slope as earlier data with large error bars. Here it does not matter that the earlier points are virtually ignored by the analysis. The inverse square weighting still puts a straight line through all data and a good value of r will result.

Non-Linear Decay and Weighting

To see what this weighting procedure has done to the CDK data, take the Table 11 data with the best 57 values in the Report. From about 1972 onward, the data had a mean error of about ± 0.004 km/s, while the mean error from 1783 to 1927 was ± 82 km/s. The inverse square weighting means that data from 1783 to 1927 were each counted 0.00015 times, while the post 1972 values were counted 62,500 times each. In other words, data prior to 1927 were essentially excluded from the analysis. All the analysis is doing is finding the decay trend post 1972. It is for this reason that Aardsma's weighted analysis is at such variance with the results from other statistical methods.

Furthermore, his method can be shown to be generally inappropriate to apply to any non-linear function, as the form of CDK clearly is. A consideration of data conforming to a physical law $y = x^2$ illustrates this well in the range $x = -10$ to $x = 0$. Allow initial measurements with large uncertainties near $x = -10$ to increase in precision towards $x = 0$ as instrumentation becomes more sophisticated with time. The inverse square error weighting technique applied to this data set will result in the conclusion that there is little discernible change in the measured quantity as only the values near $x = 0$ count at all. In fact, the quantity has changed considerably. The situation is the same as with CDK.

Radioactive Intensities

On the approach adopted in our Report, radiation has an effective mass, m , which, for conservation of energy to hold, is proportional to $1/G$. Consequently, radiation momenta, mc , are proportional to $1/c$. Thus radiation has an intrinsically lower momentum when c is higher. This is one major point.

Assume that a light or heat source emits one photon per second. Place a receptor of one square centimeter area in the path of the beam. The momentum absorbed per square centimeter per second by the receptor is the radiation pressure, W (French, 1959, p. 41). It is apparent that W is proportional to $1/c$ in a CDK scenario. However, W also expresses the energy density of radiation (French, p. 40). Thus with higher c , emitted photons have a lower energy density because of their lower momentum. If c were 10 times greater in the past, a photon's energy density would be 1/10th of one today. It would require 10 photons to give the same

energy density as one photon today. In the case of radioactive decay, 10 disintegrations then would be required to give the equivalent of one disintegration today.

If radiation is considered as a wave form, the maximum amplitude, E , of a wave is related to W by $W = \epsilon E^2 / 8\pi$ (French, p. 40). When c is higher, W is smaller, proportional to $1/c$. Thus the wave amplitudes are smaller, reflecting the lower energy density or intrinsic momentum of the wave. A simple interference experiment shows what happens when waves interfere destructively, so that the resultant amplitude is zero. At that point no light is seen: the intensity and wave energy is zero. When the resultant amplitude is a maximum, the intensity and wave energy is at a maximum. Consequently, when c is high and the wave amplitude and energy density is low, the resultant intensity will also be low. Ten waves would have to pass in one second to give the same intensity as one wave today if c were 10 times greater then. Radioactivity would therefore be less dangerous when c was higher.

What Happens to the Kinetic Energy?

Far from being 'unnecessarily abstruse' the above argument is straightforward. However, Aardsma appears to have a problem relating to the kinetic energy involved in radioactive decay and similar processes. He states that:

If the decay rate was 10 times higher, the heat production in the earth would also be 10 times higher . . . These conclusions are only dependent on the conservation of energy: the details of energy transport are irrelevant.

In the CDK scenario, kinetic energy is indeed conserved. What happens to it?

In the first place a distinction must be made between the intensity of radiation and its kinetic energy. The photoelectric effect illustrates this well. A very low intensity photon beam, barely discernible, still has sufficient kinetic energy to propel electrons from the metal and activate the circuit. With a higher value for c , each photon or wave has an intrinsically lower intensity as outlined above. Its kinetic energy, however, remains invariant with CDK. One photon at higher c still has the same kinetic energy needed to activate a photoelectric circuit.

There are two points that have eluded Aardsma. When c is 10 times higher and that photoelectron is ejected from the circuit, the electron velocity is also 10 times greater. When an alpha particle, whose kinetic energy is constant for all c , is stopped in the atomic lattice, the heat radiation produced (a form of light) also travels 10 times faster, but its intensity is 10 times lower. The constancy of the kinetic energy thereby ensures that all velocities are proportional to c , even though intensities are proportional to $1/c$. Ten alpha particles are thereby needed to produce the same radiation intensity as one alpha particle today, but the alpha particle and the radiation each have a velocity of 10 times today's value.

Thermal Conductivity and CDK

There is no heat build-up under these circumstances because of the second point that has apparently eluded

Aardsma. Refer to equation 42 (p. 57) of our Report where the thermal conductivity, K , of a substance is proportional to c . Consequently, when c is 10 times higher, and radiation velocities are 10 times greater the heat conduction rate also increases by a factor of 10. In other words there is no heat trap. When all this is coupled with the lower intensities, the temperature rise from the decay of 10 radioactive atoms will be the same as the temperature increase from one radioactive atom today. Far from being 'an enormous problem,' higher values of c seem to be a distinct advantage.

Radiocarbon

The most accurate curve for the historical period discussed by the critique in this section is the cosec² form. The other two forms presented in the Report give better results with distant astronomical phenomena at earlier times. It has been shown (Setterfield, 1986) that, from 1 AD to the present, theory and data agree well. Furthermore, the period covered by the Dead Sea scrolls has a variation due to CDK of 178 years which is well covered by the dating error ± 200 years quoted by Aardsma.

Taking the era prior to Sesostris III near 1900 BC, Pearson *et al.* (1986) give a radiocarbon age of 3579 years before present (BP). Using the cosec² form in the Report we get 5765 years BP. This is from the C-14 observations by 0.39 half-lives (if the Libby standard of 5568 years is accepted). This contrasts with 4000 half-lives suggested in the critique.

However, why is there a discrepancy with CDK theory? For theory to come into accord with observation, living systems must have been in equilibrium with an atmosphere and oceans containing 1.31 times the C-14 available today. The reason is not hard to find. About 1900 BC, the above equation suggests that c had about three times its current value. From the astronomical side of CDK theory, this suggests that the solar neutron flux (which produces C-14 in our atmosphere) was about three times its present level. The C-14/C-12 ratio would thus be systematically higher than now. The greater preponderance of C-14 would result in the observed discrepancy. The extreme results proposed in the critique are shown to be erroneous when all CDK factors are taken into account.

Conclusion

Dr. Aardsma's conclusion that there is a lack of positive, historical experimental evidence for CDK is itself negated by a quote from Dorsey (1944). Despite being opposed to CDK he was forced to concede that:

As is well known to those acquainted with the several determinations of the velocity of light, the definitive values successively reported . . . have, in general, decreased monotonously from Cornu's 300.4 megameters per second in 1874 to Anderson's 299.776 in 1940 . . .

In fact, even Dorsey's reworking of the original data left c values generally above those currently prevailing. In view of this and all the facts outlined above, it would seem that the CDK proposal rests on a far more solid foundation than Aardsma claims.

HUMPHREYS' CRITIQUE (CRSQ 25:40-5)

The Roemer and Cassini Values

I am grateful that Humphreys has clarified the situation with Goldstein and the Roemer value. I have noted the new value of 292,000 km/s above. The latest published results by Goldstein (1975) indicate a mean residual of 31.5 seconds which is an uncertainty of $\pm 18,000$ k/s over the earth orbit radius. This more than cancels any proposed CDK. However, when analysis is done with this new value, there is no substantial difference to our conclusions. For all 163 data a decay of 28.59 ± 0.0016 km/s per year results. If the Roemer point or the Cassini point are omitted individually, the remaining 162 values still give a decay. If both are omitted the decay for 161 values is 6.59 ± 0.0018 km/s per year.

Cassini's value provides further ammunition for Humphreys criticism. He states that "If an analysis like Goldstein's were done on Cassini's original data, a much more accurate value might be recovered." I agree, and in fact an analysis has been done by Delambre. It contained about 100 individual observations, including all the Roemer/Picard and Cassini data, from 1667 up to 1809 for a mean date of 1738. His treatment gave a c value to an accuracy of 0.1 seconds by this method. The result of $303,320 \pm 65$ km/s was regarded as definitive at the time of publication. This should allay any concerns about these early values. Our Report is just the tip of the iceberg. A much more extensive document is partly written in which each experiment is discussed with diagrams, as Humphreys suggested.

'The Missing Coefficient'

Humphreys makes some capital out of the omission of the correlation coefficient for the 63 aberration values. He correctly notes that the value of $r = -0.409$ which he considers "... a rather low value. Is it just coincidence that the single unreported coefficient is the one least supportive of the authors' hypothesis?" This contention is demolished when one considers that the significance of the value of r is also determined by the number of points, in this case 63. If there were only six points, he would be justified in his criticism. The test of a good r value comes from its confidence interval. If there were six points $r = -0.409$ has confidence of 78.9%. For 63 points the confidence is 99.95% that the decay trend of 4.83 ± 0.0038 km/s per year is significant.

'A Misleading Zero'

In the Report it is noted at the bottom of Table 3 that the aberration constant is disputed. There is a systematic error that involves different techniques in Europe to those at Washington and is enhanced by the large number of twilight observations at Pulkova. The details are too lengthy to discuss here. The Washington aberration constant is now accepted as definitive resulting in systematically low c values from the European observations. When this error is corrected for, many values of c in Table 3 increase by about 230 km/s, with some higher. This correction overcomes the perceived problem with Figure I. The zero takes account of this systematic error and is thus not misleading, nor is the decay trend spurious, and the vast majority of values in Table 3 are then above c.

The aberration values are very useful. The majority of Russian observations were done on the same instrument with the same errors. They display the decay trend well, which cannot be due to instrumental effects nor error reduction. The same comments apply to the results obtained from the Flower Observatory as well as the International Latitude Service. All give decay rates above 4 km/s per year. Far from being 'misleading,' the aberration values only serve to confirm the decay hypothesis.

'Bias in the Analysis'?

