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## CREATION RESEARCH SOCIETY STUDIES ON PRECAMBRIAN POLLEN: PART I - A REVIEW

GEORGE F. HOWE\*

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### Abstract

*In the last 20 years considerable interest has centered on a 1966 Creation Research Society Quarterly (CRSQ) paper in which C. L. Burdick presented photographs and descriptions of pollen grains isolated from Precambrian Hakatai shale and various other strata of the Grand Canyon, Arizona. Burdick produced this paper based on research he conducted at the University of Arizona. He relied as well on assistance from W. E. Lammerts in manuscript preparation. In defense of the macroevolutionary origins model, some workers immediately argued that these discoveries were merely contaminant modern pollen grains and not true microfossils.*

*At the prompting of the CRS Board of Directors, C. L. Burdick solicited the help of A. V. Chadwick, a creationist, to undertake further analysis of new rock samples from the Grand Canyon. At that time, Chadwick was associated with Loma Linda University, Loma Linda, CA. Based on this work, Burdick reported in a short article (1972) that the first discoveries had been replicated by utilizing essentially the same methods as he had originally used at the University of Arizona. While not altogether denying the 1972 pollen discoveries in Hakatai shale, A. V. Chadwick in 1973 wrote in a letter for publication in CRSQ that a more careful analysis of these rocks would be necessary before definitive statements could be made about the presence of microfossils in Grand Canyon strata.*

*Eight years later (1981) Chadwick, in a brief paper, indicated that by taking unusual precaution in field and laboratory to avoid contamination, and by using a different extraction procedure involving hydrofluoric acid (as well as hydrochloric acid), he had been unable to recover any pollen grains from samples of Hakatai shale.*

*Since contamination of the rock in the field and on the microscope slides in the laboratory is the usual claim of both creationists and evolutionists who discount the Burdick discoveries, W. E. Lammerts volunteered to determine just how easy or difficult it might be to recover contaminant pollen grains in nature. At about the same time, in Spring, 1983, the CRS Research Committee authorized E. L. Williams, G. F. Howe, G. T. Matzko, and W. E. Lammerts to collect new rock samples and perform pollen extraction by the old Burdick method to see if perhaps the differences in chemical processing between Burdick's method and Chadwick's would influence the final results. We recovered what are apparently pollen grains and other cellular objects from Hakatai shale by using the Burdick techniques. The methods, results, and conclusions of these recent studies will be presented in Parts II and III of this present series of papers.*

### Introduction

Finding a fossil land plant pollen grain in rocks labeled "Precambrian" is about as likely (in the uni-

formitarian macroevolutionary scheme of origins) as it would be to find a modern human skull embedded conformably in Mississippian coal layers! Evolutionary scientists have characteristically assumed that early land plants arose from the algae in the Silurian "time" while conifers and flowering plants are not supposed to have made their debut until the Permian and Cretaceous respectively. Thus even the very simplest (non-

\*George F. Howe, Ph.D., is Professor and Chairman, Division of Natural Sciences and Mathematics, The Master's College, Newhall, CA. He is also Director of The Creation Research Society Grand Canyon Experiment Station, Paulden, Arizona. He receives mail at 24635 Apple St., Newhall, CA. 91321.

flowering, non-seed-bearing) vascular land plants are believed to have evolved millions of years *after* the Precambrian and Cambrian. Yet creationists and others have reported the presence of fossil pollen from various land plants in layers designated Cambrian and even Precambrian by orthodox historical geologists—see Rusch (1982), Burdick (1974 and 1975), and Chadwick (1981). A review and a history of creationist findings in Precambrian palynology is presented here-with.

