

Creation Research Society Quarterly

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

VOLUME 24

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RENEW NOW FOR 1988/89

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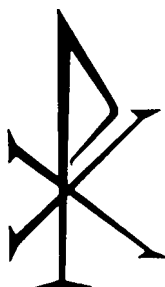
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Instructions to authors can be found in June Quarterly.

Errata

Editorial Comments (CRSQ 24:53).
The articles by DuBois, McGhee and Benton appear in the December Quarterly (Volume 24).

Cover Photograph

In an experiment on image rotation a molybdenum trioxide (MoO_3) crystal was evaporated onto a Formvar substrated grid. The crystal is seen in the center of the photograph at 85,000 magnification. The crystal was then bombarded with a beam of electrons in a transmission electron microscope. The array of white spots around the MoO_3 crystal is an electron diffraction pattern caused by the coherent scattering of electrons from the regularly-spaced lattice planes in MoO_3 (orthorhombic structure). Crystals often have been employed by evolutionists as examples in nature that violate the second law of thermodynamics. Creationists have pointed out that this reasoning is incorrect.

Photograph by Richard R. White
Caption by Emmett L. Williams

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QUOTE

I think we may be close here to the reason why the rise of psychology has not ushered in the reign of happiness. Try as it might to give us skills for living, psychology has never been able to give a reason for living. It offers no vision. And, as I have suggested, the reductionist nature of so much psychology often has the unintended effect of making life seem less important, less significant, less worth the struggle.

Kilpatrick, W. K. 1986. Why secular psychology is not enough. *Imprimis* 15(4):3. Hillsdale College.

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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 \$16.00 (\$18.00 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 \$16.00 (\$18.00 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: \$19.00 (\$21.00 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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Remarks by the President

Recently many evolutionists not only have been distorting the classical expression of the scientific method by referring to evolution as a "fact" but also they have been chastising creationists by saying they are religionists masquerading as scientists in an effort to intrude religion into science. The presupposition of these evolutionists is that science excludes religious considerations and deals only with observations of matter. But, should not the best science deal with all of reality? Many of the so-called anti-creationists themselves have what might be called an underlying agenda, and that is to promote a secularistic or naturalistic position that excludes recognition of supernatural power.

However this may be, it is true that creationists recognize a supernatural creator. This creator could be either a non-personal god (of Plato and Aristotle) or the personal Judeo/Christian God. But this whole issue of naturalism vs. supernaturalism is only one side of the coin; the other is mechanistic being based upon sensory data (facts) obtained by observing nature. The two mechanistic positions have been termed the macroevolution model and the abrupt appearance model. The former perception is that of a single tree with extinct and extant forms of life on their branches; whereas for the latter there is a forest of separate physically-unrelated trees (possibly 7,000) each representing a "kind." Most creationists do not hold "fixity of species" but rather that there has been some diversification within the kinds.

Members of the Creation Research Society believe in a supernatural personal Creator-God; and they also hold that the preferable interpretation of data from nature is the abrupt appearance model. Information about both models legally can be presented in classrooms of the United States, and it is our contention that responsible science teachers will be informing students of the difference between "macroevolution" and "abrupt appearance" with strengths and weaknesses of both positions.

Wayne Frair.

Editorial Comments

In preparing these remarks, I did a literature survey of articles and notes in the *Quarterly* that were the result of field or laboratory research work. There are 103 entries and this does not include many original projects of research primarily of the library type. Therefore the charge that creationist scientists do not perform research work is false.

It is amazing that scientists who are creationists do any research at all! Creationists do not have access to Federal money taken from the taxpayer's pockets that is available to evolutionists. Often the companies or schools where creationists work are not sympathetic to their views so their work must be extracurricular. Only dedicated creationists would expend the herculean effort necessary to do research beyond their vocational activities.

Possibly some may think that teaching in a Christian school would allow a person time to perform research. Generally the massive teaching loads, administrative duties, outside speaking schedules and other required activities leave the creationist very little time for his own projects. So you can understand why it is remarkable that any research has been done. While evolutionists have only to climb foothills to do their research, creationists must scale mountains.

Some of the articles in this issue are the culmination of years of work in the Grand Canyon area. Also many of you could help the Research Committee by collecting wildflowers in your area for our herbarium. You may have research opportunities within your area. Why not do a preliminary study and write a letter to the editor? The Research Committee needs your help, both financially (Laboratory Project) as well as some of your expertise in doing the work.

Emmett L. Williams, Editor

THE BIOLOGICAL ISOLATION OF SHIVA TEMPLE

JOHN R. MEYER* AND GEORGE F. HOWE**

Received 20 December 1986 Revised 29 June 1987

Abstract

Geographical isolation is an important constituent of most scenarios of the evolutionary origin of species. In an earlier study, Anthony (1937) compared mammals of Shiva Temple with those of the North Rim of the Grand Canyon, and concluded that isolation on Shiva was incomplete since the two populations demonstrated no significant differences. We attempt here to assess the degree of biological isolation between Shiva Temple and the nearby North Rim of the Grand Canyon. Based on limited direct observation of selected environmental variables, analysis of vegetation, and study of the known distribution of small mammals, we conclude that for some small mammals, Shiva Temple is probably biologically isolated from the North Rim. The implications of these findings are discussed in terms of the creationist concept of a recent and catastrophic origin of the Grand Canyon. Key Words: Grand Canyon, Shiva Temple, geographical isolation, biogeography, speciation, Anthony.

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Introduction

In September of 1937, Harold Anthony, mammalogist with the American Museum of Natural History, led an expedition to the top of Shiva Temple in the Grand Canyon (for details see Meyer, 1987 and Anthony, 1937). Figure 1 is a photograph of Shiva Temple from the North Rim. He intended to examine the supposedly isolated populations on top of Shiva Temple and compare them to similar populations on the adjacent North Rim. After close examination of mammals from both localities, Anthony concluded that no significant differences existed, at least in the morphological characteristics which could be evaluated at that time.

Failure to find detectable differences in the two faunal groups was attributed by Anthony to incomplete isolation of Shiva Temple from the North Rim and to a recent origin for whatever incomplete isolation barriers might currently exist. Suggestion of evidence of incomplete isolation came not only from the observation of deer antlers and human artifacts on Shiva but also from the philosophical incompatibility of the concept of complete isolation with failure to observe evolutionary changes. The isolation of Shiva Temple for small mammals, however, may be much more complete than Anthony realized. McKee (1937) has observed that there may well be strict isolation:

Almost any animal is able to ascend Shiva Temple; but few, if any of the small forest dwelling species . . . do because of the desert conditions which prevail below. Thus, the climatic belt in the area below Shiva represents as complete a barrier for some species as an ocean body.

The nature and extent of these climatic factors along the most likely migration route to Shiva (the saddle between the northeast base of Shiva and the adjacent North Rim, which we shall call "Shiva Saddle") have never been directly quantified. Although the fauna of the Kaibab Plateau and the isolated top of Shiva have been studied—see Anthony (1937), Hoffmeister (1971), and Rasmussen (1941)—no evaluation of existing data has been performed to assess the likelihood of

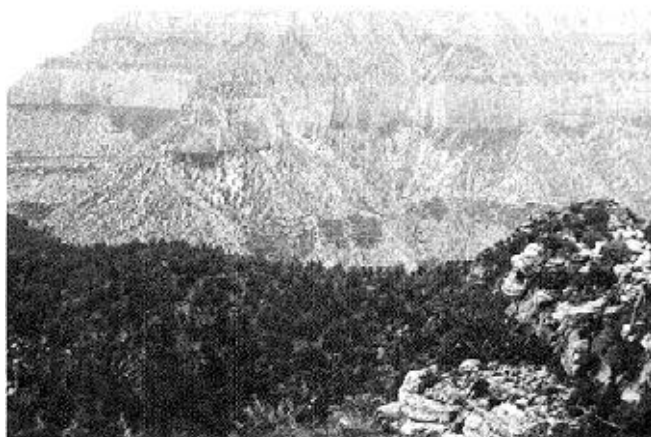


Figure 1. Photograph of Shiva Temple from the North Rim. The ridge leading out to the Saddle is seen in the foreground. The Anthony expedition reached the top of Shiva by following the ridge and then descending into the Saddle. They then ascended the talus slope seen in the center of the left third of the photograph. Final access to the top was gained at about the center of the rim.

particular small mammals existing on the Saddle itself. Thus, the degree of isolation of Shiva Temple is still an open and important question to ecologists and population biologists interested in the biogeography of the Grand Canyon vicinity and the genetics of isolated populations.

Our purpose in this study is to prepare a preliminary evaluation of the degree of isolation of Shiva Temple using direct observation of vegetation and selected climatic variables along with known habitat preferences of the small mammals of the Grand Canyon area. Implications of these data will be discussed in relation to the failure of Shiva Temple mammals to demonstrate significant differences from their North Rim counterparts.

Methods

Aerial reconnaissance photography: On May 31, 1979, we chartered a single-engine Cessna for survey of Shiva Temple. The right-hand doors were removed and Shiva was circled four times at different altitudes to allow nearly 100 photographs of the vegetation and general topography of the area. Figures 2 and 3 are aerial photographs of the top and sides of Shiva.

Data collecting stations: Measurements were made on July 2 and 3, 1985. North Rim measurements were started at 3:00 p.m. local time and were taken hourly until 3:00 p.m. the next day. Saddle measurements were started at 3:00 p.m. but were terminated the next morning at 5:00 a.m. due to declining water supplies and the need to finish the rigorous climb back to the North Rim before the heat of the day arrived.

On Shiva Saddle, data were gathered at 6300 feet at two stations, one shaded and the other unshaded. Five stations were established on the North Rim at 7650 feet. North Rim station 0 was located in an unshaded area a few feet from the edge of the Rim itself. Station 1 was located a few feet away from station 9, under the shade of a juniper tree. Stations 2, 3, and 4 were located at 10, 20, and 30 yards, respectively from the edge of the Rim in unshaded areas. Data are reported here only for stations 1 and 4 since they are shaded and unshaded areas, respectively, on the Rim which are comparable to similar areas from the Saddle.

Soil Temperatures: These were measured using standard mercury-type soil thermometers with brass probes. These instruments provide an average temperature of the first five or six centimeters of topsoil.

Relative humidity and air temperature: These data were gathered at all stations using two matched, motor-driven Psychrodyne psychrometers. Relative humidity readings were converted to vapor pressure deficits to reflect the actual water stress placed on organisms, as the vapor pressure deficit is a measure of how much more water the air can extract from nearby plants and animals. Vapor pressure deficits were corrected for the 1350 feet difference in altitude between the Saddle and the Rim. Air temperature was taken as the psychrometer dry bulb reading.

Plant identification: While hiking along trails and cross country a list was kept of plants seen at various locations. The list is by no means exhaustive as we could carry only a limited number of plant taxonomy books in our packs and we had no permits to collect plant specimens for more detailed herbarium study.

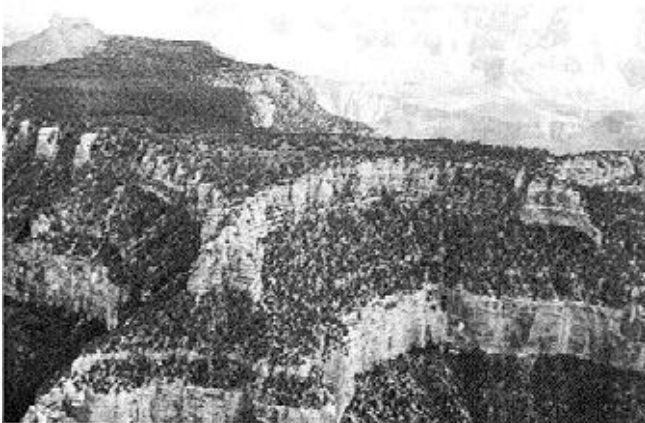


Figure 2. Aerial photograph of Shiva Temple. The northwest corner of Shiva is in the foreground and the direction is to the southeast. The upper reaches of the inner gorge of the Colorado River can be seen just above the edge of Shiva, left of center.

The identifications are thus "field calls" carried out between periods of hiking and camping.

Results

Soil temperatures for shaded and unshaded stations 1 and 4 of the North Rim and the shaded and unshaded stations on the Saddle are shown in Figure 4. Generally, soil temperatures at the shaded Saddle station were as much as 13°C higher than the shaded Rim station in the early afternoon. The unshaded Saddle station was up to 12°C higher than the unshaded Rim station at the same time (although there was one unexpected reversal of this at 3 p.m.) Soil temperatures from both locales, whether from shaded or unshaded areas, tend to converge during the night.

Vapor pressure deficit calculations derived from relative humidity measurements are displayed in Figure 5. At all times of measurement, the vapor pressure deficit was higher in the Saddle than along the Rim. These differences ranged from a maximum of nearly 0.4 inches of mercury at 6:00 p.m. to a minimum of less than 0.1 at 4:00 a.m. Changes in vapor pressure deficit closely reflect the prevailing *air temperature* as shown in Figure 6. At all times of measurement, air temperature was from 1 to 6°C warmer on the Saddle.



Figure 3. Aerial photograph of Shiva Temple. The western edge is in the foreground and the view is to the northeast. The north wall of the Grand Canyon is seen in the upper left, just below the wing tip.

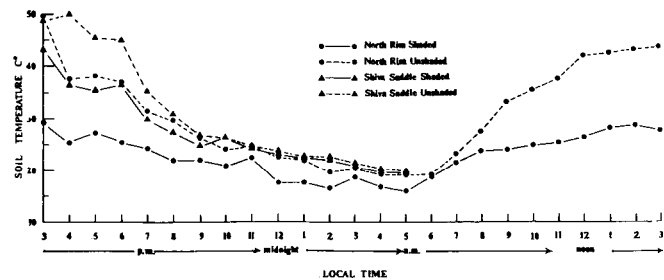


Figure 4. Comparison of shaded and unshaded soil temperatures for the edge of the North Rim and Shiva Saddle area.

General vegetational distribution as derived from both aerial photographs and ground-based observations are presented diagrammatically in Figure 4. Detailed species lists for the areas under study are given in the Appendices A and B. The Saddle area is populated almost exclusively with pinyon pine and juniper, while the sun-exposed south walls of Shiva, the ridge and the North Rim are composed of pinyon pine, juniper and shrubland. The top of Shiva Temple (see Figure 7) and the lower reaches of the Kaibab Plateau at the North Rim contain heavy homogeneous stands of Ponderosa Pine. Though not shown in Figure 8 because of the fine resolution needed, the southern edge of the top of Shiva and the North Rim also exhibit pinyon pine, juniper, and shrubland vegetation in a narrow strip of a few dozen or so yards in width. Aerial photographs provide evidence that the distribution of plants for Shiva Temple is similar to that of the North Rim.

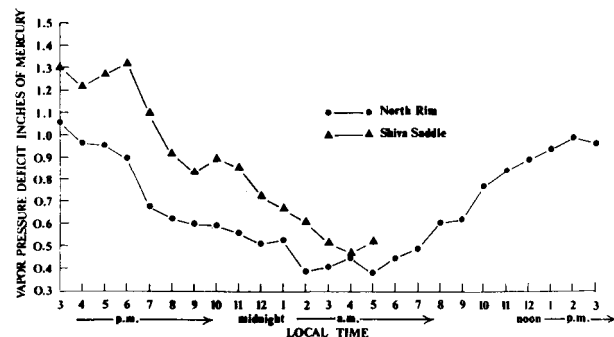


Figure 5. Comparison of vapor pressure deficit for the North Rim and for the Shiva Saddle area. Data are corrected for the 1350 feet difference in altitude between the two areas.

Discussion

Physical features: In considering the possible biological isolation of Shiva Temple, it is necessary to review the physiography of the area. Figure 9 is a cross section of the Shiva Temple/North Rim area and gives a general picture of the topography and geology. The edge of the Kaibab Plateau at Shiva Expedition Point is very nearly the same altitude as the top of Shiva, with the two areas separated by nearly two miles horizontal distance. In traversing the area from the North Rim it is necessary to descend approximately 350 feet below the Rim to a ridge which runs nearly one-half mile. A 1000

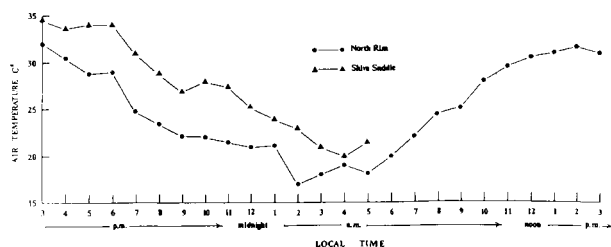


Figure 6. Comparison of air temperature for the North Rim and for the Saddle area.

additional feet descent from the south end of the ridge is required to gain access to the three-quarter mile long Saddle. From here the top of Shiva towers 1350 feet overhead and can be reached only by ascending a steep talus slope through the Hermit Shale strata, a near-vertical pitch through the Coconino, a series of several shorter talus slopes through the Toroweap and another nearly-vertical pitch up the Kaibab to the top. It will be noted, however that there are areas in the Kaibab and Coconino where the pitch is broken and it is primarily through these areas that access is gained to the top (see Figure 10).

The Saddle is narrow and flanked on either side by nearly vertical walls which descend at least another 1000 feet to the basins below. Compared to other ridges separating side canyons in this vicinity, the Saddle is small in area and remarkably flat. Thus, it appears to receive not only the direct heating from the sun throughout most of the day (except when the sun is the lowest in winter when it may be shielded by Shiva Temple for a few hours in mid-afternoon) but also the heat of rising air from the reaches below—see Malm (1974) for a more detailed description of air movements in the Canyon.

Physiography of the area: Shiva, like the Kaibab Plateau from which it is separated is capped with Kaibab limestone. This highly porous layer results in the complete lack of standing water on Shiva; and except for the tiny seasonal streams, it is almost completely lacking on the North Rim. Rasmussen, (1941) reports that standing water on the North Rim is rare and is usually found associated with sink holes which have become sealed by silt. He indicates, "They



Figure 7. Aerial photograph of Ponderosa Pine forest on Shiva Temple.

are practically the sole source of water for all animals. They are not abundant, and it is often several miles between them." (p. 234) Thus, they are not accessible to many of the smaller mammals with limited home ranges.

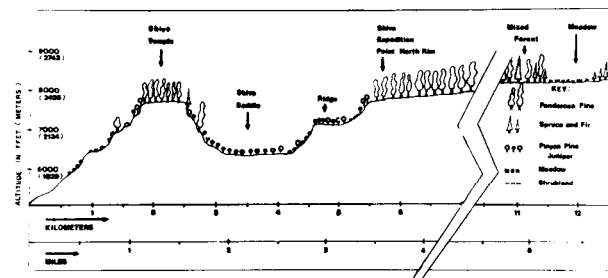


Figure 8. Cross-sectional profile through the Shiva Temple/North Rim area indicating dominant vegetation. A two-fold vertical exaggeration is used. The profile is approximately north/south in orientation with south on the left.

The physiography of the area heavily influences climate and this in turn controls vegetation. Thus, the limited, preliminary climatological data we have obtained show major differences in the environment for 24 hours or less, between the Saddle and the Rim, and the vegetation reflects this same differential. The contrasts in climatological data and vegetational observations for the Saddle and North Rim are highly consistent with the concept of a significant climatic barrier on the Saddle. Thus, small mammals which are restricted to the Ponderosa Pine forest of the North Rim would likely find the Saddle inhospitable, precisely as previously noted in the Shiva Temple file in the Grand Canyon National Park Research Library.

Vegetational differences: Although our plant list is preliminary and limited primarily to those species which yield to field identification, several important patterns emerge from the data. The pinyon pine and juniper forests of the ridge and the Saddle between Shiva Temple and the North Rim differ markedly in plant species composition from the two Ponderosa Pine forests they separate—see Appendix A. The pinyon and juniper stands have much bare rock and soil between scattered trees and are characterized by a desert component of plant life which is largely missing at both Shiva and the North Rim.

Only 16 of the 75 plant species noted in the Appendix were seen at both localities (Shiva Saddle and North Rim). Thus, most of the plant species we observed (59

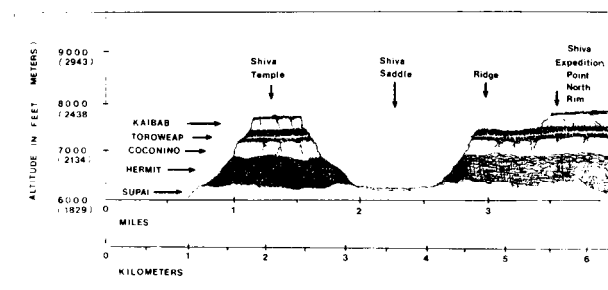


Figure 9. Cross-sectional profile through the Shiva Temple/North Rim area showing correlation between geological strata and topography. A two-fold vertical exaggeration is used. Profile is approximately north/south in orientation with south on the left.

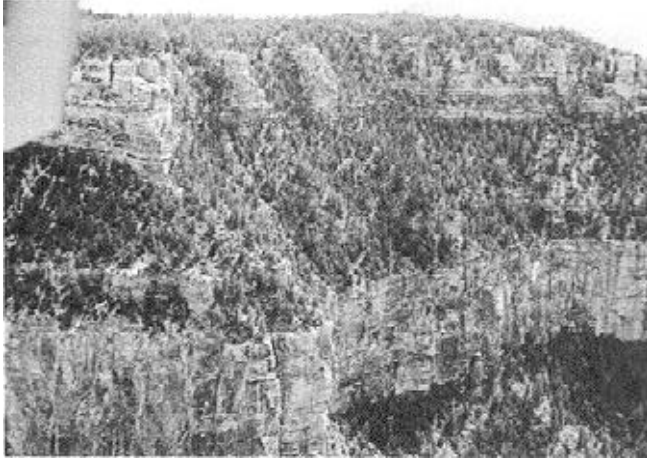


Figure 10. Aerial photograph of northeast corner of Shiva Temple. The final approach to the top for the Anthony expedition was apparently up one of the steep talus slopes along the left edge of the rim.

out of 75) were found at either the Ponderosa forests above or the pinon pine-juniper Saddle below, but not both. This difference between the plant species present in the two areas appears to provide an additional obstacle to gene flow between mammal populations of the Saddle and the Rim.

Our aerial photographic reconnaissance of Shiva Temple provides evidence that its vegetation is strikingly similar to that of the adjacent North Rim. The southern and southwestern parts of Shiva are apparently recipients of warm, dry air ascending along its cliffs and spilling over a short distance before ascending farther. Thus, the climate along these edges is apparently considerably more xeric than the rest of Shiva. This is evidenced by the dominant pinon pine and juniper, with some scattered shrubland in this area. An identical situation exists along the edge of the North Rim, as discussed by Golike and Howe (1975).

As one approaches the Rim from the north or main part of the Kaibab Plateau, there is a striking vegetational change. The homogeneous, mature Ponderosa Pine forests begin to fade into scattered stands composed of stately individuals, punctuated by increasing areas of pinon pine and juniper trees. Within eight to ten yards of the Rim, distinctively xeric desert type plants such as cliff rose and pin cushion cacti, begin to appear and the Ponderosa fade out of the picture. Thus, within a few hundred feet as one approaches the edge, one passes through a vegetational change equivalent to ascending several thousand feet on a more gentle slope. Such a zone would otherwise support much more mesic vegetation like Ponderosa Pine or even aspen. This phenomenon is known as "zonal suppression"—see Golike and Howe (1975). Zonal suppression and the difference of vegetation on the Saddle as compared to the North Rim may present barriers to animal migration.

Mammalian biogeography: Is the climatic barrier sufficient to isolate the mammals of Shiva Temple and prevent gene flow between it and the North Rim? For some mammals the answer is clearly no. Appendix B displays the biogeography of mammals of the area as obtained from several different sources. From this it can be seen that a number of small mammals of the

canyon area are equally at home in the vegetational associations of the Saddle, the North Rim, and Shiva. Many of these animals are at home on rocky slopes and can easily work their way up rock crevices. All that is needed is time before at least some of them, over a period of generations, could work across the Saddle and up Shiva. While this would reduce gene flow, it could not be claimed to completely stop it.

On the other hand, there are several species of small mammals which are at home in the Ponderosa Pine forests but do not frequent areas composed of the vegetation seen on the ridge or the Saddle itself. As seen from Appendix B, these would include the Bushy-tailed Wood Rat (*Neotoma cinerea*), the Brush Mouse (*Peromyscus boylii*), and the Canyon Mouse (*Peromyscus crinitus*). At least two other species appear to be residents of the rim rock area along the edge of the Kaibab Plateau and along the edge of Shiva Temple. These would include the Pinyon Mouse (*Peromyscus truei*) and the Cliff Chipmunk (*Eutamias dorsalis utahensis*). Furthermore, the Kaibab squirrel (*Sciurus aberti kaibabensis*) is absent from both the Saddle and Shiva Temple. The Saddle is clearly a barrier to this mammal because it is an obligate feeder on cones and twigs of Ponderosa Pine trees which are not found on the Saddle (see Meyer, 1985 for a detailed discussion of the biology of the Kaibab squirrel).

Thus, it is quite likely that these mammals are not on the Saddle and they would find the environment of this area too harsh to make a crossing. It might be asserted here that some breakdown of isolation could occur since man, raptors, or even tornadoes might have dropped individual specimens of these otherwise isolated animals. While this is possible, we believe that such events would be quite rare and would not likely constitute a consistent mechanism for significant gene flow between small mammal populations on the North Rim and Shiva Temple. Such mechanisms have clearly failed in the case of the Kaibab squirrel which does not inhabit the mature Ponderosa Pine forest on Shiva. Furthermore, some evolutionists appear open to the possibility that an occasional, limited influx of genes from the parent population could enrich genetic diversity within the isolated population and enhance speciation. For example see Stansfield, 1977.

For the above noted mammals at least, we would predict that gene flow across the Saddle is highly limited or totally absent. For these species it appears that Shiva Temple is, indeed, an "Island in the Sky," as it was originally entitled by Anthony; but its relatively recent isolation from the North Rim has been insufficient to allow even the small changes in characteristics which would be well within the "kinds" of the creationist model. The situation is thus much like that of the Kaibab and Abert squirrels which have supposedly been separated by several million years but which show differences only in tail and belly coloration for some of the individuals in each population—see Meyer (1985) and Williams (1986) for details.

Additional Support of the Model

A possible way of gaining considerable, additional information on this intriguing problem would be to make a more detailed vegetational analysis of the area and to collect additional environmental data at dif-

ferent times of the year. More importantly, direct trapping of the mammals on the saddle itself could be used to ascertain the biogeography of the area and to evaluate rates of movement of existing small mammals across the Saddle. Further research will necessitate the granting of permits to collect plants and to trap small mammals in order to do more sophisticated genetic analysis.

Conclusion

Most evolutionists believe that if two populations are reasonably well isolated for long periods of time in nature, mutations and natural selection will take their course to produce microevolutionary changes which may lead to "allopatric speciation" and, ultimately, macroevolution. Anthony originally viewed Shiva Temple as having been completely isolated for a long period of time. Therefore, at first, he fully expected to find significant differences between the mammals of the North Rim and Shiva Temple because the microevolutionary prerequisites of isolation and time had seemingly both been met. But, in the only existing study of these mammals, Anthony (1937) acknowledged that he could find no detectable differences to support his evolutionary assumptions. He therefore reversed his position and concluded that the isolation must surely have been lacking, otherwise microevolution and perhaps even speciation would have occurred.

In contrast, we present evidence consistent with a recent origin and significant isolation of Shiva Temple from the North Rim. The conclusions of our preliminary studies support the previously suggested existence of major climatic and vegetational differences between Shiva Saddle and the North Rim which would be a barrier to the migration of a number of small mammals. We have also shown from existing data that some mammals present on both Shiva Temple and the North Rim are probably absent from the Saddle. This is in direct opposition to Anthony's final conclusion which was unchallenged in the literature.

Thus, given the apparent geographic isolation of Shiva, we tentatively conclude that one of two views is likely to be true:

1. If microevolutionary changes may be expected to occur as a result of isolation *then* the isolation of Shiva Temple must have occurred very recently—so recently that no microevolutionary changes had time to occur. This is of considerable importance since most uniformitarians would assert that the large canyon separating Shiva from the North Rim was carved during the Pleistocene, up to 50,000 or perhaps 100,000 years ago—see Babbitt (1981) and Krutch (1958) for a popular discussion. In reality it appears that the isolation occurred so recently that there has been insufficient time for even microevolutionary change to have occurred. This adds strength to the argument that the Grand Canyon itself is also of rather recent origin.

2. But, *if* Shiva Temple has been isolated for considerable time, *then* it is an exceptional instance in which neither microevolution nor allopatric speciation has resulted. This conclusion would also appear to be inconsistent with a considerable segment of evolutionary theory.

Thus, it appears that data supporting isolation of Shiva Temple for some mammals provides evolutionary theory with a two-horned dilemma. On the one hand, short-term isolation of small mammals on Shiva Temple presents the problem of a recent formation of this topographical feature. On the other, long-term isolation of small mammals on Shiva Temple without concomitant changes in gene frequency is hardly consistent with allopatric speciation.

From our study of the literature on population genetics and the demonstrated changes of gene frequencies within isolated populations, we suspect that the first conclusion (recent isolation) is valid because there are numerous data supporting the belief that isolation for even relatively short periods of time may yield small, limited, but measurable degrees of biological change. Thus, we tentatively conclude that Anthony was wrong and that Shiva Temple became isolated from the North Rim only very recently.

Future Work

An approach of the type we have used here opens the possibility of indirectly evaluating the age of any geographic barrier by studying the small terrestrial animals whose ranges are dissected by that same barrier. If their separation occurred quite recently, as seems to have been the case at Shiva Temple, one would expect to find that the populations are quite similar.

Acknowledgements

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Appendix A

Comparative Biogeography of Plants of Shiva Temple/North Rim Area.

Plants observed on two hiking expeditions from the campground at North Rim to Shiva Expedition Point or Shiva Saddle, North Rim, Grand Canyon National Park, June 2-5, 1979, and July 1-4, 1985. Plants have been alphabetized by genus names.

The list is not exhaustive as we had only limited time to botanize, a small number of taxonomy books in our packs, and no permits to collect plants for subsequent study. Thus, identification was carried out in the field between periods of hiking and camping.