Humphreys calls upon systematic error to account for the trend downwards with time in the c observations. That explanation fades when it is realized that 16 different methods of c measurement all had to favor the same bias instead of the opposite one—an increase. The explanation recedes into oblivion when it is realized that a further 25 methods used to measure an additional 11 physical quantities also had to have a bias that favored CDK. That makes 41 methods acting in a united fashion. How much coincidence am I expected to accept? Contrary to suggestion in this paragraph, appropriate weighted fits have been done. The most consistent results are weights according to the number of observations per experiment.

'A Suppressed Explanation'?

Humphreys statements as well as those quoted from Dorsey are largely negated by actual history. It is also negated by the aberration series of measurements run over 200 years on essentially the same equipment with unchanged accuracy which recorded the decay. Furthermore, around the early part of this century, c values were successively dropping with each measurement. This can hardly be attributed to subjective bias and preconceived notions as to what results should be obtained. If Michelson had preconceived notions as to what he should find as the 'correct' value, then why did his data run 299,910 km/s in 1879.5, then drop to 299,853 km/s in 1882.8, followed by another plunge to 299,802 km/s in 1924.6 and final decay to 299,798 km/s in 1926.5? Dorsey's explanation is not sufficient.

At the same time as Michelson was working on his experiment in 1882, so was Newcomb independently. Furthermore, unknown to either of them, Nyren was assessing final results from a different method and concluded in 1883 that c was 299,850 km/s. Newcomb obtained 299,860 km/s in 1882.7 and Michelson had 299,853 km/s in 1882.8. The mean of these three values, 299,854 km/s lies above c now by 61.8 km/s even though the standard deviation of the three values is only ± 5 km/s.

If the quote from Dorsey is valid in attributing CDK to "... (1) the observer's exaggerated opinion of the accuracy of his own work, ..." then how is it that a standard deviation of only ± 5 km/s results when three independent experimenters work at the same time and get essentially the same value (significantly above c now) by two different methods? Furthermore, if Dorsey's next statement that the trend was the result of "... (2) his (the observer's) inability to avoid being influenced in some measure by his preconceived opinion as to what he should find, ..." how is it that neither knew of the other's result until some significant

time later? These quoted statements by Dorsey (1944) are neither in accord with history, nor is the example that is derived from them.

'General Remarks'

Humphreys appears irked by the fact that several references were repeated three times and states here that:

There are many other such redundancies, which tend to give the reader a mistaken impression that nearly 400 books and articles back up the monograph, instead of much fewer.

Note that reference 360 contains no less than 44 references under one heading to answer his inuendo. The only point of substance that remains is that concerning the rotating mass clock. This is discussed in the reply to Holt.

HOLT'S CRITIQUE (CRSQ 25:84-8.)

This falls naturally into two sections. The first deals with perceived rotation rates of macroscopic bodies under CDK, the second addresses the issue of measurement of pulsar periods.

Rotational Rates of Macroscopic Objects Under CDK

Both Holt and Humphreys had problems with this issue as it was not amplified sufficiently in our Report. Humphreys also found the terminology used in the Report unhelpful. I will clarify the concept in as simple language as possible.

A. Energy Conservation

To conserve energy in the $E = mc^2$, the rest-masses of atomic particles are proportional to $1/c^2$. Measurement of electron rest-masses are not in disagreement with this contention as Table 14, p. 32 of our Report shows. Notice that the heading IV (B) in the Report was Atomic Rest-Masses and (C) was The Atom and Planck's Constant. It is in this latter section that the conservation of kinetic energy is mentioned that Holt seizes. Note that the discussion was about atomic phenomena, not macroscopic or astronomical phenomena.

Therein lies the difference. Einstein's equation as applied to chemistry and nuclear physics is essentially dealing with mass conversion to energy at the atomic level, even when larger quantities are involved. Even in the sun, where a significant mass is changed into energy, the conversion occurs at the atomic particle level. The prime application of this equation is therefore to the atomic environment and associated light photons. As a consequence, atomic rest-masses that vary as $1/c^2$ require atomic particle velocities that vary as c to maintain kinetic energy.

B. Atomic Rest-Masses

Apart from Einstein's equation, the reason atomic particles vary in rest-mass compared with macroscopic objects is found in their electromagnetic nature which is typical of the whole atomic environment. The classical equation describing electron and proton rest-masses is given by Barnes (1986, pp. 23, 24, 27, 33) as

$$m = \mu q^2 / 6 \pi r \quad (1)$$

where q is the electric charge, r is the particle radius and μ is the permeability of free space. We note in our Report that q and r are invariant under CDK. Furthermore, from equation [1] on pp. 29, 30, it is shown that μ is proportional to $1/c^2$. From (1) above, when we measure varying atomic particle rest-masses we are essentially measuring the changing permeability of space. This is the only time-dependent term on the right hand side of equation (1). The $1/c^2$ variation in m is directly traceable to this cause. The permeability controls the way the atom behaves electromagnetically. As a consequence, since the rest-masses of atomic particles are electromagnetic in character, it should be no surprise that they vary with free space permeability.

In order to conserve energy within this electromagnetic environment of the atom with increasing μ as c decays, it becomes apparent that the electromagnetic mass increase must be coupled with a slower orbital speed for electrons and other particles. Consequently, the atomic clock ticks more slowly with CDK, and all atomic processes are slower, seen dynamically.

C. Consistency within each System—Variation without

However, no change is registered within the locked-in atomic system. Light always travels the same distance in one atomic second, rest-masses remain invariant, and radioactive decay rates are constant. It is only as we view the system from outside, in dynamical time, that any variation is noted. Conversely, if exterior phenomena are viewed from the atomic viewpoint, a change in behavior would be noted with time. Thus, orbital times for the earth or moon appear to be changing when measured by the slowing atomic clock when in fact they are dynamically invariant. A similar situation occurs for dynamical phenomena, such as Holt's rotating masses and Humphreys' rotating mass clock. Dynamically, we note no change in orbit or rotation times of objects in the solar system. Therefore, the macroscopic system governed by the dynamical clock is consistent within itself, just as the atomic system is. Only cross-comparisons show inconsistencies.

D. Where Two Systems Meet

Essentially we have two systems whereby mass is measured, the atomic and the dynamic. In the electromagnetic system of the atom, mass is determined in nuclear reactions from the equation $E = mc^2$, where m behaves in accord with (1). This is sometimes called the 'Q-value mass' (Dicke, 1960). On the other hand, in the dynamical system, mass is measured on a macroscopic scale. This is often referred to as inertial mass.

The question then arises: what happens when atomic masses measured by dynamical or inertial means, are compared with the same atomic masses measured by nuclear means within the atomic system? Historically, the calibration standard for both systems was the hydrogen atom with its single proton and electron. The atomic and dynamical masses (or Q-value and inertial masses) for the components of hydrogen would thus be expected to agree both by definition and calibration.

E. A Measured Discrepancy between the Two Systems

As the number of atomic particles increases for more complex atoms, a discrepancy proportional to μ should

be noted. A single particle, as in (1), has a Q-value mass or atomic mass proportional to $(1/\mu)$. Multiple systems with n particles in the nucleus should be discrepant by an $(n \cdot \mu)$ factor. We can therefore predict that the Q-value mass should show a progressive increase with nucleon number against the dynamical or inertial mass. In fact, this is exactly the case. Dicke (1960, Figure 1) has called attention to this discrepancy which was verified for elements up to atomic number 21, scandium. The data support our equation

$$\Delta(m - M) = K(n - 1)\mu \quad (2)$$

if M is the dynamical or inertial mass, m is the Q-value mass in the atomic system, both expressed in electron volts, n is the nucleon number, and μ is free-space permeability. Then equation 2 becomes:

$$m - kM\mu = K(n - 1)\mu \quad (3)$$

If it is desired to retain an electrical theory of gravity based on the atomic environment, as Barnes (1986) does, a 'gravitational permeability term, μ^* , is required to overcome the discrepancy between the two systems and maintain the observed dynamical consistency on a large scale. To this end, the Report places $M = m / \mu^*$, where μ^* is the coupling factor or gravitational permeability which follows from equation (3).

Measured Rotation Rates of Pulsars

A. Conceptual Error

With all of Holt's derivation in this section, he has made a conceptual error which negates his analysis. Consider a pulsar-to-earth scenario in which the emitted speed of light, c_e , is 10 times the received speed of light, c_r . The distance, L , between the two initial pulses, as the pulsar goes through one rotation, remains constant as they travel from pulsar to earth. Pulsar periods are taken to be invariant dynamically with CDK.

For the observer on earth using dynamical time, these initial two pulses will take 10 times as long to pass with the slower light-speed. As a result, the dynamical period for the pulsar will be registered as 10 times the actual dynamical period at the time of emission. However, all pulsar observations have been made using an atomic clock. This clock has been the astronomical standard since 1967, and the first pulsar (CP 1919) was discovered in 1968 (Audouze and Israel, 1985).

Now the atomic clock moves lock-step with the speed of light. In one atomic interval, light always travels the same distance. Consequently, if light were to cross a fixed distance, L , the atomic clock will always register the same period for the crossing time no matter what value c takes. When light travels 10 faster than now, the atomic clock would also tick 10 faster than now. Therefore, if we take the atomic interval needed for the distance L between the two pulses to pass the earthbound observer at the speed of light, it will always be the same as the atomic interval measured at the pulsar at the moment of emission.

Atomic clocks at the point of reception of the n th pulse will always register the same pulsar period as atomic clocks actually at the pulsar when the n th pulse was emitted, irrespective of CDK. Consequently, no

information about CDK can be gleaned from this part of the process. But there is an additional factor to consider as the pulsar period is largely invariant in dynamical time.

B. Pulsars Spin-Up Atomically with CDK

If c was 10 times faster initially, the distance L between the pulses would be 10 times greater than for pulses emitted now. This occurs since a light pulse would travel 10 times further before the next is emitted. The atomic clock thus registers 10 times the current period, even though the actual period in dynamical time was constant. With CDK the atomic period of a pulsar would lessen. There would be 10 ticks on the atomic clock at emission and reception for the initial pulses, and only one tick at emission and reception for the pulses emitted now. That is to say an apparent spin-up should occur with CDK as the pulsar period lessens atomically.