### Burdick Beginnings

In 1964 and 1965 C. L. Burdick, together with members of the Department of Geochronology of The University of Arizona, Tucson, did an analysis of microfossils recovered from rocks of the Petrified Forest, Arizona and collected by Gerhard Kremp of that same department (Burdick 1966, p. 38 and 1972, p. 25). Concerning this study, Burdick reported that "We had especially good results with fine microphotographs of many species of Chinle formation conifers" (1972, p. 25). Kremp, according to Burdick, had likewise secured rock samples from various other strata of the Grand Canyon, Arizona. These were also macerated and while examining the extracts, Burdick surprisingly recovered spores of various land plants from late Precambrian Hakatai shale (Burdick - 9/25/65)!\* In that same letter to Lammerts, Burdick noted opposition to his pollen discovery from various unnamed sources but he also named two instructors who defended his technique, one of whom was Kremp who had done the sample collecting and at this time still evidently resisted the idea that he might himself have contaminated the samples in the field or that they might have been contaminated by Burdick at the University of Arizona Geochronometry Laboratory using his (Kremp's) own standardized techniques (Burdick 9/25/65 and 1/5/66).

But departmental support for Burdick's claims was short-lived and vacillating. Burdick wrote to Lammerts that Kremp's reactions were "mercurial"—". . . one day he is as enthusiastic as a kid over a project, next day lukewarm" (Burdick 1/26/65). Upon seeing one strange spore on a maceration that Burdick had prepared, Kremp (according to Burdick) wanted Burdick to continue the work and give a paper at the April meetings (1966) of the Arizona Academy of Science at Tempe. About this co-authored report, Burdick wrote in (2/4/66): "I doubt he will let me give a paper at Arizona Academy of Science. He does not want to be a target for derision." And as it turned out, of course, Burdick was never authorized to deliver the paper.

### Possible Contamination or Sample Switching in Field or Laboratory?

At this stage of the work it was Burdick more than Lammerts who expressed concern for avoiding contamination and who was cautious to take steps avoiding the same. Relative to possible contamination, Burdick wrote (2/4/66): "Dr. Kremp made me use special techniques to avoid that but still I get the same stuff— they are not contamination!!!" On 2/11/66 he wrote:

\*In this reference and others to follow, a particular letter from Burdick to Lammerts or from Lammerts to Burdick will be listed together with the date of the letter. This correspondence is presently in the possession of the Howe and all quotations from it are used by permission of the writers.

". . . in cases where the rock was shattered in sampling, Dr. Kremp put them through washings to remove contaminants. But after washings I got essentially the same type of fossil spore.

In further reflection on the possibility that he may have contaminated the samples in the lab, Burdick cogently noted: that he had ". . . completed two years at this sort of thing and could not be considered a novice" (2/4/66).

In some of this same correspondence, from time to time Lammerts quizzed Burdick concerning the possibility that rock samples might have been inadvertently switched. "(I) . . . wonder if Dr. Kremp might possibly have gotten his sample switched with one from Petrified Forest!" (9/16/65) Relative to both sample switching and possible contamination, Burdick wrote Lammerts back on (9/25/65) as follows:

Regarding your suggestion that there might be a mixing of samples, that possibly the assumed Hakatai shale sample might be confused with Petrified Forest sample, not a chance. The samples were taken on different expeditions three months apart. Furthermore, the color of the rock is different. The Petrified Forest samples came from grey rock, while the Hakatai rock is red. Dr. Kremp discounts the possibility of contamination. He is very careful in sampling.

Believing that his instructor's original Hakatai sample was too small, Burdick returned two more times to Grand Canyon— once accompanied by H. Slusher. Burdick described his own field techniques and impressions of the Hakatai collecting area fairly plainly, as follows:

. . . the area for getting the samples along the Hakatai trail was small. The trail is usually within ten feet or so of the vertical bank . . . In rare cases the ground may be flat or low so one could pry up a slab as was done in the one Dr. Kremp brought back. The others had to be picked out of the solid rock wall, so a slab was quite out of the question. The surface of the rock was usually weathered, so we dug back a few inches to get a fresh sample. I did not see or sample in places where roots had opened up seams. Mineral laden water carrying iron does penetrate the rocks to a certain distance, but the spores of 50 microns I would think would be too big to penetrate the solid lattices of the mineral (2/11/66).