One or more numbers are listed after each plant, indicating where the particular species was found among the following regions:

- I. North Rim campground and higher altitude trails. Complex forest of spruce, fir, aspen, and Ponderosa Pine. 8200 feet plus.
- II. Sunken meadows along the upper trail. 8200 feet plus.
- III. Ponderosa Pine forests of the lower trail down to and including Shiva Expedition Point. 7600-8200 feet.
- IV. Exposed, rocky, south-facing cliffs at Shiva Expedition Point. 7600 feet.
- V. Sheltered ravines sloping both eastward and westward from center of Shiva Expedition Point. 7600 feet.
- VI. The trail from Shiva Expedition Point down to Shiva Saddle. 6300-7600 feet.
- VII. Shiva Saddle itself. 6300 feet.

More exhaustive treatments of plant communities on the entire Kaibab Plateau are found in Rasmussen (1941) and Merkle (1962).

Abies concolor, white fir: I.
Achillea lanulosa, western yarrow: III.
Agave utahensis, Utah agave: IV, VI, VII.
 Algae mats, dried with the soil: VII.
Amelanchier utahensis, Utah serviceberry: IV, VI, VII.
Aquilegia coerulea, white columbine: I.
Arctostaphylos sp., manzanita: VI, VII.
Artemesia sp., sagebrush: IV, VI, VII.
Asclepias subverticillata, poison milkweed: III, VII.
Aster sp., aster: III.
Berberis repens, Oregon grape: I, II.
Calochortus sp., mariposa lily: VII.
Castilleja sp., paint brush: I.
Ceanothus fendleri, buckbrush: I.
Cercocarpus ledifolius, curleaf mountain mahogany: IV, VI.
Cercocarpus sp., (a broad-leaved cercocarpus—not ledifolius): VI, VII.
Chamaebatiaria millifolium, fernbush: IV, VI.
Circium sp., thistle: III.
Cowania mexicana, cliffrose: III, IV, VI, VII.
Cryptantha sp., yellow borage: III.
Delphinium nelsoni, larkspur: III.
Draba asperella, golden whitlow: II, III.
Ephedra torreyana, ephedra or jointfir: IV, VI, VII.
Erigeron sp., daisy: II.
Fragaria sp., wild strawberry: I.
Fraxinus cuspidata var *micropetala*, flowering ash: V.
Fritillaria atropurpurea, chocolate lily or fritillary: III, V.

Galium boreale, bedstraw: IV.
Geaster, earthstar fungus: VI.
Geranium richardsonii, cranesbill: I.
Gilia aggregata, skyrocket gilia: V.
Gutierrezia sarothrae, snakeweed: VI, VII.
Heuchera sp., alumroot: III.
Juniperus communis, common juniper: I, II.
J. monosperma, one-seeded juniper: IV, VI.
J. utahensis, Utah juniper: IV, VI, VII.
 Lichens (small size, on ground): VII.
 Lichens (large ones up to 23 cm diameter, on ground): III.
Linum Lewisii, blue flax: II.
Lithophragma tentellum, star Bethlehem: III.
Lomatium sp.: I, II.
Lotus wrightii, deervetch: I, III.
Lupinus palmeri, lupin: I, II, III.
Mammillaria arizonica, Arizona pincushion: III, IV, VI.
Mertensia sp., bluebells: I.
 Moss pads, dried: VII.
Oenothera caespitosa, white tufted evening primrose: III.
Opuntia sp., prickly pear: VII.
Pedicularis centrantha, wood betony: III.
Penstemon palmeri, penstemon: VII.
P. sp., scarlet bugler: I.
P. virgatus, wandbloom penstemon: I, (?)VII.
Phacelia heterophylla, caterpillar plant: I, III.
Phlox sp., phlox: I, IV, VI.
Picea engelmannii, Englemann spruce: I.
P. pungens, blue spruce: I.
Pinus edulis, pinyon pine: IV, VI, VII.
P. ponderosa, ponderosa or western yellow pine: I, III, IV, V.
Potentilla sp., cinquefoil: II.
Pteridium sp., bracken fern: I.
Pteospera andromedea, woodland pinedrops: III.
Quercus gambelii, Gambel oak: III, IV, V.
Q. turbinella, scrub live oak: IV, VI, VII.
Ranunculus sp., buttercup: II.
Rhus trilobata simplicifolia, Utah squawbush: VII.
Ribes sp., currant: III, IV, V.
Robinia neomexicana, New Mexico locust: III, V.
Sambucus sp., elderberry: III.
Smilacina racemosa, false Solomon's seal: IV.
Stipa sp., needlegrass: VII.
Swertia radiata green gentian: II.
Thalictrum sp., meadow rue: III.
Townsendia excapa, ground daisy: III.
Trifolium sp., clover: III.
Yucca baccata, banana yucca or datil: IV, VI, VII.

Appendix B

Comparative Biogeography of Mammals of Shiva Temple/North Rim Area.

The left-hand column displays the scientific and popular names of mammals of the area. The remainder of the columns identify the presence of each animal in specific areas based on the indicated sources.

The second and third columns identify animals known to be on Shiva Temple, based on the American Museum of Natural History (A.M.N.H.) species list from the 1937 Anthony expedition and Hoffmeister's work (1971). Columns four and five identify animals

present on the Saddle based on the distribution indicated by Hoffmeister (1971) and the indication by Rasmussen (1941) of specific inhabitants of the *Pinus juniperus* community of which the Saddle is composed. The sixth and seventh columns identify animals

present on the North Rim as indicated by Hoffmeister (1971) and Rasmussen (1941) for the *Pinus brachypterm* (*Pinus brach.*) which compose the area along the edge of the North Rim. Note, *Pinus brachypterus* is the scientific name for Ponderosa Pine.

Comparative Biogeography of the Shiva Temple Area

Scientific Name Popular Name	Source Reference and Grand Canyon Location					
	Shiva Temple		Shiva Saddle Area		North Rim and Kaibab Plateau	
	A.M.N.H. Shiva Temple	Hoffmeister Shiva Temple	Hoffmeister Saddle	Rasmussen Pinus- Juniperus	North Rim	Rasmussen Pinus- Brachypterus
<i>Bassariscus astutus</i> Ringtail Cat	-----	Present	Present	-----	-----	-----
<i>Canis latrans</i> Coyote	-----	Present	Present	Present	Present	Present
<i>Citellus lateralis lateralis</i> Ground Squirrel	-----	-----	-----	-----	-----	Present
<i>Citellus variegatus utah</i> Rock Squirrel	Present	Present	Present	Present	Present	Present
<i>Erethizon dorsatum spixanthum</i> Porcupine	-----	Present	-----	Present	Present	Present
<i>Eutamias umbrinus</i> Uinta Chipmunk	-----	-----	-----	-----	Present	Present
<i>Eutamias dorsalis utahensis</i> Cliff Chipmunk	Present	Present	-----	Present*	Present*	-----
<i>Eutamias minimus consobrinus</i> Wasatch Chipmunk	-----	-----	-----	-----	-----	Present
<i>Neotoma lepida monstabilis</i> Desert Wood Rat	Present	-----	Present	Present	-----	-----
<i>Neotoma cinerea</i> Bushy-tailed Wood Rat	Present	Present	-----	-----	Present*	Present
<i>Peromyscus boylii rowleyi</i> Brush Mouse	Present	Present	Present*	-----	Present*	-----
<i>Peromyscus crinitus</i> Canyon Mouse	Present	Present	-----	-----	Present*	-----
<i>Peromyscus eremicus</i> Cactus Mouse	-----	-----	Present	-----	-----	-----
<i>Peromyscus maniculatus</i> Deer Mouse	Present	Present	-----	Present	Present	Present
<i>Peromyscus truei</i> Pinon Mouse	Present	Present	-----	Present*	Present*	-----
<i>Spirogale putorius (gracilis)</i> Spotted Skunk	-----	Present	Present	Present	-----	-----
<i>Sylvilagus nuttallii</i> Nuttall's Cottontail	Present	Present	Probable	Present	Present	Present
Other Major Species of the Area Not Directly Related to the Present Study						
<i>Sciurus kaibabensis</i> Kaibab Squirrel	-----	-----	-----	-----	Present	Present
<i>Lepus californicus</i> Black-Tailed Jack Rabbit	-----	-----	-----	Present	Present	-----
<i>Lynx rufus baileyi</i> Bobcat	-----	Probable	Probable	Present	Present	Present
<i>Odocoileus hemionus hemionus</i> Mule Deer	-----	Present	Probable	Present	Present	Present
<i>Urocyon cinereoargenteus</i> Gray Fox	-----	Probable	Probable	Present	Present	-----

Note: Locations listed as "probable" are those for species indicated by Hoffmeister as being found throughout the Park or specifically indicated as probable for a given location.

* = Found primarily in the rim rock area along the edge of the Kaibab Plateau.

CREATION RESEARCH SOCIETY STUDIES ON PRECAMBRIAN POLLEN, PART III: A POLLEN ANALYSIS OF HAKATAI SHALE AND OTHER GRAND CANYON ROCKS

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Abstract

Samples of Grand Canyon formations of Hakatai Shale, Hermit Shale and Supai Formation were collected using special care to avoid field contamination by atmospheric pollen grains. These samples were subjected to pollen extraction both with and without the use of hydrofluoric acid. The pollen extracts were examined and objects therein photographed under light and scanning electron microscopes. Pollen grains of Pinus, other gymnosperms, and angiosperms were recovered from certain Hakatai Shale samples, together with what appear to be fungal and algal spores.

Slides covered with open drops of water or oil were exposed to the atmosphere at various laboratories for a total of 400 days. Objects present on those slides were photographed. Not one of the objects found on these exposed slides was positively identified as a pollen grain. It is concluded that pollen-bearing plants existed while the lithification of Precambrian Hakatai strata occurred—a conclusion which fits with the young earth catastrophist view of origins but conflicts with uniformitarian macroevolutionism.

Introduction

C. L. Burdick (1966, 1972) reported finding pollen grains of seed plants in Precambrian Hakatai shale of the Grand Canyon. A. V. Chadwick *et al.* (1973) also admitted finding pollen grains in Hakatai shale while using Burdick's extraction techniques. In a later paper, however, Chadwick (1981) reported that he was unable to recover any pollen grains from other Hakatai shale samples processed by a different procedure involving hydrofluoric acid (HF). He implied that the pollen grains previously recovered by Burdick (and presumably by himself?) were of atmospheric origin and had probably contaminated the rock samples during collection or transportation and were not "... authentic examples of Precambrian pollen ..." (1981, p. 11).

The Research Committee of the Creation Research Society (CRS) authorized E. L. Williams, G. F. Howe, G. T. Matzko, and W. Lammerts to repeat Burdick's research. In February, 1984, rock samples were collected in Grand Canyon. Later pollen extractions were made and analyzed microscopically.

In Howe *et al.* (1986) a preliminary account of these findings was presented. Papers on the history of CRS pollen research and the problem of atmospheric contamination (Parts I and II of this three-part series) have been published—Howe (1987) and Lammerts and Howe (1987).

Methods and Materials: Collection of Samples

A permit to collect samples from rock exposures of Hermit Shale, Supai Formation and Hakatai Shale

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along the South Kaibab Trail was secured from the National Park Service. The samples were collected on February 10, 1984, a winter day when relatively few species of plants were pollinating in the Grand Canyon. The Hakatai Shale layer was chosen because it was from such material that Burdick (1966 and 1972) and Chadwick *et al.* (1973) had previously extracted pollen grains and that Chadwick (1981) had later reported failure to find pollen. Hermit Shale was also selected to see if similar grains could be recovered from other shales and the Supai Formation was included as a control.

Rock samples were taken after first chipping back three to four inches under the rock surface with a hammer and chisel. A new plastic bag was removed from its carton and opened for only a few seconds during rock sample collection. As inner fragments were chipped loose, they fell directly into the open plastic bag which was held directly beneath the chiseled surface. These precautions were taken to preclude field contamination with atmospheric pollen during sample collection.

After a sample was bagged, the plastic sack was immediately closed, labeled, sealed and placed inside its own plastic canister on which a duplicate label and seal were applied. Thus there was no chance of confusing the samples once they had been taken. A list of the samples and specific details of their collection is given in Table I. Samples were shipped to G. T. Matzko who processed them and performed pollen extractions in the chemistry laboratory at Bob Jones University, Greenville, South Carolina after E. L. Williams had performed some preliminary separations to observe the action of HCl and HF on the mineral samples.

An Experiment on Airborn Contaminants in the Microscopy Laboratory

Since some of the microscope work was done by G. F. Howe in two different laboratories at Reese Academic Center (RAC), of The Master's College, we wanted to estimate how likely it was that samples might have become contaminated with atmospheric pollen during slide preparation and microscopic observation. We wished to know what types of objects

Table I

Information pertaining to rock samples collected, South Kaibab Trail, Grand Canyon, 2/10/84. Altitude readings were taken directly from NPS altitude markers or with a pocket altimeter calibrated to each altitude marker appearing along the trail. Slight distances in altitude were estimated.

Layer	Sample No.	Approximate Altitude (ft.)	Collection Data
Hermit Shale	H 1	6300	50 feet vertically below Hermit-Coconino contact line.
Hermit Shale	H 2	6200	80 feet vertically below Hermit-Coconino contact line.
Hermit Shale	H 3	6200	At a lookout area under a rock ledge.
Supai	S 1	6000	150 feet vertically below Supai-Hermit contact.
Supai	S 2	5900	About midway into the Supai region. Mixed grey and red material.
Supai	S 3	5200	At lower Supai region. Whitish sandstone.
Hakatai	HK 1	3500	At the upper section of Hakatai rock exposure. Flakey. red, shaley material.
Hakatai	HK 2	3300	About half-way down the Hakatai trail area. Hard, granular quartzitic material with whitish and pink grains.
Hakatai	HK 3	3200	Considerable distance below HK 2 collection site. Brownish shaley material.

would fall on glass slides exposed to the air of these particular laboratory rooms for periods of time longer than those previously reported by Lammerts and Howe (1987).

Six slides were placed on tables in three different laboratory rooms—the two rooms in which slide preparation and light microscope observation were accomplished (RAC 11 and RAC 19, Table III) and one extra room in which none of the analysis was performed (RAC 17). Three of the slides in each room had a drop of microscope oil added to them before the exposure and the other three in each room had drops of tap water added at intervals throughout the exposure period. At the end of 7-9 days, 14 days, and to 38-57 days one oil and one water slide from each room were examined for the presence of contaminant objects and a photographic record was made—see Figures 3-7. Although these slides were untouched during their exposure, the rooms where they were exposed were being used daily for classes and laboratory sessions, presenting ample opportunity for contamination by pollen grains if such is likely.

Sample Processing and Pollen Extraction

The methods we used were modifications of the method briefly described by C. L. Burdick (1966). Certain subsamples or separations were first ground in a micromill for five minutes after cleaning under running water with a brush and abrasive cleaner—see Table II. Other samples were of loose material which was neither washed nor ground in the mill. Scrupulous care was taken throughout the processing to avoid contamination of the sample by atmospheric pollen in the laboratory.

One gram of each sample was placed in a 50 ml centrifuge tube where it was covered with Varsol (being thoroughly wetted). Five drops of non-ionic detergent were added and the mixture was stirred.

The sample was then covered with distilled water in the centrifuge tube and vibrated in an ultrasonic bath as follows: 10 minutes for samples a-f and 20 minutes for samples g-p, (Table II). The tubes were then each centrifuged for three minutes at 1500 rpm. The solution was decanted and the samples were washed by varying combinations of 10-minute HCl washes and/or 10-minute water washes, as noted in Table II.

Samples o and p, Table II, had one 10 minute wash (10% HCl) followed by a 90 minute digestion in HF and then two water washes. The HF digestion of extracts o and p was carried out by adding HF drop by drop to the sample and stirring at five minute intervals with a polyethylene stirring rod in a 250 ml polyethylene beaker. A vigorous reaction was evident when HF was first added and CaF₂ formation was evident in the sample.

After centrifuging and decanting, a zinc bromide solution of specific gravity 2.2 was added and the mixture stirred. Each sample was then placed into the centrifuge and rotated at 1500 rpm for three minutes. Since pollen of angiosperms and gymnosperms and spores of algae, fungi and other cryptogams (non-seed producing plants) have a specific gravity of about 1.5, they float in the zinc bromide solution. The technique of Chadwick (1980) was used for removing the float from the tubes. The float was then transferred to 15 ml centrifuge tubes where distilled water was added so that pollen grains and spores present would sink.

The precipitate was next washed twice in water and centrifuged for five minutes. In most samples 5 ml of glycerin was added, centrifuged and decanted. This glycerin separation was then shipped in labeled vials for analysis under light microscopy. In samples o and p, however, a pH 7.00 buffer [0.05 molar potassium phosphate (monobasic)-sodium hydroxide buffer] was added to the float material for shipment. In a few samples the pollen grain extract was left in water for analysis with scanning electron microscope (SEM) (completely eliminating the glycerin step) because we found that glycerin inhibited SEM analysis. In all instances a fresh disposable dropper was used to make the final transfer to new glass shipping vials with plastic screw caps.

Making Light Microscope Slides and Photographs

The formica or painted wood tops of laboratory tables used to prepare slides were washed with water and wiped dry before each work period. In some cases a dissecting needle was used to transfer small drops of glycerin or water from a given sample vial (Table II) to a freshly cleaned glass slide. The dissecting needle was thoroughly cleaned with soap and a scrubbing pad, flamed red over a Bunsen burner, and allowed to cool before using. On other occasions the transfer was made with a fresh disposable glass dropper, the tip of which had been heated and drawn to a narrow point to fit into the sample vial.

Each new slide was cleaned before use, rinsed in water, rinsed in 95% alcohol, flamed dry, and then allowed to cool. A fresh glass cover slide was used and

Table II

Information pertaining to pollen extracts made from the various rock samples collected, South Kaibab Trail, Grand Canyon. Each rock sample carries the same notation established in Table I. These extracts were made in the Chemistry Department at Bob Jones University. The light microscope slides were made and examined at The Master's College.

Pollen extract	Rock Sample as per Table I	Sample Texture	No. 10-min. acid given to sample followed by No. of water washes	No. slides first, No. of slides with pollen second
a.*	HK 1	solid chunks	2 : 1	3 : 0
b.	H 1	loose material	2 : 1	1 : 0
c.	H 2	loose material	2 : 1	1 : 0
d.	H 3	loose material	2 : 1	2 : 0
e.	S 1	loose material	2 : 1	2 : 0
f.	S 2	loose material	2 : 1	1 : 0
g.	S 3	loose material	2 : 1	1 : 0
h.	HK 1	loose material	3 : 1	1 : 0
i.	HK 2	loose material	3 : 1	1 : 0
j.*	HK 1	solid chunk	3 : 1	1 : 0
k.*	HK 2	solid chunk	1 : 2	1 : 0
l.*	HK 3	solid chunk	0 : 3	5 : 0
m.	HK 1	loose material	0 : 2	11 : 8
n.	HK 1	loose material	0 : 2	2 : 0
o.**	HK 1	loose material	1:90 min HF:2	1 : 0
p.**	HK 1 and III	loose material	1:90 min HF:2	9 : 8

*Had five minutes micromill treatment.

**Had HF treatment as noted between the acid and final water washes.

the mount was examined at 100 magnifications. Any object resembling a pollen grain or spore of a non-seed plant in size, shape, or overall appearance was analyzed carefully at higher magnification (400X). A time period of 15 minutes or longer was spent examining each slide with the microscope. Sometimes a slide was re-examined after a period of time ranging from only a few hours in most cases to four days in one instance.

Forty-three light microscope slides were prepared from the 16 samples as shown in Table II. Eleven out of the 43 slides were made from the 5-22-85 Hakatai I extraction (sample m). Eight of these 11 slides showed pollen of seed plants and/or cells of cryptogams. The size of these objects was measured by using an optical micrometer that had been calibrated against a stage micrometer.

Nine additional slides were made from sample p which had been digested in HF. The closed, screw-capped vial containing an extract from HK-3 had lost its water by evaporation in shipment and storage but the dried sediment remained in the tightly capped vial. Liquid from sample o was removed by clean dropper and put into the vial of dry HK-3 extract just mentioned so that sample p actually consisted of the buffer fluid from Hakatai I (sample o) mixed with dry sediment inside a vial of HK-3 extract. Thus pollen grains on such slides were from the HK-3 sediment or from the liquid on the HK-1 sample.

Photographs of biological grains were taken by dark field microscopy, see Figure 1 and following.

Scanning Electron Microscope (SEM) Techniques

SEM analysis and photography of various samples was performed by E. L. Williams. Each preparation for SEM study was taken from the top of the glycerin extract and washed in acetone to dissolve the glycerin. It was then filtered on filter paper with methyl alcohol to dissolve or wash away any residual glycerin and to retrieve the sample on the paper.

The filter paper with the sample was allowed to dry following which the sample was scraped from the filter

paper onto double-coated tape where it was gold-palladium coated and photographed using SEM.

SEM photographs were also taken of some fresh pollen grains of pine and oak (Figures 43 and 45 respectively), of fresh pollen that was subjected to HF treatment (Figures 44 and 46), and of pollen extracted from the HK-1 rock sample (loose material which had not been subjected to micromill treatment—Figures 13-17). This sample was given no acid washes but two water washes—a treatment identical to extract m, Table II. One pollen grain, one possible pollen grain, and some cellular objects were found in these SEM analyses—see Figures 13-16. Also, some objects were found by SEM analysis of another HK-1 sample that was subjected to HF extraction—see Figures 38-42.

Results

Our results are in the form of photographs of numerous objects found in rock samples, (Figures 1-32 and 38-42). In each figure caption the dimensions are given in micrometers (μm) together with the color of the object and other descriptive remarks.

Certain of our pollen grains and/or spores as the case may be were examined by an accomplished palynologist. While he worked on them he was not aware that the pictures were taken of objects present in extracts of Precambrian Hakatai shale. Following the words "palynologist's discussion," his remarks are printed under each picture that he analyzed. Qualified workers who wish to continue this research can request color enlargements of all or various figures in this paper from Howe.

Discussion

Original "Contamination"

Because of their small size, pollen grains of seed plants and spores or cells of cryptogams are readily carried by currents of air or moisture. When the various rock layers of the Grand Canyon were undergoing lithification, pollen grains present as original contami-

Table III

A survey of contaminant materials present on slides exposed to laboratory air in Reese Academic Center, The Master's College for varying periods of time. The symbol "nr" stands for "not recorded." Slides either had one drop of microscope immersion oil (o) or several drops of water (w) during the exposure. The number of water drops is indicated after the symbol "w," in column two. Although tracheids, trichomes, fungal spores, epidermal cells, and even cells resembling red blood cells or fungi were found several times, only three possible pollen grains (see slides with asterisks) appeared on the slides representing a total of 400 days slide exposure to air in laboratories that were experiencing daily class usage.

Room Number	Material on slide	Minutes spent analyzing slide	Days exposed to atmosphere	Objects present at time of study
*19	o	45	9	1 fungal spore or red blood cell(?) 1 possible disaccate pollen grain
19	w - 7	30	7	4 fungus spores
19	o	nr	14	1 fungus spore
19	w - 8	nr	14	4 fungal spores
*19	o	30	39	2 little spores 1 very dark opaque object pollen? 1 fungus spore cluster
19	w - 13	30	39	1 plant trichome 1 fungus spore
11	o	nr	7	1 plant trichome 2 fungus spores
11	w - 13	nr	7	3 fungus spores 1 epithelial cell
11	o	15	14	1 fungus object
11	w - 16	15	14	no cellular objects
11	o	30	38	2 red blood cells(?) or fungal cells(?) 3 tracheids or vessel fragments 2 fungal spores 1 epithelial cell?
11	w	30	38	5 fungus spore clusters
17	o	30	9	3 red blood cells(?) or fungal spores(?)
17	w - 8	40	9	2 fungus spores 1 plant trichome
17	o	nr	14	2 small sausage-shaped cells 1 fungus spore
17	w - 8	15	14	3 fungus spores
*17	o	45	57	4 epithelial cells 5 sausage-shaped spore 1 possible pollen grain 1 fungus spore cluster
17	w - 13	30	57	1 narrow strand of cells—algae? 10 double spores (15 x 9 m) 1 small cell (12 x 19) 1 small cell—algal spore? 2 fungal spore clusters

nants became part of the rock matrix. Without this original "contamination" of the rock with pollen, palynology would be impossible. Thus a palynologist tries to bend every effort to be certain that objects recovered from rock extracts are part of this original process.

Contamination after Lithification?

Even in the catastrophists' scale of time, however, Precambrian rocks such as the Hakatai shale are believed to have existed for thousands of years since their solidification and have thereby been exposed to the atmosphere for long time periods after lithification. During these periods of time after the rock layers were formed, they were exposed to atmospheric pollen. How can we be certain that pollen present in these rocks did not enter the rock long after lithification?

The structure of Hakatai shale rock is fine enough that it would certainly not allow objects the size of pollen grains or even considerably smaller to infiltrate the strata once the rock matrix had formed. Seams in the superficial layers, however, might allow water-borne pollen to penetrate certain areas of Hakatai shale at the surface of rock exposures. Pollen may have contaminated the rock along these seams after the time when the stratum formed and before the sample was collected. By chipping back three or four inches in solid areas which appeared to have no obvious seams or cracks, we have avoided using contaminated rocks and we believe that we thereby circumvented what could be called "post-lithification contamination." Two other workers—Burdick (1966) and Chadwick (1973)—both likewise encountered pollen grains in Hakatai shale samples which were also collected by using techniques aimed at minimizing the chance of collecting rock that had become contaminated with recent pollen. Thus in three instances where workers have attempted to get unweathered samples of rock, pollen was present in Hakatai Shale.

Contamination during Sample Collection in the Field?

There is the further possibility that rock samples from the Grand Canyon routinely get contaminated during collection. As noted earlier, we opened collecting bags only very briefly and allowed freshly chipped pieces of rock to fall directly into the bags which were then twice labeled and sealed. Based on previous studies by Lammerts and Howe (1987) we suggest that the chance of field contamination occurring during this brief collection time is very low. The chance of contaminating samples during collection was further lowered by our sampling date, February 10, 1984, as there was snow at the top of the canyon and the entire length of the south Kaibab trail was so cold that we were forced to wear gloves and heavy clothing. It was so cold that all the shrubs and trees were dormant. Thus fresh pollen would have been absent from the air and pollen from the previous year would have settled.

Contamination in The Laboratory?

The next step at which a sample might become admixed with recent air-borne pollen would be during the time it is opened in the chemistry laboratory for pollen extractions or in the microscope lab for final analysis. To examine this possibility we exposed slides

for a total of 400 laboratory slide-days in busy teaching laboratory rooms.

Figures 33-37 show representative objects encountered on these exposed slides. Other objects not shown include plant epidermal hairs, tracheids, xylem vessels, possible soot particles, charred carbon particles and fly ash (see Table III). These experiments simply show that when exposed to the air of laboratories where classes were being conducted, no pollen grains could be found on the exposed slides even though many other types of objects did fall on them. Accordingly it is extremely doubtful if any of the pollen grains or spores of Figures 1-32 and 38-42 could be the result of contamination from the atmosphere of the room where slides were examined.

On these exposed slides some tracheids, xylem vessel elements, and plant trichomes (epidermal hairs) appeared. The room in which much of the microscopic analysis was done had been used on occasion for botany classes and this might explain such contaminant items on slides from that lab.

The tracheid found in one of our Hakatai rock samples (Figure 30) most likely came from that rock sample. Another tracheid on top of the cover slide (Figure 31) may have gotten there by an accidental placement of pollen extract on top of the cover slide or it may have been a laboratory contaminant.

It is impossible to absolutely disprove pollen contamination in the laboratory but based on this study and previous data by Lammerts and Howe (1987), we conclude that the likelihood of even one or more pollen grains entering our sample vials or slide preparations is extremely small. It was actually difficult for exposed slides to become contaminated after many days of exposure in busy laboratories.

There is reasonable certainty that pollen grains discovered in this study entered the Hakatai Shale rock during its original period of lithification, having been produced by plants that were living at the time the rock was formed. The palynologist studied the pictures of our laboratory contaminants (Figures 33-37 and others) and concluded as follows:

I don't think you have a contamination problem in the lab. It may be a bit dusty, but the dust does not appear to be pollen.

Why Were Two Extraction Methods Used?

Chadwick (1981) has asserted that Burdick's method was inadequate. Chadwick recommended a different method of pollen extraction, involving HF (Doherty, 1980). We found that pollen grains were present in samples produced either by the Burdick method (extract m, Table II), or by the Chadwick method (extract p, Table II).

One wonders why Chadwick was unable to extract pollen using the HF method which would presumably remove silicates from the samples thereby making fossil pollen more visible. On p. 4 of her own methods section, L. I. Doherty had this word of caution in such use of HF:

Continue agitation until the sample is disaggregated. The length of time that sample is left in HF will depend upon the volume of the sample and the lithology. Most samples will be disaggregated within 1-2 hours. Since HF seems to have a

corrosive action on pollen and spores it is best to stir constantly until the sample is disaggregated and wash immediately. (Some samples may have to remain in HF overnight or sometimes for days to disaggregate. These samples, however, do not usually contain very good pollen or spores.) Doherty (1980, p. 4)

Neither Chadwick (1973) nor Solomon and Morgan elaborated on the length of time that their samples remained in hydrofluoric acid. The possibility exists that their negative results were caused by pollen destruction in an elongated HF phase of treating the samples. Chadwick admitted having found pollen grains earlier (1973) in Hakatai shale when using the Burdick (1966) method which involved only HCl with no HF treatment at all. We found that it was not necessary to use hydrochloric acid in the procedure we developed as with our most productive extractions. (See Table II, sample m where no acid wash was employed.) But we also noted that there were numerous pollen grains present in one of the samples we treated with HF—p, Table II.

In their own study of pollen in Twiggs Clay of the Georgia Coastal Plain, Schmidt and Wise (1979) (working with silicate-containing materials) employed no HF in extracting pollen. In private correspondence to E. L. Williams, Wise (1985) stated that HF has been used with mediocre results to extract coccoliths. Evidently it is not necessary to use HF even if silicates are present. Furthermore, the use of HF brings with it the potential problem of destroying fossil pollen grains even though the cell wall of such grains is somewhat resistant to HF. Again it should be remembered that pollen grains were present even after HF treatment in the present study so that we recovered pollen grains both with the Burdick (no HF) and the Chadwick (HF) techniques. Thus our positive results differed with those of Solomon and Morgan (1973, p. 10) who used the HF extraction method on Hakatai Shale and discovered that their results were "... total palynological sterility: i.e. no pollen grains or land-plant spores of any kind were seen."