Some x-ray pulsars do in fact show a spin-up. The current explanation is that it occurs because of a transfer of material from a companion star. However, in several instances, no companion can be detected (Shaham, 1987, p. 35). Other x-ray pulsars show spin-up followed by a spin-down phase (White, 1988). Some, like PSR 1937 + 21(4), are almost stable with a virtually constant period atomically. The majority of pulsars show a consistent spin-down.

C. Further Complications

There is a natural tendency for pulsars to spin-down because of loss of energy through one form or another, even though the details of all processes are still poorly understood (White, 1988). Superimposed upon this spin-down, then, is a counter-trend, the spin-up due to CDK. As a result, what we observe in all instances is a net effect. In the case of PSR 1937 + 21, the two tendencies seem to virtually cancel. Some spin-up cases are complicated further by the accretion of material from a companion star which enhances their spin-up.

It is apparent that pulsars do not offer a 'clean' system whereby CDK may be checked. The above complications are compounded by the fact that we have only been observing pulsars for 20 years. This represents somewhat less than 20 years of CDK at the pulsars due to the slowing of light from distant sources. In the above example where c was initially 10 times c now, our 20 years of observations would represent two years of CDK at the source.

D. Results from CDK Curves

Pulsars all fall within a few kiloparsecs of our sun (Audouze and Israel, 1985). Assuming then a distance of 15,000 light years and a mean pulsar period of 0.7 seconds, some calculations can be done. The three most favored curves in the Report produce spin-up rates ranging from 1.44×10^{-12} to 2.38×10^{-12} s/s for this mean. Given the tentative nature of these curves, this result is of the same order of magnitude as the changes being observed. When any reasonable natural spin-down is subtracted from this value, a very realistic result is achieved. Holt's quandary disappears under these circumstances.

BROWN'S CRITIQUE (CRSQ 25:91-5)

The Missing Factor

Brown made a glaring statistical error in his paper. He omitted a $\sqrt{n}$ term from his computations, where n is the sample size. The $\sqrt{n}$ factor must be employed, as we did in the Report. (See Sharpe, 1984; Spiegel, 1972; Anon, 1977). This error by Brown negates all his assertions against CDK. If he corrected his error, his results would accord with ours and support the CDK propositions.

Summary

The only alternative to counter this observational evidence for CDK is to invoke unacceptable implications. This has been done by Aardsma, Humphreys, and Holt. One of the first of these, historically, was a supposed conflict with General Relativity. This was shown to be invalid by several authors. Troitskii (1987) reached a similar CDK conclusion to ours and set the CDK postulate on a sound footing with regard to General and Special Relativity.

The final issue raised against CDK that mainly quelled the debate for the last 45 years was the problem of atomic behavior (Birge, 1934). It took four years for the framework to be laid by Dirac (1937) that held the potential to overcome the objection. It was another 27 years before Kovalevsky (1965) stated the principle clearly. It was not until the advent of the atomic clock that experiment revealed that this clock could indeed behave in the way Birge had considered unacceptable (Van Flandern, 1984).

A wealth of theoretical, statistical, and observational evidence supports CDK. If any difficulty arises, the Birge example above suggests that the perceived problem should be examined in depth to resolve all matters rather than rejecting the CDK proposition.

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BOOK REVIEWS

Proceedings of the First International Conference on Creationism. Volumes I and II. 1986. Creation Science Fellowship. 362 Ashland Avenue, Pittsburgh, PA 15228. 193 and 253 pp. \$35.00.

Reviewed by Edmond W. Holroyd III*

The International Conference on Creationism was held in Pittsburgh, August 4-9, 1986, with the theme, "the age of the earth." It was divided into three sessions: basic, educational, and technical, with many of the presentations running simultaneously. Volume I covers the basic and educational sessions. Volume II covers the technical sessions and some additional topics, including two of the evening addresses. The evening sessions and most of the technical papers have already been summarized (Holroyd, 1987). All sessions were taped and the 60 tapes are available at \$5 each or \$265 for the full set. The technical and evening sessions were video taped in addition and the 19 tapes are available at \$35 each with discounts for multiple tapes up to \$490 for the full set. The video tapes will often

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contain images of color slides, overhead transparencies, and a motion picture which are not printed in the Proceedings. All tapes will record the actual discussions at the ends of the technical sessions. Only some of the questions and comments by invited reviewers (not necessarily creationists) and by conference attendees are included after each technical paper in volume II. The author's responses printed there may be different and more refined than those recorded on tape.

Both volumes are arranged alphabetically by first author. In this review the papers of Volume I will be regrouped by subject. Robert E. Walsh reviews the correct rules for interpreting Biblical text. He then uses the rules to show that the Hebrew word for "day" in Genesis 1 can only be correctly interpreted as a solar day of 24 hours. Jerry Bergman discusses the importance of time in the discussion of origins. He reviews dating systems, the insufficiency of even 4.6 billion years or the evolutionary processes, and the Biblical understandings of time. Donald E. Chittick reviews the historical development of modern science. He points out how time and the age of the earth became important issues. Though there have been

attempts to reconcile Biblical and evolutionary time scales, there are still numerous scientific evidences that require the rejection of the old-earth theories. John G. Read faults evolutionary explanations of “gaps” in the geological column, overthrusts without physical evidence, and radioactive dating systems. John N. Moore lists words and concepts that are misused in teaching concepts of past time.

Methods of dating geological phenomena were reviewed by several authors. Robert L. Whitelaw traces the spans of years from the crucifixion back to creation. He lists items of recorded history with Biblical chronology. He sees radioactive clocks as unreliable. Several geophysical features are identified as post-Flood. Steven A. Austin presents photographs of landforms and lake deposits resulting from the Mt. St. Helens explosion. Newly-formed catastrophic deposition and erosion patterns have the same appearance as formations elsewhere thought to be many thousands or millions of years old. Deposits in Spirit Lake illustrate the initial formation stages of polystrata petrified forests and of coal beds. Paul D. Ackerman uses the reasoning-judgment style on the viscosity of lunar rocks to show that lunar craters cannot be very old. William M. Overn, in an article with similarities to one in Volume II, shows that radioactive dating systems can be unreliable. He gives an example of rubidium-strontium dates being affected by mixing of rocks during molten periods. On the other hand, Kurt P. Wise criticizes the tendency for creationists to categorically reject the three standard geological dating techniques (superpositional stratigraphy, biostratigraphy, and radiometry) just because there are special cases in which they are flawed. He calls for careful comprehensive study of each of the techniques to properly establish the conditions of their validity.

Several papers examined geological structures for other topics of interest. The paper of John D. Morris on the Paluxy River tracks was summarized in Holroyd, 1987. Walter T. Brown, Jr., offers a model of pre-Flood earth structure in which a 1 km shell of hot water is sandwiched between upper and lower crustal rocks. The catastrophic conversion of this to today's structure may help explain numerous crustal features not adequately explained by uniformitarian theories. The paper of Clifford L. Burdick is included in Volume I but was not presented at the conference. It mentions some of the history of the exploration of the Grand Canyon. He attributes the carving of the Grand Canyon in a week's time to the rising of a dam (Kaibab Plateau) and then the catastrophic release of the waters of the resulting upstream lake through a crack in that dam. “Out-of-order” strata and fossil pollen are also mentioned.

Bernard E. Northrup lists evidence he has personally observed that indicate a glacial period after the Flood. These include the direct evidence of empty U-shaped valleys, moraines, tills, scratches, and polishes. Indirect evidence of abandoned shorelines of giant lakes, catastrophically carved canyons, and subsequent regional desiccation are also listed. Two papers discussed a pre-Flood vapor canopy. R. Russell Bixler identifies the Hebrew “mabbul” with the vapor canopy as distinct from other Hebrew words for various bodies of water. Larry Vardiman reviews basic atmospheric

processes and examines Biblical descriptions relating to the pre-Flood atmosphere. He then discusses the main elements of Dillow's conceptual and mathematical model of a vapor canopy, the collapse of which contributed to the Flood. Several papers dealt with biological issues. James L. Hall discusses the topics of “kind,” blood, human embryos and other subjects while maintaining that a literal interpretation of Scripture is possible and preferred. Lane P. Lester and Raymond G. Bohlin move from design in nature to genetics and information theory to “prototype” for the various kinds of living things. They call for a re-evaluation of classifications and suggest numerous criteria while noting that problems are likely to remain. Duane T. Gish reviews evidence that the early atmosphere was rich in oxygen, which would have been destructive to the molecules necessary for the evolutionary origin of life. Even the simpler molecules in a reducing environment would be too dilute to react and would be destroyed at rates faster than their production. Douglas Sumio Jue traces the biases and inaccuracies behind the supposed relationship between cranial capacity and intelligence. There is in fact a large gap between ape and human cranial capacities throughout the fossil record.

Several papers focused on the philosophy of science. Paul D. Ackerman lists three methods of handling evidence with respect to origins. The true scientific method cannot examine a non-repeatable event. Careful judgment is needed rather than the one-sided promotion of evolutionary scenarios. Apostolos Ch. Frangos distinguishes between proper scientific investigations and metaphysical explanations of natural phenomena. Both the evolution and creation models fail to meet the criteria for testable and falsifiable scientific theories. Both are based on metaphysical assumptions. Charles W. Lucas, Jr., proposes returning to where science diverged from established logical criteria and principles by rebuilding new unified theories that are better descriptions of truth and reality. He proposes treating subatomic particles as having finite sizes and elasticity rather than as being points and rigid, as assumed by present theories.