### Which Strata Finally Yielded Pollen?

At first (1/5/66) Burdick indicated that he had found spores and pollen in Hakatai shale (Precambrian) but did not originally find a single conifer grain in all the other rock formations he tested including: Tapeats sandstone (Cambrian), Bright Angel Shale (Cambrian), Muav (Cambrian), Redwall limestone (Mississippian), Supai (Permian), Hermit shale (Permian), and Bass limestone (Precambrian). When he thought of the possibility that the Hakatai discoveries might have been contamination by modern non-fossil pollen, Burdick asked a valid question: If they are contamination why should they all be concentrated in the Hakatai? (1/5/66). Further along in the same letter Burdick noted that if contamination was present, such modern grains would be nearly colorless while all of the conifer

spores he had recovered were of reddish color-supporting their *ancient* character. Even from a creationist perspective (1/26/66) Burdick wondered why there would be so many pollen grains in the Hakatai Precambrian and seemingly few or none in the other strata.

But the picture changed when Burdick noted on 2/4/66 that he had recently macerated a sample from Supai (Permian) which was: . . . just lousy with all kinds of plant life, conifers, polyplicate, disulcate and other kinds of angiosperms. He added that he had also recovered spores from Redwall limestone and Bright Angel shale. In the face of this unexpected turn in his studies, Burdick wrote as follows:

When you get some 60 slides made from Supai samples with no spores of any kind, then suddenly with the same technique get a swarm of all kinds of spores including fifteen or twenty conifer, logic and even common sense will tell you they cant all be contamination. But it does tell me that the fossil spores were not evenly distributed throughout the sediments. There were great barren areas, then concentrations in spots . . . I am using the same technique as I used so successfully a year or so ago in the Petrified Forest. I would have a long series of discouraging blanks, then all of a sudden a whole swarm of conifers of all types . . . I never heard a suggestion then of contamination, I guess because mostly I found Triassic types of trees in Chinle. My point is that some of these blanks, according to the law of averages should show up contaminations instead of all of them being concentrated in the one sample, when the technique was the same (2/11/66).

#### The Lammerts Connection

Perhaps it has been obvious up to this point but well worth noting anyway that W. E. Lammerts (CRSQ editor at that time) lent encouragement from the very early stages of Burdick's important study to the time of its publication in CRSQ, June, 1966. Upon learning of the research, Lammerts solicited a paper and at times even coaxed Burdick to complete the laboratory work and send a manuscript.

Burdick delayed at first because he felt the original Hakatai sample collected by the University of Arizona instructor was too small (9/25/65). Later, he was concerned about possible contamination while Lammerts puzzled over possible sample switching. Such concerns seem to have vanished by January, 1966, as Burdick had located spores in newly collected Grand Canyon rocks:

Well, I am far enough along to assure you that the job will be a success, and I have now corroborated the existence of disaccate pollen (conifers) in the Hakatai shale. Samples taken from three separate locations in the Hakatai formation show up the same type of pine spore as the original one; that makes four locations in all. I dont know what more in the way of evidence is needed (1/5/66).

Burdick had come to realize by this time that he would have to produce a paper without the support of his instructor:

for a while he (Kremp) readily admitted that the one we got before could not have been a contami-

nation; and he was right. But now he says it was a contamination. Another fellow got the same type of spore or pollen from the Devonian in the Salt River Canyon. That hurts the evolutionary sequence too. What he will say when I take the microphotographs I dont know but it is preposterous to say they are all contamination (1/5/66).

Yet Burdick still tried to avoid what might have appeared to be possible conflict of interest and as a result he and Lammerts decided to limit the published data to slides from samples Burdick had personally collected:

I dont blame you for being upset due to the sudden change due to Dr. Kremp's objection to publication . . . your suggestion or a compromise is a good one, that we will now publish only the results from the samples that Slusher and I took . . . If we dont publish the results from samples that Kremp and his helpers took, then he can have no real cause for complaint (2/21/66).

