

# Creation Research Society Quarterly

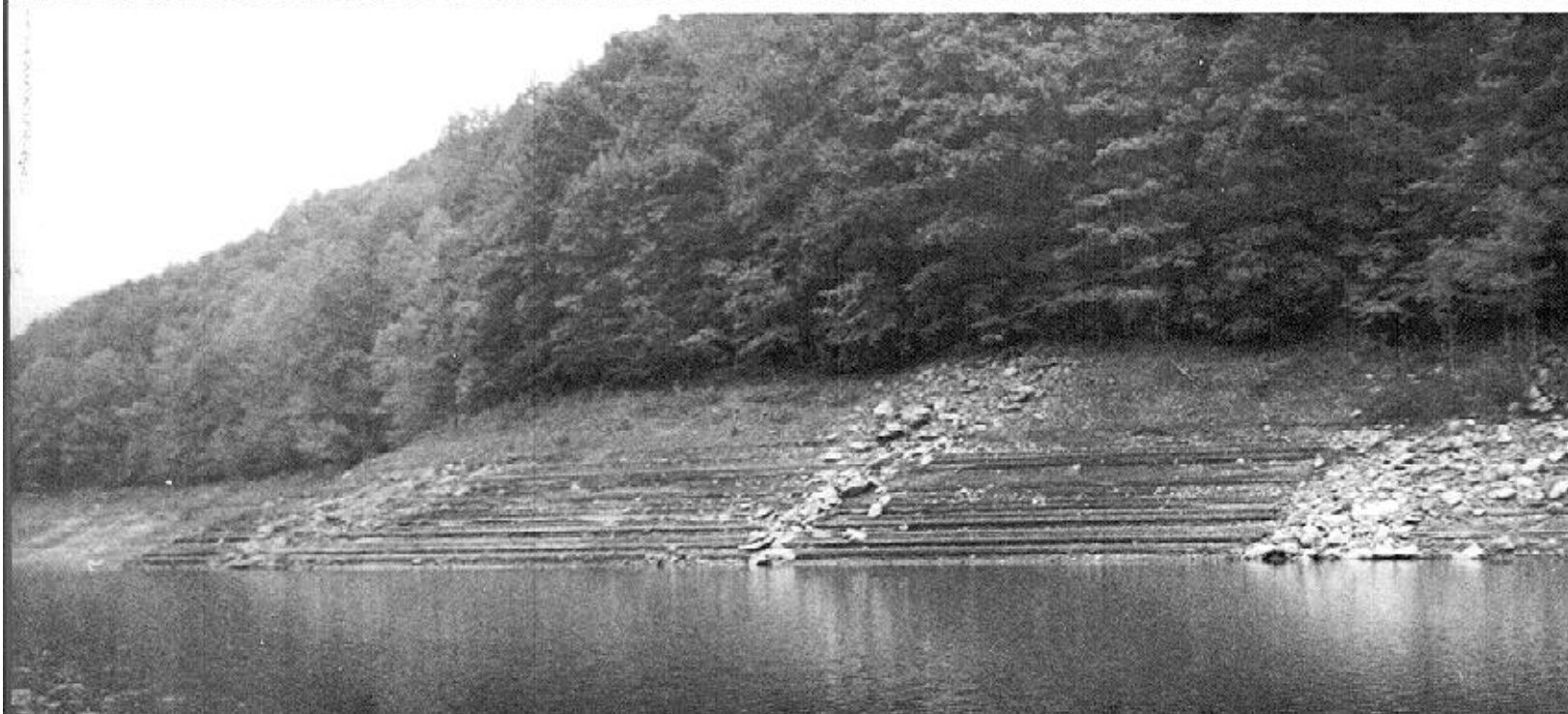