Summary

We have recovered the following objects from samples of Hakatai Shale: pine pollen, Ephedra-like pollen, angiosperm-type pollen, fungal spores and possible algal cells. Our findings support the belief that pollen-bearing plants, algae and fungi existed at the time when Hakatai shale lithification occurred. Such a finding supports Burdick's claims (1966) and casts considerable doubt on the uniformitarian evolutionists' age estimate of billions or even hundreds-of-millions of years for the dating of Hakatai Shale.

One of our Hakatai Shale pollen grains even shows evidence of preserved nuclei (Figure 23). Some of the objects recovered have the bright green color of chlorophyll—Figures 17 and 18. On this basis we also suggest that Hakatai Shale strata were formed relatively recently. Such a conclusion fits squarely with the assumptions of scientific creationism.

Further research will involve analysis of additional extracts from our rock samples with a more extensive examination of the Supai and Hermit material. Qualified workers may receive subsamples of our material

for their own use and/or we invite critical experimenters to collect their own Hakatai Shale samples to repeat this study.

Acknowledgments:

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Figure 1. Hakatai shale sample m, Table II. [32x 58 µm] Clear, light brown color. This item is very much like Burdick's (1972) Figure 4, p. 29. Palynologist's discussion: "This is most certainly *Pinus*. One of the bladders is damaged but the other is more or less intact."

Figure 2. Hakatai shale sample m, Table II. [32 x 61 µm] Clear, light reddish-brown coloration. Palynologist's discussion: "This is probably the same type as Figure 1: it looks somewhat different because of the different alignment of the grain."

Figure 3. Hakatai shale, sample m., Table II. [approximately 25 x 60 µm] Palynologist discussion: "This is the saccate type again, probably *Pinus*. The grain has dried out causing the 2 bladders to fold in onto the body of the grain."

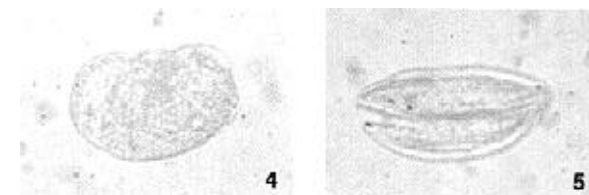


Figure 4. Hakatai shale, sample m, Table II. [25 x 56 µm] Clear. Palynologist's discussion: "Probably the same as Figure 3. As you suspect one of the bladders has been lost."

Figure 5. Hakatai shale, sample m, Table II. [25 x 56 µm] Clear. Closely resembles Burdick's (1972) Figure 5, *Ephedra* pollen and his (1966) Plate I, Figure 3 designated "*Ephedra* species." Palynologist's discussion: "Unfortunately this is not a very good image but I would hazard a guess that the grain is *Ephedra* (mormon tea). This pollen type, may have 5 to 15 parallel ridges running 'north-south' along the surface. In this case the ridge number is low. *Ephedra* pollen is capable of long distance transport and therefore means little in a paleoecological sense unless it is found in quantity."

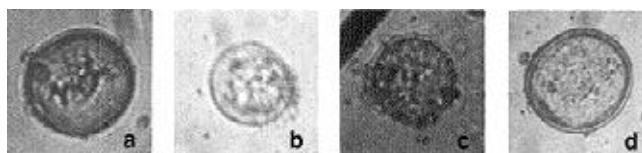


Figure 6. Hakatai shale, sample m, Table II. Four objects with diameters as follows: a-31 µm, b-25 µm, c-26 µm, d-32 µm. Each is clear with a slight yellowish color. Palynologist's discussion of a and b: "These appear to be similar although you record different diameters. Accurate identification on the basis of the photographs alone is impossible. I would say though that they are probably not pollen grains and are more likely to be spores of some kind (fungal or algal?)."

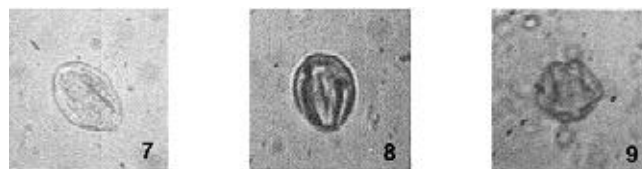


Figure 7. Hakatai shale sample m, Table II. [18 x 25 µm] Clear. Palynologist's discussion: "This almost certainly is a pollen grain—I would guess it is a tricolpate or tricolporate type. Unfortunately, this group is a large and difficult one and without a closer look I couldn't be certain what family is involved."

Figure 8. Hakatai shale sample m, Table II. Clear. [18 x 22 µm] Palynologist's discussion: "This is similar to Figure 7 and again shows the tricolpate aperture arrangement (3 furrows). *Acer* (maple) or *Quercus* (oak) might be represented here but I would have to look at the slide to be sure."

Figure 9. Hakatai shale sample m, Table II. [21 µm] Clear. This photograph was not examined by the palynologist but resembles the grains of Figures 7 and 8.

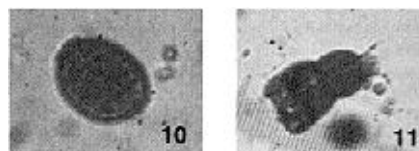


Figure 10: Hakatai shale sample m, Table II. [22 x 29 µm] Dark orange coloration, clear. Palynologist's discussion: "This could be a pollen grain but again identification is impossible because of poor resolution of the photograph."

Figure 11. Hakatai shale sample m, Table II. [18 x 29 µm] Dark brownish-orange color. Clear. Ocular micrometer is also seen. Palynologist's discussion: "This is a fungal spore possible *Alternaria*. The surface is rough and minutely verrucate (warty) and this is typical of *Alternaria*."

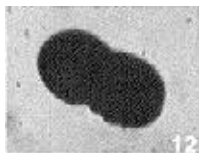


Figure 12. Hakatai shale sample m, Table II. [21 x 38 μm] Blackish brown color. Opaque. Palynologist's discussion: "This is a fungal spore (2 celled). The taxonomy of fungal spores is notoriously difficult. A similar 2 celled type that is occasionally encountered in Quaternary sediments is *Pullularia* (black yeast)."

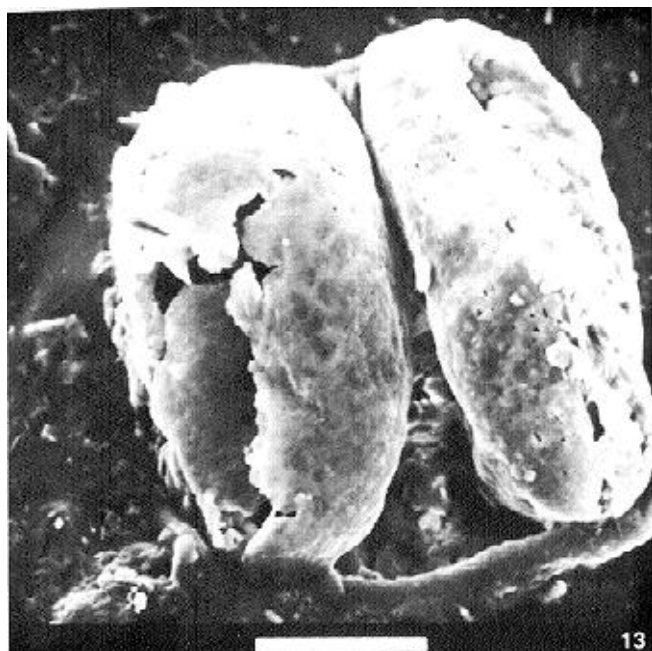


Figure 13. Scanning electron photomicrograph of an object from Hakatai shale. [30 x 35 μm] Note white bar on this figure is 10 μm . Palynologist's comments: "This is a badly corroded saccate pollen grain, probably referable to the genus *Pinus* (pine). I base this on the relatively small size of the specimen, the body of the grain being only about 30 μm . Unfortunately, identification to species or even subgenus is not possible here."

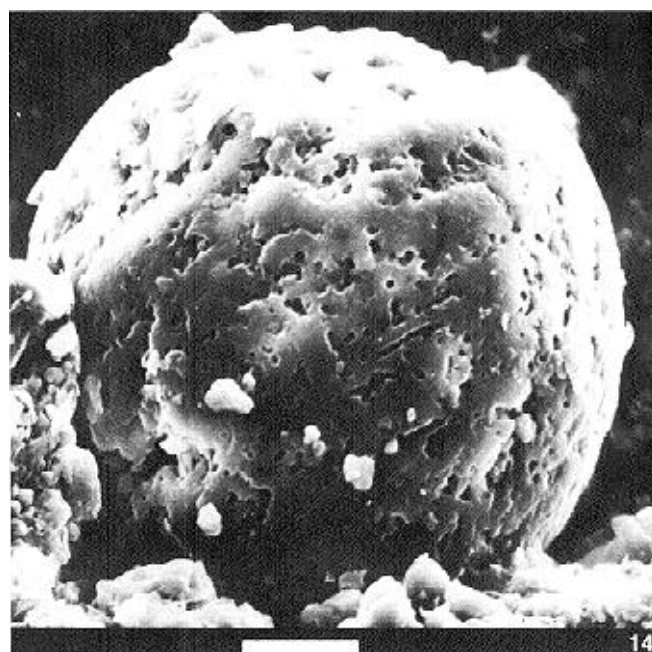


Figure 14. Scanning electron photomicrograph of an object from Hakatai [53 μm] White bar is 10 μm . Palynologist's discussion: "This is not a pollen grain but most likely a microscopic article of wood charcoal. Another possibility would be a small piece of volcanic material (Pumice?) or perhaps fly ash (industrial produced smoke particles)."

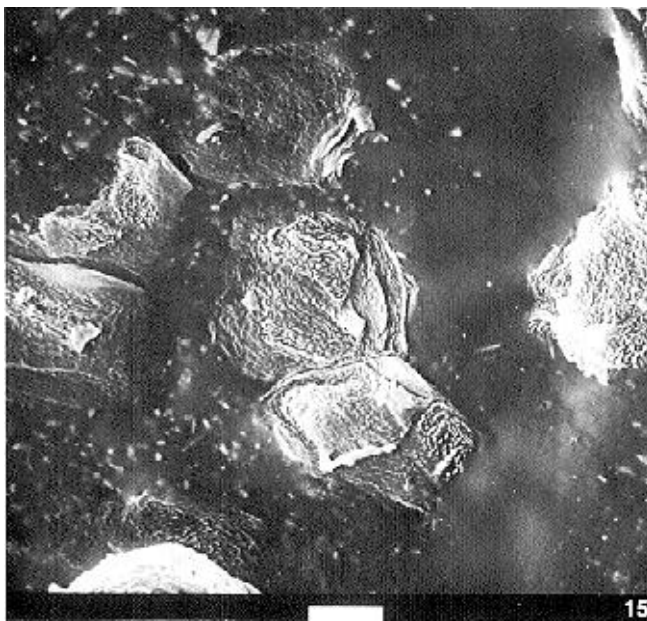


Figure 15. Scanning electron photomicrograph of objects from Hakatai shale. Object in center diameter about 26 μm . White bar is 10 μm . Palynologist's discussion for this figure and the one following: "These enigmatic 'things' are somewhat similar to *Tsuga* (hemlock) pollen. The fine detail of the surface on Figure 16 looks very much like the rugulate surface of *Tsuga* pollen. However, *Tsuga* pollen is usually spherical in shape and at least 60 micrometers in diameter, so I am not sure about the identification here."

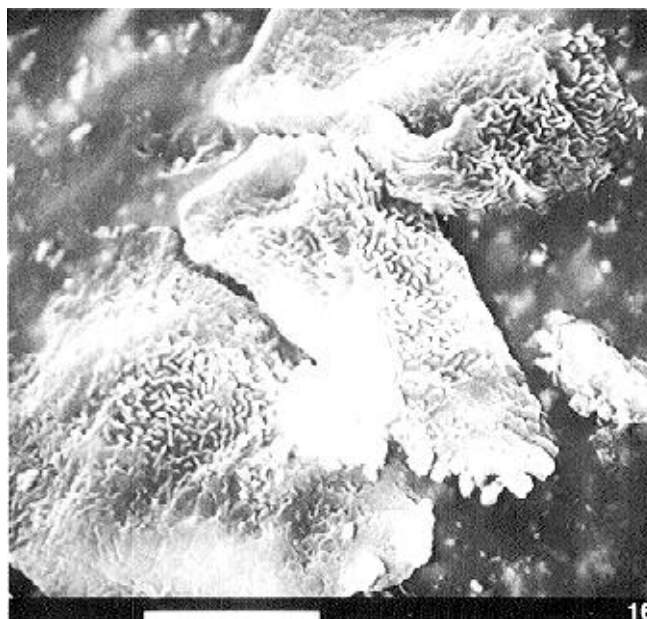


Figure 16. Scanning electron photomicrograph of objects from Hakatai shale. Object at lower center is about 20 μm diameter. Magnification is 1400x. For discussion see figure caption for Figure 15.

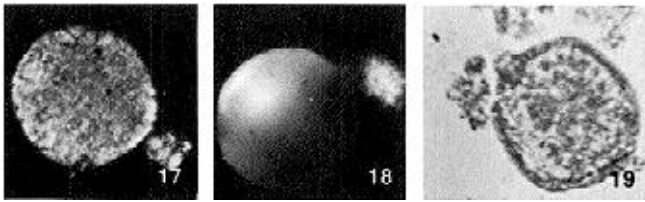


Figure 17. Hakatai shale sample p, Table II. Sidelight (darkfield) picture. Bright green color, speckled with yellow. Opaque. Granular surface. [40 µm diameter]. Palynologist's discussion: "This object is not a pollen grain. Fresh pollen is usually yellow in color and although the cytoplasm may be visible in untreated material, it does not look like the object on the photograph. My guess is that this is an algal cell."

Figure 18. Hakatai shale sample p, Table II. Sidelight (darkfield) Grass-green color. Opaque. Granular surface. [44 µm] diameter. Rock crystals visible at left and top. Palynologist's discussion: "I am afraid you have got me with this one. Possibly freshwater algae? Definitely not a pollen grain."

Figure 19. Hakatai shale sample p, Table II. Grey-orange color, clear. Estimated diameter, 40 µm. Palynologist's discussion: "This could be a pollen grain—the exine (cell wall) is evident. However, I would rather not speculate as to what species is represented."

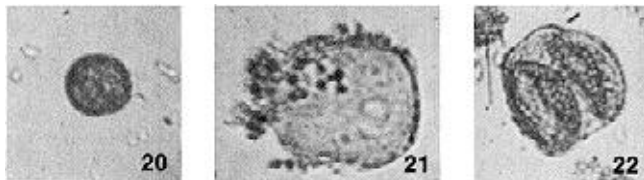


Figure 20. Hakatai shale sample p, Table II. Grey color, opaque. [19 µm diameter]. Palynologist's discussion: "Impossible to tell without more resolution."

Figure 21. Hakatai shale sample p, Table II. Brownish-orange color, clear. [43 x 35 µm]. Palynologist's discussion: "This appears to be a periporate pollen type . . . looks very much like *Plantago lanceolata*. What you are calling the 'nucleus' is actually a pore. Another possibility here might be *Ulmus*, although elm pollen only has pores around the equator. *Plantago lanceolata* is, of course, a European introduction to North America."

Figure 22. Hakatai shale sample p, Table II. Brownish tint. Clear. [35 µm]. Saccate type, bladders folded. Palynologist's discussion: "This is almost certainly *Pinus*."

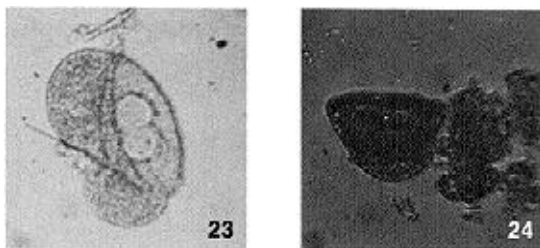


Figure 23. Hakatai shale sample p, Table II. Brownish-almost colorless. Clear. [34 x 53 µm]. Saccate type. Note cell at top showing cell contents (nuclei?). Palynologist's discussion: "This is also a *Pinus*. You could be right about the nuclei. However, this would indicate that the pollen grain was very 'young.' Cytoplasm is never preserved in fossil material of the kind I work with."

Figure 24. Hakatai shale sample p., Table II. Brownish color. Clear. [26 x 37 µm]. Saccate type. Side view, bladders folded. Concerning Figures 25-29 the palynologist had these words to say, "In my opinion these are all *Pinus*. They are all saccate, but in some cases the bladders have folded back on the body of the grain. The differences in size suggest that more than one species is represented here; however, pine pollen taxonomy is notoriously difficult and even with the best equipment it is rarely possible to separate different species."

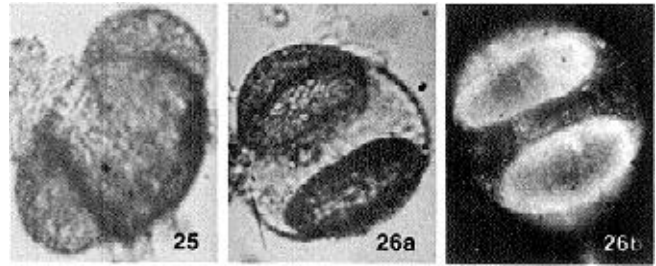


Figure 25. Hakatai shale sample p, Table II. Yellowish-brown color. Cloudy. [50 x 76 µm]. Saccate type pollen. See caption Figure 24 for discussion.

Figure 26a. Hakatai Shale sample p, Table II. Saccate type pollen. [37 x 56 µm]. See caption Figure 24 for discussion.

Figure 26b. Same pollen grain as in Figure 26a with darkfield photography. See caption Figure 24 for discussion.

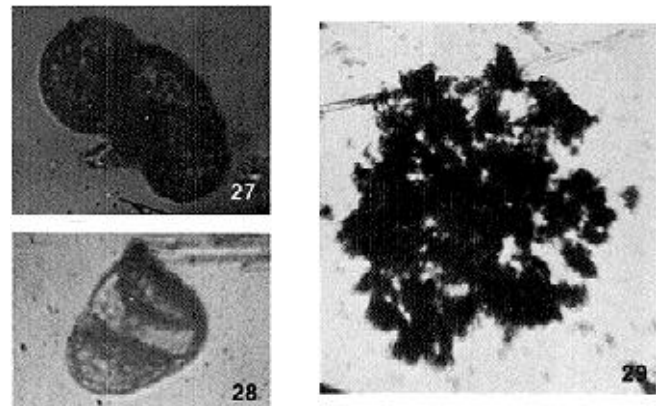


Figure 27. Hakatai Shale sample p, Table II. Saccate type pollen. See caption Figure 24 for discussion.

Figure 28. Hakatai Shale sample p, Table II. Saccate type pollen. See caption Figure 24 for discussion.

Figure 29. Hakatai Shale sample p, Table II. A view of the rock material visible after use of the HF procedure.

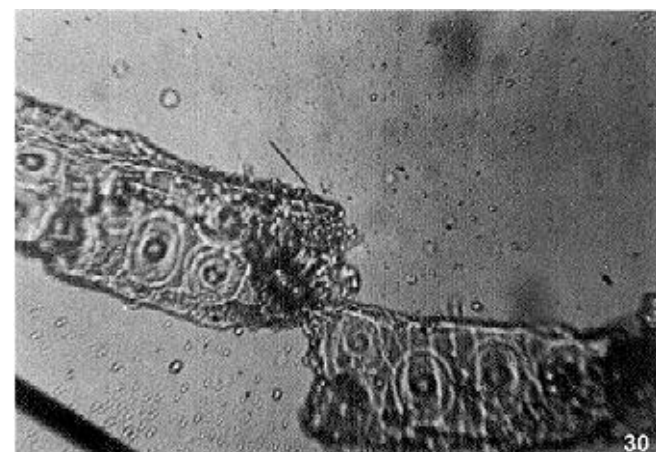


Figure 30. Hakatai Shale sample p, Table II. A tracheid with bordered pits. Tracheid width 51 µm. Width of bordered pit 26 µm. Width of pore in center of pit 6 µm. Length of tracheid 294 µm. Palynologist's discussion: "You are correct, these are tracheids. The bordered pits suggest that gymnosperms are involved."



Figure 31. Found on top of the cover slip on a slide made from Hakatai Shale sample p, Table II. Tracheid with bordered pits. Pit 20 μm diameter, pore 6 μm diameter. Palynologist's discussion: "... it appears some spiral thickening is present in the cell wall—perhaps *Pseudotsuga* (Douglas fir) or *Larix* (larch)?" Note, since this one was found on top of the coverslide it may have got there by accidental placement of pollen extract on top of the cover slide or it may have been a laboratory contaminant.

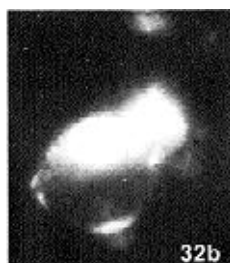
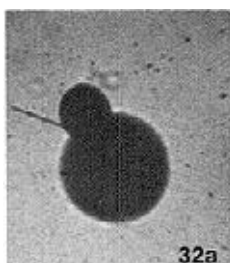


Figure 32a. Hakatai Shale sample p, Table II. Golden-brown color, opaque. Palynologist's discussion: "This is a mystery to me! It is definitely not a pollen grain, however, pollen grains are rarely smooth-walled and spherical." Note bud-like projection on cell.

Figure 32b. Same object as in figure 32a with darkfield photography.

Figure 33. Object present on slide with seven water drops exposed to atmosphere for seven days in RAC Room 19—see Table III. [58 x 15 μm]. There were many fungus spores like this one found on the various slides exposed to the laboratory air. Palynologist's discussion: "This is definitely a fungal spore. What species is involved I can't say."

Figure 34. Object present on slide with eight water drops exposed to atmosphere in RAC Room 17 for nine days—see Table III. The large object is 15 x 25 μm . Palynologist's discussion of this and Figures 35-37: "These are also fungal structures of various kinds."

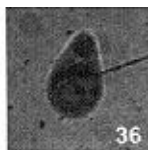


Figure 35. Object present on slide with 13 water drops exposed to atmosphere in RAC Room 11 for seven days—see Table III. [78 x 18 μm]. See caption Figure 34 for discussion.

Figure 36. Object present on slide with 13 water drops exposed to atmosphere in RAC Room 17 for 57 days—see Table III. [24 x 13 μm]. See caption Figure 34 for discussion.

Figure 37. Object present on slide with eight water drops exposed to atmosphere in RAC Room 17 for 14 days—see Table III. [7 x 19 μm]. See Figure 34 caption for discussion.

Figure 40. Object present (SEM) from Hakatai Shale treated with HF. [70 x 60 μm]. White bar—10 μm . Palynologist said this looked like a charred carbon particle.

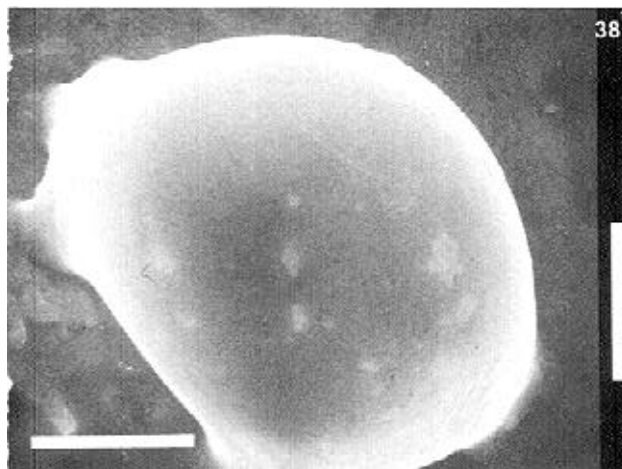


Figure 38. Object present (SEM) from Hakatai Shale treated with HF. 26 x 35 μm . White bar—10 μm . Palynologist's discussion of this one: "38 appears to be covered with coating medium. I can't help very much with your friend's SEM images."

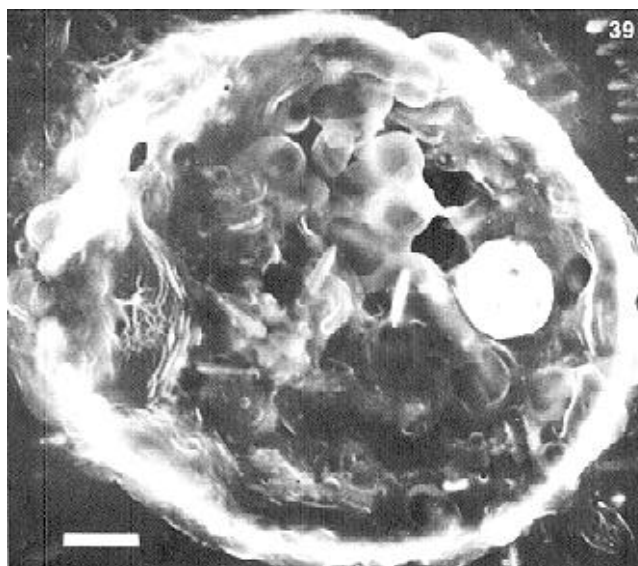
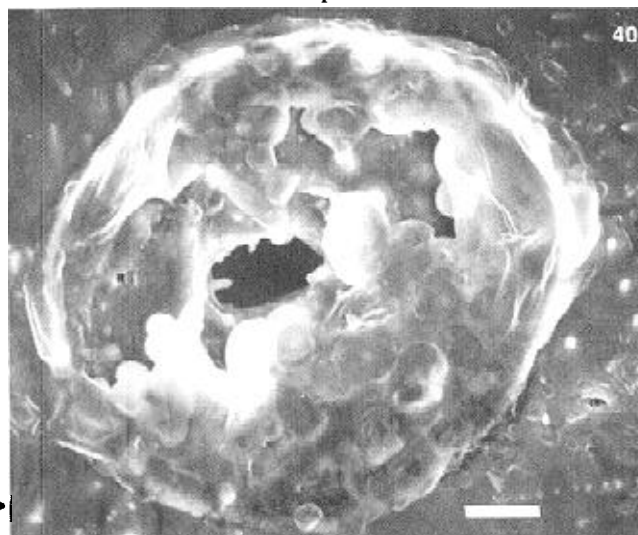


Figure 39. Object present (SEM) from Hakatai Shale treated with HF. [80 x 70 μm]. White bar—10 μm . Palynologist said this looked somewhat like a charred carbon particle.



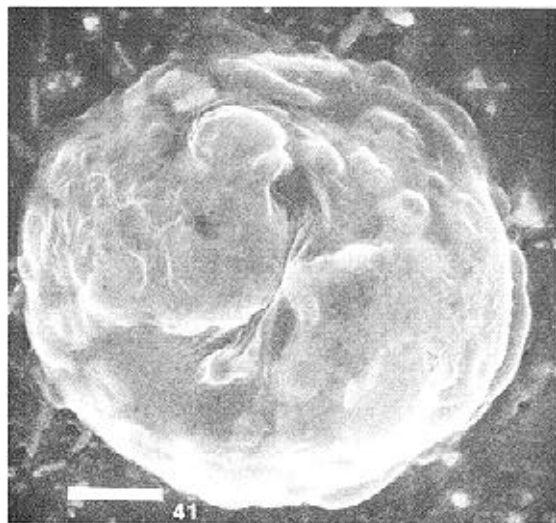


Figure 41. Object present (SEM) from Hakatai Shale treated with HF. [Approximately 50 μm] diameter. White bar—10 μm . Palynologist said this was a mystery to him.

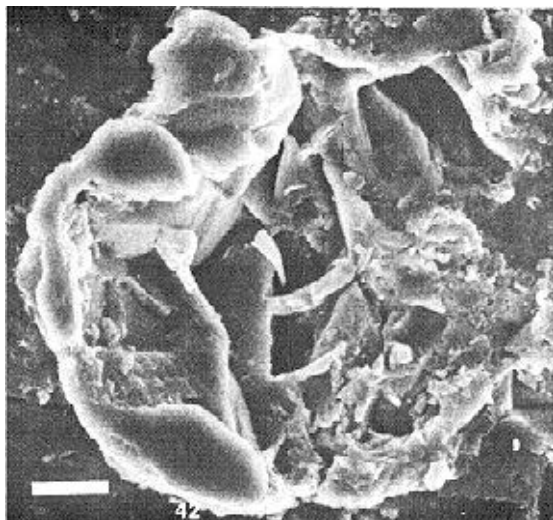


Figure 42. Object present (SEM) from Hakatai Shale treated with HF. [Approximately 70 μm] diameter. White bar—10 μm .

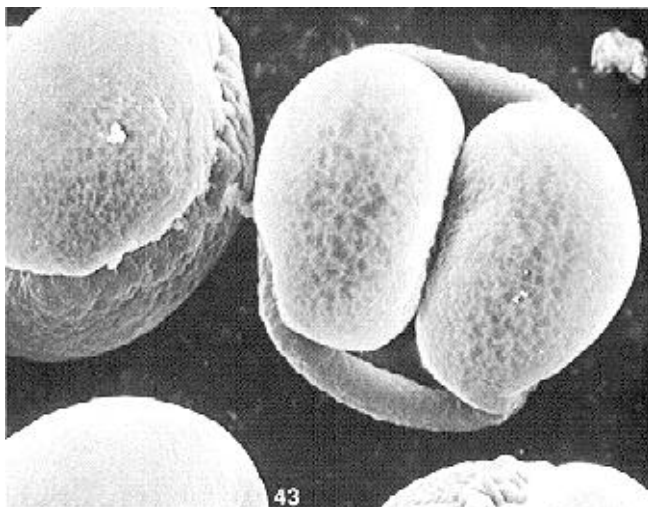


Figure 43. Fresh *Pinus* pollen gold coated, photographed before HF exposure.