Lammerts' continuing role in manuscript preparation is evident in this comment of Burdick's (2/21/66):

Since you did not return the manuscript I sent you with the introduction and maceration procedure, I will not repeat that phase, but if you wish you can pick out any parts you may care to use. All I will send now is the summary of results with description of spores and locations and photos. Perhaps you can fit together.

Finally after Burdick's instructor at the University of Arizona failed to answer a letter of inquiry sent by Lammerts, Lammerts called him by phone whereupon the man indicated that he considered Burdick's claim of having found pollen grains in Hakatai shale to be ridiculous.

#### CRSQ in The Vanguard

Here, a word should be written as well about the importance of CRSQ to the scientific world in general. Aside from whether or not Burdick's work was repeatable and aside from the cry of contamination, a radical discovery such as this certainly deserved a hearing. Had it not been accepted for publication in CRSQ, it is extremely doubtful that the editors of any other peer-reviewed, scientific journal would have printed these results. If Burdick had sent the paper to some Christian magazine instead, it is probable that evolutionary scientists would have sneered even more— further discounting his work for having been published in a "religious" journal.

#### What Was The Source of Pollen for Precambrian Shale?

During this correspondence Lammerts and Burdick periodically discussed the problem of how, even from a creationist point of view, one might have expected pollen to be deposited in Precambrian rocks anyway. While some creationists like B. Northrup and W. G. Peters have assumed that Precambrian strata were formed in the early stages of creation— when plants had been created but not animals— it is apparent from their correspondence that Lammerts and Burdick, were agreed on the idea that Hakatai shale was formed during the Flood event itself. Accordingly, Lammerts proposed the following to Burdick on 1/16/66:

Incidentally though you will think it a bold theory, could not the source of pollen have been the pines of the petrified forest, the wind blowing the spores into the Hakatai shale as it was being formed. We must remember that from our viewpoint all these were alive at the same time. Or were there nearer sources of pine forests as shown by the geological record?

On 1/26/66 Burdick agreed to this proposition and Lammerts on 2/1/66 added the following insights:

. . . pollen is windblown and evidently the forest was still standing at the time the Hakatai was being deposited. Then the Flood evidently overwhelmed this forest and naturally after that there would be no source of pollen supply. Let us remember that even from our point of view there is some time sequence, though it is a matter of days or at most months. In fact we may draw some remarkable inferences as to the mechanics of the Flood . . . One is that while formations were being deposited in one area, forests and presumable life in them (at nearby areas) were not completely annihilated as yet. Studies such as these should eventually allow us to picture more vividly just how the Flood progressed in its destructive paths.

#### Enter Dr. MacGinitie

In the fall of 1964 Lammerts enrolled at the University of California (Berkeley) in a paleobotany course which at that time was taught by the well-known and widely-respected micropaleobotanist H. D. MacGinitie. Lammerts and MacGinitie had many long conversations about paleobotany in relation to evolution. Thus in either late 1965 or early 1966, when concerned about possible contamination, Lammerts showed some of Burdick's pollen slides and pictures to MacGinitie. The latter, after carefully examining the Hakatai material, affirmed without hesitation that some of them were gymnosperm pollen grains. MacGinitie further expressed interest in the unusual nature of the pollen grains and spores, saying that they were quite unlike any from plants now growing on Grand Canyon walls. Although Lammerts told MacGinitie the material came from Grand Canyon, he did not disclose the fact that these were samples from Precambrian Hakatai shale.

