portions of the Mississippi Gulf Coast had previously been radiocarbon dated to Late Pleistocene and this is believed to place them within another older sea-level cycle (Foxworth, Priddy, Johnson, and Moore, 1962). These dates were later reinforced by the investigations of Otvos (1979) along both the Alabama and Mississippi Gulf Coasts. He conducted an extensive well-drilling program in an attempt to understand the geologic and stratigraphic history of this area. He also conducted radiocarbon dating of shallowly buried woody materials and humus to understand when these deposits were formed. While most of his age dates for the surficial woody materials were within the age range of Holocene to recent, he suggested that this "younger" woody material had been contaminated by groundwater and other (unnamed) things because directly beneath this shallowly buried material were underlying sedimentary and peat deposits which dated to the much older Pleistocene, i.e., >40,000 years before present (Otvos, 1979, p. 297). Because he believed that contamination was a problem, he then suggested that the "younger" materials (i.e., wood, paleosols, and sediments) all dated to the Pleistocene too (Otvos, 1979, p. 297). The few studies of this area that have occurred subsequent to this time have only served to reinforce the Pleistocene dates for the paleosols and tree stumps found within the surf zone at Dauphin Island, Alabama.

Recently the Pleistocene age issue for the tree stumps found within the surf zone at Dauphin Island has again been called into question. In June of 1996, the Alabama Geological Society conducted a portion of their annual field trip to the Dauphin Island area. As part of this trip the issue of the age of the paleo-maritime forest within the surf zone was a point of focus for the group. New radiocarbon dating of the tree stumps found within the surf zone was conducted as part of this field trip and the age of the wood was estimated to be 235 ± 80 years before present (English and Haywick, 1996, p. A-10). Hence, what were once Pleistocene ages for these tree stumps become very recent ages—ones which Otvos and others had previously rejected. Rather, these more recent dates support what the field trip leaders suggested as reflecting rapid erosion of the island.

Previously, I had suggested that although I had no means to date the tree stumps I did not believe them to be more than 200 years old (Froede, 1995, p. 148). Additionally, I suggested that the island is continuing to erode at an alarming rate (Froede, 1995, p. 146). It is satisfying to have uniformitarian confirmation of my present research. I share this with *CRSQ* readers in an attempt to show that we too can conduct successful research apart from a university setting. Let us get out there, look at the earth, and interpret it within the Young-Earth Flood Model! (Froede, 1995b).

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CRSQ—Creation Research Society Quarterly.

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Carl R. Froede, Jr.
2895 Emerson Lake Drive
Snellville, GA 30278-6644

Precambrian Pollen—A Response

Editorial Comment and Introduction

Recently a message critical of CRS-sponsored research was forwarded to the CRSnet. The work in question had to do with a finding of pollen grains in Precambrian rocks from the Grand Canyon. The author of the message refused to grant permission to publish his letter in the *Creation Research Society Quarterly.* Dr. Emmett Williams, the CRS president, prepared a response to the message, which is printed below. Due to the refusal of the author of the attack to allow his name to be used, he is referred to as "A" in Dr. Williams's response. According to conventional science, vascular land plants did not exist during the Precambrian. Hence, A took issue with whether there was contamination of the rock, the Hakatai shale, with modern day pollen. The original research was done by the late Clifford Burdick, but it was followed subsequently by more detailed studies by Emmett Williams, Walter Lammerts, George Howe, George Matzko and others. A mentioned some work by Arthur V. Chadwick, which initially gave negative results. Since the CRS sponsored research produced positive results, A accused the CRS of using sloppy technique, and also of being unwilling to publish Dr. Chadwick's research, which was instead published in *Origins,* a creationist publication based at Loma Linda University. A went on to criticize a tract by D. Russell Humphreys called "Evidence for a Young World" published by *Answers in Genesis.* Below is Dr. Williams's reply.

Recently a message was posted on CRSnet which took issue with the various field and laboratory efforts of the Society in isolating fossil material from Hakatai shale in Grand Canyon National Park. The post contains misrepresentations and outright falsehoods. Apparently,

from the abysmal ignorance shown in the post, the author has not studied the original papers!

The work on Hakatai shale is contained in the following Quarterly reports and letters.

1. Burdick, C. L. 1966. Microflora of the Grand Canyon. *CRSQ* 3:38-50.
2. Burdick, C. L. 1972. Progress report on Grand Canyon palynology. *CRSQ* 9:25-30.
3. Burdick, C. L. 1982. Reply to Rusch. *CRSQ* 19:144.
4. Chadwick, A. V., P. DeBord and H. Fisk. 1973. Grand Canyon palynology—a reply. *CRSQ* 9:238.
5. Howe, G. F., E. L. Williams, G. T. Matzko and W. E. Lammerts. 1986. Pollen research update. *CRSQ* 22:181-182.
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8. Howe, G. F., E. L. Williams, G. T. Matzko and W. E. Lammerts. 1988. Creation Research Society studies on Precambrian pollen, Part III: A pollen analysis of Hakatai shale and other Grand Canyon rocks. *CRSQ* 24:173-182.