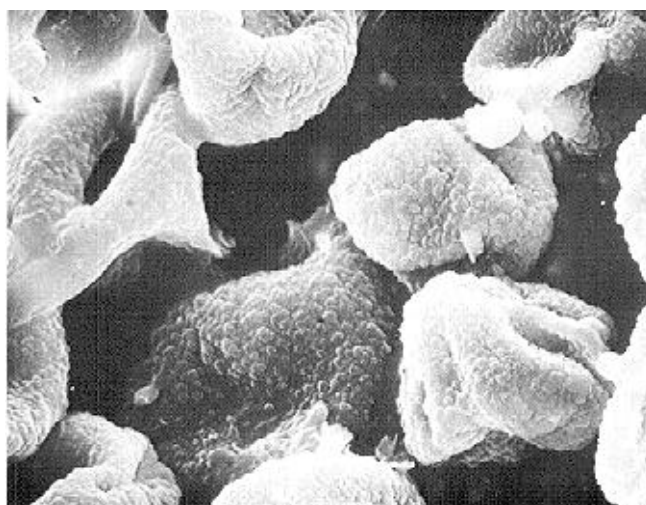
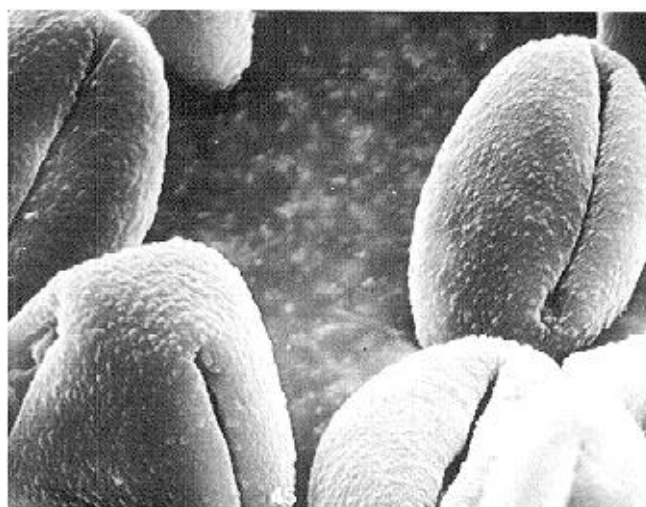
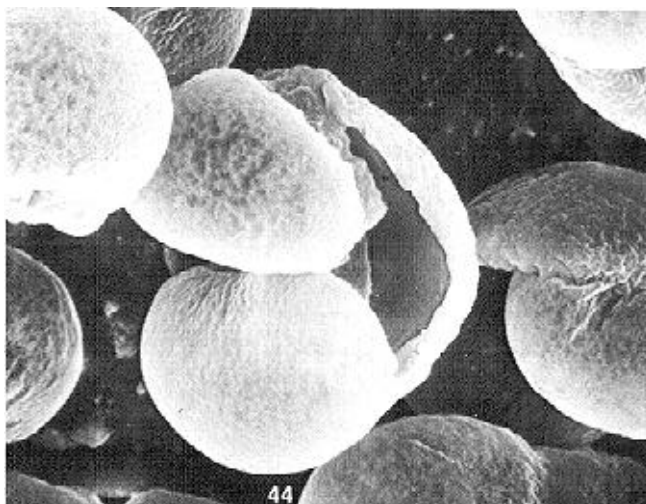


Figure 44. *Pinus* pollen gold coated, photographed after HF exposure.

Figure 45. *Quercus agrifolia* (coast live oak) pollen, fresh, gold coated, photographed before HF exposure.

Figure 46. *Quercus agrifolia* (coast live oak) pollen after exposure to HF, gold coated.

PANORAMA OF SCIENCE

A Theory for the Birds?

Introduction

"The origin of birds is largely a matter of deduction. There is no fossil evidence of the stages through which the remarkable change from reptile to bird was achieved" (Swinton, 1960). Does the recent unearthing of a new avian creature from the Triassic of West Texas require a complete reversal of this candid admission by a well-known evolutionist? Is the new genus, *Protoavis*, the long-sought-after fossil which demonstrates evolution's postulated transformation of reptile into bird? A preliminary review of the evidence reveals the elusive link between these two orders has still not been found.

Stratigraphy

Popular ideas about the alleged evolution of birds have recently been turned "inside-out" by the 1986 discovery of two crow-sized fossils in the West Texas Dockum formation. The mudstone quarry from which the delicate remains were extracted is located near the small town of Post, roughly 40 miles southeast of Lubbock.

The Dockum group is a series of non-marine, Upper Triassic deposits which composes the entire Triassic system of Texas (Sellards, Adkins and Plummer, 1958). These poorly lithified shale beds overlie Permian formations, producing an unconformity between the two systems. To account for this problem, most geologists believe the area was simply not receiving sediments during the Lower (Early) and Middle Triassic times.

Since vertebrate fossils found in the Dockum include Parasuchians (crocodiles), amphibians and a freshwater species of fish, it is presumed the ecological conditions in which these sediments were deposited were reminiscent of a braided stream or flood-plain environment. Skeletal remains of birds might have been anticipated in such a setting, were it not for the presumed age of the strata containing them! Assigned to a new genus by Texas Tech paleontologists, *Protoavis* or "ancestral bird," extends the pedigree of our fine feathered friends back 75 million years before the time of *Archaeopteryx*, previously considered the oldest known bird (Benton, 1983).

Morphological Comparisons

From the two partial individuals recovered (one believed to be an adult; the other a juvenile) a fairly accurate structural comparison indicates the new Triassic vogel possessed more "advanced" avian qualities than *Archaeopteryx* (1), even though it appears much earlier in the evolutionary scheme.

Unlike that of its Late Jurassic "progeny," the *Protoavis* jaw is devoid of posterior teeth. Careful studies have yet to be conducted on the *Protoavis* dentition, but the obvious lack of rear teeth is a condition seemingly inconsistent with the evolutionary

assumption of gradual tooth reduction in avian phylogeny (Raymond, 1939, p. 186).

Both creatures apparently share features common to most birds: elongated forelimbs, furcula and a typically bird-like quadrate. However, *Protoavis* has a keel on its sternum and a cnemial crest on its tibia (Beardsley, 1986), whereas *Archaeopteryx* lacks both (de Beer, 1954, p. 21).

Although it is questionable that quill nodes, for the attachment of feathers, can be discerned on the ulna of *Protoavis*, the presence of flight muscle attachment sites on the humerus have been reasonably well established (Beardsley, 1986). According to its discoverer, Sankar Chatterjee, its wings "are so well developed, there's no doubt it could fly" (Weisburd, 1986). Certainty is also lacking in regard to the possession of a feathered tail, like that of *Archaeopteryx*. For this reason it is premature to reconstruct the bony tail of *Protoavis* with a fleshy covering as depicted in a recent *Scientific American* article [255(4):84-67]. Such tactics are subtly misleading because they imply an unproven, highly speculative relationship between two zoological classes—reptiles and birds.

Phylogenetic Relegation . . .

Archaeopteryx Bites The Dust!

After enjoying a lengthy popularity in the literature as the first bird, the celebrated *Archaeopteryx* will soon be unceremoniously toppled from its lofty perch. Actually, as the ancestral bird, it has long been rejected by some paleo-ornithologists (Benton, 1983, p. 99) and now seems destined to be relegated by most evolutionists as another insignificant side twig leading nowhere in the imaginary "tree of life."

Even the controversy over the genuineness of the feathered imprints on two of *Archaeopteryx*'s slabs (2) will no longer matter, for its transclassical reputation will eventually be forgotten in much the same way that the ancient pterosaurs are no longer cited as links between mammals and birds (Langston, 1981, p. 122).

With *Protoavis* as the "bird in the hand," *Archaeopteryx*'s downfall is already becoming increasingly evident by recent comments like the following:

It (*Archaeopteryx*) probably represented a side branch of avian evolution . . . The similarity of some of these birds (of 100 million years ago) to modern families has long suggested they share some ancestor older than *Archaeopteryx* (Anon. 1986, p. 86). Many paleontologists have long suspected that *Archaeopteryx* is not on the direct line to modern birds. It is in some ways more reptilian than *Protoavis*, and the period between the Late Jurassic *Archaeopteryx* and the worldwide radiation of birds in the Cretaceous has to some seemed suspiciously brief (Beardsley 1986, p. 677).

(1) Morphological details ascertained from this genus are taken from studies conducted on three of the five known *Archaeopteryx* slabs. The solitary feather imprint from Solnhofen, first described by von Meyer (1861), should not be classified as a sixth specimen, as it cannot be positively assigned to any taxon.

(2) Claims to the effect that fraudulent feather impressions were added to the two world-renowned *Archaeopteryx* specimens in London and Berlin, shortly after their extraction from the quarries, were publicized by such notables as Sir Fred Hoyle and N. C. Wickramasinghe. For a review of their evidence, see the series of articles in the March 8th, 29th, and June 21st, 1985 issues of *The British Journal of Photography*.

A Creationist Alternative . . . Protoavis Gets His Feathers Wet

Since most creationists maintain that all types of birds were brought into existence on the fifth day of creation, as recorded in Genesis, it is assumed that species representing both of these mosaic creatures (possessing characteristics also possessed by other animals) were still living at the time of the Biblical cataclysm commonly referred to as Noah's Flood. Theoretically those specimens found as fossils would have perished during this event.

In this scenario since *Protoavis* was discovered in sedimentary layers positioned "lower" in the artificial geologic column than *Archaeopteryx* it may have been among the first winged denizens to be buried in the rapidly depositing sediments. Interestingly enough, its discoverer surmised that *Protoavis* probably drowned in a flash flood, its fragile bones preserved only because they were covered immediately by a blanket of mud and debris (Weisburd, 1983). Nearly 30 years previous to this statement, Texas geologists reached identical conclusions: ". . . it is only in the disturbed beds (of the Dockum formation), which bear evidence of having been deposited by great flood washes, that the remains of animals and plants have been found" (Sellard, Adkins and Plummer, 1958, p. 244).

But whether or not *Archaeopteryx* and *Protoavis* lived millions of years apart, as alleged by the vast evolutionary timescale, or were contemporaries thriving within different habitats, one conclusion is certain. Neither of these "classical examples of evolution" is really compatible with the claim that living birds are the end product of a process which started with cold-blooded reptiles.

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Contributed by Ronald Calais and Gerald Duffett

A Critique of Molecular Homology (Knaub)

Quoting a source that had declared that molecular homology serves as an independent proof of macroevolution, Knaub demonstrates that such is not the case. Knaub claims that errors in amino acid sequence determination for "closely-related" organisms can cause problems.

The degree with which serum from organism A reacts with antibodies of organism B assumes a relationship. Yet reciprocity (the serum of organism B reacting with the antibodies of organism A) often is not the same. This means that the evolutionary distance from A to B is not the same as B to A.

Other problems encountered in molecular homology are that the same amino acid can have more than one codon, some proteins may be forced by the researcher to be homologous and evolutionary assumptions are used in data analysis. The cytochrome C phylogenetic tree is used as the best example of relationship yet it represents only one out of 40 possible choices available from the modeling parameters used.

Rather than forcing the assumption of a common ancestor on all modeling programs, another possibility that is ignored is a mosaic pattern of trait distribution that may reflect plan and purpose. After much of the data from molecular homology is examined, the idea of descent from a common ancestor has been falsified.

The different fields of fossil evidence, immunology, protein sequencing and DNA hybridization, chromosome banding and electrophoresis each yield a different phylogenetic tree and when they are combined they produce a tree different from all the others. This state of confusion is definitely no independent proof of macroevolution.

The concept of "molecular clocks" is examined. Scientists try to determine "ticks" that have never been observed and if the "truth" of macroevolution is assumed in the study, molecular clocks are of no value. In his conclusion Knaub offers suggestions for creationist research. This thesis offers a good starting literature review and critical examination of molecular homology.

Reference

- Knaub, C. W. 1983. A critique of molecular homology. M.S. Thesis. Institute of Creation Research. El Cajon, CA.

Contributed by Emmett L. Williams

Philosophy of Progress and Evolutionism (Carson)

The material prosperity of the latter part of the 19th century provided the framework within which the Idea of Progress gained sway over the minds of many in Europe, and especially in the United States. The Idea of Progress refers primarily to the belief that things are getting better generally and, more broadly, that improvement with the passage of time is a virtual law of the universe. Much evidence appeared to support this belief in the latter part of the 19th century, particularly on the material side. New discoveries and inventions abounded; sanitation and health facilities were clearly improving; population was increasing, perhaps as never before in history; and people generally had the means for producing and had a greater variety of goods available than ever before. Indeed, this variety and abundance continued to increase into the 20th century, but before long other sorts of development began to cast doubt upon the universality of progress.

The Idea of Progress was not new to the 19th century. It had been around since the 17th century, at the latest, and more and more people championed it in the 18th century. Modern thinkers even ceased to believe that even the ancient Greeks were their superiors. The Idea of Progress was mightily bolstered by the material improvements and expansion of information in the 19th century, and by the 20th century was widely accepted by people generally. At its simplest level, it translated into the view that the latest is the best, or, as the French publicist of the 1920s Emil Coué, put it "Every day in every way, we are getting better and better." By the 1920s too, American automobile makers were taking advantage of the popular idea and bringing out new models annually, each year's model presumably an improvement over that of the preceding year. More deeply, the Idea of Progress got intellectual support from the idea of evolution which gained sway in the 19th century. This was especially the case with the Darwinian conception of the survival of the fittest. If the fittest survive in the struggle for survival, it would appear to tend to make some sort of progress inevitable. In any case, the Idea of Progress enjoyed great vogue in the midst of the growth and expansion of America in the late 19th and early 20th century . . .

Looked at in one way, evolutionary theories gave great impetus to the Idea of Progress. But from another angle, they tended to undercut the very foundations of individual liberty, limited government, and the bases of freedom and responsibility. The theory of natural law was abandoned by some thinkers, and with it the natural rights on which individual rights were based in the United States. (That does not necessarily mean that people ceased to believe in rights, but they tended to attribute them to other sources than nature and nature's God—usually government.)

A great wave of materialism swept through intellectual circles, and, supported by evolutionary theo-

ries, thinkers began to conceive of natural law as an active force rather than as neutral laws imbedded in the nature of the universe. Above all, evolutionary hypotheses were used in an attempt to explain the origin of life and of man without requiring or having a creative and providential God. On this view, man arose from nature and was wholly a natural being, not a special being created by God to have dominion over the earth. Naturalistic explanations of all sorts of things become commonplace . . .

The full impact of these hypotheses and theories were not felt in the 19th century. For one thing, those who advanced these theories did not have the power to impose their ideas on people usually. For another, those thinkers greatly influenced by these ideas, or who originated them, were by no means in agreement as to their significance or how they were to be applied. The materialistic and evolutionary ideas were interpreted by some as meaning that man was caught in the grip of natural forces which determined his conduct. He was powerless, in this view, to alter the course of development. On the other hand, some thought that in a universe without God man could do all things. The central belief to come out of this attitude was the belief that men working together could build a utopia. Indeed, utopianism undergirds many of the social and political beliefs of the 20th century.

The main point here, however, is that the seeds of the 20th century developments are to be found in the intellectual currents of the 19th century. The fruits of these seeds have often been such things as the massive uses of governmental power, uses based on naturalism, revolution, evolution, and a host of particular theories either for forcing man into the current of "progress" or remaking him.

Reference

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WHY COLLECT PLANTS?

As an introduction to Joneen Cockman's article which follows, two questions deserve consideration: 1. Why should we develop a CRS herbarium? 2. Why should we have CRS members collect plants for it from different locations? The first question is like asking "Why have a CRS insect collection or a CRS library?" The answer in all three instances is that the library, insect collection and herbarium will each be exceedingly important as CRS laboratory buildings are constructed in Arizona and Oklahoma. In these buildings there must be the books, space, equipment and collections needed to pursue creationist origins studies. Some of the goals of the herbarium in particular are:

1. Collect specimens of the same plant over a period of years to determine if any micro or even macroevolutionary changes have occurred.
2. Collect specimens of the same species of plant from all over the country to determine its distribution and its differences from place to place.

3. Apply these data to a recent Flood-climatic-change model of origins.

Ms. Cockman gives us helpful rules we would all do well to follow as we collect for the CRS herbarium. She has taken the plants that have been gathered by E. L. Williams from the Southeast, G. F. Howe from Arizona, and Steve Hagberg from the GES in Oklahoma and has begun classifying a creationist plant collection.

Other collectors from various regions are invited to join in this activity. When they collect, they should abide, of course, by their local and regional laws regarding plants. They should also try to follow as many of the suggestions given here as is possible. But by all means collect plants and send them to Joneen for analysis, mounting and ultimate inclusion in this growing herbarium.

George F. Howe

COLLECTING PLANTS FOR CREATION RESEARCH SOCIETY HERBARIUM

JONEEN S. COCKMAN*

Received 19 November 1986 Revised 31 May 1987

Abstract

Several suggestions are offered to interested readers who would be willing to collect plants for the Creation Research Society (CRS) Herbarium. Help is solicited in this important project.

Introduction

In the late 1970's while pursuing an undergraduate degree in Range Science, I was fortunate to be a student of a group of botanists at Humboldt State University in Arcata, California. These men also had an amusing side as they began a nation-wide contest to identify the "worst dichotomy." A "dichotomy" is a series of paired choices employed to unlock a plant's identity. For example, "petals present vs. petals absent" is a pair of choices used to arrive at an end or the identity of a particular plant.

Being neophytes of *A California Flora* by Philip A. Munz, my classmates and I thought the description of California poppy, *Eschscholzia californica*, would win by a landslide. It was, "sepals cauducous at anthesis," which means that the sepals fall off when the flower is ready to bloom.

A sepal is similar to a petal. In many flowers it is green in color and a number of them are located just below the petals. In the California poppy the sepals are fused. It is as though the poppy were wearing a sweater of sepals and pulling the sweater off over its head. Since a dichotomy is composed of a pair of choices, the taxonomist must assume that the alternate choice is for the flower to have "normal" sepals. As I learned to speak "Munzese" the former description took on certain meaning. The winner of the dubious award went to "flowers blue," an innocuous description that came in from another university.

Some plant specimens if not properly collected can be as innocuous as "flowers blue." On the other hand, a plant fragment with a few meaningful field notes can become a favorable herbarium specimen once it is identified. The purpose of this paper is to lend some pointers to those field biologists who will be collecting treasures for someone else to identify.

Building a Model Herbarium

A model herbarium is one which includes properly identified voucher specimens to use as reference material and historical documents. Documentation is the key point and the weak link in many herbariums. Often collectors and curators simply will not be bothered with field notes.

Among the documentation for plant specimens is a list of botanical uses a plant may have. Much of the literature researched is based on American Indian uses of certain plants. As the CRS collection grows, some individual plants or taxonomic groups may become more important as their uses as medicines are revealed.

Currently the CRS herbarium collection is focused on grasses, forbs, trees and shrubs. However, lichens,

mosses and their associates are not to be excluded. A few creationists have already begun to collect plants. As a result a herbarium now exists and has collections from five different field seasons. The collection has grown to include about 150 different species. Most of the plants are from Arizona, Alabama and Georgia. Presently the specimens are catalogued at The Master's College, Newhall, CA. Ultimately they will be kept at the CRS Grand Canyon Experiment Station when a suitable building has been constructed. As a taxonomist identifying plants from numerous areas of the United States, I like to have contact with experts from each region. If you are a botanist who is able to do verification of plant identities on finished herbarium specimens, your help is needed. Currently someone who can verify plant taxonomy on specimens collected from the Southeast could help us greatly.

The plants being contributed to the experiment stations should be of high quality. The herbarium collection for the Grand Canyon Experiment Station is just beginning and it can be done correctly from the start which will save many future headaches. All plant collectors can help accomplish this goal.

Collecting Complete Specimens

Consider grasses in the genus *Bromus*. Many collectors have a way of picking grasses that resembles a horse grazing. One hand grasps a wad of culms (stems) and pulls. If the roots do not come voluntarily, they are broken. The blades (leaves) are torn from the culm and a million pieces go into the press—none of them resembling a whole plant.

Referring to Hitchcock and Chase's *Manual of the Grasses of the United States*, the taxonomist turns to *Bromus* in the key. Are the plants perennial or annual? How does one know without roots? If it appears somewhat robust the plant may be perennial. If the plant is a perennial did it have creeping rhizomes (underground stems) or not? Again, the specimen has no roots so how can the taxonomist really know? You are left guessing with three dozen brome grasses from which to choose.

How much better it would have been if the grass collector had taken the time to secure several complete specimens of the same brome grass including the roots and/or underground stems. A small garden trowel proves to be invaluable when collecting roots. However, I admit to having used antlers, pencils, pocket knives, branches, lug wrenches, finger nails and screw drivers.

As the collector becomes familiar with different flowering families he will learn what makes a good specimen. The best specimen is a complete specimen. However, not all plants need to be collected wholly in order to run them through a key. A collector may get away with "top snatching" in the *Geranium* family since most *Geranium* keys focus on flowers and fruits.

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But do not try that with the Buttercup family. The best rule is to never be guilty of "top snatching."

Table I was compiled by Dr. James Payne Smith, Jr., Dean of the School of Science at Humboldt State University (Smith and Simpson, 1977). It lists features emphasized in the identification of common flowering plant families.

Preparing the Specimens

If possible it is best to place fresh plant specimens directly into a plant press. However, good plant presses are at least 12" x 18" in size and somewhat cumbersome to carry in the field. A good substitute is two small pieces of cardboard sandwiched around newspaper and tied with a bungi cord. At the least plants should be placed in plastic bags, one specimen per bag. Do not plan to leave the plants in plastic bags for any prolonged period of time. Fungus such as mold or mildew may grow on the plants. Fungal growth can distort the appearance of pubescence, alter flower color and cover up important anatomy. If it is necessary to leave plants in plastic for several hours or more, refrigerate them to retard fungal growth. Field presses and plastic bags are only for temporary holding; specimens should be transferred to a regular plant press as soon as possible.

A plant press is used to dry and flatten the specimens. Better presses are composed of two lattice-work frames allowing for more air circulation, however, plywood can also be used. Between the two frames are layers of newspaper, "blotters" and ventilators. Newspaper holds individual plant specimens. Field notes may be written on the newspaper right along side the specimen. (Using a bright colored ink will help to highlight the notes making them easier to locate later.)

Blotters are thick, and absorbent to aid in drawing moisture away from the plants. Ventilators are similar to corrugated cardboard and aid in air circulation. These materials are combined in a specific order. The plant specimen is centered between newspaper. On either side of this are two blotters, then ventilators (Figure 1).

Sponge pads may also be useful in drawing moisture from plants. However, the primary purpose for sponge pads is to cushion bulky specimens that would otherwise be crushed or broken in the press. Shrub branches, cacti, heavy thistles and fruits may benefit from the addition of sponge pads. Photographs illustrating these items and the plant press are found in Figures 2-4.

The quality of a specimen depends largely on how it is placed into a press. It should be positioned as if it were going on display. Shake off excess soil. Arrange the plant so that it looks good and will fit easily onto an 11½" x 16½" paper. Try to display the anatomy of the plant by turning leaves and flower heads so that some are seen from the front and some from the back. On grasses, pull a few leaf blades away from the culm (stem) so the inner collars and ligules can be seen. Taller specimens may have to be bent into a V, N or W shape. Very small specimens may need to be enclosed in an envelope so they do not get lost in the press. Some characteristic features of plant anatomy are shown in Figures 5 and 6.

Heavy, fleshy specimens such as cacti and large thistles need extra preparation before going into the

Table I. Features Emphasized in the Identification of Common Flowering Plant Families.

Family	Under-ground Parts	Leaves	Flowers	Fruits	Take Note of:
Amaranthaceae				x	Monoecious or dioecious
Araceae	x	x		x	
Araliaceae	x		x	x	
Asclepiadaceae			x	x	Position of fruiting pedicel
Betulaceae				x	
Boraginaceae	x		x	x	
Cactaceae			x		Spines & glochids
Campanulaceae			x		Corolla shape
Caprifoliaceae				x	
Caryophyllaceae			x	x	Style number
Chenopodiaceae					
Commelinaceae			x		Very delicate flowers
Compositae	x	x	x	x	Ray flower color
Cornaceae				x	Branchlet color
Crassulaceae		x	x		Basal rosette
Cruciferae		x	x	x	
Cucurbitaceae				x	Monoecious or dioecious
Cyperaceae	x			x	
Ericaceae			x	x	Fruit surface
Euphorbiaceae			x	x	Watery or milky sap
Fagaceae		x		x	Growth form
Geraniaceae		x	x		
Gramineae	x	x		x	Mature spikelets
Hydrophyllaceae	x		x	x	
Iridaceae	x		x	x	Flowers very delicate
Juglandaceae		x		x	Nature of pith
Juncaceae			x	x	Flat or terete leaves
Labiatae	x		x	x	Flower color & markings
Leguminosae	x	x	x	x	
Liliaceae	x	x		x	Bulb morphology
Loranthaceae			x	x	Host plant
Malvaceae			x	x	Flower color
Nyctaginaceae	x			x	
Oleaceae		x		x	
Orchidaceae			x		Flower color & markings
Plantaginaceae		x	x		
Polemoniaceae	x		x	x	Flower color
Polygonaceae	x			x	
Portulacaceae			x	x	Stipule morphology
Ranunculaceae	x	x	x	x	
Rhamnaceae			x	x	
Rosaceae	x	x	x	x	Flowering & sterile stems
Rubiaceae				x	
Salicaceae		x		x	
Scrophulariaceae			x	x	Flower color & markings
Solanaceae			x	x	
Ulmaceae		x		x	
Umbelliferae	x	x		x	
Violaceae		x	x		Flower color & markings

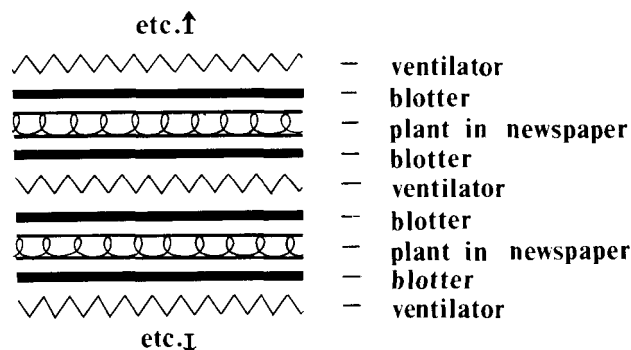


Figure 1. Cross sectional diagram of the specific order of materials surrounding a plant specimen in a plant press.

press. Large thistle heads should be sliced in half vertically with one-half remaining intact with the stem. Cacti pads and fruits also should be sliced in half, parallel to the plane of the pad or fruit. The halves should be scooped out while taking care not to injure the outside structures, i.e. spines and bristles.

Once the specimens are in the press, check them periodically. Place plants on dry blotters if any sign of mildew appears. (Do not separate the specimen from the original newsprint.) The length of time required to dry a specimen depends largely on the succulence of the specimen. Most grasses will dry without having to change the blotter. Heavier plants such as thistles may require a change. It may take weeks to dry heavier plants like cacti. It is preferable to mail specimens when they are completely dry. But if you must mail damp specimens, include a note to the taxonomist so he can transfer them to a dry press and continue to monitor them against mildew formation.

If plants are collected when green and then dried, they should hold together well. But you may find that a dried specimen begins to lose its fruit or seeds. Thistles do this frequently and soon the downy seeds are scattered. Collect these and any other lost anatomy into an envelope. Label and keep it with the main specimen.

Packing Specimens to be Shipped

Dry plants are very fragile, however, they can be shipped without a plant press. If the specimens are still

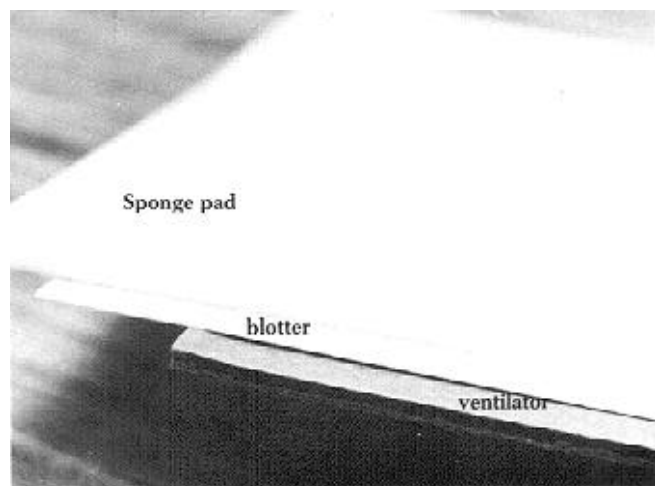


Figure 2. Equipment items shown in Figure 1.

loose between newsprint, layer them as flatly as possible in a snug fitting box. It may be necessary to insert heavy paper such as poster board between the layers. Do not crush the specimens, they should be packed loosely. If there is any room to spare, use foam or other packing to fill the space.

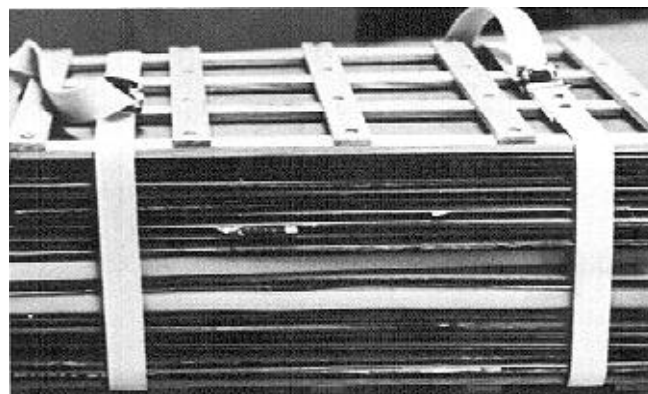


Figure 3. A hardwood, lattice frame press with adjustable straps.

If the plants are already mounted onto herbarium paper, use a box that is only slightly larger than the herbarium mount. Treat a herbarium mount as if it were fine art. Do not let it become smeared with newsprint. Make sure that any glue used in mounting is dry. It is best to insert a piece of clean paper between each mount.

Always ship specimens with a return receipt. It is nearly impossible to place a value on them. They represent time spent in the field, time for taxonomy and literature review. They are valuable and should be tracked as if they were worth a fortune in gold, because if properly prepared, they are priceless.

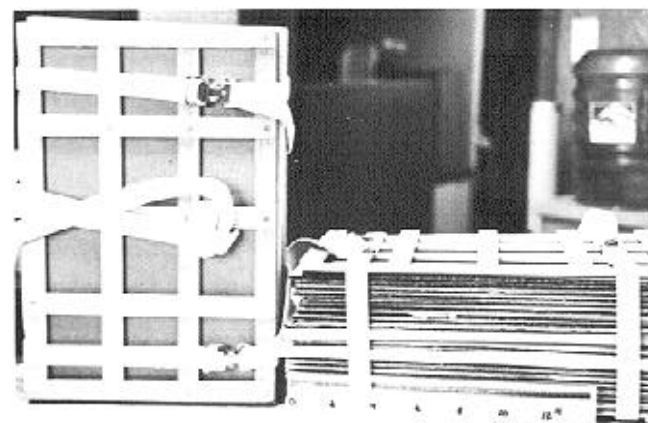


Figure 4. Two views of a suitable plant press.

Field Notes

These are best obtained either while collecting and inserted into the press with each corresponding specimen or recorded directly into a field notebook for permanent record.