The reactions of MacGinitie stand in strange contrast to the assessment of A. V. Chadwick who (1981, pp. 9-11) asserted that the poor quality of Burdick's photographs made them difficult to identify but that even so, "Burdick's grains approximate the modern pollen grain found in surface samples in the area of the Grand Canyon where he collected samples." In Lammerts opinion, Chadwick's assessment of poor quality in Burdick's 1966 photographs was essentially correct. Lammerts remembers that Burdick's slides themselves did show much more detail and variety of pollen grains than is evident in the 1966 photographs. Thus MacGinitie was puzzled as to just where in the Grand Canyon Burdick could have found such a complex of spores and pollen grains as was manifest on the slides.

With problems such as these seemingly settled, Lammerts made the Burdick paper a part of the June 1966 Quarterly.

#### Repetition Attempted at the University of Arizona— 1970

The Creation Research Society sponsored an attempt to repeat the Burdick study and encouraged a corollary project sponsored by Loma Linda University of California (Burdick— 1972, p. 25). Burdick, accompanied by D. Delevan, geologist from the University of Arizona, returned to Grand Canyon in June, 1970, to collect the fresh rock samples. Concerning their technique, Burdick wrote that:

These were from fresh unweathered exposures and immediately sealed in sterile plastic bags. Samples were also taken from the shaley layers in the Mississippian Redwall formation. Further samples were cut from Cambrian formations, chiefly the Bright Angel shale. Getting down into the Precambrian, the Proterozoic, samples were taken from the Hakatai shale and the bass limestone (Burdick, 1972. p. 26).

Two workers who did not go on the collecting trip but were asked to analyze the samples make the following report in Geotimes:

The trip could not be made at that time so Burdick found someone to do the collecting and upon his return asked to have the samples extracted for pollen with safeguards against contamination. The extractions were done as requested. The results were total palynological sterility; i.e. no pollen grains or land plant spores of any kind were seen (Solomon and Morgan— 1973, p. 10).

Burdick attributed these negative 1970 results of Solomon and Morgan to problems with their processing of samples. He reported that:

. . . Mr. Delevan then turned the samples over to Mr. Morgan, a palynologist from the geochronology department of the University of Arizona for processing. Mr. Morgan used the acid technique, which has been the vogue in the past. When the spore residue was placed on slides and examined through the University microscopes, they were so clouded with undissolved rock salt that if spores were present they were completely obscured. Therefore, I would conclude that the University of Arizona phase of the investigation was inconclusive. However, sufficient samples were available for a repeat performance, but Mr. Morgan has been too busy to repeat the analyses (Burdick, 1972 p. 26).

Other comments by Burdick concerning this interesting phase of the repeat analysis include the following:

I would have had some definite results from the redoing of the Grand Canyon pollen had it not been that the graduate students under Dr. Kremp messed up the processing in the laboratory. They don't seem interested in re-doing the samples (2/25/71).

The University of Arizona messed up the samples they processed, so we have to depend on the Loma Linda samples (2/19/71).

#### Loma Linda University Was Also On The Scene - 1970-71

Burdick maintained (1972, p. 25) that on this same July 1970 tour, "Drs. Bullas and Arthur Chadwick of Loma Linda took rock samples and processed them in

their Loma Linda laboratories." On page 26 Burdick added that:

The announced tour of Grand Canyon had been rather widely advertised by The Bible Science Association, and two scientists, Drs. Bullas and Arthur Chadwick, were sent along to also take samples, which they did from the same rocks where Mr. Delevan sampled. they too followed specific procedures to avoid contamination (1972, p. 26).

Correcting Burdick's assumption that Bullas was from Loma Linda University, Chadwick *et. al.* reported that:

Dr. Burdick incorrectly implied in his paper that Dr. Leonard Bullas . . . was a part of the Loma Linda team . . . Aside from his assistance in the initial collection of Grand Canyon samples and continued interest he has not been associated in our palynological investigations (1973, p. 238).

Later, Chadwick recounted this collecting trip as follows.