The rest of the papers address the teaching of creation and some legal aspects. Richard B. Bliss examines skills that are factors leading to excellence in science education. He questions the relationship of each skill with respect to the creation/evolution controversy. Ronald E. Johnson outlines the creationist features that are found in the curriculum of Accelerated Christian Education (A.C.E.). Dan I. Rodden proposes the Caleb Campaign in which a small local group of lay people are trained to write and speak about creation evidences mostly in local newspapers. Luther D. Sunderland presents two papers advocating a purely secular approach contrasting evolution with abrupt appearance of life forms. Teachers are already using his slides, filmstrip and book without, legal challenges. He points out that both are not truly scientific theories and then shows that the fossil record supports abrupt appearances. Robert Russell Melnick lists a variety of legal arguments that can be used in support of the creationist positions in society. A highlight of Volume I is Jerry Bergman's article documenting contemporary religious discrimination against crea-

tionists in academia. He calls for some precedent-setting court cases to alleviate such discrimination. In Melvin A. Cook's second paper he shows that dendrochronology calibration of radiocarbon dating techniques can be reinterpreted better according to a nonequilibrium model. The observed rate of radiocarbon production is 37 percent greater than the rate of decay. Graphs, equations, and tables are given to correct equilibrium model dates to dates of the nonequilibrium model, thereby bringing them into closer agreement with Biblical dates. Volume II, Section 4 contains additional topics, including the evening addresses by Gentry and Kennedy previously summarized. John J. Davis lists the theological inadequacies of the "Gap," "Day-Age," and theistic evolution theories. John W. DeVilbiss offers some additional discussion about the past and recent observations of the footprints in the Paluxy River. David R. McQueen examines the geological structure of the Southern Appalachian Mountains in terms of a 6000-year history. Hugh S. Powlison offers a circular periodic table of the elements based on symmetries. Volume II ends with a tribute to Byron C. Nelson whose books defended creationism a half century ago.

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A Brief History of Time: From the Big Bang to Black Holes, by Stephen W. Hawking. 1988. Bantam Books New York. 198 pages. \$18.95.

Reviewed by Donald B. DeYoung*

British author Stephen Hawking is perhaps the leading cosmologist of our day. He is an expert on the theories behind the Big Bang, black holes, and grand unified equations. With his world-wide reputation, his first book written on a popular level will be widely read. The author suffers from ALS, or Lou Gehrig's disease, a motor neuron disorder that confines him to a wheelchair. However, this affliction has in no way impeded his career in theoretical physics. Hawking's gifted mind races through complex derivations; he requires assistance only in writing his mental conclusions. He has been an inspiration to all who have seen his achievements in spite of a severe handicap. He is a devoted family man with a wife and three children.

The chapters have intriguing titles such as "Black Holes Ain't So Black" and "The Arrow of Time." The topics vary widely in their depth of discussion. Some can be easily read like a newspaper; other subjects such as negative energy, light cones, and cosmic strings are written on a more challenging level. By design, only one equation appears in the entire book, $E = mc^2$ (p. 20). Unfortunately, some of the abstract, technical topics simply cannot be explained clearly when not accompanied by their mathematical descriptions. Of the 32 figures, most are helpful but a few raise more questions than they answer. Still, the book is very worthwhile to glean for its rich assortment of ideas from physics and astronomy. Hawking discusses quantum mechanics and relativity, and predicts that their unification into a single elegant theory is imminent.

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Concerning black holes, he explains his theoretical discovery that these collapsed stars may emit radiation and gradually shrink away or evaporate with age. Also reviewed is the anthropic principle, the idea that the universe is especially designed or man's well-being. Instead of seeing creative design in this principle, Hawking takes the standard approach: "if [the universe] had been different, we would not be here!" (p. 125). That is, conditions "just happened" to be ideal for the evolution of life. Included in the book are short appendices on the lives of Newton, Galileo, and Einstein. Hawking paints an especially unkind picture of Isaac Newton. This is surprising since Hawking's present position at Cambridge University was once held by Newton himself. Hawking's discussion of Einstein is interesting since these two men share an ultrahigh level of theoretical thinking. Einstein's rejection of the Biblical idea of God is noted (p. 177). His concept of God seems to be synonymous with nature, a pantheistic view. One is left to wonder if Hawking shares Einstein's low opinion of Scripture and a personal Creator.

The book attempts to answer several basic questions (p. vi): Where did the universe come from? How and why did it begin? Will it come to an end, and if so, how? Sometimes, Hawking sounds very religious as he seeks to "know the mind of God" (p. 175). At other times he becomes sarcastic: if the universe does indeed "need a creator . . . who created him?" (p. 174). Concerning origins, Hawking supports the standard Big Bang theory. Although he often refers to God, his conclusions are not particularly honoring to the Creator. Theoretical presuppositions lead Hawking to propose that space and time form a four-dimensional universe with no singularities or boundaries (p. 173). This universe has no beginning or end because it continually expands and contracts. The resulting implication is that the universe must be completely self-contained and unaffected by anything outside itself, including God (p. 136). Hawking concludes that God had "no freedom at all to choose initial conditions" (p. 174). Carl Sagan gloats over this arbitrary conclusion in his preface to the book, describing the universe as existing without "beginning or end in time, and [with] nothing for the Creator to do" (p. x).

The book ends with a helpful glossary of terms and an index. There is no mention of many current topics in astronomy such as missing solar neutrinos, red shift controversies, and Supernova 87. The book is good reading, but it certainly is not friendly to the Creation cause. Hawking's work shows the depth and power of theoretical science. How closely the resulting theories mirror reality remains open to question.

Men of Science, Men of God; Great Scientists of the Past Who Believed the Bible by Henry M. Morris. 1988 (Revised Edition) Master Books. El Cajon, CA. 107 pages. \$4.95.

Reviewed by Emmett L. Williams*

Often the impression given in the popular "media" is that most scientists do not believe in a higher Being particularly a Creator and that the better scientists are practical atheists. The Christian viewpoint or belief in

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the supernatural by past and present scientists is often not known and if it is, the information is suppressed or ignored. Dr. Morris tries to remedy the situation by presenting brief statements about many giants in the world of science and revealing their beliefs in God. This is a much needed work and should be in all school libraries.

Introductory material is provided to lead into the actual biographies. A final section summarizes the modern revival of creationism. Some of the 108 scientists discussed by the author are Kepler, Huygens, Newton, Linnaeus, Faraday, Cuvier, Maury, Mendel, Pasteur, Carver and von Braun. This book is well worth an evening of study. The reviewer challenges the reader to do an in-depth study on one of these men from a creationist perspective, an excellent means to combat the historical revisionism of today's materialists.

The Rise of Evolutionism: The Escape from God, by David Herbert. 1987. Hersil Publishing, London, Ontario, Canada. 47 pages. \$6.00 Canadian.

Reviewed by Clifford L. Lillo*

This is a book of revelation vs. reason and the author uses those two words repeatedly in the process of showing that each is the antithesis of the other.

There are three fundamental questions to which man has long sought answers:

Where did we come from?

Why are we here?

Where are we going?

Herbert states that the essential purpose of Revelation from God is to provide answers to those questions. "Whether it be Christianity, Judaism or Islam, . . . there is a Sovereign God who desires to reveal Himself. He, . . . has given a written record [the Bible] . . ." (p. 3). In contrast, human reason also attempts to provide answers, but must do so without acknowledging a divine Source.

Evolutionism was a known theory before the time of Darwin. Herbert says that

It is commonly thought by many, especially those of the scientific community, that the rise of evolutionism occurred in Darwin's lifetime because it was only at this time that sufficient scientific data had been found to warrant its acceptance. (p. 7)

He contends that it was not because of scientific evidence, but rather it was because of a change in the world view or religious base of Western society.

Herbert says that in the time period between the 16th and 18th centuries, "there was an ever-increasing growth in rationalism or dependence on human reason in Europe. Historians have termed this rise of rationalism as the Age of Reason or the Enlightenment . . . In other words . . ., a new world view emerged called Naturalism." (p. 8)

According to the author? France "adopted the rationalistic philosophy of René Descartes (1596-1650)" (p. 30), and "Unlike in Britain the Bible was never revered as the Word of the living God." (p. 30) Herbert continues, "When it was put under the scrutiny of Reason, the Scriptures were deemed to be spurious and mythical." (p. 30)

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In England,

The writings of John Locke (1632-1704), an influential philosopher of both political science and psychology, can best typify the view of how the Bible must be subjected to reason . . . In his most famous work, *An Essay Concerning Human Understanding* (1690), he stated most clearly: "Reason must be our last judge and guide in everything." (p. 28)

To this, Herbert adds, "Thus Reason became the final arbiter of all truth, including the Bible." (p. 28)

In any discussion of the effect of reason on the acceptance of naturalism, the activities of Thomas Paine must be included. Herbert says, "Thomas Paine (1737-1809), an Englishman by birth, . . . wrote the most vitriolic attack on the Bible. In his book, *Age of Reason* (Part I: 1793; Part II: 1795), his intention was to demonstrate once for all that the Scriptures were of human not divine origin." (p. 34)

After describing the contributions of several other prominent writers of the 18th and 19th centuries, Herbert concludes: "By 1859 [date of publication of Darwin's *Origin of Species*], the naturalistic world view based solely on Reason came to the fore and has since gained in its influence until today it pervades every aspect of our society." (p. 43)

Herbert does credit the scientific work of Boyle, Newton, Lamarck, James Hutton, and Charles Lyell for contributing to the acceptance of evolutionism, but says that the primary factor was the change in world view.

God and Evolution by R. J. Berry. 1988. Hodder. 189 pages. £6.95.

Reviewed by David C. C. Watson*

When a new history of the warfare of science with theology is written for the 20th century, this book may well be seen as the last *cri de coeur* from an endangered species—the Darwinian "evangelical." One can feel some sympathy for Professor Berry (RB). Here is a man whose whole career is based on the scientific consensus about origins. He has been repeatedly told that Darwin is the Newton of biology. In his 20's Sir Julian Huxley was vociferating that "Evolution must now be considered no longer a theory, but a fact." Now, 30 years later, it must be disturbing, at least, for RB to see a tidal wave of new books which threaten to shatter that confidence—titles like *Darwin Re-tried* (1971), Macbeth; *Where Darwin went Wrong* (1982), Hitching; *The Great Evolution Mystery* (1983), Taylor; *The Bone Peddlers: Selling Evolution* (1984), Fix; *Darwin was Wrong: A Study in Probabilities* (1984), Cohen; *Darwinism: the Refutation of a Myth* (1987), Lovtrup; and *Adam and Evolution* (1984), Pitman. Not one of these books was written from a Christian-apologetic point of view: they are concerned only with scientific *truth*—as was Sir Ernst Chaim when he called evolution "a fairy tale." Attacks come also from the much-maligned "creationists" who assert that the traditional straightforward interpretation of Genesis is the only one that makes sense. With his professional reputation at stake and his integrity as a Christian, RB clutches at every straw that promises hope of survival for his theory.