When God created, He designed and it is this design that taxonomists like to tear apart, examine, pigeon hole and quantify. This system of identification may not be the best but it is our way of keeping track of the Creator's outline.



Figure 5. *Verbena* sp. Monadelphous stamens form a tube surrounding the style with filaments diverging at the top in members of the family *Malvaceae*.

Always try to collect a complete specimen including flowers as well as ripe fruit and enough root to tell whether it is a taproot, fibrous root or rhizome. But if you cannot obtain a complete specimen, then include a discussion of missing details in your field notes. If the plant is so large that it cannot be collected (a tree or bush) then carefully describe it in your notes. Include height, growth form, trunk size, type of bark, etc.

Field notes are important not only to the taxonomist but they are also indispensable to the ecologist. Reggie Fletcher, Botanist for the United States Forest Service Southwest Region, has verified the identification of many of the plants collected for CRS. He claims that one of the most important elements on an herbarium specimen sheet is a list of associated species. If a botanist has an idea of what a plant was associated with, he will have an idea of where to look for it in other localities.

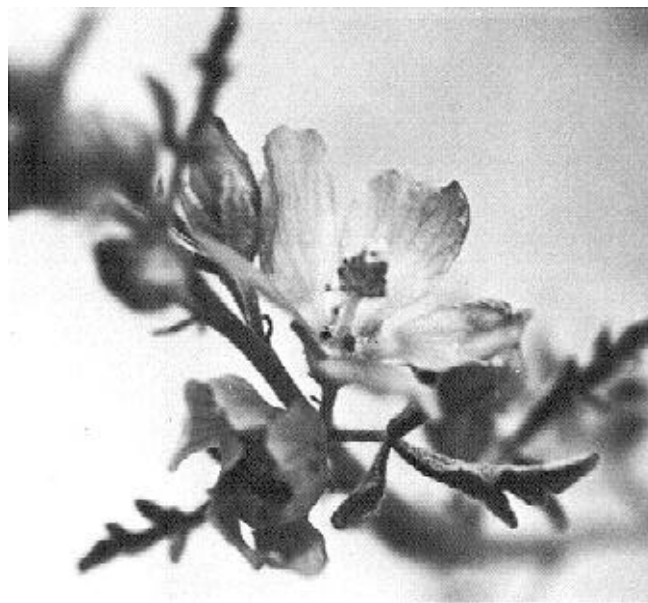


Figure 6. *Sphaeroclea* sp. Monadelphous stamens form a tube surrounding the style with filaments diverging at the top in members of the family *Malvaceae*.

Field notes should include as much of the following as possible:

- List of associated species.
- Community type (riparian, dry meadow, woodland, etc.)
- Soil type (or your best description)
- Elevation, percent slope, aspect
- Geomorphic setting
- Location (Section, Township, Range; name of Land Grant, vicinity; county; highway marker, etc.)
- Description of missing anatomy
- Site notes (overgrazed, old field, fenced, spring, etc.)
- Commonness of the species (abundant, common, few, rare)
- Flower color (flower color may change after the plant is dried, descriptions are written for live plants)

When field collecting, taking notes may get to be a redundant chore. One set of notes may suffice for several plants. Specific details can be noted on the newspaper or drying paper covering the particular plant in the press. Dr. George Howe, who has already contributed many specimens to the CRS herbarium, suggests carrying along a small tape recorder which may be used to store field observations. These notes can be transcribed at a later date and attached to the particular specimen when it is more convenient. Either way, notes should be collected into a field notebook for permanent record and frequent review. Your personal collection number should be recorded and a list maintained of the next available number (see section on organizing data for use of the Personal Collection Number). Recorded data will be invaluable to the taxonomist who inherits the plant specimens.

Learning Basic Plant Anatomy

Anatomical criteria are needed to support identification. If you cannot collect the complete plant, attach a descriptive note. It does not have to be as glamorous as Munz's "sepals caudaceous at anthesis" but it could be as follows. "This plant wears pixie caps which when pulled off expose the petals." Or, "This plant had a long root that broke before I could find the bottom of it."

Although a field biologist need not be a taxonomist, it is still helpful to have a basic understanding of plant anatomy to take to the field. Knowledge of anatomical criteria is needed to understand the botanical literature. There are a number of small-sized references that can be handy in the field such as *The Peterson Field Guide Series* and the *Finder* series. Pocket sized references can be purchased for trees, flowers and berries. These references contain glossaries and many pictures as well as illustrations of plant anatomy. Most college book stores will offer a local version of plant field guides.

Being Aware of Other Disciplines

At the March, 1986 meeting of the Society of Ethnobiology, one of the biggest complaints voiced concerned plant anatomy. It was reported by the keynote speaker, Dr. Vorsila Bohrer, a leader in ethnobiology in the Southwest, that scientists from other disciplines are frequently not learning skills from botany that could help them understand the botanical literature. Unfortunately many universities are not

willing to let their people cross over into other departments to learn such related skills.

The problem with being a purist in your own particular discipline is that much valuable information on plants is never recorded. One summer I had the delightful duty of monitoring permanent range transects in a Northern California wilderness area. Transects are long lines along which vegetation is recorded and analyzed. Some of the transects had been established for as long as 20 years. Most of the transects had been inventoried fairly well for the important range grasses, but the forbs (a range management term for wild flowers) had not been treated carefully since the purpose of these transects was to determine forage production rather than to develop a complete vegetation inventory. The last 10 feet of one transect crossed a bog which contained populations of "rare" or "threatened" plants. These had never been recorded on the transect readings or any other file. Meanwhile, the managers of that forest had contracted a botanist to look for one of these species which had been growing in one of their own transects without them knowing about it. The contracted search was unsuccessful in locating the plant. Had these plants been mentioned on previous transect readings, even as a note, it could have saved the U.S. Forest Service some expense.

Table II. A Table of Plant Uses for Certain Specimens in CRS Herbarium. The Herbarium Number is Given for Each Example.

85 - 45	
Family —	<i>Lamiaceae (Labiatae)</i>
Plant —	<i>Prunella vulgaris</i> L.
Name —	"Selfheal, Heal-all"
Sept./Oct. 1985	
Place —	Marietta, Ga.
Uses —	Various teas were made for bruises, burns, diabetic sores, cuts and acne; it was cooked as greens; it flavored other medicines. (Hamel & Chiltoskey, p. 54)
85 - 50	
Family —	<i>Fabaceae</i>
Plant —	<i>Cassia fasciculata</i> Michaux.
Name —	"Partridge pea"
Sept./Oct. 1985	
Place —	Marietta, Ga.
Uses —	A root medicine was used to keep Indian ball players from tiring; mixed with wild senna, a tea was made to treat fainting. (Hamel & Chiltoskey, p. 54)
85 - 58	
Family —	<i>Asteraceae</i>
Plant —	<i>Solidago odora</i> Aiton
Name —	"Sweet goldenrod"
Sept./Oct. 1985	
Place —	Horse Pens 40, Ala.
Uses —	Various teas were used for fever, colds; diaphoretic; tonic; stimulant; nerves; measles; female obstructions; tea used for bloody discharge from bowels; summer complaint; neuralgia; sore mouth; tuberculosis. (Hamel & Chiltoskey, p. 36)
85-62	
Family —	<i>Asteraceae</i>
Plant —	<i>Anaphalis margaritacea</i>
Name —	"Pearly everlasting"
Sept./Oct. 1985	
Place —	Marietta, Ga.
Uses —	Used for catarrh, colds, throat infection, bronchial cough, headache, sun blindness, chewing tobacco. (Hamel & Chiltoskey, p. 48)

Organizing the Data

Fifty percent of a taxonomist's time is spent in actual microscope work. The other half goes to managing the data and preparing the plants for the herbarium. Ethnobiology research is additional. In order to access the abundance of information that comes in from the field it will become increasingly necessary to computerize the CRS herbarium.

Even in the age of computer technology there is nothing like the original source for gleaning information. As a collector you are the original source and should be keeping track of your own specimens. To do this you will need a numbering system.

The CRS Herbarium will provide a number of its own. However, the collector should also have his own number which will go onto the herbarium sheet label along with the collector's name. This number is completely separate from the number assigned by the herbarium.

The personal collection number is one that you assign to a plant. If it is the first plant you have ever collected in your career as a field botanist, it is assigned the number "1." The second specimen is assigned the number "2" and so on. According to Smith, new numbers are assigned anytime you move to a new specimen, new location or new day. He has described a few examples. (Smith, 1979, p. 6).

1. If I collect 10 different kinds of plants at a certain site, I will have 10 collection numbers.
2. If I collected each of the 10 plants in duplicate or triplicate, I would still have only 10 collection numbers, each in duplicate or triplicate. This is the only situation in which a collection number is used more than once.



Figure 7. Mount of *Polygonum* showing envelope used to corral small seeds.



Figure 8. *Cassia fasciculata* with ethnobotany notes; Cherokee ball players used a root medicine to keep them from tiring.

3. If I move to a second site and collect five more plants, I will have five more collection numbers. This is true whether or not any or all of the five



Figure 9. *Prunella vulgaris*. A good example of a specimen collected with the root.

plants duplicate species collected at the first site. New numbers are assigned because this is a different collection site.

4. If I should return to any of these sites at a later date, all of the plant collections made at that time would get new numbers.

Photographs of finished herbarium sheets are illustrated in Figures 7-10

Conclusion

Bohrer (1986) reminded field biologists that they are at best interpreting rather than identifying. She said that we draw on clues to give our best guess of what "it" is. Therefore, she said that we must document our way to a conclusion. Dr. Bohrer reminded her colleagues that redundancy of work can provide an insight that cannot be acquired in any other way. Her final reminder to the ethnobiologists was "treat each step as important and worthy." We as creationists should be even more concerned. God created in completeness and wisdom. His design is exquisite. Collect intelligently so that your work can become an integral part of the CRS herbarium.

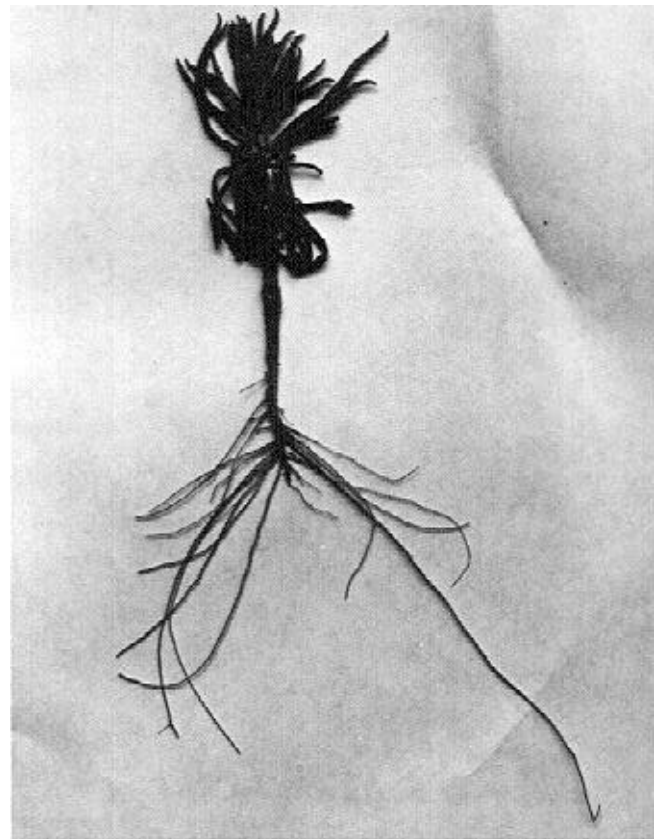


Figure 10. *Dithyrea wislizenii*. A better example of collecting the root.

Acknowledgements

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ARTICLE REVIEWS

Star formation, luminous stars and dark matter by Richard B. Larson. 1987. *American Scientist* 75: 376-85.

Reviewed by Don B. DeYoung*

Introduction

This article addresses some basic questions in astrophysics. New theories and observing instruments are keeping this field of study very active. In particular, the 1983 Infrared Astronomical Satellite (IRAS) produced much data concerning star formation and the nature of interstellar matter.

The author's position is that nearly all of the dust and gas in space are the remnants of former stars. Evidence is searched for rapid star recycling, with a consequent interest in stellar masses and lifetimes.

Points of Interest

Larson's paper brings out much useful information related to stellar evolution. Some of the points are:

—Stars of one solar mass have a lifetime of 15 billion years if their energy source is nuclear. Because this is the assumed age of the universe, star recycling is thought to be limited to more massive, shorter-lived stars.

—50 percent of stellar clouds are found to have infrared sources embedded in them, usually taken as protostars. The nearest such clouds are in Taurus, 500 light years away.

—The long age view requires that new stars form at the rate of about six solar masses per year in the Milky Way (p. 379).

—IRAS detected regions of extremely intense infrared emission in galaxies. If the radiation is really from protostars, stellar formation is rapid enough to deplete all galaxy dust in less than 10 million years (p. 379). This is implausibly rapid in stellar evolution theory.

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—Our neighbor galaxy Andromeda is moving toward us despite the general expansion of the universe. Gravitational attraction by unseen "dark matter" is suggested as the cause (p. 384).

Star Formation

Do stars form spontaneously? This question does not have a definite answer, either from stellar evolution or from creation viewpoints. Concerning the latter, the denial of any present day star formation has often been made a point of orthodoxy. However, we simply do not know! Stars are inherently "simpler" than living systems. If a cloud of hydrogen gas is squeezed to within Jean's gravitational limit, it may well heat up and turn on as a star. Remember that the sun is a gaseous sphere as well as a stable star. If star formation does indeed occur today, it certainly remains a rare event, only in regions of pressure waves. There remains a total inability to explain the host of visible, isolated stars. A timescale problem also remains, since computer models give a million-year startup time for new stars (p. 382). However, just as rapid star decay has been observed—(DeYoung and Whitcomb, p. 86), so formation could also sometimes be rapid. Having raised this controversial point, let me be very clear: All assumptions regarding present day star formation remain speculative. All infrared protostar data remains open to other interpretation—(Steidl, p. 102). No "turning on" of a star has ever been observed.

Consider Larson's data on star formation. *First*, he describes "stellar nurseries" as clouds with densities of at least 10^4 hydrogen molecules/cm³ and temperatures near 10K. However, (Mulfinger, p. 11) has clearly shown that such clouds cannot form stars. *Second*, Larson reports that instead of collapse, many such clouds often show an extremely rapid outflow of material. Natural explanations of this mass dissipation are lacking. The motion seems to resemble star decay

more than it does star formation! *Third*, the formation of massive stars is thought to occur only near other massive stars. After all, strong interactions would be needed to squeeze a cloud to a critical protostar size. This is said to be happening in the Trapezium region of the Orion Nebula (p. 378). However, there is something obviously wrong here. For the well known supergiant stars with masses greater than 10 solar masses (Betelgeuse, Rigel, Antares, etc.), no massive binary partners or unusually dense surroundings are known. The conclusion from these three points is that natural star formation, if it does occur, remains poorly understood, both from data and from theory.

More on Massive Stars

The formation and eventual supernova shattering of massive stars is thought to be the source of dust and gas, the "dark matter" of the universe. The author holds out the hope that such stellar debris may contain the key to a closed universe, one which will eventually collapse on itself. Because much dust is required, Larson is forced to suggest that massive stars formed much more rapidly and frequently in the past than in at present. With such speculation, one can arrive at any conclusion desired! However, even this thinking leads to observational trouble. Many galaxies, such as the Milky Way and the Magellan Clouds, still have simply too many massive stars (p. 383). Considering the "short" lifetimes of such stars, galaxies should have lost their massive stars by now. The search for dark matter in the universe will continue, but there are a growing number of dissidents. These critics demand nothing less than a total rethinking of current cosmology—(Horgan, p. 22).

Conclusion

This article clearly shows how little is really understood about stars and galaxies. The boundary conditions on current thinking are shown both by silence—no mention of gravitational contraction for stellar energy, and by assumption—massive stars must be younger than normal stars, since giants last only a few million years at most. The alternative view of the creation of the heavens, in all their present beauty and diversity, is refreshingly free of such constraints.

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The evolution of early land plants by P. G. Gensel and H. N. Andrews. 1987. *American Scientist* 75:478-89.

Reviewed by Walter E. Lammerts*

In this article the authors claim to show some of the steps involved in the evolution of vascular land plants (having water conducting tissue). They claim that it was not until the Silurian period 400 to 450 million years ago that plants became adapted to a land environment and became well established there. What evidence do they present? As the authors point out if

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one assumes the evolution of land plants from algae or plants resembling them, many changes are involved. Thus plants had to develop a waxy outer coating, or cuticle to prevent the loss of water, pores called stomates to allow gas exchange, reproductive structures that could function on land, and a system which is quite complex in most vascular plants to circulate water and food. What do the authors present as to how this was accomplished by an evolutionary process?

After a discussion as to how fossil plants are preserved they present in their Figure 2 the usual series of stratified rocks from the Ordovician to the Quaternary with the "remarkable burst of evolution" of land plants. As so effectively pointed out by Henry M. Morris in *The Genesis Flood* (pp. 132-7) this succession of the stratified rocks cannot be found anywhere on earth. Only a few formations are ever superposed in any one locality, and only by the use of 'index fossils' can these formations ever be put together into a complete series such as is shown by the authors. To the unsophisticated reader Figure 2 appears most impressive. Also it is interesting that all of the lower formations have English names. This is because the stratified rocks were first studied in England and the names are those of the various places and tribes who lived in the areas where these formations were found. Thus Cambria is the Latin name for Wales where the Cambrian rocks were first studied. Similarly the Ordovician was named after the Ordovices, an ancient Celtic tribe of North Wales. The Silurian was named after the Silures, an ancient tribe who long ago lived in that part of Wales where these rocks are found. And finally the Devonian was found in Devonshire, a small county in southwestern England.

Remains of possible land plants from the lower Silurian are shown. These include a fragment of cuticle, a spore with resistant wall and divided into three parts, and three tubes, one banded with a regular pattern of lignin-like material, and the other two smooth. The authors make a most interesting statement (p. 481) that it now seems probable that most early land plants were buried in low-lying freshwater or marginal marine environments such as flood plains or coastal marshes, probably after being transported a relatively short distance. Now this is precisely what one would expect as the result of a world wide Flood, since as the earth became gradually filled with water both fresh from the intense rainstorms, and marine from the disturbed oceanic basins, these marsh inhabiting plants would be the first to be covered with sediment. Also the authors admit that we still lack any precise information as to the presumed aquatic ancestors of the earliest land plants and that the search for evidence of these precursors and of probable transitional stages continues.

The first plant discussed in detail is *Cooksonia*, a very simple plant found in the upper Silurian. It has upright branched stems terminating in sporangia which produced spores typical of vascular plants. These plants evidently attained a height of only a few centimeters. Though first described from the lower Devonian by Lang in 1937, new fossil specimens discovered within the last decade in Wales and Ireland push its occurrence back to the middle Silurian. Although *Cooksonia* shows several features of vascular plants,

tracheids have never been found in its stems. These water conducting elongated cells have always been considered a sure identification of vascular plants. No evidence of any transitional forms is given.

The next plant discussed is *Renalia hueberi*, a late Devonian plant discovered on the Gaspé peninsula of southeastern Quebec Province, Canada. The superbly preserved specimens are found in large quantities in a gray shale, indicating a pure stand of these plants buried close to the area at which they grew. *Renalia* plants range from 20 to 30 cm in height; have a main axis with lateral branches bearing kidney-shaped sporangia. In contrast to *Cooksonia* the sporangia of *Renalia* split along a special row of cells to shed spores. A dark central strand of tissue can be seen after chemical treatment and the authors believe this strand reveals the presence of tracheids in the stem. Both of these genera, *Cooksonia* and *Renalia* show many features characteristic of primitive vascular plants: small size, lack of differentiation into stems and leaves, a simple vascular system (in *Renalia*) and a rather simple branching pattern. There is also one most interesting genus called *Prototaxites* which has stems measuring up to 15 cm in diameter. The number of known vascular plants increased from one in the late Silurian to about 28 at the end of the Lower Devonian. From then until the end of the Devonian, the numbers remain the same, with earlier genera becoming extinct and new ones appearing. *Zosterophyllum* was first described by Penhallow in 1892 and he thought the stems of this plant were like those of our modern eelgrass, *Zostera*.

Baragwanathia longifolia is the oldest lycophyte and has a robust stem 1 to 2 cm in diameter thickly covered with long slender leaves. The stems were up to a meter in length, and probably grew along the ground. It closely resembled our shining club moss, *Lycopodium lucidulum* which grows in the mountains of North Carolina. First reported in early Devonian, it has been revised by discoveries in Australia by Tims and Chambers as being in the late Silurian. This raises the intriguing possibility of the Silurian occurrence of vascular plants much more complex than *Cooksonia* or *Renalia*.

By Middle Devonian times several other lycophytes are known. *Leclercgia complexa* consists of abundantly branching axes covered with small leaves divided toward their distal end into five units. The lycopods continued to evolve at a rapid rate throughout Devonian. Some had become arborescent with trunks up to a half meter in diameter and heights of 8 to 10 m. Some Middle and Upper Devonian plants such as *Archaeopteris* reached diameters of approximately 1 m and heights of about 25 m.

First appearing in the middle of the early Devonian and extending into the Middle Devonian are the trimerophytes, best characterized by the genus *Psilophyton*. A number of species of this genus have been described. An exceptionally well preserved species in this genus is *Psilophyton crenulatum*. It shows the characteristically varied branching patterns of the slender mostly erect stems. The plants were probably up to 30 cm high, with many branches terminating in masses of paired sporangia producing large numbers of spores. Another species found in abundance in northern Maine is *Pertica quadrifaria*. It had a strong

central axis with side branches arranged in four clearly defined rows. These were three-dimensional, some being sterile and probably photosynthetic, while others terminate in compact, densely divided branchlets bearing a mass of sporangia. A most interesting progymnosperm is the tree type *Archaeopteris macilenta*. These trees had diameters of about 1 m and heights up to 25 m. The distinctly branched stem was two-dimensional and had leaves. Thus leaves originated in the Early Devonian. The trimerophytes also lead to the first seed plants now known from the uppermost Devonian horizons. The earliest appearance of heterospory or the production of male and female gametes is found in a late Lower Devonian plant called *Chaleuria cirrosa*. It had a main stem about 1 cm in diameter, with spirally arranged side branches. When the sporangia were analyzed, some were found to contain megaspores and others microspores. As the authors say what is revealed is a landscape first of very tiny plants which changed to a very diverse array of erect plants from 0.5 to 9 m high. At a distance the Devonian landscape must have resembled a modern salt marsh.

Now what should a creationist's reaction be to the presentation of the above evidence of evolution? As Moore, R. C., C. G. Lalicker and A. G. Fischer. 1952. *Invertebrate Fossils*. McGraw-Hill. New York. p. 7 write: 'Fossils are an indispensable means for classifying and correlating sedimentary rocks throughout the world.' Or as Henry Morris writes in *The Genesis Flood*, p. 132: 'The importance of the so called 'index fossils' in the geologic identification of rocks is indicated by the Yale geologists Charles Schuchert and Carl Dunbar as follows: 'A trained paleontologist can identify the relative geologic age of any fossiliferous rock formation by a study of its fossils almost as easily and certainly as he can determine the relative place of a sheet of a manuscript by looking at the pagination. Fossils thus make it possible to correlate events in different parts of the world and so to work out the history of the earth as a whole.' That is why Tims and Chambers working in Australia could claim that *Baragwanathia* was found in older Australian tentatively dated as late Silurian, instead of early Devonian as first reported by Lang and Cookson in 1935. Incidentally the determination that Lang and Cookson's discovery was really early Devonian was based on the presence of graptolites which the authors of *Invertebrate Fossils* state: 'are especially useful for stratigraphical zonation and correlation. Graptolites are among the most important index fossils in Ordovician and Silurian strata.' (pp. 716 and 718)

So then what we essentially have in the Gensel and Andrews paper is the presentation of a series of plants of increasing complexity as one considers them as occurring first in the middle Silurian and then later in the lower and middle Devonian. *Cooksonia* evidently consisted of several species. *Renalia*, a late Lower Devonian plant is considerably more complex, especially in that the sporangia split along a special row of cells to shed spores. It is also thought to have tracheids. *Prototaxites* had stems measuring up to a meter in diameter, actually large logs. Though several others are discussed, *Baragwanathia* is one of the most interesting having robust stems covered with slender, closely arranged leaves about 4 cm long. It evidently was a

sort of grandiose club moss similar to *Lycopodium lucidulum*.

Now all of these species were found over a period of at least 50 million years according to evolutionary geologists' reckoning of the time it took to form these sedimentary rocks in which these plants were discovered. Yet each one of the species appears as a fully developed one even though of course as one studies them in the later formations more complex types do appear such as the genus *Baragwanathia*. Surely if evolution is the explanation of the origin of these plants one would expect to find many intermediate forms, in fact a whole series of them. This is especially so if one postulates that species developed by the accumulation of mutations, which is the usual evolutionists' mechanism for species differentiation. The fact that during this immense period of time not a single series of variations can be found is surely remarkably good evidence that these species were created and then destroyed by the world wide Flood as recorded in Genesis 7:17-24. Of course all of these species are now extinct. In fact the big problem is that so very many species are becoming extinct each year.

BOOK REVIEWS

Blowing the Whistle on Darwinism by Lester J. McCann. 1986. 7555 Co. Rd. 10, No., Waconia MN 55387 124 pages. \$9.95.

Reviewed by P. Lawrence Pusey*

Dr. McCann has been a professor of biology at the College of St. Thomas for 30 years. He has written this book in a manner that is interesting and easily understood by the lay person. At the beginning he reviews the 125 year struggle between Darwinism and "non-Darwinism," the rise of neo-Darwinism, and the anti-Darwinist resurgence in recent years. He then talks about the man, Darwin, and portrays him as an amateur scientist who had "trouble keeping straight his theology from his science." According to McCann, to enshrine Darwin or build him up as a bulwark around which the field of biology gravitates, is a judgment matter that calls for review.

Modern Darwinism is viewed in two facets: the unassisted origin of life or abiogenesis, and the fortuitous production of new species, or neobiogenesis. McCann contends that for protoplasm to have originated by pure chance would have been a gigantic abridgement of the Law of Entropy (second law of thermodynamics). He emphasizes the inability of chance to operate as a causative force since only controlled forces construct. Natural selection, as the term is used by Darwinists, is discounted for basically the same reason; it has no way of controlling energy. In answer to the question, "Can mutations generate evolution?" McCann says that the mutated gene qualifies as a miscue. It is a cardinal principle of information theory that mistakes cannot improve a code of information; they can only reduce a code's ability to transmit anything meaningful.

That is why the California Native Plant Society continually stresses the need to preserve the species which we have left. Thus in the April 1983 issue of their journal *Fremontia* Peter H. Raven has an article entitled 'The Importance of Preserving Species.' On p. 11 he states: "The Smithsonian Institution, in 1978, listed 1485 kinds of plants as endangered, 1408 as threatened and 360 as extinct." So then although there is a remarkably long list of extinct plants if we include all of those found in the various stratified rocks along with the ones which have become extinct since the United States became populated, there is not a single instance of any new plant having arisen by any process of evolution. Because of our realization that the various species of plants were originally created we are in a better position than evolutionists to appreciate the need to conserve the plants which God created for us. Certainly if they really believe that species arise by any sort of evolutionary process they must be less concerned about the loss of a few of them for after all, more will evolve to take their place! Let us then cooperate with organizations such as the California Native Plant Society to conserve the plants we still have left.

In one chapter McCann argues against the contention by Darwinists that if an occurrence could conceivably happen, then we can count on it having taken place. Statisticians are in general agreement, he points out, that when the probability of an event calculates to be less than one in 10^{50} , it is impossible for the happening to occur at all. He then gives various scenarios involving protein and gene formation with odds that are far less than this.

McCann considers Darwinism a hindrance to the science of biology. According to McCann, Darwinists tend to be preoccupied with chemistry and are generally mechanistic. He discusses the inadequacies of chemistry and physics in explaining many phenomena in living systems. Concerning the Darwinist's ardor for genetics, McCann asserts that there are many cellular activities which the genes give no evidence of controlling. He says that by focusing only on nuclear genes, Darwinists are obstructing an understanding of the full scope of accomplishments of which the living cell is capable.

McCann outlines the prerequisites for any construction. These are: (1) the right materials, (2) the right kind of energy, and (3) intelligence. He makes an application to the living cell and provides an interesting discussion on intelligence in cells. Attributes of cells covered include sentience, self-direction, know-how, and information transfer.

In separate chapters he addresses the questions, "Could so many be so wrong?" and "How did Darwinism do it?" In his final chapter, entitled "What can be done?" McCann suggests that those who are apprehensive about the ill-omened effects of Darwinism on the philosophic foundations of society may need to join efforts with those who are disturbed over the menacing effects of Darwinism on the integrity of science. It would help, McCann says, if non-scientists become thoroughly knowledgeable about the details of Darwinism and its scientific failings.

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Earth, Fire and Sea: The Untold Story of Creation by R. Russell Bixler. 1986. Baldwin Manor Press, Pittsburgh, PA. 214 pages. \$16.95.

Reviewed by George F. Howe*

This book contains theological, linguistic, and scientific argumentation about origins and hence three reviewers will deal with its contents. My own comments are followed by those of R. Nettelhorst, a specialist in ancient language, and G. Morton, a geophysicist. If the reader senses a difference in our evaluations, that simply reflects the wide range of reactions which will likely greet this book.

The author identified himself as a creationist and spoke out against macroevolutionism. He placed considerable emphasis on the miraculous element in creation. But at the same time Bixler showed a strange predisposition to the belief that God used natural means to form the pre-Flood vapor canopy. On p. 169 he made the general statement that "God operates within His own established laws." Thus Bixler viewed miracles as merely manifestations of God's own natural laws.

It is a very small step from this position to the devastating thrust of the higher critic who attempts to explain all miracles as the fortunate consequences of purely natural events—a view which Bixler would probably reject. It is true that God can and often does work in accordance with "natural laws"—the procedure by which He governs His handiwork. But we should also recognize that God can and does produce effects without any "natural" causes. Such non-repeatable activity of the Creator falls outside the domain of human inquiry. Miracles are thus governed by direct spiritual causality which, by its very nature transcends all temporal "natural causality." God can and does break what we call His "established laws" whenever miracles are performed. Miracles demonstrate that natural law is of God but not *above* God, as I have explained elsewhere—Howe (1972). Bixler affirmed that God's creation laws are the same laws God operates under today. I would submit that the two are quite different and if they were not different we would never recognize miracles.