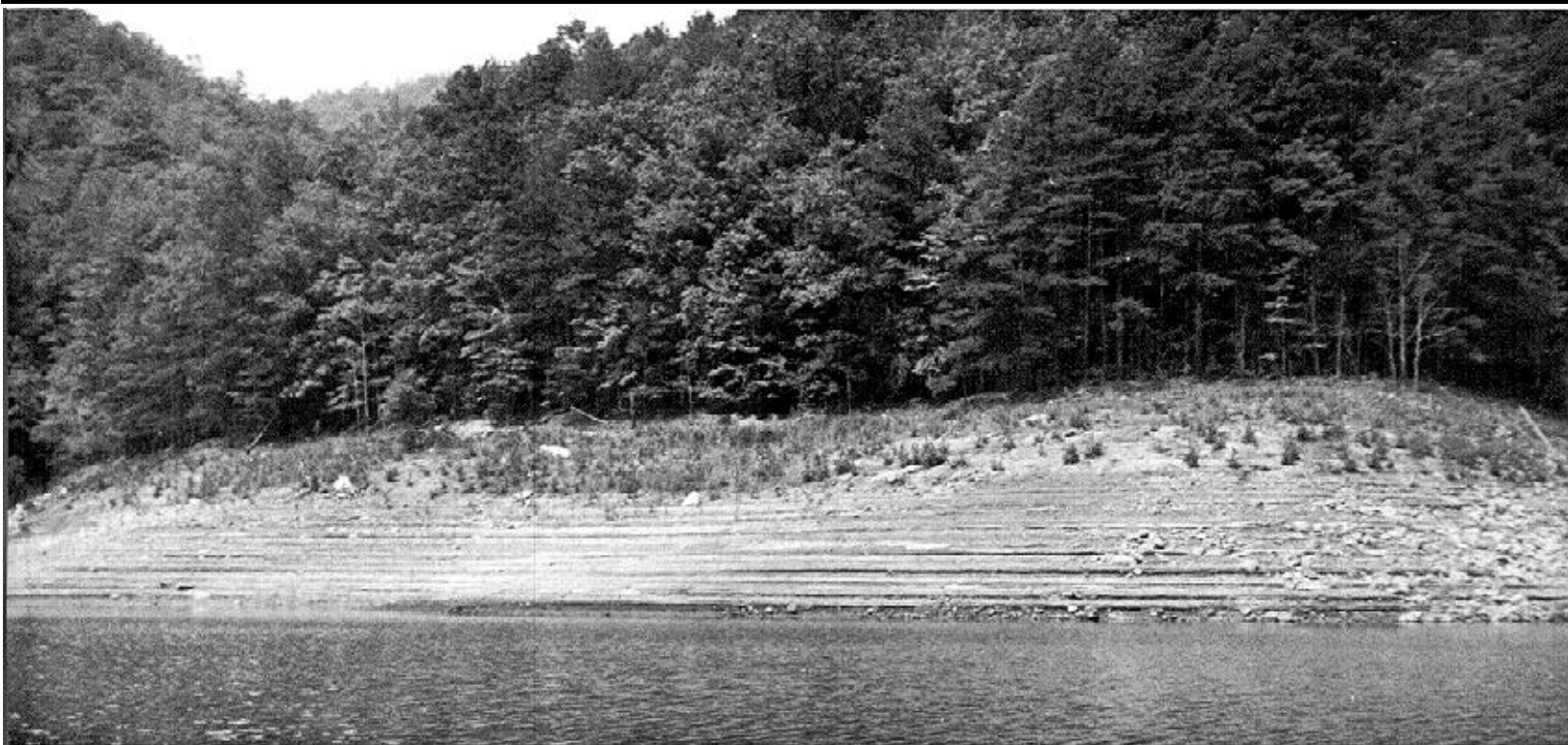
Haec credimus:

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 25

JUNE 1988

NUMBER 1



# CREATION RESEARCH SOCIETY QUARTERLY

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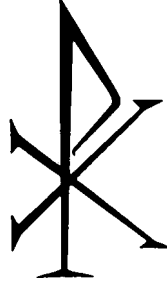
JUNE 1988

NUMBER 1

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## Cover Photographs

These pictures (September, 1987) show the shoreline of Fontana Lake in western North Carolina on the boundary of The Great Smoky Mountain National Park. This lake is used by TVA to maintain constant water levels of lakes farther down the Tennessee River system. Thus the lake water level at Fontana constantly fluctuates and during the late summer the water level is dropped to supply water to the downstream lakes. These photographs show a water level of about 40 feet below maximum. As the water level drops, terraces are seen on the shoreline revealing the previous higher water levels. Creationists (Whitcomb and Morris, 1963, pp. 313-24; Lammerts, 1974, pp. 101-3) have used terraces as evidence of gradually retreating Flood waters. As illustrated in these photographs, falling or retreating water will leave terraces and the creationist postulation is quite reasonable.

### References

Lammerts, Walter E. 1974. On the rapid formation of beaches and a graded series of flattened and rounded stones. *Creation Research Society Quarterly* 11:101-3.  
Whitcomb, Jr., John C. and Henry M. Morris. 1963. *The Genesis Flood*. The Presbyterian and Reformed Publishing Co. Philadelphia.

Emmett L. Williams

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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 606 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 49566. 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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## DEDICATION TO GEORGE L. MULFINGER, JR.

George Leonidas Mulfinger, Jr., a member of the Board of Directors of the Society since 1975, died after an extended illness on December 21, 1987 at the age of 55. He is survived by his wife, Joan, 11 children and five grandchildren. George was born in Syracuse, NY, his father being a professor at Syracuse University. He received a B.S. in chemistry and a M.S. in physics from Syracuse and did graduate work in the physical sciences at Syracuse, Harvard and the University of Georgia.

He became an instructor of physics and cello at Bob Jones University, Greenville, SC in 1965. In later years he added astronomy, geology and philosophy to his teaching expertise. Truly a brilliant man, George was able to convey the gist of any subject matter he taught. A talented musician, his performing career included principal cellist of the Syracuse Symphony, cellist with the Krasner String Quartet, principal cellist of the Bob Jones University Symphony Orchestra and member of the Bob Jones University String Quartet. Also George had many original hymn arrangements to his credit.

His writings in defense of creationism were clear and incisive. The more well-known selections that appeared in the Quarterly were "Examining the Cosmogonies—A Historical Review," "Critique of Stellar Evolution," "Review of Creationist Astronomy," "A Biblical Framework for a Course in Physical Science" (coauthor) and "Deposition of Calcium Carbonate in a Laboratory Situation" (coauthor). George acted as the major catalyst and did the initial research work in the Society's series on rapid  $\text{CaCO}_3$  deposition.

He coauthored many creationist science textbooks for Christian schools including *Physical Science for Christian Schools*, *Science for Christian Schools* (elementary series) and *Earth Science for Christian Schools*. He wrote a chapter in *Chemistry for Christian Schools* and was a technical consultant for Bob Jones University Press in the science area. George felt strongly about the publication of creationist textbooks and one of his specialties was the brief biographies of Christian men of science that appeared in many of the works to which he contributed.

This amazingly talented man still found time to maintain a ham radio station in his home and do some



"star gazing" upon occasion. A firm supporter of the local church he willingly plunged into the work of any assembly of which he was a member. George, above all else, wanted to serve his Creator in any way he could. He was a dedicated fighter against the evolutionary hypothesis. Being a very kind man, he turned his intellectual powers full force against naturalistic error and was relentless in pointing out fallacies in the humanistic philosophy. He was a tireless speaker for creationism.

I was George's office mate for 12 years at Bob Jones University. It was a pleasure and extremely stimulating to work with him. When George and I felt it was necessary to write a creationist physical sciences book, he plunged into the project with vigor. It was my privilege to be his "teammate."

George assumed the responsibility of Chairman of the Publications Committee for the Society and asked me to edit the first technical monograph, *Thermodynamics and the Development of Order*. I, in turn asked him to write a chapter for the book. He edited the second monograph, *Design and Origins in Astronomy* and requested that I contribute a chapter. That was the type of relationship we shared, a true friendship in the service of the Creator. George was one of my most trusted peer reviewers and at the time of his death was working on an article review for the Quarterly. His contributions to modern creationism are endless. His writings appeared in many other publications and he authored a chapter ("Transient Lunar Phenomena") in the excellent book, *The Moon: Its Creation, Form and Significance*.

I will miss his advice and help. George literally gave his life to honor his Savior. When we completed the physical sciences book, he chose Revelation 4:11 as his dedication verse:

Thou art worthy, O Lord, to receive glory and honour and power: for thou has created all things, and for thy pleasure they are and were created.

Now George has heard these words: "... well done thou good and faithful servant..." (Matthew 25:21).