Also another paper on the palynology of the Hakatai shale (HS) that was often referenced and discussed in the 1986-1988 CRS reports was:

9. Chadwick, A. V. 1981. Precambrian pollen in the Grand Canyon—a reexamination. *Origins* 8(1):7-12.

Individual comments by this anonymous author (A) will be examined and discussed.

A claimed that Burdick (Ref. 1) employed scanning electron microscopy in examining pollen from HS. This is not true. Burdick used optical microscopy.

A noted that. "The pollen isolated was pine, juniper, and Mormon tea." He failed to mention that fungal structures, plant stems and lycopods were also detected (Ref. 1, p. 45).

Later A stated that Burdick took no precautions against contamination. This is another falsehood. Burdick took precautions against contamination (Ref. 3). Then A said, "The work of Howe et al. (Refs. 5 and 8) was interesting in that they didn't guard against contamination as much as Chadwick (Ref. 9) did." [parenthesis ours] Earlier A mentioned that Chadwick ". . . washed the samples in filtered water, trimmed the samples to remove the exterior surfaces and took care to analyze his samples in a clean and sterile lab. Guess what? No pollen?

A failed to mention that in an earlier investigation Chadwick, DeBord and Fisk (Ref. 4) found fossil material in HS. When Howe et al. processed the HS samples, they trimmed them to remove the exterior surfaces. Then samples were cleaned in Varsol and non-ionic detergent and later cleaned in distilled water within an ultrasonic bath in clean laboratories with filtered air supplies. All containers were carefully cleaned with distilled water and soap, acetone, etc. where necessary. Matzko has a Ph.D. in analytical chemistry and Williams worked in a scanning electron microscopy (SEM) laboratory. We understand and practiced careful laboratory techniques. One does not perform SEM work without keeping the laboratory clean and samples contamination free, for any foreign material on SEM samples is readily apparent upon examination in the microscope.

The processing of samples was done according to Doher (1980) except that in some cases, the sample dissolution step using HF was unnecessary. Chadwick (1981) employed the Doher (1980) technique including the final step of placing the samples in an HF solution. Actually, we used the entire Doher/Chadwick method (with time in HF solutions) on two HS samples and obtained positive results in one instance!

Before undertaking the laboratory work, we discussed the sample dissolution procedures with several palynologists and analytical chemists. They warned that if the fossil pollen was silicified within the shale matrix, excessive time in HF solutions would destroy the grains. Many of the samples were completely dissolved in the HCl solution rendering the HF dissolution step unnecessary. The HF dissolution was only to be employed if the rock had not been completely dissolved in the HCl solution.

We repeat, lengthy exposures to HF can destroy fossil pollen incorporated in the shale matrix without affecting any possible contaminating modern pollen. We demonstrated that HF exposure would not destroy modern (contaminating) pollen. Fresh oak and pine pollen were placed in 48% HF solutions for periods of 64-70 hours. The pollen grains were still present after these exposures (Ref. 8, Figures 43-46).

Likewise, the samples were collected in the field in February on a bitterly cold day to avoid the pollinating season of native plants if possible. The collection technique was described (Ref. 8, p. 173) illustrating the careful procedures we employed. Howe et al., noted that "Scrupulous care was taken throughout the procedure to avoid contamination of the sample by atmospheric pollen in the laboratory" (Ref. 8, p. 174). This was done in all phases of the field and laboratory work. Thus the probability of field or laboratory contamination with modern pollen was extremely unlikely. Contamination of another sort will be discussed later. Again A observed ". . . that the positive results (finding fossil pollen in HS) comes from either one or two rock samples. Don't they even know?" [parenthesis ours]. If A had consulted the original report and the tables of collected data (Ref. 8), the answer to his question would have been evident. A concluded by stating that

"No graduate student in any geology department I've been in would have gotten away with such sloppy work." An amazing conclusion from a person who never read our reports or observed our work in the laboratory.

Another phase of our work included a study of contamination of laboratory slides during the pollinating season of various trees by Lammerts and Howe (Ref. 7). They showed that it is difficult to contaminate greased slides with pollen (outdoors and in the laboratory) even when you are trying to accomplish that goal! Lammerts was a plant breeder and geneticist and Howe is a botanist, so such work was within their area of expertise. A did not mention this study.