In 1971. I obtained a collecting permit from the National Park Service and accompanied C. L. Burdick to the Grand Canyon. His previous sample localities were relocated and new samples were collected, returned to my laboratory at Loma Linda University and processed by C. L. Burdick using techniques similar to those he had employed in his earlier work at the University of Arizona (1981, p. 7).

In 1972 Burdick summarized the Loma Linda results as follows:

. . . readers will observe, the Loma Linda results largely follow the same pattern of the palynology investigation as performed by Burdick in 1964 and 1965, while doing research for the University of Arizona. If there is any divergence in results it would appear that Loma Linda secured a slightly greater proportion of Angiosperms (1972, p. 27).

In this 1972 paper Burdick published six photographs of pollen grains— some gymnosperms and some angiosperms reportedly taken by Chadwick and others at the Loma Linda laboratory.

#### Was Howe Hoodwinked In 1973?

The editor of CRSQ at that time, Howe, received, modified, and published the 1972 Burdick manuscript, assuming that it had come with the approval of the Loma Linda workers through their collaborator, C. L. Burdick. Howe was surprised later to receive communication from A. V. Chadwick *et. al.* (1973, p. 238) that the Loma Linda workers ". . . were not contacted directly . . ." by Burdick as regards this manuscript and that it had been published with neither their knowledge nor approval. Howe decided thereafter to run closer checks on the source of data being published in CRSQ.

#### Who Actually Did The 1972 Research Chadwick *et al.* or Burdick or Both?

In comparing the 1973 disavowal of Burdick's 1972 paper by Chadwick *et al.* with the 1981 paper also penned by Chadwick, an apparent discrepancy arises regarding who actually performed the pollen extractions reported in Burdick's 1972 paper. Writing of this

1972 research in his 1981 report, Chadwick treated it as if it had been entirely Burdick's production:

. . . new samples were collected, returned to my laboratory at Loma Linda University and processed by C. L. Burdick using techniques similar to those he had employed in his earlier work at the University of Arizona (1981, p. 7) (Emphasis is mine not Chadwick's).

Evidently Chadwick had forgotten that back in his 1973 communication to CRSQ he had spoken of these same 1972 data as his own, avowing that Burdick had had little or no part in the analysis:

Aside from his assistance in the initial collection of Grand Canyon samples and continued interest *he has not been associated in our palynological investigations.* (1973, p. 238) (Emphasis mine not Chadwick's.)

We have thus far been unable to extract pollen or spores from the Hermit although the *Hakatai* has contained numerous well preserved palynomorphs some of which have been identifiable. That these may be contamination has not been ruled out. (1973, p. 238) (Emphasis mine, not Chadwick's.)

Thus in 1973 Chadwick and his coworkers were evidently discovering pollen from ancient rocks because they wrote in 1973, p. 238, that they had extracted "well preserved palynomorphs" from Hakatai shale and they were finding "anomalous pollen grains in Early Paleozoic sediments . . ." as well.

In 1981 Chadwick made no comment about his own out-of-place pollen grains. It is hoped that in some later publication Chadwick will describe and share pictures of his own 1973 palynomorphs from Precambrian strata, even if he feels they were contaminants.

#### Chadwick's Message in 1981

Chadwick clearly indicated in a short results section of his 1981 paper that:

A total of fifty samples from the same strata which Burdick had studied were processed. All slides were completely scanned. No single example of an authentic pollen grain was obtained from any of these samples. In fact, the slides produced from the Hakatai Formation were in most cases completely free from any material of biologic origin, modern or fossil (1981, p. 8).

He presented numerous reasons why Burdick's (1966 and 1972) pollen grain discoveries should be considered to be: ". . . modern contamination picked up either during collection and transportation or infiltrated into the sample itself prior to collection" (1981, p. 9). He maintained that Burdick's discoveries failed the test of reproducibility.