Emmett L. Williams

### QUOTE

In other words, if science was to be born, nature had to be de-animated. Animism, which was always an essential feature of pantheism, had to retreat in the measure in which the monotheistic doctrine of creation was gaining ground. Animism was no match, in the long run at least, for the impetus of the doctrine of creation when the doctrine was taken in terms of the New Testament. Its doctrine of Christ as the "only begotten Son," in whom the Father created everything, put a damper on any flirtation with the idea that any other being might be a divine begetting in terms of an emanationism which always carries an animist touch. Animism—the entire history of philosophical learnedness in the Muslim world is a witness—held its own when confronted with the impetus with which the Koran carried the doctrine of creation.

Jaki, Stanley L. 1985. The physics of impetus and the impetus of the Koran. *Modern Age*. 29:159.

### Editorial Comments

A minisymposium on the speed of light will be featured in this issue and the two Quarterlies that follow. After a full presentation of the various positions, the debate will continue for a few issues in the Letters to the Editor section. Wendell Bird writes an article for us using material from his new book. A chapter is taken from Clarence Carson's history series and should be of interest in tracing the influence of naturalism and evolution on our culture.

A. W. Mehlert discusses the supposed reptile to mammal transition suggesting that it is nonexistent. Since the Research Committee will be performing some research work in the Big Bend area of Texas, an introduction to these efforts is offered in the Letters to the Editor. I hope you will find much material of interest, and I will be glad to consider your comments on the origins controversy. Please note on page 2 a new book just published by the Society on Charles Darwin.

Emmett L. Williams, Editor

## INSTRUCTIONS TO AUTHORS

1. Manuscripts shall be typed and double spaced.
2. An original plus two copies shall be submitted to the editor of the Quarterly.
3. All submitted articles will be reviewed by at least two technical referees. The editor may or may not follow the advice of these advisors. Also, the prospective author may defend his position against referee opinion.
4. The editor reserves the right to improve the style of the submitted articles. If the revisions of the editor and referees are extensive, the changes will be sent to the author. If the changes are not suitable to the prospective author, he may withdraw his request for publication.
5. Due to the expense involved, manuscripts will not be returned to authors.
6. All references (bibliography) must be presented in the style shown in the Quarterly. If a prospective author is not familiar with the CRS format, the editor will furnish an example reference page.
7. All figures and drawings must be prepared professionally. No sloppy hand drawings or freehand lettering will be accepted. The editor reserves the right to approve submitted figures. Unacceptable illustrations will result in rejection of the manuscript for publication.
8. Any manuscript containing more than 25 pages is discouraged. If a topic cannot be covered to the author's satisfaction in this length of pages, the author *must* divide his material into separate papers that can be serialized in the Quarterly.
9. The Quarterly is a journal of original writings. Only under unusual circumstances will I reprint previously published manuscripts. Never submit an article to two or three journals, including ours, hoping all of them will publish your work. I consider this practice unethical. When submitting an article, please state if the material has been published previously or has been submitted to other journals.
10. Book reviews should be limited to 500 words or less.

### Remarks by the President

In early 1988, on a radio talk show, two participants, one a creationist and the other an evolutionist, were discussing the issues. Somebody called in and asked the evolutionist a question regarding the evolutionary argument from embryology. The evolutionist appropriately pointed out that the evolutionary community generally did not regard the recapitulation concept in embryology as a good general principle for understanding past evolutionary processes. The creationist then took the occasion to point out that in this particular field, a former evolutionary "proof" had now been dropped down from the condition of a so-called "law" to possibly a hypothesis, but that some texts still were considering embryological data as good evidence for evolution.

In contemplating this, I recollected back to the late 1940's when as a freshman at the university I found the botany and zoology classroom presentations on evidence for evolution from embryology very convincing. At that time I wondered how anybody could doubt macroevolution on the basis of the embryological evidence. But even then professional embryologists were realizing that accumulated data were making them less and less comfortable with recapitulation

("Biogenetic Law") as a working principle in their discipline. By the time I had completed a master's degree in embryology (1955), I effectively was disillusioned about the general value of evolutionary thinking in embryology.

My master's advisor was a prominent embryologist and a very strong evolutionist, but he had informed me in no uncertain terms that this so-called "Biogenetic Law," which considered the developmental processes a summary of past evolutionary history, was an idea that had impeded progress in the field of embryology. His feeling was that we would be much further ahead had it not been for this particular evolutionary position which, though it may have stimulated some research, had led to many scientific dead ends. Also it had been the reason why many competent investigators had felt impelled to force their embryological data into an untenable mold.