A continued by claiming that Chadwick's (1981) results were not reported in *CRSQ* — another lie. "The *CRSQ* simply will not publish results critical of young-earth creationism." Chadwick's work (Ref. 9) was discussed in detail in Refs. 5, 6, 7, and 8. More evidence that A had not consulted the original reports when he attacked our work.

Then A wrote that Chadwick stated that "Some of them regarded me as an evolutionist by then, . . ." We do not know who is meant by "them" but a careful reading of all *CRSQ* papers will reveal that Chadwick was never regarded as an evolutionist and all of us held his work in high regard. In all conversations and correspondence with the workers on this project, no one ever referred to Chadwick as an evolutionist. Possibly since A so easily misrepresented our work on this project, he did the same to Chadwick?

Finally A criticized Russell Humphreys for mentioning our pollen work in his tract Evidence for a Young Earth and accused Humphreys of dishonesty because he did not mention Chadwick's work. Humphreys had read our reports (More than A can claim—we wonder who is really dishonest?) and realized that we discussed Chadwick's work. Thus anyone that consulted our reports would have known of Chadwick's studies.

Comments by Burdick and Rusch

Burdick (1974) noted the discovery of pollen grains in formations designated as Precambrian by other geologists. Later Burdick (1975) and Rusch (1982) referenced reports concerning the detection of microspores of vascular plants and tracheids having bordered pits in Cambrian strata. None of these studies were isolated incidences ". . . since several workers in India, Australia, and the U.S.S.R. have reported these findings" (Rusch, p. 143). The point being that the results of the CRS studies were not unusual!

Another Type of Contamination and Models of Earth History

Realizing that HS is classified as Precambrian using the standard geologic column, could the shale have been contaminated later in geologic history? For instance, could the catastrophic forces of the universal Flood with associated massive erosion, deposition and tectonic activity have affected the HS if the formation had been deposited before the Flood? Also if the conjectures of Austin (1994) and Brown (1995) are viable, could the draining of the postulated lakes on the Colorado Plateau after the Flood to form the Grand Canyon have affected the HS? (Also see Williams, Meyer and Wolfrom, 1991; 1992a; 1992b; Oard, 1993.) Would the violent processes involved in these two events have deposited pollen and other plant material from more "modern" eras into the HS which was later incorporated into the rock structure? We think that such a circumstance may be possible, but we have no way of knowing if it happened.

Creationist models of earth history also will determine how an individual creation scientist will view our findings. For instance in 1991, Snelling asked the question, "Where do the Precambrian strata fit?" He suggested that much of the Precambrian fossil-bearing strata should be considered as Flood deposits. However, Wise (1992, p. 70) felt that ". . . Precambrian sediments were formed in a pre-Flood global water catastrophe." He suggested a regression of water in day three of the Creation week as a possibility. Steve Austin (1994, pp. 58, 62-67), in his excellent monograph on the Grand Canyon, listed Precambrian rocks as pre-Flood and Creation week formations. Also, old earth creationists would reject the presence of fossil material in HS for there would be no "advanced" plant life on earth at that time according to their model.

If a portion of HS was a Flood deposit, there would be no problem with our findings. Also, if the standard geologic column is not a reliable basis for any earth history model as discussed by Reed (1996), our results would cause no problems. However, if it is assumed that a Precambrian sediment was formed during the Creation week or before the Flood, there should be no fossils of any significance in HS. Thus as always the model determines "reality." This is one of the weaknesses of scientific models. How you visualize earth history is how you determine the validity of scientific evidence. You pigeonhole evidence based on which model you believe. Thus any fossil pollen and trachids in HS have to be, by definition, a result of contamination.

This circumstance was realized a few years ago when the former editor of the Quarterly, Don DeYoung, received a belligerent letter from a creationist criticizing an article. Since our HS work was mentioned in the paper the critic proclaimed that what we found was certainly not Precambrian pollen! We respect all creationist positions on various subjects, even if we do not agree with them. We admire those who have developed comprehensive creationist models and appreciate the

professionalism required to outline the necessary details. We have no argument with those who disagree with us if they prefer to believe that the fossil material we detected is from later contamination due to the Flood or other catastrophic activity. We realize it is a matter of personal opinion.

As a matter of interest, as Howe and Williams descended from the south rim of the Grand Canyon, sliding along the icy North Kaibab Trail to collect the rock samples, Howe asked Williams what he expected we would find. The answer was that we would find no fossil material in the rocks. Howe agreed. We were astonished at our findings! We have "no axe to grind" and realize that the results of our work have caused much disagreement among creationists.

It was not our intent to be self-serving in that investigation. However, we deeply resent any accusations that our field and laboratory techniques and procedures were remiss and that we contaminated the rock samples. We have spent the majority of our professional careers involved in laboratory work. We know how to follow and develop procedures to avoid contamination. It is easy for someone to sit in front of a computer and claim contamination when he has not read the original reports or does not know what was done! It is the height of arrogance and laziness and we reject such allegations as spurious.