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His strategy is to ignore (almost entirely) what he calls "secular criticism"—such as Denton's devastating *Evolution: A Theory in Crisis* (1985)—and concentrate on ridiculing "creation science." Much of his material is taken uncritically out of American anti-creationist literature: he appears never to have read the *Creation Research Society Quarterly*, whose articles for 25 years have been up-dating, correcting and improving on the basic work of Whitcomb and Morris. Thus John Lightfoot's alleged pinpointing of creation's date and time—always good for a laugh—has been lifted straight out of A.D. White (1890) who concocted the story by mis-reading the record. The myth was exploded by John Klotz (*CRSQ* 23:173-4), but here we find it coolly resuscitated.

He criticizes (p. 108) Thomas Barnes' (but RB gives him no credentials!) book on earth-magnetism. However, Barnes updated his theories (*CRSQ* 23:30-3; 21:109-13). RB seems to know nothing of this, and Barnes is not even mentioned in the bibliography. (It is worth pointing out that RB, a natural-scientist, here claims to know more physics than a Professor of Physics, yet scorns all attempts by non-biologists to invade the sacred precinct of biology). In defense of biological evolution he offers sweeping generalizations by members of his own party, e.g. Cuffey: "The fossil record displays numerous sequences of transitional fossils . . ." but forbears quoting the numerous evolutionists (including Darwin himself) who have said exactly the opposite. Duane Gish is criticized for lumping all 850 species of bat into one 'kind.' RB makes no attempt to trace the pedigree of any *one* species. Evidently Sir Karl Popper's dictum is still right up to date: "Neither Darwin, nor any Darwinian, has so far given an actual causal explanation of any single organism or any single organ." RB cannot resist the temptation to sideswipe (*twice*) poor Philip Gosse whose

famous "God created the rocks with fossils already in them" has no relevance whatever to the modern debate (see Whitcomb, *The Early Earth* p. 42).

RB also follows Russell (*Cross Currents*, 1985) in repeating the old, old fallacy, "Darwin's case is a re-run of Galileo's." Is it? Consider: 129 years after Galileo, every European astronomer had accepted the Copernican system. 129 years after Darwin, ten thousand doubts and criticisms remain unanswered. Listen to the Science Editor of the world's most prestigious newspaper: "Although Darwin's books are said to explain the origin of species, in fact they do no such thing" (Pearce Wright, *The Times*, 30 August 1988). Also, the only words that *seemed* to contradict Galileo were three verses of Bible poetry, whereas Darwinism contradicts nearly everything in 11 chapters of Bible history. The 'parallel' is hopelessly crooked.

Finally RB resorts to psychology: "creationism is a symptom of *fear of a changing future*" (my italics). If only RB had talked to some of these "fearful saints," he might have realized his blunder. As it is, he is content to copy page after page from Americans who try to explain away religious movements in terms of politics and economics. He knows nothing of the success of creationism in Europe, Australia, India, and Korea. He asserts that it produces "stunted" Christians and suggests that St. Paul's command to 'be transformed' means swallowing evolution! He would be surprised to find that hundreds of these "stunted" Christians are preaching the Gospel in dark and dangerous corners of the world. It is indeed regrettable that one so highly qualified would waste his talents defending the pagan philosophy of which a more mature believer has written: "What is Darwin's theory of evolution but a far-fetched exercise in credulity that may well amuse posterity for generations to come?" (Malcolm Muggeridge, *Conversion—A Spiritual Journey*, p. 28, 1988).

LETTERS TO THE EDITOR

Seeds Available For Experiment

Having three leaves rather than two at the beginning of growth would appear to be an advantage to a plant, yet William J. Tinkle's experiments (1971, 1975, 1977) with mutant plants having three cotyledons indicated that it was a disadvantage. Lammerts (1982) had similar results. My letter (1983) reported my experience with a three-cotyledon tomato plant which had inferior fruit production and quality compared with two controls.

This spring I again found a three-cotyledon plant among my tomato seedlings. This mutant was from a pure strain of yellow tomato which my mother began raising about 50 years ago and which I have been raising for about 25 years, saving seed as she did.

As I was unable to plant this mutant with controls suitable for a fair test, I did not keep production records but have saved seed from both the mutant and normal plants which I will send to anyone interested in experimenting as Tinkle did. He raised several generations of plants and studied their characteristics.

This could also be a good Science Fair project for a student. In just one season useful information could be gained on seed viability, number and nature of abnormal seedlings, plant vigor and productivity.

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"Protoavis"?

Calais and Duffett (*CRSQ* 24:183-5) discuss the announcement of the discovery of what was termed the 'oldest known bird,' by Sankar Chatterjee of Texas Tech University. Dr. Chatterjee has been calling his find "Protoavis," but this name has not been published in the scientific literature; therefore, it is not yet a valid name. (Your authors should not have italicized it as if it were.) More seriously, Chatterjee has not published *any description whatsoever* of his find. You are, of course,

welcome to think what you like about Chatterjee's announcement of his interpretation before he had submitted his ideas and evidence to the scrutiny of others. However, his actions do not change the facts. There is no such thing as "Protoavis," at least in the scientific community, until Chatterjee publishes it according to the Rules of the International Commission on Zoological Nomenclature.

The restoration of "Protoavis" distributed to the press by Chatterjee was not based on his fossil find. It is, instead, a silhouette of *Archaeopteryx*, still the earliest known bird (Late Jurassic, West Germany), upon which Chatterjee appears to have colored in the bones that he thinks he has in his "Protoavis" material. Therefore the restorations and speculations offered by your authors, Calais and Duffett, probably should be reconsidered.

As for the facts surrounding "Protoavis," these will have to await what we can only hope will be a meticulous description by Chatterjee. He will need to explain how and in what condition this material was found. He will need to establish that all parts really belong to the same animal, and that he has correctly identified them as sternum, wishbone, etc. Contrary to Calais and Duffett, Beardsley's (1986) news report in *Nature* only reported Chatterjee's claims (and was careful to do so as "claims"). Other paleontologists who have seen this material are not so far inclined to agree. John Ostrom of Yale University, probably the world authority on the earliest birds, is highly skeptical of Chatterjee's interpretation, and could find no evidence of quill nodes even under a microscope. Siegfried Rietschel of Frankfurt, an *Archaeopteryx* expert, regards the material only as "an arrangement of bones on a slab," and Rupert Wild of the State Museum at Stuttgart notes that "Protoavis" is lacking one absolute character of birds: a fused metatarsus (Wolf, 1987).

So you see there is considerable question about "Protoavis," and nothing about it has been established. Calais and Duffett do not cite any study of fossil birds later than Swinton (1960) and Rand and Wetmore's (1968) encyclopedia article. The study of the origin of birds and their flight underwent a renaissance in the 1970s and 1980s spurred largely by John Ostrom's demonstration that they evolved from small carnivorous dinosaurs (not the reptile order "Thecodontia" [sic] as thought in Rand and Wetmore's time). Ostrom's work was discussed and elaborated in a vast number of papers. It culminated in the First International Conference on *Archaeopteryx* held in Eichstätt in 1984. A volume of proceedings from the conference was published the following year (Hecht *et al.*, 1985).

Calais and Duffett also make a point of citing Hoyle and Wickramasinghe's controversial claims that *Archaeopteryx* is a forgery, but they do not cite Charig *et al.*'s (1986) refutation of this, published in *Science*. The British Museum (Natural History) has recently published a short popular book, "The Feathers Fly!," in which the claims of both sides are evaluated against the evidence. Suffice it to say that Hoyle and Wickramasinghe have found even less support in the scientific community for their ideas than Chatterjee has found for his.

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Response to Padian

Error Corrected*

One of several accusations in Padian's letter concerns his apparent inability to detect identical "mistakes" in the writings of his evolutionary colleagues. He charges Duffett and me with italicizing of the name "Protoavis" as though it were a valid genus for this fossil, but overlooks the same name italicized in Beardsley's *Nature* article (Beardsley, 1986). Not that it makes much difference, but it was not Duffett and Calais who italicized the name, but the editor of *CRSQ*. The name "Protoavis" was never underlined in our original manuscript, so Padian's critical remarks on this point are without foundation. If *Protoavis* is not yet considered a proper scientific name for Chatterjee's find, as Padian seems to emphasize, then why did the editors of *Scientific American* also use the name in italics? Are evolutionists permitted this privilege, but creationists are denied it?

News Reports?

Among the non-substantive complaints Padian makes about our article, one of the most glaring deals with the sources of data for the *Protoavis* piece. He incorrectly infers that Duffett and I used, of all things, news reports to gather our information even though they were taken from such respected periodicals as *Nature*, *Scientific American* and *Science Newsletter*. I can only imagine how Padian arrived at the conclusion that we used questionable sources.**

What's In A Name?

Padian spends a disproportionate amount of space in his letter with a discussion dealing with a proper name for *Protoavis*, when actually it matters little whether or not the Chatterjee discovery retains the generic term, "Protoavis." Since these fossils must be referred to by some generic designation, in order to avoid confusion, Duffett and I chose to call *Protoavis* by the same name which its discoverer used. Would Padian have been happier if we devised our own name for this creature? Duffett and I dared not be so presumptuous! When all

*One minor mistake, made in an earlier article dealing with *Protoavis* and repeated in later creationist periodicals, requires correction. It concerns not the biological or nomenclatural status of this controversial fossil, but its geologic position on the theoretical "time scale." Gish (1987, p. 4) asserts an early (lower) Triassic "age" for *Protoavis*, when in fact the Dockum sediments are classified as lower-upper to middle-upper Triassic. This is according to the Correlation of Stratigraphic Units of North America (COSUNA) S.W./S.W. mid-continent chart, published by the American Association of Petroleum Geologists.