In the 1950's professional embryologists had good reasons for backing away from their prior commitment to evolutionary thinking. However, during these same years there were other scientists, for example Swedish geneticist Heribert Nilsson, British biochemist G. I. Kerkut and Canadian entomologist W. R.

Thompson, who also discovered that their data did not support evolution. But, without turning their clocks back a century or more, what alternative could they embrace? Clearly, for them and others, evolution was not the "great unifying concept," but rather it appeared to be false and even an impediment in science. But the problem was to find an acceptable alternative theory.

Fortunately this no longer is the case. In the early 1960's the Creation Research Society pioneered in popularizing within the scientific community what commonly today is termed the Abrupt Appearance Model (or Theory). Because of the reasonableness of

this concept thousands of scientists have experienced relief from a compulsion to force data into an evolutionary mold. It has become easier for careful investigators to go only as far as evidence is compelling, most often clearly a small amount of change (what some have termed microevolution). Even though currently some well-meaning scientists unfortunately are opposing our efforts, CRS members and other scientists have opportunities, unprecedented in the 20th century, for presenting the Abrupt Appearance Model as the best way to understand data from nature. Yes, there now is a well-supported and increasingly popular alternative to evolutionary thinking.

Wayne Frair

## A CRITIQUE OF THE ALLEGED REPTILE TO MAMMAL TRANSITION

A. W. MEHLERT\*

Received 30 March 1987 Revised 8 June 1987

### Abstract

*For many years evolutionists have claimed that of all major groups of plants and animals, the fossil record of the origin of Class Mammalia is easily the best. (Olson, p. 207). However it will be shown that the case for the reptile to mammal transformation is extremely shaky and is based largely on inference, supposition and wishful thinking. Although I quote large-scale time frames and use standard geological terms in respect of epochs, eras and periods, I do not imply my acceptance of the time scales. I use the time periods for the sake of argument, but even so, the fossils do not provide a strong case for evolution.*

### Introduction

First I will examine the general evidence in broad terms and second I will investigate the detailed paleontological features of the various fossil candidates for the alleged transformation. In both cases the conclusions drawn by evolutionists belong more to the field of speculation than to science. In broad outline, the standard theory of the origin of mammals is as follows:

1. 'Early' members of a class of reptiles known as the synapsids arose from *unknown* amphibian ancestors in the early Pennsylvanian period about 300 mya (million years ago). These early mammal-like reptiles (Order Pelycosauria) were differentiated from other early reptile-like reptiles (!) of the same period *from their 'first' appearance* in the fossil record. Unlike the other reptiles, the pelycosaurs possessed a small space—the temporal fenestra—in the cheek region of the skull, but the origin of this feature is unknown.
2. The pelycosaurs suddenly became extinct around the mid-Permian, some 260 mya, and a few million years later, in the late Permian, the first members of the next phase of mammal-like reptiles appeared (Order Therapsida), which "... evolved from an *unknown lineage* of the sphenacodonts (pelycosaurs)." (Kemp, 1982a, p. 581)
3. The therapsids had larger temporal fenestrae, and "... even at their *first appearance* the therapsids had *already diverged* into several groups." (Kemp, 1982a, p. 581)
4. At the end of the Permian some 230 mya, most therapsid groups became extinct and the first cynodonts made their appearance, equally as suddenly as

the previous groups. The cynodonts, more mammal-like than the previous orders, dominated the Triassic period some 180-230 mya. They possessed cusped teeth and although some had impressive canines, many were herbivores. Their skeletal structure shows that they carried their bodies well above the ground and locomoted more or less like some land mammals.

5. According to Kemp the cynodonts also became extinct by the late Triassic, 180 mya, "... leaving only their tiny descendants—true mammals—to persist into the Jurassic and beyond." (Kemp 1982a, p. 583)

M. R. Dawson (1967, p. 12) tells us:

... the fossil record presents a picture of several lines of therapsids approaching the mammalian level of development in the Triassic. It appears likely that at least three or perhaps six or more phylogenetic lines passed from the therapsid to the mammalian grade.

Personally I find one line hard enough to believe let alone six! Although Dawson does not name the six groups, a study of Romer's 1966 book (pp. 176-86) reveals that they probably were members of the sub-orders Theriodontia, Anomodontia, Ictidosauria, and included genera such as Diarthrognathus, Thrinaxodon and Cynognathus, all of which belonged to the Upper Permian to Late Triassic. A single genus (Diarthrognathus) appears to have persisted into the mid Jurassic, before becoming extinct.