Those readers who are interested in this investigation should obtain and consult the various articles and notes mentioned and decide for themselves how to interpret the evidence.

Recently a fellow creationist and I exchanged amiable letters over the pollen grains found in Hakatai Shale. The gentleman noted that a person who he thought was familiar with the Society's palynological work had claimed that all of the pollen detected in the shale was closely similar to the pollen of plants which grow in the Grand Canyon region today. This statement is not correct and a portion of my answer is given as follows:

In one place in your letter you mentioned that you were puzzled as to why there should be such close similarities between the pollen found in Hakatai Shale and the plants which grow in the region today. Below are two paragraphs from the Burdick paper Microflora of the Grand Canyon *(CRSQ* 1996. 3(1)38-50)

> It is important to note also that an entirely different type of vegetation is indicated by the fossil spores than that which is now growing in or near the Grand Canyon. Thus *Podocarlpidites* simply means a fossil plant resembling the genus *Podocarpus,* a group of species in the yew family or Taxaceae. This genus is found in rather moist areas, such as Japan, and indicates that prior to the deposition of the formations of the Grand Canyon, the climate was not so dry and arid as now.
>
> None of the plants resembling the fossil species or rather genera with the exception of *Ephedra* now live near the Grand Canyon. Thus the pollen grains of the yellow pine, pinion pine, spruce, and Douglas fir do not resemble those found in the various formations. This also indicates that the climate in the past was warmer and less arid than now. In other words these species survived because of their genetic variability potential to adapt to the increasingly arid conditions.

Burdick noted that most of the pollen found by him was different from pollen released by plants now. In other words these species survived because growing the region now. And fossil genera names were assigned to them. Also algae and fungal spores were detected. Once Burdick showed slides of his fossil pollen to the CRS Board. The ones removed from the Hakatai shale had the red color of the shale indicating they have been incorporated in the shale matrix and were not modern contamination.

In our study (Howe et al. 1988. *CRSQ* 24:173-182), the palynologist suggested that one pollen grain may be maple. Another set of pollen may be hemlock (Figures 15,16). Figure 21 may be elm pollen. Much of the pollen could not be identified but a great deal of it was not similar to plants which grow in the region today.

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Emmett L. Williams
5093 Williamsport Drive
Norcross, GA 30092-2124

Comment on "Mississippian and Cambrian Strata Interbedding . . ."

As a licensed professional geologist (TN-0129) with 24 years experience working in Nashville, I keep track of the creation/evolution controversy as a diversion. My attention was called to Mr. Douglas S. Sharp by an article on Talk.Origins Archive (http://earth.ics.uci.edu:8080/faqs/knee-joint.html) that showed him willing to correct errors when errors were demonstrated. There was one file on his web site, (http//www.sojourn.com/-revev/download.html), "The Faults Say No!," that I found to contain errors. I pointed these out to him in two e-mails and, to his credit, he withdrew the article from his web site.

One of the articles he used as a reference was:

Waisgerber, William, George F. Howe, and Emmett L. Williams. 1987. Mississippian and Cambrian strata interbedding: 200 million year hiatus in question, *Creation Research Society Quarterly* 23:160-165.

Sharp's article stated, "Mississippian Redwall limestone and Cambrian Muav limestone are interbedded, with the rock strata alternating back and forth? As this was unconventional, to say the least, I considered this worthy of further exploration. I was supplied a copy of the *CRSQ* article by the National Center for Science Education. I found the article to contain numerous instances of less than rigorous application of the scientific method. These are:

1. First sentence of abstract. "Two field trips were made to study" Two trips to study a purportedly very important outcrop in depth is entirely too little time to do a credible job. In the next phrase "the supposed unconformity," the author's use of the word "supposed" and later in the article "alleged" violates the scientific method rule of tentativeness, and implies all previous work is faulty. Indeed, it borders on an *ad hominem* against all of science. The authors use the words "supposed," "supposedly," and "alleged" 24 times in the paper. They have announced their conclusion in the first sentence!
2. Third sentence. The seven criteria of an unconformity are absent in the case of a paraconformity, the existence of which as a type of unconformity was not mentioned in the paper. Most paraconformities do not show any lithologic or structural evidence and generally require detailed work to map.
3. "Do great damage to the macroevolution model of origins." What a positive statement! Was the writers' purpose to do science or act like Sherman in Georgia?
4. In "Nature of the contact line," The Temple Butte was mentioned as "appears absent" at the locality. They recognized its existence at other locations but failed to report an unconformity at the trail site which was required whether the Temple Butte is present there or not. When the Temple Butte is present, there are actually two unconformities at this horizon. The first between the Cambrian Muav and Devonian Temple Butte, and the second between the Devonian Temple Butte and the Mississippian Redwall Limestone. Not recognizing this was a very serious error.
5. A formal description of the lithologies and a detailed stratigraphic study of the two formations at the contact was not given. Little in the way of lithologic description of the strata was given, and no thin sections were reportedly prepared on the rock samples to properly classify them. From what little information was presented, one cannot determine from the article if there is or is not an unconformity present.
6. What specifically are the facies changes at the site? From Figures 5 and 6 the transition appears sudden and knife sharp. If true, it is a channel cut rather than a facies change. Facies reflects a change in the environment of deposition, which is reflected in different lithologies and occurs over considerably longer distances.
7. Figure 2 (10 meters along trail) and Figure 5 would appear to show a 6 m deep erosion channel cut in the Muav. The break in lithologies seems sharp at the vertical contact. In Figure 1 (Shelton, Fig. 270, p. 274) is a photograph (permission for use granted by Dr. John S. Shelton) of an exposure of the Temple Butte Limestone in the east wall of Pipe Creek, west of Kaibab trail. Shelton describes it as:

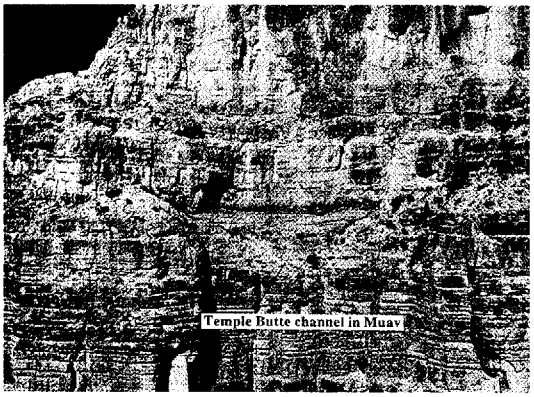

Figure 1. Shelton, Figure 270, p. 274.

> Between them (Redwall and Muav) is the most *inconspicuous* formation in the Canyon, the *Temple Butte Limestone.* In this view its upper contact is sharply defined and slightly wavy. Its

lower contact is partly concealed by talus but is distinctive for its sag just below the center of the scene. No strata are bent, no deformation is involved; in this place the uneven, gnarly beds of the Temple Butte fill a channel about 30 feet deep cut into the top of the Muav Limestone. A dozen or more such filled hollows can be found in the Canyon walls, most of them in relatively inaccessible places and partly obscured by loose debris. Between some of the channel fillings, and throughout many miles of the Canyon, the Temple Butte Limestone is missing altogether and the Redwall Limestone rests directly on the Muav Formation.

8. The other supposed Muav that was shown as interbedded with the Redwall may well have been either misidentification of the lithologies or a Muav-like facies in the basal Redwall. These alternatives were not mentioned. The lack of an alternate hypothesis was a substantial violation of the scientific method. Other alternate hypotheses must be advanced.

9. On page 165 of the article, they quote from Shelton (1966, p. 276) to show how flat a surface the disconformity is. The quote presented in the paper is:

> The extraordinary flatness of the disconformable contact at the base of the Redwall limestone, which generally lacks even the minor channeling seen beneath the Temple Butte, implies that the landscape that developed in the interval between the accumulation of the two formations possessed remarkably little relief.

One would expect that this was the end of the statement by Shelton, but the authors deleted the final, most important sentence. "Some of the smoothing may have been accomplished by wave action as the sea advanced, for *the basal Redwall beds contain recognizable detrital ingredients derived from the Temple Butte Limestone."* (Emphasis added).

Here the authors have made that most egregious error, the out-of-context quote. The "recognizable detrital ingredients derived from the Temple Butte Limestone" satisfies the author's requirement number 2 (p. 162) for an unconformity. Given the author's apparent desire to show the opposite, it is unsurprising that they deleted this sentence. It destroys their entire thesis.

10. The authors, while trying to show the conformable nature of the contact between the Muav and Redwall, appear to have ignored other pertinent parts of Shelton's book. On page 288 is figure 288 (Figure 2 of this letter) showing an aerial view of the west face of Frenchman Mountain 10 miles east of Las Vegas, Nevada. Describing it on page 289, Shelton says:

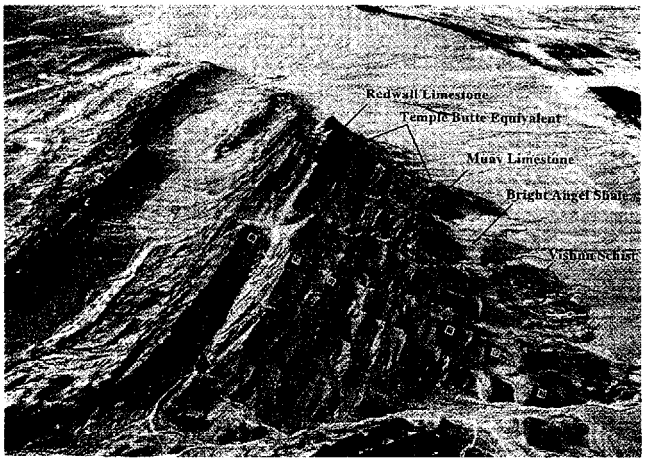

Figure 2. Shelton, Figure 288, p. 288.