When an individual tries to force the Bible into some scientific mold by a bizarre pattern of interpretation, we speak of the resulting scheme as a "concordant theory." Chapter eight contains a valuable rejection of various concordistic theories such as (1) the "gap-theory" by which it is assumed that a time gap occurs between Genesis 1:1 and 1:2, (2) the "day-age" theory by which the interpreter attempts to solve origins problems by asserting that each day (yom) of Genesis 1 was a vast age of creation, and (3) the "literary framework hypothesis" by which some scholars try to argue that the Creator merely *spoke* to Moses on the six days about events that actually had taken long time periods to occur.

Concerning the gap theory Bixler did not allow eons of time between Genesis 1:1 and 1:2 and asserted wisely that "... a billion year gap in the middle of a sentence is incredible" (p. 89). Regarding the day-age view Bixler argued that it fails because "yom" really means day.

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Likewise he stoutly rejected the literary framework hypothesis by writing that "If any version of the literary framework theory is correct, then Genesis 1 must be incorrect!" (p. 91). Chapter eight is therefore a valuable defense against these unfortunate concordant theories of Genesis interpretation.

Bixler asserted the belief that the *Biological* creation was recent and thus placed himself in the company of CRS members who hold that all creation occurred in relatively recent time. He also argued that the geologist who misses evidence for the Flood is one who cannot see the forest for the trees. Thus Bixler was unimpressed by the uniformitarians' body of so-called "evidence" that is offered as support for an ancient origin and evolution of life. He is to be commended for a stand which puts him in company with the Flood geological catastrophists who oppose gradualism in the formation of the fossil record. These sections of the book will be of value to creationists.

But Bixler seemed unwilling to stand against the so-called "evidence" offered by the same uniformitarians as support for an ancient origin of *matter* and the universe. Bixler has devised his own concordistic theory in which he avowed that matter and the universe itself antedated the biological creation of Genesis 1. He asserted that the Bible actually supports such a rendering of Genesis 1:1, that the creation of matter and the outside universe "out of nothing" (*ex nihilo*) are not even mentioned in Genesis 1:1, and that they may have occurred billions of years earlier.

Some of the linguistic and theological problems attending this theory will be analyzed in greater detail by R. Nettelhorst. A few other theses in the book should also be challenged such as the unnecessary and unfounded dualism which Bixler envisioned between what he saw as a good Creator God and the evil forces of nature!

It is unfortunate that Bixler succumbed to the uniformitarians' demands for eons of radioactive and stellar time. He somehow imagined that he has brought peace between the Bible and science concerning the age of matter and the universe. But Bixler has left the more serious conflict between science and Scripture regarding the age of fossils untouched. Furthermore, he has ignored the solutions that creationists have proposed to the various problems of vast universal age, as published in *CRSQ* and elsewhere.

Bixler's interpretation of Genesis 1:1 is a long-shot, in my opinion. If it were somehow to prove correct, Bixler would have helped creationists see that there is no need to fight any battles regarding the long ages of universal history. They would still need to confront the problems related to the recent creation of life, however. But if Bixler's interpretation is largely incorrect, as I suspect, then readers of this book would wrongly be led to believe there is no conflict between vast stellar or radioactive "dates" and the Biblical record. As a result, research on these problems would suffer as whole segments of creationists would wrongly "make peace" with a false vast "age" for matter and the universe. Much is at stake with this book.

Reference

- Howe, G. F. 1972. Miracles and the study of origins. *Biblical Research Monthly* 37(10):3-6.

Reviewed by R. P. Nettelhorst*

Introduction

If you reject creation *ex nihilo*, then you will like this book. If you subscribe to an inconsistent and contradictory methodology, you will like this book. If you believe logical fallacies are a legitimate form of argument, you will like this book. The rest of us are just appalled. Bixler's book is a difficult book to review as it has so many problems one wonders just where to begin. I have decided to address some of these problems under two major headings: methodological problems and interpretive.

Methodological Problems

The author has a tendency to be inconsistent. On pp. 17-18, he decries tradition and argues that the reader must not rely on it for interpreting the Bible (a position with which I would agree). Then he almost immediately falls back on tradition to buttress his position, as on p. 24 where he quotes Jewish sources, and relies on Jewish tradition: "Ginzberg notes that Jewish traditions insist . . ." The author spends much of the book simply quoting from scholars and others who agree with the translation and the ideas he proposes for Genesis 1:1-3. Those who disagree are dismissed as "traditionally minded." It would seem traditions should be ignored when they are uncomfortably opposed to the author's thesis, but other traditions that support the author's thesis are acceptable (pp. 161 and following are particularly interesting in this context.)

The author spends an inordinate amount of time in attempting to prove that the concept of "*ex nihilo*" creation derives from a heretical source, as if thereby to prove that the doctrine is also "heretical" or somehow tainted. Even accepting that the doctrine was first expounded by a heretic (which is not the case), it is of no value to the author's point. To argue that the source of the doctrine is tainted is to fall victim to the logical fallacy of *ad hominem*.

A more minor point, the author has been delinquent in checking the reliability of his sources. He uncritically accepts the account of the *Letter of Aristas* regarding the formation of the Septuagint, though the letter is demonstrably fiction. He also quotes from the *Book of Jasher* as if it is a legitimate source. He does, at the very least, state the book's "antiquity has been challenged." That is an understatement, since the *Book of Jasher* is an eighteenth century forgery.

Interpretive Problems

Bixler argues that "scriptural exegesis reveals that the Bible does not speak of creation out of nothing, but out of existing material—material which stubbornly resisted God's creativity," (p. 181) and that "This exegesis provides the only interpretation of Genesis 1:1-3 that is not self-contradicting. It confirms itself throughout the Bible." (p. 181). Bixler argues that Genesis 1:1-3 must be translated along the following lines:

When God began creating the heavens and the earth—the earth being chaotic and empty, and darkness being over the surface of the deep, and

the Spirit of God hovering over the water—then God said 'let there be light' and there was light. (p. 28,184)

Keil, writing in the nineteenth century, indicated: "... this construction is invented for the simple purpose of getting rid of the doctrine of *creatio ex nihilo*, which is so repulsive to modern Pantheism." (Keil and Delitzsch) Yet the author has set for himself the task of arguing, not convincingly, that creation *ex nihilo* is not Biblical. He fails to adequately handle Genesis 1:1-3 in the context of the rest of chapter one of Genesis, or within its cultural context. Moreover, he argues for a dualism, that Genesis one is a description of God's battle with evil matter and of how He forced it to behave according to his wishes. Bixler wishes to describe a modified gap theory, with the gap preceding verse one, rather than following it. He accepts the gap theorists's view that "*tohu ve bohu*" refers to "chaos" which God then subdued in His creative acts of Genesis 1:2ff.

Bixler argues that those passages which refer to Leviathan/Rahab are speaking of God's struggles with the primeval forces of evil matter which already existed in Genesis 1:1. He fails to take account of the fact that all the references to Leviathan/Rahab (except those passages where an actual animal is being described as in Job 41) are references to Egypt, usually in the context of the Exodus of the Israelites from bondage.

The grammatical arguments marshalled by Bixler are not overwhelming, or even overly useful. To begin with, the only place that "*reshit*" (beginning) is preceded by the preposition "*be*" (in), and is then followed by a verb in the perfect aspect (completed action) is here, in Genesis 1:1. The common translation "In the beginning God created the heavens and the earth, etc." is perfectly reasonable grammatically. Furthermore, the context of the whole chapter virtually requires it.

Bixler's most serious error, however, was to attempt to view the first three verses in isolation. On p. 50 he mentions in passing that some commentators have taken Genesis 1:1 as a title. He dismisses that proposal without a second look, stating "the titular hypothesis is quite compatible with the overall thesis maintained herein." Yet the titular hypothesis does not fit with Bixler's view! Verse one should most definitely be understood as a title, or perhaps more accurately, as a summary statement. What occurs in Genesis 1:1 is what occurs in any good newspaper article where the first sentence or paragraph presents, in a nutshell, the contents of the article which follows. So here, in Genesis the first verse is an introductory statement of the sum of God's creative activity: He created the heavens and the earth and this statement is the Hebrew (and Near Eastern) way of saying that God created the *universe* (Bixler's protestations notwithstanding to the contrary). And this universe was created "in the beginning." There is no room here for pre-existing matter. Everything but God is said to have its start in the descriptions of Genesis 1:2. Then in Genesis 1:2-2:4a, Moses goes into some detail of how God accomplished this feat. Genesis 2:4b-25 which follows, details how He created the most important aspect of His creation: human beings. (See Table for the structure of Genesis 1:1-2:25.)

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Table I. Structure of Genesis 1:1-2:25.

1:1	General summary statement: God created the heavens and earth.
1:2-2:3	How God created the heavens and earth.
1:2	Day One: earth
1:3-8	Days One-Two: heaven
1:9-13	Day Three: earth
1:14-19	Day Four: heaven
1:20-31	Days Five-Six: earth
2:1	Summary
2:2-3	Day Seven: expansion of summary: heaven
2:4-2:25	Expansion of 1:2-2:3. Details of how man was made.
2:4b-7	Creation of home for man.
2:8	Man placed in his home.
2:9-17	Description of man's home.
2:18	The need for a helper
2:19-25	Fulfillment of the need for a helper.

Some other Biblical examples of this writing style in which the passage begins with a summary are given here:

1. Jonah 3:3-10

Jonah 3:3-5 explains that Jonah obeyed God's word, went to Nineveh, proclaimed a message of judgment, and that the people responded, from greatest to least, with a fast.

Jonah 3:6-10 gives the reader details of the king's response and reproduces a copy of the king's decree for a fast. If one fails to notice the topical expansion involved between these two sections, the actions of the king make no sense. If 3:6-10 followed chronologically from 3:3-5, why would the king declare a fast after a fast had already been called, and after his subjects were already fasting?

2. Ecclesiastes 3:1-8

Ecclesiastes 3:1 states: "There is a time for everything, and a season for every activity under heaven." In 3:2-8, the reader then learns in detail what those activities for which there are times and seasons.

3. Proverbs 1:10-19

Proverbs 1:10 states: "My son, if sinners entice you, do not go."

Proverbs 1:11-14 describes how sinners entice then 1:15-19 warns that a person should not go.

And of course Hebrew is not the only ancient Near Eastern language to use this topical structuring technique. In fact, it was the norm for the cultures around them. For instance there is an Ugaritic text that well illustrates the principle (1.4VI:55-59):

The gods still ate and drank.

And the waiter prepared a breast with a shiny knife
a slice of fatted meat.

They drank goblets of wine;

from a cup of gold, the blood of the vine.

Notice how these few lines of an ancient poem begin with a *statement* of what the gods were doing: eating and drinking. The lines that follow then explain in detail what was involved in that eating and drinking.

In the final analysis, Bixler's thesis should be rejected as untenable in the extreme. The structure of Genesis

1-2 does not permit his interpretation, nor do other passages of the Bible lend credence to his position. Space does not permit a point by point discussion of other Biblical passages he misuses. A thorough refutation of each of his errors would also require a book of its own. Perhaps it would be good to end with a Biblical passage unmentioned in Bixler's book, perhaps because of the havoc it would create to his theory:

Blessed be your glorious name, and may it be exalted above all blessing and praise. You alone are the Lord. You made the heavens, even the highest heavens, and all their starry host, the earth and all that is on it, the seas and all that is in them. You give life to everything, and the multitudes of heaven worship you. Nehemiah 9:5b-6.

Reference

Keil, C. F. and F. Delitzsch. 1986. The Pentateuch. Eerdmans. Grand Rapids: p. 46.

Reviewed by Glenn Morton*

I examine only the scientific aspects of this book. Although very little space in the book is devoted to the scientific issues of creation, the major thesis, that Genesis 1:1 does not refer to the creation *ex nihilo* of the universe, contains major implications for our understanding of creation. Bixler touches on many of these implications. For instance, if God did not create everything in the universe a few thousand years ago but merely made the earth and solar system out of pre-existing material, then at least some of the old radioactive dates can be explained by the fact that the matter is truly that old. As Bixler says, "this work renders much of the creationist argument against radiometric dating as moot . . ." (p. 151). This would also allow for a very easy explanation of the missing isotope problem. The only disappointment was that Bixler did not address the issue of old radioactive dates from igneous rocks embedded within sedimentary rocks.

His view that the universe itself is older than the earth would also lay moot objections to creationism based upon the time of travel of light from distant stars and galaxies. In Bixler's system there would be no need for light to travel on Riemannian surfaces or for the speed of light to have been different in the past as both Morton and Setterfield have suggested. The universe is old so light could have had the time to make the trip. God could also not be charged with deception in creating a young universe with an appearance of age.

Bixler has been greatly influenced by the works of Donald Patten and his description of the creation of the earth resembles Patten's description of the Flood. However, Bixler is one of the few holding to such views who admit that

Most of the ice would have become steam prior to striking proto-Earth due to conversion of initial potential energy of separation into kinetic energy from its acceleration to Earth's fiery surface. (p. 164)

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Bixler envisions masses of ice cooling the earth's surface rapidly providing an alternative explanation for Gentry's radiohalos. Since there is much more involved in creating those halos than merely cooling the granite, Bixler's mechanism here leaves a little to be desired.

The most important contribution to the scientific understanding of creation concerns the relation between the supernatural and the natural. Bixler devotes a full chapter to this discussion and concludes that even many creationists are anti-supernatural. Bixler implies that creationists cannot afford to ignore the miraculous working of God when discussing the creation. Unfortunately, says Bixler, many Christian leaders do.

Within the light of Bixler's supernaturalism, the usual objections to cosmic theories of Patten and Velikovsky just do not apply. Many have pointed out that such views violate the laws of conservation of energy and angular momentum. Bixler, using similar means at the time of creation, would simply reply that God was involved in the creative process at the time.

I have never understood why some people write books that say the same thing as others have said before them. A book is a lot of work to achieve nothing more than redundancy. Bixler's book is novel and its message is well worth considering.

The author's" response to reviews.

Earth, Fire and Sea is admittedly revolutionary; I stated this fact repeatedly in the book. But revolutionary writings can stir up very emotional responses, leading to quick, negative judgments. I will comment on each review.

Howe's review is a case in point, alternating between praise and condemnation. He contradicts himself and, in doing so, sets up a "straw man," then proceeding to demolish it. Howe states, "He [Bixler] placed considerable emphasis on the miraculous element in creation." Three sentences later uses contradictory language: "Thus Bixler viewed miracles as merely manifestations of God's own natural laws." However, I did not say "natural laws." I said "established laws," which include both the natural and the miraculous. Howe misses this vital distinction, and it is the error which provides the basis for this straw man. As a whole, *Earth, Fire and Sea* is actually more oriented to the supernatural than the natural.

Howe further asserts that I devised my own "concordistic theory." This is not so. I have merely recovered the creationist views of many of the earliest Christians, including the great Augustine himself. I clearly defended against this "concordistic" accusation in my book, anticipating such a criticism.

Concerning Nettelhorst's review, I really cannot respond to it briefly. His theology appears to be a curious synthesis of 19th century pietism and rationalism; but the Bible was not written in 19th century terms. I have tried to recover a first century Christian view. Nettelhorst obviously resents this effort. Challenging religious *a priori*s usually sparks more heat than light. Some of his language is unnecessarily pejorative. The reader of *Earth, Fire and Sea* needs to examine

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Nettelhorst's criticisms in the full context of the book. They are almost all answered there. *Earth, Fire and Sea* itself provides my response to Nettelhorst, and I continue to stand on it.

Morton's review is encouraging. He brings up the subject of Gentry's radiohalos. Morton's question concerning the radiohalos provides a reminder of my plea in *Earth, Fire and Sea* for further research by open-minded creationists. Not being a scientist myself, I must depend upon others to attempt to falsify the various theses and origins models I have presented.

Thirty years ago I was a thoroughgoing liberal; today I find that I am more of an inerrantist than most members of the International Council on Biblical Inerrancy. I only ask that creationists allow their *a priori*s to be challenged—Scripturally, historically and scientifically. We creationists must not be found as intransigent and narrow as the evolutionists!

The Amazing Body Human by Mark P. Cosgrove, Baker Book House, Grand Rapids, 1987. 202 pages, \$7.95.

Reviewed by Wilbert H. Rusch, Sr.*

Very occasionally I encounter a book that makes me say 'I wish I had written this book.' *The Amazing Body Human* by Dr. Mark Cosgrove falls into the category. Contrary to the majority of the books on humans and human origins that solely emphasize the similarities between apes and humans, Cosgrove takes the opposite point of view. Hence, much of the book is a masterful discussion of examples of just how great and numerous the differences are between all of the anthropoid apes and humans. His opening words to the body of the work are as follows:

It is interesting that a human is the only creature with the exception of birds who does not have to raise its head to make an utterance. Our erect posture generates an arrangement of the windpipe and vocal organs that permits us to talk while maintaining the normal head position. . . . human beings have the capability to maintain face to face interaction. Such a difference from the animal world is important because human faces are uniquely designed for the expression of personhood. (p. 23)

The subtitle is aptly named *God's Design for Personhood*. The author holds that contrary to Charles Darwin and others who share his views, our bodies speak of humanness, personhood, value, design and meaning. he considers that we have overlooked the uniqueness of the human body.

I also found the section on sexuality to be one of the best that I have ever encountered from a Christian point of view. This section alone makes the book worthy of being in every church library as a reference work for pastoral pre-marital instruction courses. Certainly it is high time that in this day of all kinds of advice on how to educate our children regarding AIDS, an abdication to a purely secular scare and prevention technique is utterly inadequate. The author gives many reasons for using 'No' as a means of prevention of

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pregnancy as well as venereal diseases. He also points out that the hymen is unique to the human female. He says:

It almost seems as if the body human is saying that the first sexual encounter for a woman is not to be entered into lightly. Something personal and hal-
lowed is at stake. (p. 93)

The author is not ignorant of Jane Goodall's work on chimpanzees. This is obvious from his frequent references to her findings. (See p. 93.) He is certainly not writing in a scientific vacuum. In the discussions on the many comparisons between apes and men, he is very penetrating and the result is the development of an overall view that is invaluable to the student of human origins.

A list of section headings follows:

1. Face to Face
2. Feeling is Skin Deep
3. Sex as if People Mattered
4. The Long Childhood
5. Rising to the Occasion
6. The Matter of Mind
7. Till Death Do Us In
- Epilogue—In the Image of God
- Bibliography

The only criticism that I would have of this work is the absence of a key-word index. Time and again I wished I could have found page references to some topic. However, this deficiency could be and certainly should be rectified in a future printing.

I certainly recommend this book to all CRSQ readers as a valuable addition to their library.

The Blind Watchmaker by Richard Dawkins, 1986.
Longmans Group 332 pages. £12.95. Harlow, Essex
England.

Reviewed by David C. C. Watson*

Reading this book reminds me of a little rhyme made up by the students of Balliol College in the 1870's:

First come I: my name is Jowett,
There's no knowledge but I know it.
I am the Master of this College:
What I don't know isn't knowledge.

A certain air of omniscience still emanates from Oxford dons: one gets the impression that even if the author himself does not quite know everything, yet Dawkins + friends + colleagues to gether come very close to encyclopaedic learning. To be compared with Galileo (back cover blurb) is enough to turn the head of any scientist; so the reader must brace himself to resist intimidation.

Dawkins (RD) is a full-blooded Darwinian evolutionist, a missionary with a gospel to proclaim to benighted Lamarckians, creationists, theists, nay, even to deists. He is Richard the Lion-Heart, attacking the armies of 'infidels' who espouse the Argument from Personal Incredulity and refuse to believe (e.g.) that spiders' webs could have evolved by minute modifications over millions of years. His reply to this heresy is simple: "I, Richard Dawkins, have some experience of spiders and their webs . . . and I firmly believe that the web could have been produced step by step through

random variation" (p. 39) (in other words, the Argument from Personal Credulity). So, what RD believes must be true, with or without evidence. Living as he does in the post-Christian era, he can afford to be much bolder than Darwin,—who had to tread cautiously through the Victorian religious minefield. He admits he writes as an advocate as well as a scientist.

Dawkins' *Watchmaker* is more a brief than a textbook and the oftener one reads it the more tricks of advocacy are discovered—special pleading, begging the question, false analogies, demolition of straw men, imputing guilt by association (with Jehovah's Witnesses), denigration of opponents, and withholding of vital evidence. He is most persuasive when playing with computers and least convincing when writing about real live animals.

It was Dr. Samuel Johnson, I think, who remarked that "one may scold a carpenter who has made you a bad table, though you cannot make a table. It is not your trade to make tables." (*Boswell's Life of Johnson*, Oxford University Press, p. 290) Nor is biology my trade, but I do not have to be a biologist to notice facts ignored or misconstrued. When RD says that "no serious biologist doubts the fact that evolution has happened, nor that all living creatures are cousins of one another" (p. 287), one has to assume that he reads only those authors who agree with him.

Michael Denton's *Evolution: A Theory In Crisis* completely refutes that allegation, and one might have expected RD to make some attempt at answering Denton's objections; but the book is not so much as mentioned. Inevitably one forms the impression that he is writing for the cosy coterie of Oxford students and scientists who regard him as a near-infallible authority, who know little of American-Australian science and much less of creationism—except from the pen of its opponents. Not a single modern creationist book is listed in the bibliography, and even G. R. Taylor's *The Great Evolution Mystery* is excluded, no doubt because the author dares to doubt natural selection as an omnipotent miracle-worker. RD gives no references for his quotations . . . perhaps just a matter of style, but unfortunate when the quote happens to be inaccurate. The opening paragraph of the book states: ". . . Our own existence is a mystery no longer because . . . Darwin and Wallace solved it . . ." (p. ix) (emphasis added) In fact, of course, Wallace did not agree with Darwin about the descent of man from monkey; he believed natural selection could not account for man himself, thus provoking Darwin to protest: "I differ grievously from you and I am very sorry for it. I hope you have not murdered too completely your own and my child." (Macbeth, Norman. 1973. *Darwin Retried*. Delta, p. 103) Dawkins' inexactitude, at the threshold of a scientific treatise, does not inspire confidence.

Now we shall look at some of the methods by which RD seeks to persuade the world that Darwin got it right, utterly, gloriously right, whereas poor old William Paley was "wrong, utterly, gloriously wrong" (p. 5)! Fortune favors the bold, so he begins by directing our attention to one of the most complex systems in all nature—echolocation in bats. By describing this in some detail he elicits our awe and wonder; but then, instead of explaining how it could/must have evolved by infinitesimally small stages, he wades in to attack the

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Bishop of Birmingham's *The Probability of God*. This book includes (a) an exceedingly weak argument against the whiteness of polar bears being due to natural selection: RD has no difficulty in refuting it. (b) a paragraph on spiders' webs (see above) (c) a paragraph on eyes, which RD answers thus: Eyes don't fossilize . . ." (p. 40). It is hard to believe he did not know this statement is untrue. One of the most impressive pages of *The Great Evolution Mystery* describes the discovery of fossilized trilobite eyes by Levi-Setti and Clarkson in 1973. He goes on to brood over the difficulty we have in imagining geological time.

In a few thousand years we have gone from wolf to Pekinese . . . think of the total quantity of change involved in going from wolf to Chihuahua, and then multiply it by the number of walking paces between London and Bagdad. This will give some idea of the amount of change that we can expect in real natural evolution. (p. 40)

Here is a classic example of false analogy, for the following reasons:

1. There is no evidence that dogs were originally bred from wolves. Historical records in every ancient civilization mention them as distinct species from the beginning.
2. All freaks such as Chihuahuas are man-made, artificial breeds. They would never survive in the wild; so there is no true parallel with "natural evolution."
3. Are we to believe that because we now, after 2000 years of breeding, have dogs as diverse as the Great Dane and the Peke, therefore by 4000 AD we shall have dogs weighing a ton, and others weighing *One ounce*? Extrapolation is a risky sport: all farmers know there is a limit to breeding, both up and down. So it is highly misleading to use this analogy to 'prove' that anything can be changed into anything, given enough time.

Another argument in the Bishop's book is the Cuckoo Story. Canon C. E. Raven, a fine ornithologist of the 1930's, pointed out that cuckoos require (a) a mother who will not build her own nest but lays her egg in others, (b) a chick that will turf out other chicks and eggs from its host-parents' nest, (c) a foster-mother willing to feed the monstrous intruder who has murdered her own offspring. For the system to work, says Raven, "the whole 'opus perfectum' must have been achieved simultaneously." (p. 41) "Not so!" replies RD "It isn't true that each part is essential for the success of the whole." (p. 41) But instead of showing *how* the system could have developed bit by bit, *he walks right away* from the whole question, and contents himself with the feeble unsupported assertion that ". . . a simple, rudimentary . . . cuckoo parasitism system, is better than none at all." (p. 41). Since it is obvious that a chick that pushes his own brothers and sisters out of the nest would be worse than none at all, even a non-ornithologist might be tempted to doubt whether Professor Dawkins has thought through this one. This time it is Canon Raven, we opine, who is utterly, gloriously right.

Meanwhile all this peripheral argumentation has diverted our attention from the central problem of bat-evolution: how did a ground-mammal ever become an air-mammal? RD entirely omits two vital facts:

(1) The oldest bat-fossil skeleton is exactly the same as a modern bat's, bone for bone; (2) All bats have their pelvis twisted 180° from that of ground mammals, and no fossil ever discovered shows any sign of a 90° or any other degree of twist. What has happened to all the intermediates? The question is not even raised. So, in the chapter Good Design, after 20 pages expounding the marvels of echolocation, the 'proof' that this evolved from nothing is reduced to the meager proposition that half a loaf is better than no bread, half an ear better than no ear, *therefore* echolocation and the bat must have been evolved, not created!

What about the famous bombardier beetle? Here is RD's explanation: "The bombardier beetle's ancestors simply pressed into service chemicals that already happened to be around. That's often how evolution works." (p. 87) Translated into human terms this could be: "My 6-year old *simply* pressed into service some loose gunpowder and metal that *happened* to be around, constructed a double-barrelled 12-bore gun and cartridges and fired them at the burglar." (Children are much more intelligent than beetles.) A tall story; but no taller, we think, than RD's fantasy. And why is the world not overrun with bombardier beetles, if its armament is so easy to make, so effective in use?

It is refreshing to find *one* phenomenon of zoology that remains a mystery even to our learned Professor. This is the periodical cicada which divides into three species, and each species has both a 17-year and a 13-year variety. All attempts to explain this in terms of evolution have failed. Why not 10, 11, 12, 14, 15, 16, 18, or *any* number of years? Why have not all three species been ousted by the much faster-breeding two-year and five-year species (not mentioned by RD)? By the Darwinian law of differential reproduction, the race is to the swift and the battle to the baby-boomers. But here the slow are equally successful and show no signs of becoming extinct. Once again the mysterious balance of nature points to an all-wise Creator whose arbitrary arrangements defy human analysis.

RD takes a whole chapter to explain what he calls the 'arms race' between cheetah and gazelle, each animal running faster and faster (another just so story) to catch or escape being caught. How much more convincing the theory would be if fossils had ever been found of a slow cheetah or a slow gazelle. But RD is very economical indeed in his references to fossils . . . and he leaves untouched the far more basic problem: why should any animal *become* carnivorous when surrounded by an abundance of vegetable food which requires *no* catching?

Throughout the book, superficial resemblances are stressed and real difficulties are evaded. When trying to persuade us that fish turned into amphibians, RD talks about different kinds of lungs but says not a word about the revolution in *skeletal* design that was necessary to enable a fish to walk on four feet—a revolution for which, of course, there is not one scrap of evidence, living or dead. Likewise the transformation of an amphibian egg into a amniotic (reptilian) egg—involving eight separate 'improvements'—is nowhere discussed. Again, he asks, how did wings get their start? Then goes on to wing *flaps*, as if they could automatically be turned into proper wings with

feathers! The origin of feathers is not even mentioned. Snake venom gets the same treatment. "There is a continuous graded series from ordinary spit to deadly venom." (p. 90) So . . . 'Once upon a time the cobra was a beautiful harmless creature . . .—or was it? RD makes no attempt to explain what *advantage* accrued to the poisonous snake by reason of its poison, when it is obvious to all the world that non-poisonous ones have survived and multiplied just as well as their dangerous 'cousins.' In fact 92 percent of all snakes are non-poisonous. The blind watchmaker that produced the other eight percent must have been not only blind but barmy. (The creationist, of course, has quite different and satisfactory reasons for the existence of venom in some snakes.)

Much space is devoted to 'convergence,' a word invented to disguise the fact that evolution cannot account for similarities in unrelated creatures. Flight technology and sonar are supposed to have been 'discovered' quite independently by several kinds of animal. But if these extraordinary powers were acquired independently, why should not the corti and every other piece of specialist equipment have been invented 10, 20 or 100 times? Wriggle as he may, RD cannot prevent us from seeing that sonar is explicable only as God's gift to bird, bat and dolphin for their particular needs in different environments. And if the common possession of extra-ordinary faculties does not point to a 'common ancestry,' why should the possession of 'ordinary' faculties prove it? Why should not the creator have given to all vertebrates *independently* that very useful organ, a backbone? When he comes to DNA, RD claims that human genes' 99 percent similarity to chimpanzees must prove our close cousinship and common ancestry. He overlooks the fact that (a) this may well be another case of 'convergence,' and (b) there is a wide variation of DNA among frogs, which "proves" that we are nearer to chimps than frogs are to frogs!

RD boldly repeats Darwin's challenge: "If it could be demonstrated that any complex organ existed which could not possibly have been formed by numerous successive slight modifications, my theory would absolutely break down . . ." (p. 91) but overlooks the fact that instincts, though invisible, are just as really part of an animal as an organ and need explanation no less. Hence, he leaves bird-migration alone. It is easy to say that a short flight would be better than none and a bird *might* be naturally selected by being able to fly one mile further than his fellow-bird. It is absolutely impossible to say that an experimental flight of 10 miles from Alaska towards Hawaii would be better than none for an American golden plover. The bird would certainly drown—unless it had the capacity to cover the 3000 miles non-stop, with correct navigation, the very *first time it tried*. When the penalty of failure is death, all theories of trial and error can be immediately discarded.