**As incredible as it may seem, a recent article co-authored by John Ostrom, whom Padian praises, contains as a source, a newspaper reference! The item appears on page 213 of the Spring, 1988 issue of *Paleobiology* (14:213) and was taken from the New York Times of Aug. 14, 1986.

is said and done, Padian may have to eat crow for his remarks about there being “no such thing as *Protoavis*.” However, none of this alters the weaknesses exposed in our note against the contemporary claims for avian megaevolution.

Is *Protoavis* A Bird?

Not having had the opportunity to examine Chatterjee's fossils neither Duffett nor I can say for certain if *Protoavis* is avian. But according to Ostrom, an evolutionist familiar with fossil bird remains, “the material is so fragmentary that the identification cannot be certified, but it certainly looks as though that's the right interpretation” (Weisburg, 1986). The editors of *Scientific American* had this to say about *Protoavis*: “Two sets of bones found in western Texas have turned out to be those of crow-sized birds . . .” (Anon., 1986, pp. 84, 88). That sounds like a definite statement to me, unlike the remarks Beardsley made, questioning the claims that *Protoavis* is a bird.

As for Rietschel's quoted statement in Padian's letter, I can only emphasize that he is entitled to think what he will of Chatterjee's fossils, but it does seem a bit derogatory to refer to *Protoavis* simply as “an arrangement of bones on a slab.” Such an amateurish connotation could be used to describe virtually any fossil specimen consisting of more than one bone or tooth on a lithic matrix.

If *Protoavis* is “lacking one absolute character of birds: a fused metatarsus (Wolf, 1987),” as Padian claims then *Archaeopteryx* is disqualified along with *Protoavis*. That *Archaeopteryx* possessed unfused metatarsal bones is a fact widely publicized in the literature (i.e., Welty, 1963, p. 480), yet it is still considered a true bird by most evolutionists: “. . . I think conclusively show that *Archaeopteryx* is a genuine bird . . .” (Martin, 1985, p. 182) “*Archaeopteryx* was a bird . . .” (Feduccia, 1986, p. 131) “There can be no doubt that *Archaeopteryx* was a true bird . . .” (Ostrom, 1985, p. 174). “In conclusion, *Archaeopteryx* is a bird . . .” (Tarsitano and Hecht, 1980, p. 179). *Protoavis*, may be avian despite the lack of fused metatarsal bones.

Just prior to this comment Padian states that John Ostrom “could find no evidence (on *Protoavis* remains) of quill nodes even under the microscope.” Incredibly, Duffett and I plainly stated: “Although it is *questionable* that quill nodes, for the attachment of feathers, can be discerned on the ulna of *Protoavis* . . .” (p. 183). But even with this “disclaimer” about the possible quill nodes, Padian manages to fault us for neglecting to mention the absence of said nodes! Quill (calamus) nodes or feather attachment “bumps” on the forearm of birds are not a prerequisite for the presence of feathers, so their absence on *Protoavis* proves nothing either way. Even *Archaeopteryx* lacked such nodes, according to Ostrom (Desmond, 1976, p. 150).

Dubious Reconstructions

Not much can be added to the revelation that the *Protoavis* restoration is not even based on Chatterjee's find. If this is true, then it seems rather dishonest to me, using *Archaeopteryx*'s skeletal silhouette as a basis for a *Protoavis* reconstruction. It reinforces the oft-repeated insistence by creationists that most evolutionary reconstructions should be taken with the proverbial “grain of salt.”

Duffett and I can hardly be held accountable for using what may prove to be a faulty illustration, because the very same drawing was published in the pages of *Scientific American* (1986, p. 84) and *Nature* (1986, p. 677). To my knowledge the editors of neither of these science journals have been criticized for using Chatterjee's *Protoavis* illustration.

Tracking-up The Evolutionary Landscape

If the evolution of birds from reptilian ancestors is a false premise, and we believe it is, then it is no less false without *Protoavis* in the picture. Let us agree, for the moment, to dispose of *Protoavis* until more is ascertained about its zoological ranking. Even without *Protoavis* in the Triassic, there still exists considerable evidence, gathered by evolutionists, which seemingly contradicts the lofty status of *Archaeopteryx* as the first bird. While much of this evidence is in the form of fossilized skeletal fragments, a number of examples are represented by tantalizing ichnological (trace fossil) remains. The osteological evidence, some of it belonging to geological periods supposedly older than the upper Jurassic (where *Archaeopteryx* resides), is controversial (Molnar, 1985; Anon. 1978; Marsh, 1881; Simpson, 1926; Jensen, 1981). None of these discoveries, however, deserves a casual dismissal. Neither can we ignore the footprint evidence suggesting even earlier avian activity, some of it before the Mesozoic!

It was during the 1830's that Edward Hitchcock, President of Amherst College, first published accurate descriptions of the fascinating array of fossil footprints scattered over a large geographic area of the New England countryside known as the Connecticut Valley. All of the tridactyl prints found here are now thought to be those of bi-ped dinosaurs, but Hitchcock was convinced at least some of them were birdprints.

These fossil footprints are sometimes composed of lengthy track-ways in the arkosic sandstones and shales, which are assigned by geologists as either upper Triassic or lower Jurassic (Olsen and Galton, 1977). This would place them at approximately the same geologic age as *Protoavis* and about 60 to 70 million years before *Archaeopteryx*.

Figure 1A depicts one such imprint relatively common in this formation, geologically designated as the Newark Basin. The footprint is morphologically similar to that of a living Dunlin, but about twice the size. Footprint 1A also has an unusual divarication (angle between digits II & IV), unlike that of a dinosaur print. This feature is believed to be the major difference between three-toed dinosaur footprints, which are usually more acute (30° - 45°), and most birdtracks (about 90°). Not all living birds seem to possess this large angle of divarication (Currie, 1981, p. 261). In fact, studies show that the species and conditions under which a birdtrack is produced allow divarications of 75° to 128° in modern birds. According to size, shape and divarication, Figure 1A could easily be classified as a birdprint.

The *Plesionis* footprint shown in Figure 1B has an acute divarication compared to most birdtracks (about 75°), but may be the footstep of a bird. An entire trackway of this same genus was reported as being accompanied by a groove mark in the stony foundation, which strongly resembled the dragged impression

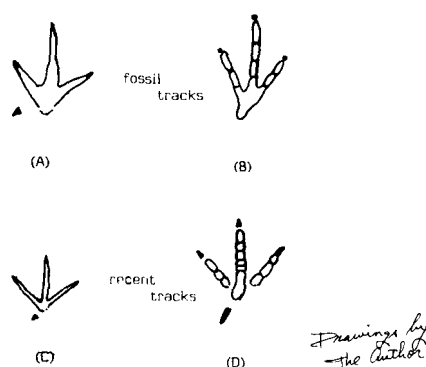


Figure 1. A comparison between fossil and recent footprints . . . (A) Fossilized footprint of *Sillimanian gracillior* from the Upper Triassic (Portland formation) sandstones and shales of the Conn. Valley. Actual length: 1.75 inches. (B) Fossilized example of *Plesiornis* sp. from the same area and geological formation. Actual size: approximately two inches in length. A trackway of this same genus was reported as being accompanied by a groove mark about six inches long which strongly resembled the dragged impression of a feather! (C) Foot impression of a modern Dunlin (*Tringa alpina*). Actual size: 0.75 inches. (D) Footprint of a modern pigeon (*Columba domestica*). Actual size: 1.25 inches long.

of a feather! (Lull, 1953, p. 164). This delicate drag mark was about six inches in length, while the entire trackway was several feet long, so it was not likely to be from any kind of reptile's appendage.

Skepticism over some of these fossil footprints as being those of birds eventually gave way to acceptance. In time some of the great names of paleontology: Silliman, Deane, Owen, Mantell, Murchison accepted an avian origin for many of the tracks (Merrill, 1964, p. 553-63).

Much "older" tracks than those of the Connecticut Valley exist, which, if confirmed as those left by birds, would completely demolish the flighty foundation of bird evolution. Two in particular were discovered in the Hermit formation (Arizona) and in Carboniferous strata of Nova Scotia.

The single large track (Figure 2C) from the Grand Canyon region was pictured by Gilmore in 1927, but was listed as "unidentified, No. 11,528-U.S.N.M." It is on a slab from the Permian and strongly resembles the tracks left by modern birds, such as the one shown in Figure 2A. Divarication on this fossil print is nearly 90° and is probably the track of a relatively large bird. This would place it some 20 to 30 million years before *Protoavis* and 90 million years earlier than *Archaeopteryx*! These dates are too early for any contemporary scheme of avian evolution.

Another example of possible bird tracks appearing too early in the geologic column is illustrated in Figure 2D. Though it has a smaller divarication than most living birds, it is morphologically identical to tracks left by modern shore birds (Sternberg, 1933, p. 959). It is especially similar to the track left by the Great Blue Heron (*Ardea herodias*), Figure 2B. Despite Sternberg admitting that this trackway of several prints resembled those of wading birds, he could not bring himself to admit the possibility that they were produced by birds, no doubt because of some ingrained evolutionary bias.

Even if it could be agreed upon by everybody concerned with the study of ichnites that this series of fossilized footprints are those of a reptile, the evolu-

tionist is still left without an explanation of why a bi-ped creature walked in the Carboniferous Period when bi-pedality supposedly developed sometime during the Triassic (Romer, 1968, pp. 127-8), tens of millions of years later!

When all of the evidence is objectively considered, Padian's vision of *Archaeopteryx* being reinstated as the "earliest bird" becomes much cloudier.