> In short, here is the whole Grand Canyon Paleozoic section, tilted about 55 degrees to the left, (down to the east). The top and bottom formations and the Redwall Limestone near the middle are almost identical in lithology and fossil content with their counterparts in the Canyon walls, but the total thickness here is a little over 8,000 feet—twice that of the Paleozoic strata in the Canyon. Two changes are noteworthy. Between the Muav and Redwall, where in the Canyon we found less than 100 feet of Temple Butte Limestone, there are here over 2,000 feet of limestone and dolomite, much of it undoubtedly deposited during the time represented by the two pre-Redwall disconformities in the Canyon.

11. Conclusions: The conclusions reached were entirely unjustified on the basis of two field trips to the site. Resting their evidence on a two-day examination of one outcrop, the authors would have us rend asunder all of geology.

On the basis of the agenda stated by the writers in the very first sentence, the cursory and unsubstantiated examination of the exposure, the failure to report pertinent information in a quoted reference, the use of an out-of-context quote, the failure to report an obvious disconformity, and the conclusions reached, this paper is deemed to have no scientific merit. Its unimportance is witnessed by being ignored in the refereed journals in the nine years since publication. Had the authors uncovered something of importance, further study would certainly have been performed and published.

Reference

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James L. Moore
13th Floor, L&C Tower
401 Church Street
Nashville, TN 37243-0445
vsrn89a@prodigy.com

A Reply to James Moore's Comment on "Mississippian and Cambrian Interbedding . . ."

Thank you for allowing me to respond to the Moore critique. It has been nine years since the aforementioned article was published. I will answer parts of the critique even though time may diminish ones remembrance for details.

Before I do, allow me to establish my credentials. I am a licensed geologist in California (No. 58) and Oregon (No. 11) and a practicing engineering geologist in California (No. 33) and Oregon (No. 11). I have had a consulting practice in California since 1962.

I have been a member of the Geological Society of America since 1958 and a member of the Association of Engineering Geologists shortly thereafter. I am also a member of the American Institute of Professional Geologists.

Rebuttal of Parts of Mr. Moore's Letter to the Editor

Moore's opening statement that he, ". . . found the article to contain numerous instances of less than rigorous application of the scientific method." must itself be subject to questioning with reference to earlier investigators of the Grand Canyon. What is meant by "rigorous?" In reading the works of earlier investigators of the Grand Canyon, I have found a decided lack of a, ". . . rigorous application of the scientific method." among them because the earlier investigators applied western European geologic time to Grand Canyon formations as though western European geologic time was truth. Need I remind Mr. Moore that, during the 19th century, western European geologic time was determined on the bases that 1) there was an unconformity at the commencement of a geologic period, and 2) there was an unconformity at the end of that same period. This belief in unconformities delimiting geologic periods was discarded though the results (the belief in a Paleozoic Era, Mesozoic Era and Cenozoic Era) were continued as truth the world over.

Why did early Grand Canyon investigators not make use of multiple working hypotheses when each viewed supposed Grand Canyon unconformities? It is my opinion, that they did not suggest alternative explanations because these geologists did accept evolution and geologic time (ever changing in duration during the 19th century) as factual. Therefore, Mr. Moore and those of like mind have never made "rigorous application of the scientific method." because belief in evolution and geologic time has blinded them to alternative options.

Moore's first page indicates that Waisgerber, Howe and Williams made a ". . . less than rigorous application of the scientific method." in 12 instances. Some of these "instances" will be discussed by Waisgerber herein.

Instance No. *1 (part a).* "Two trips to study a purportedly very important outcrop is entirely too little time to do a credible job."

What is a "purportedly very important outcrop"? Since the word "purport" can mean "false being," or "false appearance," or a "false intention," the use of the word "purportedly" in "purportedly very important outcrop" seems to suggest a geologic structure exhibiting false features. I could agree with such a definition except that is not what Mr. Moore meant.

Also, what is the definition of outcrop in "purportedly very important outcrop."? My definition of "outcrop" has always been that part of a formation which crops out of the ground, thus implying that other parts of the formation nearby are concealed. Along the study site, both the Redwall and Muav are totally visible. There are no outcrops in the study area. The supposed unconformity is open for macro-inspection.