Coming to sexual selection, RD fills 20 pages of print to "prove" the reasonableness of the black widow bird having evolved an 18-inch tail. This is easier, of course, than explaining the peacock or the Argus pheasant, whose tails are much more conspicuous and an encumbrance to flight. Even so, it cannot be said that RD has succeeded, because the anomalies of the bird-

world are legion and (it seems) wholly arbitrary. Tailless birds find mates just as easily as black widows; crows reproduce no less plentifully than peacocks; birds that nest in tree-tops have no more progeny than groundnesters. There seems to be absolutely no correlation between beauty, brains and babies. Why are ducks and drakes different, but male and female swans indistinguishable? To these and a thousand other questions there is no answer except, God made them like that! RD's abstract reasoning avails nothing against (e.g.) the field-evidence adduced by Norman Macbeth (*Darwin Retried*, 1973), who concludes: "Have the birds, through their patterns of sexual choice, established a system in which the race is not to the swift and the battle is not to the strong? If so, they have shaken the whole structure of Darwinism." (p. 85)

The next chapter is Puncturing Punctuationism and it will be interesting to hear Stephen J. Gould's reaction to RD's thesis—that there is no real difference between the two schools of thought: in fact, punctuationists are gradualists in disguise! This is a familiar ploy in politics. If your party feels threatened, form a coalition against the *enemy*. In this case the enemy are creationists, and at all costs they must not be allowed to overthrow the Establishment. To an impartial observer it seems obvious that the gradualists are right in pointing out that nature knows nothing of 'hopeful monsters,' and the punctuationists are right in retorting that the fossil record knows nothing of gradual evolution. Each group can clearly see the beam in the other's eye, and each group is blind to its own.

The climax of RD's fallacious thinking is reached in these words: "The genetic code is universal. I regard this as near-conclusive proof that all organisms are descended from a common ancestor." (p. 270) So—what kind of world would he *expect* to find if a Creator had populated it with a variety of creatures who all need to breathe the same air, enjoy the same sunlight, drink the same water and eat (somewhat) similar kinds of food? Must a horse have six legs to prove it is no relation of the cow? (In fact, of course, evolutionists cannot even account for the different digestive systems of these animals.) Ought we to expect apples to use one kind of photosynthesis, and oranges another? But, to quote Michael Denton, (*Evolution: A Theory in Crisis*)

. . . the new molecular approach to biological relationships could potentially have provided very strong, if not irrefutable, evidence supporting evolutionary claims . . . all that was necessary to demonstrate an evolutionary relationship was to examine the proteins and show that the sequences could be arranged into an evolutionary series. (p. 277)

In fact, ". . . the molecules reaffirm the traditional view that the system of nature conforms fundamentally to a highly ordered hierarchic scheme from which all direct evidence for evolution is *emphatically absent*." (p. 278) (emphasis added) So, what RD regards as the strongest evidence *for* evolution is considered by a specialist in this field to be decisive evidence *against* it.

RD's crowning unwisdom is to out-Darwin Darwin in atheism. Whereas Charles postulated ". . . life breathed into a few forms . . . or one" (*Origin of Species*) by the Creator, RD will not allow the Creator

even one finger in the pie. To set the ball rolling 'in the beginning' required no one and no-thing, only the laws of physics. One is reminded of Francis Bacon '... a little philosophy inclineth man's mind to atheism; but depth in philosophy bringeth men's minds about to religion.' (*Essay X, VI*) The whole book illustrates the fact that Darwinism is a folk-religion, an object of genuine piety (loyalty to the Guru), and passionate devotion, on the part of its disciples. RD is unable to show any hard evidence for the evolution of the eye, the ear, echolocation, long tails, or indeed of life itself; but unshakeable faith in the great god natural selection lends wings to his imagination and carries him soaring into Cloud-Cuckoo-Land. Notionally he peoples the world with intermediates between humans and chimps and seriously suggests that we have been spared many headaches by not having to discriminate (in law) between various levels of ape-men. "It is just sheer luck that these embarrassing intermediates happen not to have survived." (p. 263) No evidence is offered that any of them even existed: it is simply *assumed* that Lucy & Co. were a thriving business. Unsurprisingly no mention is made of Piltdown Man, Nebraska Man, Java Man and all the other failures.

One great virtue of this book is to demonstrate the incompatibility of Darwinism and theism. RD points out the inconsistency of those sophisticated theologians who wish to appear in step with modern science, while at the same time retaining a corner for God in their cosmogony. But the great weakness of *The Blind Watchmaker* is its total failure to come to grips with creationism and deal seriously with the objections to evolution. "It would obviously be unfairly easy to demolish some particular version of this theory (that life was created)..." (p. 316)—thus he excuses himself from entering the tournament, because, forsooth, no foe is worthy of his steel! Instead, he uses ridicule: "... the Genesis story has no more special status than the belief of a West African tribe that the world was created from the excrement of ants." (p. 316) So—from Olympian heights our professional zoologist dismisses in a sentence those magnificent chapters which men of real genius like Bacon, Newton, Boyle, and Galileo were content to receive as divine revelation, which Christ himself quoted as literally and historically true, and which the finest scholars of Europe, masters of ancient lore, have recognized as superior beyond comparison to all rival cosmogonies. In a last desperate attempt to win support for his party, RD besmirches the motives of his opponents. They are all either cranks (Bible-thumpers), snobs (who do not relish kinship with monkeys) or journalists (making a quick buck by attacking the Establishment). The idea that some anti-evolutionists may actually be interested in *truth*, never seems to have crossed his mind. So writers like Macbeth, Denton and Geoffrey Harper, who are neither Bible-thumpers nor snobs nor journalists—yet disbelievers in Darwinism—are kept carefully locked in the cupboard.

RD is very concerned that his name should go down in history and I believe it will... much the same as that of Titus Lucretius Carus, who flourished 2000 years ago. Lucretius was a Roman philosopher, a fervent believer in the atomic theory and disbeliever in the traditional gods. A keen observer of nature and a fine

poet, he is still read with pleasure today even though his science is hopelessly out of date. Similarly RD is an elegant writer—humorous, satirical, sparkling with felicitous comparisons. It is just an enormous pity that these golden gifts should be wasted in defence of the indefensible, in keeping the life-support system going on a body of pseudo-scientific ideas which must soon join Ptolemaic astronomy and phlogiston in the Mausoleum for Extinct Hypotheses. As Fred Hoyle has written, William Paley is still in the tournament, with a chance, a very *good* chance, of being the ultimate winner.

The Creation/Evolution Bibliography and Directory, Third Edition by Ernie Lazar, 1324 G Street, #4, Sacramento, CA 95814-1543. 338 Pages. \$44.00.

Reviewed by Emmett L. Williams*

The author claims what this reviewer has often said:

... there is a serious problem evident with respect to obtaining access to creationist literature. My personal opinion is that creationists are correct when they assert that bias exists in our public libraries (Table of Contents page).

Mr. Lazar hopes that his bibliography (over 4400 citations) will help those who are interested in the creation/evolution controversy and will be the starting point of a truly definitive work. If public and university libraries would obtain a copy of this bibliography/directory, it would open new vistas to their patrons as to some of the historical, sociological and scientific literature available on the subject.

The author makes no claim as to absolute inclusiveness of all books, theses and journals on the subject in his directory. For instance he samples only a few *Creation Research Society Quarterly* for content. However an interested scholar could learn of the *Quarterly* and get some idea of the articles featured for certain years.

Included in the bibliography/directory are lists of

1. Doctoral and masters' theses (creationist and anti-creationist)
2. Books and monographs (creationists and noncreationists)
3. Periodical and journal articles (creationist and non-creationist)
4. Newspaper articles
5. Court cases and legal opinions
6. Debates, panels, conferences and symposia
7. Audiovisual material
8. Chronology of creation/evolution controversy (1921-1986)
9. Directory of creationist organizations and publications

Some errors are evident in the work, i.e., the Creation Research Society is given two different addresses (p. 5 and p. 18 in the *Directory of Creationist Organizations/Publications*). However this is an excellent index and could be of great value to students of the creation/evolution controversy.

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The Bone Peddlers by William R. Fix, 1984. Macmillan. New York. 337 pages. \$18.95.
Reviewed by Clifford L. Lillo*

Reading *The Bone Peddlers* is a little like listening to a radio broadcast of a football game announced by someone whose real interest is baseball. The author, William Fix, tells everything wrong with evolution theory, everything wrong with creation theory and everything right with parapsychology.

To the creationist, the book is of value for two reasons. It serves as a review of evolution fallacies, helpful in any discussion of the theory of evolution with acquaintances. Secondly, it brings to light a belief system not often discussed in creationist literature but one with which we should be intimately familiar.

In commenting on the tendency of paleontologists who find bones of animals near ashes or hearth sites in strata and who then leap to the conclusion that the animal made the fire, the author says,

The fossils around an ancient hearth may be merely the remains of a victim, even a dinner, of the man that made the fire—a man who was under no obligation to deposit his skeleton in the same place (p. 7).

Regarding the accuracy of the fluorine dating technique, Fix says, "In one famous case, an expert dated part of the Piltdown skull at roughly 50,000 years old, and then a few years later the same expert concluded it was only 500 years old" (p. 8). The potassium-argon method also receives the author's scorn: "In a recent important case, one laboratory dated an ash layer at 1.8 million years, while another dated the same layer at as much as 2.9 million years" (p. 9).

Fix calls those who would parade their bone collections before the world "bone peddlers." He criticized Stephen Jay Gould for claiming that evolution is a fact and saying that people evolved from ape ancestors when there is no proof. Fix says that Carl Sagan "invokes accidents [as the fundamental factor in the emergence of life forms] the way others invoke God" (p. xxiii) brackets added. Others scored by Fix include Charles Dawson and Teilhard de Chardin for their Piltdown man hoax, Franz Weidenreich for his claims about Peking man and Giganto-pithecus, and many other "scientists."

After ridiculing other "bone peddlers" including Raymond Dart, Robert Broom, Louis and Mary Leakey, Richard Leakey and Donald C. Johanson, Fix says, "To say that human evolution is impossible would be just as absurd as to claim it is a fact" (p. 152). He then goes on to show that many scientists privately admit that Darwin was wrong and adds,

If the leading biologists do not believe that Darwin found the answer to the origin of species, what do they believe? Well, they certainly still believe in evolution; that much is beyond doubt. Despite the numerous difficulties and contradictions presented by the fossil record, most scientists simply see no reasonable alternative to evolution (p. 182).

Does he consider that creationism is viable? Evidently not, for he says, ". . . creationism not only lacks scientific credentials but is objectionable on other grounds . . ." (p. 182). An insight into his opinion of certain scientists is shown by his statement, "So, when it comes to the fossil record, or any other body of evidence, . . . they [leading biologists] accentuate the supportive data and ignore or minimize as far as possible the contrary indications" (p. 182). Brackets added. He further adds, "They [the same leading biologists] also derive no little succor from other concepts and areas of inquiry that opened up even as the Darwinian explanations were collapsing" (p. 182). Brackets added.

Fix looks realistically at the mathematical difficulty faced by evolutionists when he states:

. . . there is an enormous probability problem at the core of Darwinist and new-Darwinist theory, which has been cited by hundreds of scientists and professionals. Engineers, physicists, astronomers, and biologists who have looked without prejudice at the notion of such variations [either mutations or gene flow] producing ever more complex organisms have come to the same conclusion: The evolutionists are assuming the impossible (p. 196). Brackets added.

Even though he has presented many evolutionary problems, this does not mean that Fix is anything but an evolutionist. However, he does acknowledge that evolution is a religion when he says, "In a detached, wider perspective it is almost impossible not to conclude that what we are witnessing is a conflict between competing systems of religious belief." (p. 208). Fix then amplifies on this by quoting several authorities, including Gertrude Himmelfarb, "Darwinism had emerged as an agreeable religious myth," (p. 211) and Edwin G. Conklin, "The concept of organic evolution is very highly prized by biologists, for many of whom it is an object of genuinely religious devotion . . ." (p. 211).

Where, then, should scientists turn for a correct system of beliefs? Fix supplies the answer:

Many serious investigators have now moved to a view of nature that brings them nearly full circle . . .

It's about time. Across all history, from the earliest pyramid texts and Hebrew prophets down to stories in contemporary magazines and newspapers, human experience is replete with accounts of paranormal and psychic experiences: telepathy, clairvoyance, and precognition via voices, visions, and dreams (p. 253).

But is this view truly scientific? Fix says,

The beginnings of the attempt scientifically to document and study psychic or parapsychological events in modern times may be traced to the founding of the Society for Psychical Research in London in the latter half of the nineteenth century. A counterpart was later formed in the United States, called the American Society for Psychical Research (p. 254).

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The author then describes several examples of “out-of-body” or EOB experiences documented in scientific literature, and adds,

If we widen our view and include clairvoyance, telepathy, and similar experiences in which human consciousness has gathered veridical information unimpeded by distance and matter, one can find hundreds or thousands of examples in the literature. . . it is only fairly recently that such experiences have been monitored under highly controlled scientific conditions (p. 259).

Lest his readers think this is not “scientific,” Fix says,

Perhaps the most noteworthy work of this kind has been described by Russell Targ and Harold Puthoff . . . [They] are not parapsychologists but physicists, senior researchers at Stanford Research Institute with experience in ultra-high vacuum technology, optics, lasers, plasma, and quantum physics (p. 259). Brackets added.

What tremendous scientific feat did these men perform? According to Fix,

Under the most controlled conditions imaginable, Targ and Puthoff repeatedly demonstrated that extrasensory perception and remote viewing in particular are undeniable realities (p. 262).

With all of the above introducing his subject, Fix is ready to reveal the real purpose of his book:

We see, then, that the noosphere is getting hotter all the time . . .

. . . Some people can “fly” thousands of miles and are able to target their flight to a specific destination (like Mrs. Wilmot [who “flew” out to a ship in the Atlantic to visit her husband while he lay on his bunk]); others (like Miss Z [who merely floated near the ceiling of her bedroom]) seem unaware of these possibilities and remain within a

few feet of the physical body (p. 263). Brackets added.

Since Fix believes that these events actually occurred, he says,

So there is hard evidence, if not of the soul itself, then at least of the ethereal covering of the soul, the elusive second body described in both ancient and modern accounts of out-of-body experiences . . .

The progress of parapsychology has confirmed the beliefs of many people, but despite its relevance it has not entered the public debate between the creationists and the evolutionists (p. 265).

Fix seems to want to make some kind of peace with creationists by placing parapsychology on their side, as opposed to what he calls “literal fundamentalists” when he says,

Not all creationists are afflicted with this kind of ideological desperation [i.e., fear that Jesus’ miracles will be unmasked as nothing more than “documented psychic phenomena” such as is “happening in the world today”], but none that I know of plays the excellent cards dealt him by the parapsychologists and physicists. The findings of parapsychology tend to confirm rather than deny the reality of certain of Jesus’ feats (p. 265). Brackets added.

Do creationists want the kind of help that could be dealt to them by the parapsychologists? I think not. If the “ancient Hebrews” were not describing miraculous events performed by the Son of God, then we have no basis for our faith. Far from agreeing that modern day parapsychology confirms the reality of Jesus’ feats, we need to investigate the claims of authors like Fix so that we will be in a knowledgeable position to refute such an unholy alliance whenever and wherever it is proposed.

LETTERS TO THE EDITOR

Why You Should Support Creation Research

Currently the United States Government, in addition to the various states, other national governments, National Geographic and other “scientific” societies, etc. spend *billions* of dollars annually to fund research that is used to propagandize the youth of the world to accept materialism as their life philosophy.

Interestingly, it takes relatively little research to combat a false philosophy. Surveys indicate that half the people in the U.S. still believe in a created earth that is only a few thousand years old. However, it does take *some money* to support the “ox that treads out the grain.” Even oxen must eat.

The *Creation Research Society* (not affiliated with CSA) has over 2100 members and engages in basic and applied research related to origins. Since 1963 the Society has published a refereed quarterly scholarly journal; nearly 80 scientific research papers, almost 100

theoretical papers, over 200 scientific review papers and hundreds of more popular works. This work is extremely valuable to creation scientists, therefore it is valuable to the nation and to the world. CRS is now building a permanent research station at Grand Canyon. Three research projects are already underway there. Twenty more are underway in other parts of the nation.

If you would like to support these efforts contact us at CSA for more information or write:

CRS Laboratory Project
R.D.1, Box 101A
Clarks Summit, PA 18411

Used by permission, first appeared in CSA News (March 1986).

CSA News
Rt. #1, Box 247B
Cleveland, MO 64734

Should the Expanding View Of The Earth Include A Canopy?

Upon reading Howe's review (CRSQ 24:37-8) of *The Geology of the Flood* by Glenn R. Morton and having had the privilege of reading that book plus several of his articles, I would like to make three observations.

First of all, the work is very detailed with a wealth of data and a multiplicity of evidence that he has painstakingly gathered and analyzed to support his thesis. It is apparent that Morton has considered the subject very carefully and has put an enormous amount of work into what is clearly a controversial theory. Thus, Howe concludes, his view deserves an active consideration. And while I am not anxious to embrace the idea that the permittivity of free space has changed, it is appropriate to suspend judgment until further analyses can be performed.

Secondly, Morton has defended well the idea of earth expansion and has put forth a mechanism that would effect such an expansion. This model, with a different free space permittivity, ϵ_0 , is one that can be analyzed using the tools of mathematics and thus, make a more objective assessment of this theory possible. Since the speed of light is related to the permittivity (ϵ) and permeability (μ) of any medium by

$$v_c = 1/(\mu\epsilon)^{1/2},$$

and since the permittivity of a medium is the product of the relative permittivity and the permittivity of free space, $\epsilon = \epsilon_r \epsilon_0$, it is clear that a permittivity change would indicate a change in the speed of light. Modifications to the "big bang" theory have been proposed due to the fact that the speed of light limits the interaction between the central portion and outer portion of the "primeval expanding universe." This modification has been called "the inflation hypothesis." There is also the conflict between the width of the universe as extrapolated from "red shift" measurements (and the like) and young earth theories. So, although Morton's theory seems revolutionary, it should not be prejudged and it does stand the chance of reconciling contradictory evidence.

Thirdly and finally, a preliminary analysis indicates that abandoning the canopy theory in the face of a different pre-Flood permittivity is neither necessary nor adequate. This is due to the sweeping consequences of Morton's most basic premise, that the permittivity of free space, ϵ_0 , was lower before the Flood. This change results in a different world with modified laws of physics and biophysics and, perhaps, a different "life as we know it."

Before discussing the effects of the permittivity on Morton's analysis of the canopy theory, I point out a few assumptions which he makes that, in general, may result in an incorrect canopy model. Coincident with this critique, however, I admire the fact that he has taken the time to pick a model and put it to the quantitative tests of atmospheric physics.

Morton has made a fairly convincing case against a canopy that consists of vapor. Included in his model of a canopied atmosphere are the following assumptions:

1. The detailed variations of transmissivity with frequency are disregarded. This may have an effect on radiative heat transfer. For example, ozone causes an

absorption peak just below 10 microns wavelength (290 K) which falls off just below 9.5 microns (305 K). This tends to regulate the temperature below 305 K (89° F).

2. It is assumed that the atmosphere of Venus can be compared directly to that of a canopied Earth and since Venus is very hot, an Earth with a canopy would also be very hot. The planet Venus is 0.723 astronomical units from the sun and is almost the same size. Based upon the inverse square law, the solar irradiance of the Earth is 52.3 percent of that of Venus. The atmosphere of Venus is carbon dioxide. The surface pressure is nearly 100 times that of today's Earth or 40 times the pressure estimated for a canopy with 50 feet of precipitable water. All the above differences would lower the surface temperature for the Earth.

3. It is assumed that the heat of fusion from the rain would not escape. Since the canopy was not the only source of water for the Flood but the "fountains of the deep" were also opened up, it appears that only enough rain for a 40 day period is required for the canopy, say around 80 inches. There is an area of the earth today that has a "canopy" much of the time and it receives between 70 to 160 inches of rain per year. I am referring to the Amazon rain forest. Interpolating from Morton's Table 4-4 and correcting for the different span of time gives us an equivalent solar irradiance of 111 per cent for a rainfall of 115 inches (9.6 feet). The Sahara desert receives about 100 percent solar irradiance and radiates heat away to a dark sky (2.7 K) almost every night and is virtually unlivable. Life thrives in the Amazon. Water in the atmosphere absorbs a great deal of energy for a small rise in temperature and it is possible that Morton's model has not accounted for this.

To summarize, it is not clear to me that the canopy theory has been modeled adequately to warrant throwing it out at this point.

But notwithstanding the above discussion, let us assume that Morton's model of the canopy is accurate and that the permittivity of free space changed in the time of Noah precipitating the Flood. The model of the canopy must still be corrected for a pre-diluvial permittivity.

One law basic to the analysis of the canopy is the ideal gas law,

$$PV = nRT.$$

The ideal gas law relates the pressure, P, the volume, V, the absolute temperature, T, and the number of moles of a gas. As with many of the equations of physics, this expression contains a constant that is arrived at empirically, the gas constant, R. If the Earth's elements all expanded, then R has also changed. Its value before the Flood was lower because the volume was lower, since

$$R = PV/nT.$$

The pre-Flood gas constant can be related to the present gas constant by

$$R_{p-f} = R/m,$$

where m is a correction factor greater than one, given by the volume increase ration,

$$m = V/V_{p-f}.$$

So, for the pre-diluvial world, the ideal gas law can be expressed by

$$PV = nR_p T = nRT/m.$$

Therefore, to calculate the corrected pre-Flood temperatures simply divide the temperature in Morton's tables by the expansion ratio, m . Based on the expansions in Morton's Table 6-1 in his book, it is estimated that N_2 and O_2 would have expanded by a factor of about 2.5, at least. Using this expansion factor in Table 4.1, it is found that livable conditions would prevail for a canopy having 10 feet of precipitable water and an albedo of .8. These values seem reasonable for a water vapor canopy.

Remember that the boiling point of water, which is dependent largely upon electronic, hydrogen bonding would probably be higher before the Flood (permittivity change). Bonding among proteins would also probably be stronger. This could mean that life would have done well at higher ambient temperatures than is observed in the present world. These factors would allow a canopy with an even lower albedo and a larger amount of water.

Therefore, I conclude, from this preliminary analysis of the effects of an Earth-expanding permittivity change, that the existence of a canopy would be even more likely.

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CRS Conferences?

I fully agree with the editor (CRSQ 23:141) that the CRS should be and remain a publishing and research organization, and not attempt to sponsor an annual or biannual conference or speaking symposium. Much as I would like to see such a conference established, I do not think it wise for the CRS to sponsor it. Perhaps there are enough Bible-believing churches who would be willing to pool their resources and sponsor such a conference. A technical conference on creation would be a valuable and wonderful thing; my understanding of the purpose for which the CRS was founded did not include sponsoring conferences or speaking engagements. Is such a conference (technical, not popular!) a good idea? Yes. Is such an idea within the stated purposes the nature of the CRS, even if finances are available? No.

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Material Hypothesis and Medical Profession

The material hypothesis (MH) discussed by William P. Riemen (CRSQ 23:141-5) has two strikingly appropriate examples underway today in the medical profession, one having begun about a half-century ago, the other within the last few years.

It is questionable that any "scientific" endeavor that adheres completely to MH is actually scientific at all. Such a hypothesis typically starts with unwarranted initial assumptions that have never been proven: a conclusion is assumed, then evidence sought to prove

the conclusion. Real science is observation and experimentation with DISCOVERED physical principles and laws: evidence is analyzed to arrive at the indicated conclusion.

The assumption of MH has bogged down the medical profession (note: "profession," not "science") for half a century in the battle against cancer. After the expenditure of untold number of manhours and billions of dollars, the objective of a cure is no closer than at the start. The reason for this is that the real cause of cancer has been ignored. The cause is very simple in principle: cancer is caused by internal physical abuse.

This abuse takes two forms, the two being somewhat mutually interdependent: 1. excessive insertion of foreign substances into the body (tobacco, alcohol, additives in processed food) and 2. lack of proper nutrition to equip the body to fight its own battles (something it is designed to do when furnished proper nutrition). One now catches an occasional glimmer of the medical profession finally shifting to this conclusion, but only after 50 non-productive years (within a "cure" context).

The only cure for abuse is cessation of that abuse. The "cure" for a sore finger continually pounded with a hammer is cessation of pounding with the hammer. This is, of course, quite obvious in the physical realm. This same principle translated into the moral realm is not so obvious and is, therefore, subject to being lumped into the loony category, especially by the MH adherents. Nevertheless, the moral realm is just as real as the physical and abuse occurs in both realms.

The most startling result of abuse in the moral realm that history has ever witnessed is AIDS. And all the eggs of "cure" for this disease are now placed in the basket of medical research, whose record for "cures" of abuse is dismal indeed. Most unfortunately, the medical profession, ever obsessed with MH, is again off on a similar tangent as that initiated by cancer.

Since morality can hardly be more drastically abused than it is within homosexuality (this should not be news to the Christian), it is not surprising that AIDS in this country originated within homosexuality and is still largely confined therein. Since this disease can be transmitted by the transfer of body fluids via blood transfusions, injection needles, etc, the disease now occurs in heterosexuals.

What really boggles the mind is the fact of this disease occurring in heterosexuals only by contamination from homosexuality is completely ignored by everyone with responsibility for the public good. If this base of contamination were severely restricted and contained (as laws in many states stipulate), the disease within this base would eventually (pardon the morbid pun) literally die out. Were this to happen, then the relatively few cases appearing among heterosexuals would be easy to contain and control, setting the stage for elimination of the disease.

But as long as this base of contamination is allowed to remain intact with no chance of diminishing the base and neutralizing its spread of contamination, then all efforts, medical or otherwise, will remain peripheral in nature and results.

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Some Thoughts About Evolution (In Poetry)

I have trouble accepting the Theory of Evolution because
 It conflicts with my religious beliefs. I believe that God's laws
 Are the sole guiding force in the universe. The Bible tells us
 That God is the Creator, Sustainer, and Ultimate End plus
 Of all things. The wide variety of forms of life
 I interpret as a reflection of His love and inspiration rife.
 The Bible states the humans were created in the image of God.
 Thus they were elevated above all other life forms that trod
 The earth. It is difficult to reconcile these Bible teachings view
 With the concept that life on earth has evolved through
 natural processes, I feel that the evolutionary theory, too,
 Conflicts with the Bible's teachings regarding the reality true
 Of sin and of redemption from the guilt to sin. I believe
 That sin tends to become mere imperfection if people conceive
 This to be in the process of evolving from a lower state.
 Thus, the Gospel of redemption from the guilt of sin tends to berate
 And lose its meaning. Scientists, in the past, did concentrate
 On theories purposely to push God out of the creation straight
 Process. But, I am sure there are fence-straddlers who think by trait
 They can co-exist with these opposing views to equate.

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The Flood and Time Limits

I wish to briefly comment on some things written by Dr. Howe (CRSQ 24:9-12). He has slightly misunderstood my position. On p. 11, he wrote that in CRSQ 20:133-85 I had advocated a tectonic effect superimposed on ecological zonation. I do include ecological zonation as one of the viable explanations for stratigraphic separation, but as far as my new model is concerned, it was tectonic action acting upon *bio-geographic* provinces, not ecological zones. That is, in fact, why I called the tectonic units TABS, or Tectonically-Associated Biological Provinces. Note that the TAB concept explains many things that ecological zonation by itself cannot completely explain, such as why there are marine forms (of varied type) at different stratigraphic levels. Clearly, living things were passengers on subsiding tectonic blocks, which is why there is a cross-section of ecological types (marine, terrestrial, etc.) in nearly every geologic period.

He claimed that I had not discussed how the Creator may have induced tectonics during the Flood. No, that had not been my aim, but I did mention that my model has relevance regardless of whether the cause was direct divine miracle, providentially-timed naturalistic cause(s), or purely naturalistic. I also mentioned that geologists, be they diluvialists or uniformitarians, have difficulty accounting for orogeny in terms of deep-earth processes. The reason for this is plain: not that much is understood how the inner earth operates.

Another point concerns Morton. I think that Howe gives Morton and his positions too much credence—see Howe's review of Morton's book, (CRSQ 24:37-8). I agree that Morton has raised points that need to be answered. However, Morton's "evidences" that he "accommodates" via his absurdly bloated chronology are the same as those used by Davis Young and Daniel Wonderly, among other uniformitarians, to reject the Flood. Young, Wonderly and Morton are all alike: they all cite "evidences" for long time periods that are little

more than uncritically-accepted standard uniformitarian interpretations for earth strata. I think that I have given at least tentative refutations for Morton's old-earth claims (see my article in the *Proceedings of the First International Conference on Creationism*, Vol. 2, pp. 205-18). In Howe's book review he urges readers not to treat Morton in the same class as a theistic evolutionist or a day-ager. I disagree. Since Morton bloats the chronology so that the earth is 125,000 years old and the Flood occurs over 5000 years, he is in fact an old-earthier. *Dumping that much time into the Biblical chronology so completely distorts it that he might as well make it 4.5 billion years.* After all, 150 miles up is just as much outer space as 15,000 miles up. Likewise, 125,000 years is so far in excess of the traditional 6,000 years that it might as well be the uniformitarians' 4.5 billion years. Recall that, from a historical standpoint, the 6,000 years first gave way to about 75,000 years. By then, Biblical chronology had so lost its credibility that it was no difficult feat to keep piling on the years until it went into millions and eventually billions of years.

Elsewhere, Morton has insisted that expanding the chronology from 6,000 to 125,000 years is essentially no different from expanding 6,000 years to 10,000 years. How ridiculous. A chronology whose time is enlarged by 60 percent is still a viable chronology, but surely a 20-fold increase in time makes the chronology a mockery. Use an everyday example: 15 minutes is only slightly different from 10 minutes, but expanding 10 minutes to 3.5 hours surely makes a mockery of time.

Howe's comparison of Morton's views with those of Northrup and Austin is somewhat invalid. Austin is talking only of Tertiary (primarily Miocene/Pliocene) and Northrup of Mesozoic/Cenozoic as being post-Flood, *but both still keep an earth under about 10,000 years.* I also am willing to allow parts of the upper column to be postFlood in specific regions, but only where *absolutely demanded* by very strong evidence. I believe in first refuting all the alleged evidences for a long time in the sedimentary record and only then allowing for more time. In other words, I believe that Northrup is too quick to invoke additional postFlood catastrophes and Morton much too quick to invoke vast spans of time. *Let us stick with the basic Price/Morris one-year Flood* and then only allow for deviations in terms of additional time when and if the year proves unworkable despite prolonged and careful research.

John Woodmorappe

Creationist Insect Errors

I have read the recent paper "Insects Indicate Creation" (CRSQ 22:166-70). As an evolutionist who has paid close attention to the efforts of CRS I was not surprised by the collection of unsupported assertions: "The origin of such exciting morphological and behavioral adaptations would be difficult to explain in terms of mutation and natural selection" (p. 169); debater's tricks such as using the words "suddenly arose" in the same sentence with "slow processes" (p. 167) to caricature evolutionary thought; and an outdated bibliography. I enclose the reference list from a recent article to help bring you up to date (Weygoldt, 1986).