An Ode To Ostrom

The next phase of Padian's critique concerns his observation of our use of "outdated" references and our failure to "cite any scholarly study of fossil birds later than Swinton (1960) and Rand & Wetmore's (1968) encyclopedia article." This charge is not exactly accurate. We did list a more recent source from the 1980's—a review of the evolutionary controversy centering around *Archaeopteryx*. This two-page article only reiterates what we intimated: "The most recent work on the 'London' specimen (of *Archaeopteryx*) reflects the inconclusive nature of the arguments surrounding this contentious creature" (Benton, 1983, p. 99).

Somehow I get the impression that Padian considers most claims made about avian evolution prior to the 1970's as now outdated and irrelevant. I wholeheartedly concur! The truth is, during the last 20 years nothing really substantial has changed in the search for reptile/bird transitions, contrary to Padian's paleo-ornithological "renaissance." While it may be true that speculations about bird origins have evolved into more sophisticated discussions over the years, no new hard evidence has been uncovered which would drastically alter Swinton's 1960 admission quoted in the introduction of our note (p. 183).

Padian obviously places a great deal of faith in John Ostrom's proclamations, but according to R. A. Thulborn (Department of Zoology, University of Queensland, Australia), Ostrom's concept of "Proavis," which represents the crucial—but hypothetical—"missing

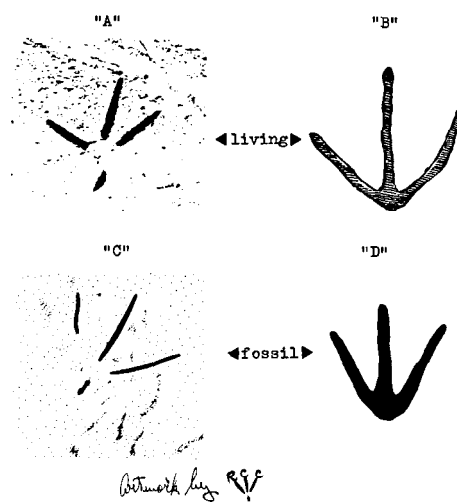


Figure 2. Paleozoic Bird Tracks . . . Drawings "C" & "D" are those of fossilized footprints from the Hermit formation (Permian)—Grand Canyon, Arizona, and Carboniferous strata of Cumberland County, Nova Scotia, respectively. Illustrations marked "A" and "B" are the footprints of a living Heron in sand and a track left by a modern Canada Goose. Note the remarkable similarity between the size and general morphology of the fossil and modern prints.

link" wedged between theropods and *Archaeopteryx*, has "outlived any usefulness it may once have had, and should be abandoned" (Thulborn, 1984, p. 119).

It is nothing short of absurdity to believe Ostrom has demonstrated the evolution of birds from small carnivorous dinosaurs. His research demonstrates nothing of the sort. Ostrom has merely formulated an hypothesis based on what he considers homologous skeletal elements between *Archaeopteryx* and Coelurosaurs. But Ostrom's thesis is by no means universally accepted among paleobiologists, as implied in Padian's letter. For example, Feduccia, author of a successful volume on bird origins, states:

However, the analysis of Tarsitano & Hecht (1980) disputed all the claims of Ostrom, and they stated (p. 175), 'From our morphological description and analysis, it is clear that there are no synapomorphies that relate *Archaeopteryx* to the Theropoda or any similar group' (Feduccia, 1986, p. 129).

Tarsitano's and Hecht's concluding remarks include the following analysis: "Therefore the interpretations of theropod physiology by Hopson (1977), Bakker (1975) and Marx (1978) are not supported by the morphology of *Archaeopteryx*" (p. 179).

There is perhaps more controversy now than ever among evolutionary "experts" regarding the reptile group from which *Archaeopteryx* and/or birds allegedly sprang. Padian may have convinced himself of the merits of Ostrom's ideas, but many workers disagree with him vehemently. Ostrom himself makes it plain there are at least two separate schools of thought circulating around this question, besides his own. They are the crocodilian/avian relationship and the pseudosuchian origin for birds (Ostrom, 1985, p. 17).

A little reading of the literature would have spared Padian the embarrassment of making such a foolish statement as the one in which he assumes the reptile order Thecodontia is no longer considered a viable alternative (among evolutionists) as ancestors for modern birds. At least two of the world's leading authorities on the subject—S. F. Tarsitano and M. K. Hecht (Department of Biology, Queens College, New York)—dispel the notion of abandonment of this theory: "Our contention is that the present data indicate that *Archaeopteryx* is derived from the Thecodont level" (Tarsitano and Hecht, 1980, pp. 178-9). More recently, Hecht reaffirmed this conclusion in the Proceedings of the International *Archaeopteryx* Conference in Eichstatt, W. Germany (Hecht, 1985), and Charig (1985) emphasizes that the "thecodontians remain one of the two most popular candidates."

Scaly Feathers and Feathery Scales

Ostrom's related notions on the origin of flight and the development of feathers from reptile scales, "supported" by volumes of speculation from his proponents, also evaporate when compared with a careful study of the fossil record. Feduccia (an avid evolutionist) demolishes one of Ostrom's cherished beliefs when he relates the following:

Feathers are features unique to birds, and there are no known intermediate structures between reptilian scales and feathers. Notwithstanding specula-

tions on the nature of the elongated scales found in such forms as *Longisquama* (Sharov, 1970) . . . as being featherlike structures, there is simply no demonstratable evidence that they in fact are. They are very interesting, highly modified and elongated reptilian scales, and are not incipient feathers (Feduccia, 1985, p. 76).

Furthermore, Ostrom conducted an extensive search for fossil feather or "proto-feather" impressions on the *Compsognathus* slabs, the very reptile genus to which he compares *Archaeopteryx* so favorably. But, as Ostrom admits, it was all to no avail (Ostrom, 1978). Evidence of feathers was especially sought after on the famous Munich specimen of *Compsognathus*, a small dinosaurian form superficially resembling *Archaeopteryx*. According to Ostrom, this particular specimen was considered a crucial one in the search for fossilized feathers on extinct reptiles. Ostrom (1978, p. 116) conceded defeat when he wrote:

If feathers had been present in *Compsognathus*, it is inconceivable to me that no evidence of them would be preserved . . . but the fine-grained matrix shows nothing

If, as Ostrom insists, coelurosaurs like *Compsognathus* are responsible for the evolution of *Archaeopteryx* — a true bird—then why does one not find this reptile in possession of feathery plumes? According to Ostrom's evolutionary scenario, *Compsognathus* should have been slowly developing feathery structures of some type while in the process of evolving toward *Archaeopteryx*, being that *Archaeopteryx* already sported primaries of asymmetric design—fully modern feathers identical to those attached to living birds (Feduccia, 1979).

The only alternative is to accept the rather doubtful possibility that these complex integumentary structures just suddenly sprouted from the skin of some creature, but fully developed and somehow managing to leave not a trace in the extremely fine-grained solnhofen limestones. It would be much too kind to attribute this brand of transformist "logic" to wishful thinking!

Fabulous Feathered Fossil Fake?

The last point in Padian's letter is a bold assertion that Duffett and I "make a point of citing Hoyle's and Wickramasinghe's controversial claims that *Archaeopteryx* is a forgery." Quite the contrary! We briefly referred to Hoyle's claims by saying: "Even the controversy over the genuineness of the feathered imprints on two of *Archaeopteryx*'s slabs will no longer matter . . ." (Calais and Duffett, 1988, p. 183). We admitted Hoyle's claims of forgery were not only controversial, but clearly expressed the opinion that it would not matter whether the feather impressions proved eventually to be authentic or faked. But by no stretch of the imagination did we imply *Archaeopteryx* was decidedly a hoax! In fact Duffett published an excellent paper in 1985 in which he concludes the hoax thesis was untenable. This is nearly a year before the British Museum (Natural History) proved Duffett was correct (Charig, *et al.*, 1986).

We purposefully neglected to cite Charig, *et al.*, 1986 and numerous others in a similar vein, which refute the

hoax claims of Hoyle and company because the purpose of our note was not to assault or acclaim these suppositions. If Padian will carefully read our note, he will realize its purpose was to address some of the implications of the *Protoavis* discovery. The hoax controversy has been adequately dealt with elsewhere by Hoyle's detractors (Dickson, 1987; Gould, 1986; 1987; Rietschel, 1985).

Permo-Trias Flooding?

Because the concept of global, diluvial catastrophism is often a favorite target for derision by modern Darwinists, I was surprised that Padian failed to attack the comments relating to Flood geology. It may interest the readers that additional data supporting this theory can be found in the same geological survey volume, authored by Sellard, Adkins and Plummer (1958).

The evidence is further indication of a rapid, sequential deposition of not only the Triassic Dockum sediments (containing *Protoavis*), but also a similarly rapid deposition of the underlying Permian formation. On page 243 of volume I a discussion of the contact between these two systems, alleged to have required millions of years to be deposited separately, discloses the following observation:

Gould records that locally on Trujillo Creek the upper red shales of the Quartermaster (Permian) have been removed down to the alibates dolomite lentil, and throughout the region, although very little Permian has been subsequently removed, the Triassic lies unconformably upon it. However, there are no fossils near the contact, the beds on both sides of it are in general similar (except for some color differences), and some writers have stressed the gradational appearance of the boundary. Thus Case states that he has repeatedly crossed the boundary in every county in Texas where it occurs, and has been unable to draw any line that can be used to separate the two formations.

If, as the evidence may imply, the strata belonging to both these geologic systems in this region of west Texas were deposited in relatively quick succession during a time of catastrophic changes, then the vast geological age separating the Permian and Triassic periods becomes nothing more than a figment of the evolutionist's imagination.

The event represented by this flooding may not have been merely regional, as overwhelming evidence of mass extinctions from many other areas of the globe at the Permo-Triassic "boundary" are well documented in the literature (Pitrat, 1973; Raup, 1979; Holser, 1987). The late Triassic biosphere as well seems to have endured a faunal crisis of devastating proportions (Monastersky, 1987). Are these destructive events related to the Texas flooding which apparently drowned *Protoavis*?