In my opinion, two trips to study an outcrop can be sufficient when one is able to conclude (based on 25 years of professional field studies) the weakness in the belief that 200,000,000 years elapsed from the end of Cambrian (Muav) depositional time and the coming of Mississippian (Redwall) depositional time. Where is the geologic evidence for a withdrawal of the sea after Cambrian time and a return during Redwall time? The mere presence of detritus from a lower formation within the upper formation is not evidence for 200,000,000 years of geologic inactivity. It can be evidence for localized submarine dynamism on unconsolidated sediments which have not yet been raised from beneath the sea and consolidated diagenetically as are the formations seen today.

Instance No. *1 (part b).* Along with the first criticism regarding only two days of field study comes another Moore criticism. Mr. Moore states that the phrase "supposed unconformity" and other words used the writers ". . . violates the scientific method of tentativeness, and implies all previous work is faulty."

Our rebuttal comes with questions. Firstly, where and when in the known geologic literature did the

geologic column developed for the Grand Canyon conform to a "scientific method of tentativeness. . ."?

Secondly. Where in past literature and current literature does any evolutionary geologist admit that geologic time is "a trial study done on a provisional basis," for that is a meaning of the word, "tentative"? Since evolutionists declare evolution, geologic time and paraconformal unconformities to be factual, it becomes mandatory that all previous work be labeled faulty because evolution, geologic time and paraconformities are not now deemed "tentative."

Instance No. 1 (part c). Mr. Moore objects to usage of words as "supposed" and "alleged" because it ". . . borders on an *ad hominem* against all science."

Let me make it clear that "supposed" and "alleged" were never intended to border on an *ad hominem* against "all science." It is most decidedly an *ad hominem* aimed directly at all those scientists who have abandoned scientific skepticism as an attribute because they accept evolution, geologic time and paraconformities as settled questions and then pontificate accordingly.

Instance No. 2, on page 1. Mr. Moore criticizes that the word "paraconformity," was not mentioned in our report as a geologic structure lying between the Cambrian and Mississippian strata, even though Mr. Moore admits that: "The seven criteria of an unconformity are absent. . . ." in paraconformities.

Are not the absences of seven criteria of an unconformity sufficient grounds for a conclusion that "paraconformity" is merely a convenient word of evolutionary derivation which was brought into existence by those who prefer evolution and geologic time to any other explanation?

A common understanding of the word, "paraconformity" is that it is an unconformity at which strata of different ages are parallel and the contact is a simple bedding plane. The identification of a "paraconformity" therefore, is based less on Moore's ". . . detailed work . . ." philosophy and more on the questionable use of fossils as time markers. Considering the "detailed work" aspects offered by Mr. Moore, if that is part of the definition of a paraconformity, then there might be hundreds of intraformational paraconformities within Grand Canyon along with known interformational unconformities. Most Grand Canyon strata lie horizontally; the bedding plane of one stratum lying on a lower stratum. All that is needed therefore is an observed difference of fossil content, above and below a selected bedding plane to "prove" intraformational paraconformities and interformational paraconformities.

Since other unconformities can be determined usually via lithologic and/or structural evidence, and since, by Mr. Moore's own admission, the seven criteria of an unconformity are absent in paraconformities, it can be concluded hypothetically that a paraconformity is an evolutionary crutch. It is a convenient word, much like the word "peneplain" conjured up by evolutionists, to satisfy their own deficiencies in the application of multiple working hypotheses. They do not practice the scientific method rule of tentativeness themselves, because evolution and geologic time are permanently imbedded in their literature.

Instance No. 5, re: The Temple Butte formation being absent at our study site, and whatever follows in the remnant part of Mr. Moore's Instance No. 5 paragraph.

Mr. Moore is wrong in suggesting that the Temple Butte's existence elsewhere demands the presence of an unconformity between the Mississippian Redwall and the Cambrian Muav at our locality. Mr. Moore will not admit to any other hypotheses as to why the Temple Butte is absent along our study area, which is his professional shortcoming.

Where the Devonian, Temple Butte Formation associates with the superjacent Mississippian Redwall and the subjacent Cambrian Muav is a study in itself. Conclusions drawn from elsewhere within Grand Canyon and applied to the trail site we studied seems no more correct than Mr. Moore's objection to our conclusion about geologic time determined along the trail site, except that nowhere along the trail site is there any kind of evidence for a withdrawal of the Muavian Sea 200,000,000 years before the return of a Redwallian Sea.

Our hypothetical conclusion (based in small part on the two-day Grand Canyon study) will continue to be that there is no such segment of geological time as the Paleozoic Era anywhere in the world. The term Paleozoic Era was given to the world, non-tentatively, because of 19th century misunderstanding of one of the great sacred cows of evolution and geologic time; the principle known as Uniformitarianism. Nineteenth century "stratigraphers" did not apply it to their own studious efforts.