#### Rusch and Burdick Reply - 1982

While not necessarily defending the earlier Burdick discoveries, W. H. Rusch responded to Chadwick and to Solomon and Morgan by asserting that even if Burdick's findings could all be explained as modern contaminants, the fact still remained that various other workers had found such items as microspores and tracheids of vascular plants in the Cambrian (Rusch, 1973 and 1982). Burdick had also referred to these Cambrian findings in a 1975 Panorama of Science article and to workers who had located microfossils of

land plants in the Precambrian of Venezuela (1974).

In a final statement about the whole matter, Burdick drew attention to Chadwick's withdrawal from his (Chadwick's) own discoveries and apparent change of stance.

I saw slides of the work, which seemed to me to parallel the ones which I had obtained in Tucson. Later Chadwick seems to have changed his mind, and doubted that any ancient spores had actually been found; although I do not believe that all workers at Loma Linda share that view (1982, p. 144).

#### CRS Involvement

By Spring, 1983, the Research Committee decided that CRS had a major stake in this intriguing series of pollen papers! If, as Chadwick had asserted, reproducibility is a cardinal principle of the scientific method, then perhaps Chadwick's own negative results should be tested to see if they also are routinely reproducible. It was decided at the CRS meeting in April, 1983, that a wholesale reanalysis of both the Burdick and the Chadwick-Doher methods should be undertaken. E. L. Williams, G. F. Howe, and W. E. Lammerts agreed to collect samples at the Grand Canyon. Williams located a chemist, G. T. Matzko, who agreed to carry out the pollen extractions according to both procedures. G. F. Howe agreed to examine and photograph the resulting samples with a light microscope while Lammerts volunteered to carry out his own research on how likely it is that environmental pollen will contaminate microscope slide samples.

Although a brief mention of our findings has already appeared in CRSQ (Howe et al., 1986) the extensive results of these studies will be reported in two subsequent papers. It is hoped that this preliminary review will lay the foundation for later papers and perhaps challenge other workers— both creationists and evolutionists to go back to examine these supposedly ancient strata for the presence or absence of anomalous microfossils.

#### Acknowledgements

I thank Clifford L. Burdick for making the original discovery of spores in Precambrian Hakatai shale and other strata of Grand Canyon and thereby initiating this whole pattern of creationist interest in palynology. I am grateful, as well, to him and to Walter E. Lammerts for their permission to reproduce portions of their private correspondence. I appreciate the interest, encouragement, and financial support lent to these studies by members of the Research Committee of CRS. I also wish to acknowledge the contributions of many CRS members and friends to the Laboratory Project Fund— interest from which has covered some of the expenses involved in the investigations being reported in this series of papers. I thank Phyllis Hughes for assistance in preparation of the manuscript.

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## DILUVIOLOGY AND UNIFORMITARIAN GEOLOGY— A REVIEW

A. W. MEHLERT\*

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#### Abstract

*Interpretation of the fossil record from Flood and uniformitarian geology are compared. The different approaches of Morton and Woodmorappe to Flood geology are considered. The Flood model is superior to the uniformitarian model. Likewise the fossil record does not support any long-age concept.*

#### Introduction

When carefully considered in the light of questionable assumptions and practices engaged in by many historical geologists, the fossil "succession" can be very reasonably harmonized with modern diluviological concepts, even though much work remains for creationist geologists. Over the past few years there has been considerable discussion and controversy on the subject of Flood geology versus uniformitarian geology, and the fossil "succession" in the pages of *CRSQ*.

\*A. W. Mehlert, Dip.Th. receives his mail at P.O. Box 30, Beenleigh, Australia, 4207.

The main proponents involved have been Morton (1982, 1983, 1984) and Woodmorappe (1978, 1980, 1981, 1982, 1983a, 1983b, 1985).

The controversy boils down to one basic argument— does the fossil record accord more with the short duration, non-evolutionary Flood geology theory, or with the historical geology theory involving long time periods and evolution? I would like to examine features which can be used to support or detract from either, but firstly we must establish the differentiations involved in both Flood and uniformitarian geology, and the questions of superficial or detailed support of the competing models.