Such are the usual techniques of scientific creationists. Why, then, have I sent this letter? I have done so because I am an entomologist and I am disturbed to see the audience, for which you write, given grossly inaccurate statements about insect life. Which current worker believes that the collophore is an organ of adhesion? I count 112 families of beetles in Borror *et al.*, 1976. Do you still claim there are 23? Why did you use “the dragonfly,” a hemimetabolous insect, to illustrate metamorphosis? Careful, Mr. Sanders! These are mistakes so glaring that they may be spotted by the untrained. Certainly no person making them can properly call himself an entomologist. In the interest of honest scholarship please correct these mistakes in future publications.

Reference

Weygoldt, P. 1986. Arthropod interrelationships—the phylogenetic approach. *Zeitschrift für Zoologische Systematik und Evolutionsforschung*. 24:19-35.

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A Couple of Insect Errors But Ample Evidence for Creation: A Reply to Wells

We appreciate the reading and critical attention that J. D. Wells has given to our paper—Sanders and Howe (1986). We often think that too many non-creationists simply ignore the papers in CRSQ—reading only those journals that reflect viewpoints which coincide with their own preconceived notions.

We are likewise grateful to Wells for pointing out our mistake regarding the published number of beetle families. His count of 112 is closer to the present number of 123 by various authors and we acknowledge our typographical error. He was also correct when he chided us for a bibliography that did not include enough current references.

We quoted Borror and DeLong, however, regarding the amazing ability of the spider wasp to construct a cell, capture a spider, paralyze it, drag the spider to the cell, and finally oviposit in the spider's paralyzed body, which in turn serves as a nursery for their young. Then we shared E. N. Smith's idea that “. . . the origin of such a delicate interaction would be hard to understand in terms of evolution”—Sanders and Howe (1986, p. 167). Wells thought this was “an unsupported assertion.” We disagree and hereby invite Mr. Wells to reply by giving CRSQ readers a clear, step-by-step description of how these amazing activities of the spider wasp might be expected to arise by natural selection acting on gene mutations, most of which are harmful. These dragonfly behavior patterns and thousands of others *are truly* “. . . hard to understand in terms of evolution” (p. 167).

We wrote about the dilemma which faces evolutionists when they try to explain the origin of the basket-like apparatus by which dragonflies are able to catch their insect prey while flying—Sanders and Howe (1986, p. 167). Here Wells accused us of using “debater's tricks” when in fact we were simply show-

ing that some “pressing” or “sudden” requirement for such a structure would need to arise or else the ancestors of the dragonflies would have gone on feeding just as they had done previously. But many evolutionists and one of us—Howe and Davis (1971)—have established that the workings of natural selection upon gene mutations are woefully slow, so slow that the evolving dragonfly ancestors might well have starved before the result would have been achieved by evolution.

Wells also asked “Which current worker believes that the collophore is an organ of adhesion?” Here he was referring to our remarks (1986) about the springtails in which we wrote that:

Members of this supposedly ancient and primitive springtail group also have a ventral tube which serves as a sucker to hold them onto a leaf or some other object. Hutchins even describes the water springtail as using its ventral tube to penetrate the surface film on water, thus anchoring the insect near the surface (p. 168).

Before relating which current workers still hold to this idea that the collophore can and does serve as an organ of attachment, let us begin by discussing the little structure more fully. Maynard (1951) described it as follows:

This structure, which originates as a pair of embryonic appendages, is bilobed. From its apex a pair of sacs or filaments may be exerted. Blood pressure is said to be responsible for the extrusion of these vesicles; their retraction into the tube is by muscular action (p. 7).

See Figure 1 for a sketch of a typical springtail (Collembola) with the collophore (also called ventral tube) labeled. The word “collophore” derives from the root words *colla* (meaning “glue”) and *embolon* (“bar”) so that the collophore is a “glue bar” in the root languages—so named because it was and still is believed to serve such adhesive function.

Perhaps Wells got his idea that all current workers—except Sanders and Howe—had shifted their opinions about collophore function from Borror, DeLong, and Triplehorn (1976) who wrote that:

It was originally believed that the collophore in some way enabled the insect to cling to the surface on which it walked (hence the order name), but it is now believed that this structure may play a role in water uptake (p. 152).

These writers imply that all entomologists have changed their minds years ago about the function of the ventral tube—but have they?

As recently as 1984 Wootton wrote that:

On the underside of the springtail's abdomen is a ventral tube which is variously considered to be concerned with respiration, water absorption or to act as an adhesive organ enabling locomotion over slippery surfaces (p. 182).

In 1978 K. Christensen called the collophore of springtails “. . . a respiratory adhesive osmo-regulatory organ . . .” p. 51. Also in 1978 Atkins described the ventral tube as “. . . an adhesive device on the first abdominal

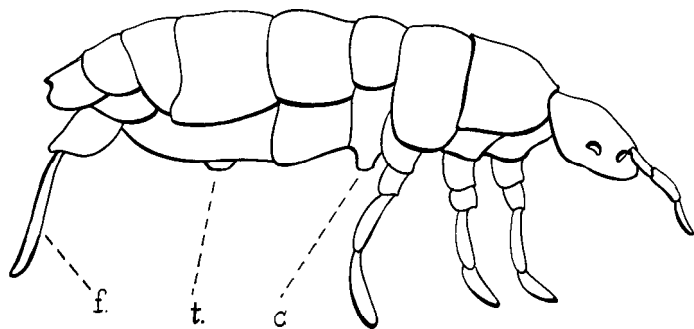


Figure 1. A typical springtail insect, order Collembola, showing collophore (c), furcula (f), and tenaculum (t). There is some disagreement at present about the function of the collophore (also called the ventral tube). Evidently it has been shown to serve in water-uptake but some workers still maintain that it also helps the insect adhere to surfaces. The furcula, held in place at the tenaculum, helps the springtails leap several centimeters when it is abruptly released. Devices like these are strong support for a rapid and distinct creation.

Drawn by Ross Marshall from Lanham (1964)

segment . . ." p. 455. Fomoser (1973) affirmed that its function ". . . is not clear . . ." p. 60. Scott (1968) wrote about the collophore that: "Although its purpose is not fully understood, this tube is presumably for adhesion or for some respiratory function" p. 74. Putting things a bit more firmly in 1966, R. E. Hutchins wrote that:

A pair of adhesive sacs are present at the end of the collophore and these can be everted and used to anchor the insect to any smooth surface. In the case of the water springtail, the collophore is 'water-loving' or hydrophilic and penetrates the surface film, anchoring the insect in place (p. 107).

Fox (1964, p. 340), Lanham (1964, p. 136) and Little (1963, p. 76) each list adhesion to surfaces as one of the functions of the springtail ventral tube.

One of us (Howe) traced the words "collophore" and "Collembola" from 1972 to 1987 in *Biological Abstracts*. There was only one title reference to the collophore and it did not relate to function. There were many references to the springtails (Collembola) but none of these titles contained mention of the function of the ventral tube. Based on the survey of current and older literature given here, it appears we were correct when we stated that springtails use their collophores also for adhesion to surfaces. Collophore physiology might be a choice area for creation researchers to study as it seems very little has been published in this regard.

Wells scored us for having used the dragonfly ". . . to illustrate metamorphosis." Actually we did use the word "larvae" in reference to the young of the dragonfly when we were pointing out that the young feeds with an armed lip whereas the adult dragonfly feeds by means of a basket-like trap it makes with its peculiarly bristled legs. But we did not talk about metamorphosis, as such, of dragonflies. But even if we had, the term metamorphosis was used by P. Aguesse (1986) and P. Corbet (1968) in reference to dragonflies to designate their conversion from aquatic larvae to aerial adults. Also E. C. G. Pinhey (1951 and 1981) refers to the development of dragonflies as ". . . almost always hemimetabolous" which means he considered

them to undergo metamorphosis. He also referred to the young as "Larvae (nymphs)" which he said were "campodeiform."

We would like to suggest that Wells consider the strong arguments for a rapid and special creation of many distinct insect types—unrelated to each other by organic evolution. Going back to the dragonfly, consider the way it ". . . scoops its prey out of the air with the bristly front legs, which it holds under the thorax to form a basket . . ." Lanham (1964, p. 185)—see also Little (1972, p. 81). We maintain that unique morphological and behavioral features like these support the creation of insects.

The springtails previously mentioned have ". . . a pair of appendages forming a structure known as the catch or *tenaculum*, which holds the furcula when not in use" Little (1963, p. 75). See Figure 1 for sketch of the furcula and tenaculum. It is not "a piece of creationist misinformation" nor is it a "debater's trick" to suggest that the furcula and the tenaculum (organs which permit the tiny springtail to leap for several centimeters) are exactly the sort of evidence one would seek to support the belief that a Designer *directly* formed insect organs.

Evidently evidence for the evolutionary origin of the Collembola in particular and the insects in general is not complete. As Salmon put it (1964):

Within the present state of our knowledge of the Collembola it is still extremely difficult to be precise on evolutionary sequences. Indeed, in many cases it is also difficult to define accurately the evolutionary relationships that exist between apparently similar forms (p. 4).

Callahan (1972) has also pointed out that the fossil record is of little help in supporting the origin of the springtails or insects at large by macroevolution:

The earliest known fossil insect is considered to belong to the order Collembola, although this is not certain . . . There are at least five major theories on the ancestry of insects, with one or more leading evolutionists arguing for each (p. 72).

The profound morphological peculiarities of the furcula and tenaculum strongly support rapid creation of distinct insect types. We challenge Wells to demonstrate otherwise.

Meanwhile, rather than having given our audience grossly inaccurate statements about insect life (as Wells asserted), we were trying to show general readers and scientists whose specialty is in other areas that macroevolution is just as bankrupt for solid evidence in entomology as it is elsewhere. In a very stirring section on "The Religion of Science," the noted insect physiologist V. B. Wigglesworth (1976) gave some advice it would be well for evolutionists and creationists to use in their origins dialogues:

The moral is a simple one: to practice the maximum degree of toleration towards different opinions. We are adjured in the parable of the tares to 'let both grow together until the harvest'—but that, I am afraid, is a counsel of toleration that is rarely followed either in religion or in science (p. 208).

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Portland Cement Dripstone

Emmett L. Williams (1987) presented an example of a rapidly-formed (less than 50 years) dripstone on a concrete-mortared sandstone dam. He claims that the dripstone, derived from the concrete mortar, has a rate of growth that is comparable to cave stalactites. Williams thus feels that examples of rapidly-formed, concrete-derived dripstones can be added to the list of evidences for rapid cave formation. A basic understanding of the chemistry of concrete makes this conclusion very doubtful.

In the process of cement production the raw materials (usually a mixture of a carbonate and a silicate) are first mined, then crushed, and finally heated to temperatures approaching 1400 degrees. In this process the CaCO_3 in the carbonate is converted to Ca(OH)_2 and CO_2 . There is no CaCO_3 in the resultant cement unless the raw materials were improperly processed, and regulations make this very uncommon. There is thus no CaCO_3 in the final concrete product unless there is CaCO_3 in the aggregate which is added to the cement. Most natural limestone caves contain deposits of calcium carbonate, thus differing chemically from concretes.

Ca(OH)_2 which is found in concrete is on the order of 100 times more soluble in water than CaCO_3 . As a result, rates of dissolution of Ca(OH)_2 and CaCO_3 cannot be compared directly. Thus, as Moore (1961)

concluded, the rate of dripstone formation from concrete cannot be compared with rates of stalactite formation in natural limestone caves.

Williams, however, claims:

... this presents no problem for the creationist viewpoint of a recent Flood. If limestone precipitated and hardened in the Flood waters similarly to the hardening of mortar or Portland cement (Williams, House and Herdtklotz, 1981, pp. 207-8), would it contain considerable amounts of Ca(OH)_2 and be easily dissolved as the Flood receded? (pp. 18-19).

My answer to Williams' question is NO. The Ca(OH)_2 in artificial cements is present because of the artificial burning of CaCO_3 . What is the source of the Ca(OH)_2 in flood sediments? I am aware of no mechanism whereby large amounts of Ca(OH)_2 were produced and deposited in preflood times. Neither has a mechanism (e.g. burning) been proposed for the conversion of calcium carbonate (in preflood shells and sediments) to Ca(OH)_2 in the process of, or immediately following the flood. Carbonate present in water can be precipitated out of the water as Williams and others have shown (e.g. Williams, 1975; Williams, *et al.*, 1976 and 1981; Williams and Herdtklotz, 1977 and 1978), but almost exclusively as CaCO_3 . Finally no mechanism has been proposed for the production and deposition of Ca(OH)_2 during and immediately following the flood. Nowhere has Williams proposed a reasonable source for the Ca(OH)_2 which he feels was common in the post-flood sediments.

The only reason Williams gives for concluding Ca(OH)_2 may have been present in flood waters is the analogy between the hardening of laboratory limestone and the hardening of Portland cement (see Williams, *et al.*, 1981, p. 208). The analogy is extremely weak. The precipitation of CaCO_3 about an aggregate is simply the precipitation of a salt from a saturated solution. The hardening of Portland cement in the presence of water is a set of complex chemical reactions which hydrate calcium oxide compounds ($3\text{CaO} \cdot \text{SiO}_2$; beta $2\text{CaO} \cdot \text{SiO}_2$; $4\text{CaO} \cdot \text{Al}_2\text{O}_3 \cdot \text{Fe}_2\text{O}_3$; $3\text{CaO} \cdot \text{Al}_2\text{O}_3$ to Ca(OH)_2 , $3\text{CaO} \cdot \text{Al}_2\text{O}_3 \cdot 6\text{H}_2\text{O}$, calcium silicates, etc.) The "similarity" between the hardening of concrete and the precipitation of carbonate is at best illusory.

Unless Williams can produce a viable mechanism for the production of Ca(OH)_2 in flood sediments, rates of formation of concrete-derived dripstone cannot be considered comparable to rates of formation of limestone-derived cave formations without an appropriate time-scaling factor. Examples of rapidly-formed, concrete-derived dripstone should not be used as evidence for the rapid formation of stalactites and stalagmites in limestone caves.

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Kurt P. Wise

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Reply to Wise

Misleading Commentary vs. Actual Statements

Wise makes some misleading statements in the first paragraph of his letter. For instance in the second sentence he claims that I state "dripstone, derived from concrete mortar, has a growth rate that is comparable to cave stalactites." A careful reading of the note (Williams, 1987) indicates that the terms used for the formations were stalactite-like and cave-like. "Dripstone" was placed in quotes in Figures 2 and 3 for a reason. No statement as indicated by Wise can be found. Likewise a *careful, thorough* reading of the references of which I was coauthor and footnoted by Wise will reveal an entirely different message.

In our research work (Williams 1975; Williams, et al. 1976; Williams and Herdtklotz 1977; Williams and Herdtklotz 1978; and Williams, House and Herdtklotz 1981), we were very careful to claim that we produced *simulated* stalactites and stalagmites in the laboratory. Note the following conservative statement:

Thus neither creationists nor uniformitarianists should use laboratory or field information on dripstone to claim any age for them. However as creationists we can point to various conditions under which these structures grow rapidly. (Williams and Herdtklotz, 1978, p. 90)

Franke (1965) and Gardner (1935) were quoted extensively to reinforce these conclusions concerning cave dripstone. If the size of cave formations or their present rate of formation cannot in themselves be employed as an indication of age, then certainly laboratory-derived data and growth of CaCO_3 structures from cement also must be used with caution.

Remarks Concerning Portland Cement

Quoting another paper which has remarks about Portland cement (Williams and Herdtklotz, 1977, p. 197),

The authors liken the deposition of limestone to the setting of Portland cement. Both are based on the chemistry of calcium. One can imagine dead organisms particularly those with shells, and various forms of debris such as stones, as an aggregate upon which calcium compounds precipitated. In other cases a cementing-precipitation reaction out of water could possibly have occurred. Deposition of limestone may be a chemical rather than a geological problem (Johnson and Williamson, 1916).

We were pointing out that possibly limestone deposits could have hardened fairly rapidly similarly to the hardening of Portland cement. The creationist model needs the rapid formation of sedimentary material,

thus cementing reactions *in general* should be considered and researched.

Later in the paper it is stated (p. 198),

One kind of evidence offered for the quick growth of stalactites is that they are found under and around concrete structures. Moore (1962) claimed that such measurements are not valid since the Ca(OH)_2 in cements is more soluble in water than CaCO_3 and will dissolve more readily in rain water. This brings up another interesting aspect that should be investigated by creationists. Would calcium-rich cement precipitated from the Flood water contain Ca(OH)_2 which later combines with CO_2 to form CaCO_3 as it ages, just as Portland cement does?

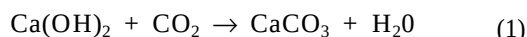
Then a warning was noted in a later paper (Williams and Herdtklotz, 1978, p. 90) "The growth of dripstone on Portland cement structures should be handled with care."

Calcium Hydroxide

I will quote Moore (1962) p. 99:

Calcium hydroxide is approximately 100 times more soluble in water than calcium carbonate, and a solution containing it may combine rapidly with carbon dioxide from the atmosphere to produce fast-growing stalactites.

Notice how careful Moore was with his statement to the certainty of the Wise assertions. (In my 1987 note I used the wrong Moore reference and this incorrect citation was referenced by Wise indicating he did no checking of original sources—a dangerous practice.) Both Moore and Wise miss a very important reaction that occurs on Portland cement and mortar surfaces. As soon as they are exposed to air the following reaction occurs:



Thus a layer of CaCO_3 exists on the surfaces of these materials that are exposed to air or water containing CO_2 . This carbonate may act similarly to a freshly-precipitated limestone and in turn can dissolve upon exposure to CO_2 -laden water, precipitating later as "dripstone." This was the implication of our previous statements concerning "dripstone" on cement structures. Creationists should point out examples of rapidly occurring processes such as the formation of "dripstone" from cement as long as they are employed carefully to show how such objects might form rapidly under the proper conditions.

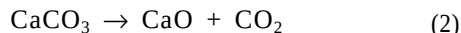
The solubility of Ca(OH)_2 in water has no meaning for the aforementioned process. The Ca(OH)_2 in the interior of the cement structure does not enter into the process or the entire cement body would be "spongy" from the removal of the compound and destroy the integrity of the mass. Obviously this does not happen.

Portland Cement Chemistry

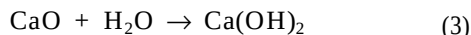
Wise then suggests that the author needs an understanding of the chemistry of Portland cement and he launches into a very elementary discussion of the kiln firing of the raw materials to be used in cement. Since my doctoral research was in cementing mechanisms, I am aware of much of the proposed chemistry of

cement although it is still not completely understood (Hull, Hass, and Franklin 1954, p. 831). Likewise the role of $\text{Ca}(\text{OH})_2$ in the product is still controversial (Lea 1971, p. 264).

Wise has demonstrated that he understands nothing about the aging reaction (1) which I mentioned previously and then goes on to show that he is misinformed about the firing process. He states that the raw material, CaCO_3 , is heated to form $\text{Ca}(\text{OH})_2$ and CO_2 . This is incorrect. The reaction desired in the firing operation is the formation of lime (CaO) as follows;



$\text{Ca}(\text{OH})_2$ is not found in the cement clinker immediately after burning (Lea, 1956, p. 34). $\text{Ca}(\text{OH})_2$ is formed when water is added to the cement by the reaction:



Also $\text{Ca}(\text{OH})_2$ can form during the grinding of cements (Lea, 1956, p. 34) by the action of water released from gypsum. I strongly suggest that those interested in the chemistry of Portland cement consult Lea 1956, 1971 or Bogue 1955 rather than Wise's incorrect comments.

Mortar

The discussion of Portland cement has nothing to do with the formation of mortar which was between the sandstone blocks discussed in my 1987 note. Mortar is prepared by mixing $\text{Ca}(\text{OH})_2$ with sand and water. It dries and sets on exposure to air by adsorbing CO_2 and forming CaCO_3 by reaction 1 (Nebergall, Schmidt and Holtzclaw. 1972, pp. 807-8). I repeat, CaCO_3 is present on the exposed surfaces of mortar and cement due to this aging reaction. The solution of $\text{Ca}(\text{OH})_2$ likely is not involved in the solution-deposition reaction for "dripstone" from mortar and cement.

Misinformation

Wise informs us that the precipitation of CaCO_3 about an aggregate is simply the precipitation of a salt from a saturated solution. No references are given for this broad statement. I urge readers to consult our original research work and the quoted references to realize the nonsense of that statement. Calcium carbonate precipitation can occur from solution because of pressure and temperature changes, pH differences and chemical composition differences in the solution water to mention a few. This entire problem of the precipitation of CaCO_3 from water would fill volumes considering the amount of work done on this seemingly simple process (to Wise) and it is still not completely understood. This was one of the major points of my 1987 note, to hopefully encourage research work, not get involved in an armchair, hot air contest.

Aging of Portland Cement

The compositional range of Portland cement before hydration is given by Lea (1956, p. 151) as:

	<u>Percent (by weight)</u>
CaO	60-67
SiO_2	17-25
Al_2O_3	3-8
Fe_2O_3	0.5-6.0
MgO	0.1-5.5
$\text{Na}_2\text{O} + \text{K}_2\text{O}$	0.5-1.3
SO_3	1-3

However after the cement is hydrated and set an aging process proceeds as mentioned earlier and noted by Lea (1965, p. 225):

The ultimate fate of any cement product stored in air is to become converted to calcium carbonate and hydrated silica, alumina, and ferric oxide, since all cement compounds are decomposed by carbon dioxide.

Assume for the sake of discussion a cement composition as follows:

	<u>Percent (by weight)</u>
CaO	67
SiO_2	25
Al_2O_3	2
Fe_2O_3	2
MgO	4

Hydrate this composition and allow it to completely age in the presence of CO_2 (This would not happen throughout a structure since CO_2 cannot penetrate into the interior.), the final composition of the cement (excluding water content) would be:

	<u>Percent (by weight)</u>
CaCO_3	76.2
SiO_2	15.9
Al_2O_3	1.3
Fe_2O_3	1.3
MgCO_3	5.3

Compare this "final, aged" composition of the hypothetical Portland cement to some carbonate rocks (natural dolomites and limestones) (Edmondson, 1958, pp. 114-24):

	<u>Composition Range (percent)</u>
CaCO_3	50.54-98.66
SiO_2	0.07-20.24
Al_2O_3	0.02- 2.66
Fe_2O_3	0.04- 1.32
MgCO_3	0.40-43.91

Do not these interesting circumstances suggest some research possibilities? The cementing compounds that concern Wise are metastable and will decompose in the proper environment. This is no illusion as he suggests.

Mechanism for Formation of Calcium Hydroxide

Before any mechanism for the formation of $\text{Ca}(\text{OH})_2$ in the Flood or postFlood waters is suggested, several questions need to be answered based on a creationist, not a uniformitarian model.

1. Were there carbonate rocks present in the crust of the Earth before the Flood?
2. Was there any concentration of lime in the crust of the Earth before the Flood?
3. What was the pH, temperature and chemical composition of the great Fountains of the deep when they burst forth during the Flood?
4. Were the Flood waters heterogeneous or homogeneous? Smith and Hagberg (1984) suggest the waters were heterogeneous for biological reasons.
5. What was the extent of volcanic activity during the Flood?

Any so-called mechanism of $\text{Ca}(\text{OH})_2$ formation or the presence of the compound will depend greatly on the answers to these questions. The mixing of water containing $\text{Ca}(\text{OH})_2$ (high pH) with water containing large amounts of CO_2 (lower pH) will result in the formation of CaCO_3 .

Several experiments come to mind and these will have to be pursued to suggest other possible mechanisms since considerable literature is available on the alkalinity of waters and the CaCO_3 precipitation phenomena. Again this was the point of the note (Williams, 1987). The creationist model needs development in these areas and Wise's interest in the subject is appreciated but a "quick draw" NO with associated arrogant remarks and inaccurate statements are not the answer. Uniformitarian scientists often offer imaginative solutions to their dilemmas. Likewise creationists can carefully utilize known rapidly occurring phenomena into their model if based on an experimental and observational level. Laboratory and field work combined with some creative thinking is what is needed now, not warmed-over uniformitarianism.

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Emmett L. Williams

The Confusion Between Evolution and Development

"The system has evolved . . ."

"The evolution of computers . . ."

"The evolution of Man . . ."

These kinds of statements are rampant throughout the literature of scientific and technical publications. Is it truly valid to use the term "evolution" when applied to a system which has undergone change?

Change itself happens to every system. As the law of increasing entropy is not to be defeated, the system is destined for final burnout. That system may be the Sun or a man-made system such as a computer network. Left to itself, everything comes to a halt. Only by the introduction of intelligence and energy into the system will it be able to ward off the doom of the second law of thermodynamics.

So what is it that justifies using the term "evolution"? The popular view of the word conjures up images of slimy creatures crawling up upon the newly formed land masses of the earth leaving behind a watery history of millions and millions of years. Yet we know the term is used for almost every conceivable situation where a supposed improvement of a thing has occurred. Is then the alleged improvement of something a reason for supposing that it evolved?

Humpty Dumpty, in the classic, *Through the Looking Glass*, by Lewis Carroll, said that he could make a word mean whatever he wanted that word to mean. Today, any word which is ambiguous in meaning, or tautologous is said to be a Humpty Dumpty word. "Evolution" has come to be such a word. The biologist uses it to describe a defined process of origins, whereas an engineer uses it to describe a system which has been improved over the years. At the same time, the sociologist uses it to refer to the way society has changed over the years, good or bad.

The reason for the overuse of the word has been because the original use of the word was ill-defined. The hypothesis of evolution indicates that chance alone is responsible for the increasing structure and increasing information content of a creature. It indicates to us that living organisms are getting better and better, more and more complex. If something is not better, then the chance process of evolution will weed it out through something called natural selection. Thus only the most fit things will survive. "Better" in the context of biological evolution could be redefined as "adapting to the environment" or in the case of man, "subduing the environment."

If chance is the qualifier of evolution, then there is certainly something wrong with our use of the word applied to things not related to chance. Take for example, the aforementioned phrase, "The system has evolved . . .". Let us be specific and say that the system is a computer, made up of various subsystems, including the memory board, the central processing unit, mass storage devices, input/output devices, etc. If this system were left exposed to the natural elements, I will

give you 10 to 1 that it will not evolve into something better, or even stay working. Thus to use the term "evolution" to this computer "system" is incorrect.

Obviously, the only way this computer is going to be improved is the introduction of intelligence exterior to the boundaries of the computer system itself. This intelligence will, of course, have to direct some energy transfer mechanism to actualize any improvements, but it is important to point out that energy alone is not enough! Throwing the computer into a blast furnace will certainly subject it to ample energy, but no one is going to argue that this will improve the product. The energy must be directed, and the direction of this energy can only be available as a result of an *a priori* intelligence, i.e., the computer designer.

The use of the term evolution has been inappropriate in much of the technical literature. The word "development" would be much more appropriate, for this word has behind it the connotation of intelligence behind the product. Let us hope that the improvements of our complicated electronic systems are not left to chance!

Cam de Pierre
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Fountain Valley, CA 92708

Thomas G. Barnes Institute of Physics

Thank you for announcing the formation of the Thomas G. Barnes Institute of Physics. As all of your readers know, he has proposed a new theory of physics which explains many of the phenomena that have defied science in the past, such as gravity, inertia, the light bearing medium as well as a new model of the atom. The Institute was formed to carry on Dr. Barnes' work and to publicize it.

The avenues of research that have been opened up by this new theory are almost limitless. A few of the most obvious and which the Institute is planning to pursue are as follows:

Inertial Velocity.

The Sagnac experiment and the laser gyro bear out the fact that motion can be measured with respect to the medium. Some have tried to explain this in terms of relativity but the arguments seem very weak. The Institute is planning to conduct an experiment to measure the linear velocity. This is an improvement over similar experiments that have been done by E. W. Silvertooth of Olga, Washington and by Stefan Marinov of Graz, Austria. They have both found that the earth is moving through the medium at a speed in excess of 300 km/s. If they are correct, it will verify the basic assumption that there is a medium.

Permittivity and Permeability.

There is a very meager understanding of permittivity and permeability. We use them extensively in both engineering and physics but we have no concept of what they are. With the insight which Barnes has given into the nature and the medium of light transmission there is reason to believe that progress in this area can be made.

Laws of Gauss, Faraday and Maxwell.

Just as Barnes has explained the mechanism involved in Newton's laws, we should be able to explain the mechanisms of Gauss' and Faraday's laws and of Maxwell's equations.

Model of the Atom.

The Bohr model of the atom has been used for many years but it has many problems. Barnes has proposed a new model of the atom as well as new models of the electron, proton and neutron. He is the first to admit that it is not complete but he has suggested an approach which should be carefully examined.

Spectral Lines.

The crucial test of whether a model of the atom is reasonable is whether it can predict the spectral lines of radiation of an excited atom. Since these lines are extremely repeatable, it should be possible to explain why they always have the precise value of frequency.

Intrinsic Spin of the Proton.

It has been accepted that there is an intrinsic spin of a proton due to its magnetic moment. This is a part of the puzzle of the mechanism involved in the atom. Barnes shows that this intrinsic spin would induce a spin to the electron which would generate a magnetic field in opposition to that of the proton. He proposes that this is the force which keeps an electron and a proton apart. The nature and cause of this spin is critical in any model of the atom.

Radioactive Decay.

Any adequate model of the atom must also predict the nature and cause of the decay of radioactive atoms. We know statistically that a particular substance decays at a very precise rate. This is not a random process since it is so predictable and seemingly invariant.

Semiconductors.

We have been using semiconductors since the advent of the crystal radio. Although, we have some empirical models which allow us to use them, we do not have a consistent atomic model to understand precisely how they work.

Superconductivity.

There has recently been much excitement over the discovery of some substances which exhibit superconductivity at relatively high temperatures (90K). A good model of the atom should help to develop other compounds with even better characteristics.

The research projects listed above are ones that have perplexed scientists for years. However, with the direction that has been given by Barnes, they no longer seem so formidable. The Thomas G. Barnes Institute of Physics has been established to pursue these problems and to honor Barnes for his great contributions to science. For further information, write to:

Max W. Callen, President
The Thomas G. Barnes Institute of Physics
3521 Chelmsford Road
Minneapolis, MN 55418

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