If *the present is the key to the past,* as suggested by *Uniformitarianism,* then the seas of ancient times could have supported contemporaneous environments that differ one from another in faunal content. Any lands which were raised from beneath the sea could have supported continental life forms on a contemporaneous basis.

The hypothesis of faunal contemporaneity suggests that there were littoral, neritic, bathyal, abyssal and hadal benthic, marine environments which existed contemporaneously, much as they do today. Fauna were created to fit the various environments. If such a hypothetical conclusion were accepted as an alternative

option then the Grand Canyon formations would reveal to the currently blind that "what you see is what you got." The formations within Grand Canyon could have been deposited continuously, with differences in deposition and in minor structures occurring within different localities.

During the course of deposition (alternative geologic time), minor erosional sequences could have occurred because submarine dynamism was as functional then as the modern sea is today The Temple Butte Formation need never have been deposited along the trail site.

Instance No. 6. "A formal description of the lithologies and a detailed stratigraphic study of the two formations at the contact was not made."

There was no need to prepare a detailed petrologic (including a thin section) study of the two formations at the contact because a paraconformity does not exist along the trail site.

As for the need for thin sections, anyone viewing the supposed contact between the Muav and the Redwall along the trail site would have recognized that preparation of thin sections would be meaningless because both the Muav strata and the overlying Redwall strata varied vertically and horizontally. What was done instead was to make "field calls." along the trail of the kind with which the writer has had great success in courts of law. The "field calls" indicated that Redwall and Muav strata along the trail exhibited lithologic similarities as well as lithologic differences. A five power hand lens view of the strata and the absence of geologic structures indicative of an erosive, off lapping sea followed 200,000,000 years later by a depositional on lapping sea were sufficient to conclude in the field that a belief in a 200 million year hiatus was in fact, beyond belief.

Instance No. 10. Moore's belief that ". . . the authors have made that most egregious error, the out-of-context quote concerning Shelton (1966, p. 276), because we did not admit to an added, following statement by Shelton that: ". . . the basal Redwall beds contain recognizable detrital ingredients derived from the Temple Butte Limestone."

The criticism by Mr. Moore that we deliberately deleted Shelton's added statement is invalid because those suggested detrital ingredients are not part of our field site. Even so, the fact of detrital inclusion in a higher formation from a lower formation, when not related to dynamic derived erosive structures suggests the need for alternative explanations.

Shelton's statement "The extraordinary flatness of the disconformable contact at the base of the Redwall limestone, which generally lacks even the minor channeling seen beneath the Temple Butte . . ." was cited because it offers similarities to the Redwall-Muav relationship along the trail. It was important that Shelton's implication: ". . . that the landscape that developed in the interval between the accumulation of the two formations possessed remarkable little relief" be applied to our trail site. His "detritus" statement is of no local value.

Had we the time and intent to analyze the Shelton article to its fullest, we could have questioned Shelton's opinion that there was an: ". . . interval between the accumulation of the two formations . . ." We could have asked for proof of such an opinion. Our objective, however, was to study the trail site rather than argue with anyone.

Instance No. 12. The conclusions reached were entirely unjustified on the basis of two field trips to the site.

We will stand with our conclusions. Our intent with the report was to "rend asunder all of geology" because the Mr. Moores of the world, geologists and biologists, have misled the world the past 150 years, that geologic time and evolution are the only scientific explanations worthy of consideration.

That our opinions are being ignored ". . . in the refereed journals in the nine years since publication. . . ." mean nothing to me. The history of science is replete with examples of ignorance in "refereed journals." Ignorance often associates with the development of pseudoscientific standards developed by technicians who support narrowly devised explanations in accordance with their own technically inspired biases. There will come a time in the 21st century when more rational, learned men will re-evaluate the work done in the 19th and 20th centuries and find that evolution and geologic time are as mythical as any Babylonian, Egyptian or Greek myth, because evolution and geologic time were framed originally in a context of insufficiency of data.

I challenge Mr. Moore and any other believer in evolution and geologic time to go to the trail site and take photographs of the Redwall-Muav "paraconformity" along the trail and across the canyon from the trail, along the opposite wall. Then identify the "paraconformity" on the photographs. Then send the photographs to the *Creation Research Society Quarterly* for inclusion in a later publication. After that, I will critique your "paraconformity" photographs.

Should George Howe and Emmett Williams desire to critique those "Instances" not discussed herein, they are welcome to them. As for me, my answers herein are deemed sufficient to challenge Mr. Moore.

William Waisgerber
18445 Majestic View Drive
Anderson CA 90067