Evolution's Empty Nest

Padian must surely be aware of the new mechanical preparations and judicious studies conducted on five of the six *Archaeopteryx* slabs,** some of which have

***A sixth (virtually complete) specimen of *Archaeopteryx* has been recently reported in *Science* (June 24, 1988, p. 1790-2).

completely dispelled old die-hard dogmas about this magnificent creature. These recent scientific reassessments prove, for example, that the skull and brain of *Archaeopteryx* were as birdish as any aerial denizen of today's world (Whetstone, 1983; Buhler, 1985; Haubitz, 1988). In shape and implantation mode, even its teeth, long thought to be dinosaurian, are unlike those of any known dinosaur (Martin, 1985, p. 179).

A more reasonable assumption credits *Archaeopteryx* as an originally created member of a now extinct order of ancient birds that, as one evolutionist said, "might have nothing whatever to do with the (evolutionary) origin of birds" (Charig, 1985, p. 23). Whether or not *Protoavis* eventually is placed on a similar parallel side branch of avian phylogeny remains to be seen. But one thing is becoming increasingly obvious: When all of the evidence is carefully considered in the cold light of reason, the case for avian evolution begins to resemble nothing more than an empty bird's nest.

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The Speed of Light—A Critique of Aardsma's Statistical Method

I read with interest Aardsma's article in *CRSQ* 25:36-40, particularly his analysis of Setterfield's data. Aardsma contends that a method that applies a weighting to measurements depending upon their accuracy is a statistically sound method of checking the amount of change that any set of measurements may have. I have no qualifications in mathematics but may I be allowed to demonstrate a simple proof that using this method on these values is invalid.

Case 1

I examine a simple curve—say x^2 , I can determine the best fit of an average slope of the values from say 0 to 10 using the least squares method. It will be found that the slope of the line is $y = 7.857 * x$.

Case 2

I can introduce an error bar into the data and increase this as the curve moves away from the origin by making it equal to say 0.1 or .01 of the value of x . The value of the difference (between the curve and the straight line being tested for a 'best fit') can then be 'weighted' in inverse proportion to each measurement's accuracy. This is most simply done by dividing each 'difference value' by the error; i.e. those with a high error will have a smaller weighted value than those with a low error.

Then each weighted result is used in a least squares summation. It will be found that the best fitting slope will then drop to $y = 5.5 * x$. Thus, although the error bars may be small, such that the there should be negligible change in the optimum slope. This alone surely suggests that the method is incorrect. I have weighted the values before using them in the least square method. Aardsma does not say whether his weighting is applied after the squaring—in which case the decrease in the slope would be less, but still very significant.

Case 3

The weighting method can be shown to be even more erroneous if we say the error bars are in proportion to the square of their distance from the origin, as this is nearer to the data collected by Setterfield i.e. the measurements are less accurate as you move away from the origin. In this case, the optimum slope falls to $y = 1.89 * x$. These differing optimum slopes are illustrated in Figure 1. It is interesting that whether the error is 0.1 or .01 * x makes no difference to the value of the best fitting slope in both cases 2 and 3.

It is surely obvious that the weighting method used on this curve—and by inference on the "c" data—is totally inapplicable. The effect of weighting quite a small error in the data results in a change in the optimum slope out of all proportion to the error itself. The reason for this marked change is surely that while the method may be suitable for straight lines, it is clearly inappropriate for curves. It is the high weighting that is given to the extremely accurate results of recent years that swamp out the much less (but adequately accurate) results of earlier years.

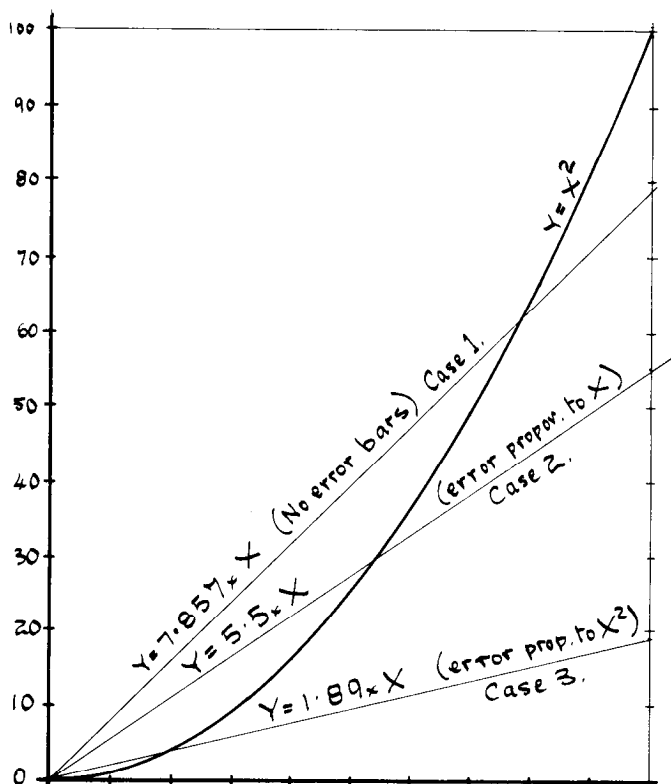


Figure 1.

Norman and Setterfield only quoted average slopes of the results to emphasis the fact that “c” had decreased. To add to this a weighting inversely proportional to the error bars will obviously result in a spurious decrease in the slope, as I have demonstrated above and which Aardsma also presented in his paper.

I would suggest that if my simple analysis is correct, then the whole basis of Aardsma’s major criticism of Setterfield’s data is invalid.

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Response to Bowden

Bowden has attempted to prove that my analysis of Norman and Setterfield’s data—using a standard, properly weighted linear least-squares fit to search for an overall trend in the data set—“is totally inapplicable,” by devising several test cases which he feels serve as clear counter examples. Mr. Bowden’s proof falls short of its intent on several counts, however. A principal difficulty is that Bowden’s cases are not analogous to my analysis of Norman and Setterfield’s data. I have fit the equation $y = mx + b$ to their data, where both the slope, m , and the y intercept, b , are free parameters. Bowden has used the equation $y = mx$, constraining the y intercept to 0 as his graph clearly shows. To avoid comparing apples and oranges it is necessary to leave the intercept unconstrained. I will discuss the results of applying a two parameter fit (i.e., $y = mx + b$) to Bowden’s test cases below. It is necessary to clear up another point before proceeding along these lines, however. Bowden seems to think that the decrease in the absolute magnitude of the slopes going from his case 1 to case 3 is proof that weighting the data points properly obscures the real trend in the data. This is not correct; it is not the absolute slope of the fitted line which is of interest at this stage of the analysis.

Norman and Setterfield have made an extraordinary claim—that the speed of light has been changing throughout history; they require extraordinary evidence to substantiate their claim. The measurements of the speed of light which have been made over the last 300 years would certainly seem to be the right place to look for such evidence. And the most obvious analysis is to search for a decay trend in these measurements by fitting a straight line to them. But the absolute magnitude of the slope of the fitted line is not of interest; what is of interest is whether the slope is statistically significantly different from a slope of zero. This can only be determined by comparing the slope of the fitted line to the estimated uncertainty in the slope which is provided by the linear least-squares analysis. If the fitted slope is less than three times the estimated uncertainty in the slope away from zero slope, Norman and Setterfield do not have extraordinary evidence. To say this in another way, Norman and Setterfield’s claim would be better if their data gave a slope of -0.1 ± 0.01 than if it gave a slope of $-1,000 \pm 750$.

Table I shows the data and Table II the results I obtained using the standard, weighted, linear least-squares fit for four separate test cases. The first three are the ones which Bowden suggested; the fourth is one

Table I

X	Y = X ²	Weight			
		case 1	case 2	case 3	case 4
1	1	—	0.1	0.1	10 ⁻⁹
2	4	—	0.2	0.4	10 ⁻⁸
3	9	—	0.3	0.9	10 ⁻⁷
4	16	—	0.4	1.6	10 ⁻⁶
5	25	—	0.5	2.5	10 ⁻⁵
6	36	—	0.6	3.6	10 ⁻⁴
7	49	—	0.7	4.9	10 ⁻³
8	64	—	0.8	6.4	10 ⁻²
9	81	—	0.9	8.1	10 ⁻¹
10	100	—	1.0	10.0	10 ⁰

which is a better analogue to Norman and Setterfield’s data in that the error bars span nine orders of magnitude (the error bars on Norman and Setterfield’s data span eight orders of magnitude) starting with a relative error in y comparable to the relative error in modern c measurements (about one part per billion). Table II shows a statistically significant slope for all cases; thus, none support the contention that weighting the data points obscures the true trend in the data. In fact, the weighted fit of the test data which is most analogous to Norman and Setterfield’s data (case 4) shows that the decay trend in the test data (i.e., the fitted slope) is real (i.e., statistically significantly different from zero) far more forcefully than the unweighted fit (case 1) does.

Table II

case	slope	uncertainty in slope (1 σ)
1	11.0	0.9
2	8.09	0.05
3	5.41	0.21
4	3.01	4×10^{-8}

Returning to the Norman and Setterfield data set and analysis, the following conclusions apply:

1. In no case is it permissible to ignore the relative precision of the various data points and treat them all equally as Norman and Setterfield have done by not weighting their linear least-squares fit. To do so is equivalent to the claim that the best estimate of the diameter of a microscopic pollen grain is obtained by averaging a measurement made with a reticle under the microscope with another made with a one-foot ruler using the unaided eye.
2. Weighting the Norman and Setterfield data set in the normal, proper fashion reveals no statistically significant slope. This supports only two alternatives:
 - a. The speed of light was constant over the time interval spanned by the data set, or
 - b. The data is not of sufficiently good quality to show the decay trend.

Option ‘a’ is the only one which is consistent with other considerations brought forward in the minisymposium on the decay of light. In any case, Norman and Setterfield’s claim that the historical data supports their hypothesis is not a logical alternative. This, I recognize, is devastating to their whole presentation—which is unfortunate and was not the intention of my investigation of their work. But Norman and

Setterfield's analysis is so clearly flawed and the proper analysis is so simple to do and clear in its results that, in my opinion, there really is no alternative but to reject it.

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