6. The group in which I am most interested is the cynodonts, and the last lingering genera of the mammal-like reptiles also include Morganucodon, Kuehneotherium, Probelesodon and Probainognathus which are found in the Triassic/Jurassic deposits. These genera have attracted much debate and most evolutionists believe that all have a combination of

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

mammal and reptile features, particularly in the skull, jaws and teeth. Such claims will be examined in more detail.

7. By the late Jurassic some 140 mya four allegedly distinct mammalian orders are believed to have been in existence—the multituberculates, the triconodonts, the pantotheria and the symmetrodonta. Opinions as to their status and to their alleged descendants vary widely and most fossils of this time consist of a few scraps of teeth and jaws. The actual reptilian ancestors are *not known*.

8. Even by the Late Cretaceous, some 80 mya, very little is known about mammals. M. R. Dawson (1967 p. 14) admits—“. . . the postulated relationships (of early mammals) *among themselves* and with other (later) mammals are *highly speculative*.” On page 13, he also admits that the Mesozoic (Permian, Triassic and Jurassic) mammal fossil record is “woefully inadequate,” which is rather strange as this era is just at the very time when the alleged “finest record of the origin of a major class” (Olson, 1966, p. 207) was taking place!

Of the Mesozoic, particularly the Late Triassic to Late Jurassic, where a huge time gap exists with only a pitiful handful of fossil scraps available, Olson (1966, p. 206) had this to say—“After the early Jurassic, a very curious thing happened. During the next 80 million years, very little seems to have gone on. It was a period of marking time.” I find it truly amazing that 80 million years could pass with so little evidence!

In the upper Cretaceous of North America is found the ‘oldest’ marsupial—the opossum (family didelphidae) which is very similar to modern Virginia opossums. Like all the other orders, the marsupials appeared suddenly in the rocks without known ancestors.

9. By the early to mid Tertiary, 50 mya, we find a great radiation of mammals including all the 30 odd known orders, none of which is clearly linked to previous alleged ancestors nor to each other. (Simpson, 1944, p. 105)

Having broadly listed the various steps in the transformation we can clearly see that not one assumption in the whole process can be substantiated. There is no fossil proof that any of the so-called progressions actually took place and it is impossible to properly document the ancestry of the pelycosaurs, the therapsids or the later cynodonts. Of the mammal-like reptiles as a whole, Kemp (1982a, p. 583) writes—“The main groups . . . are more or less fully *evolved when they first* appear in the fossil record . . .”

Even down to the species level, evolution is in a great deal of trouble as Kemp (p. 583) further writes—“Each species of mammal-like reptile . . . appears *suddenly* in the fossil record and *is not preceded* by the species that is directly ancestral to it.”

W. E. Sheele (1955, p. 24) wrote—“The first true mammals were small insectivores whose relationship to (reptiles) is not at all clear.” F. J. Ryan (1956, p. 330) commented—“The origin (of marsupials) is extremely ancient and its sources are not known.”

The fossil record of the monotremes (platypus and echidna) is very poor and attracts this comment by Australian zoologist Lyne (1967, p. 5)—“No intermediate forms between these highly specialized animals

are known and they so little resemble each other that their relatively close relationship would hardly be suspected.” Hugh Tyndale-Biscoe, Professor of Zoology in the Australian National University, Canberra, is totally unable to find the ancestors of even a single marsupial family! On page 30 of his 1973 book, *Life of Marsupials*, he published a fossil ancestry chart of all the marsupial families, living and extinct. Each family is designated by a solid line going back in time to form its known temporal range. *In every case* the solid line degenerates into the familiar dotted line which we so often see in evolutionary trees of life (phylogenies). The caption to the chart states—“Known records shown by closed (solid) lines, and probable but *unknown* relationships shown by dotted lines.”

J. W. Valentine (1978, pp. 112-5) also published detailed ancestral phylogenies of the mammal-like reptiles and all of the mammalian orders. Once again, *in every case* the solid lines of ancestry quickly degenerate into the dotted lines which fizzle out into nothingness at the very stages at which we would expect to see some solid evidence of the transitions. Because nobody knows which mammal-like reptile gave rise to which mammal, the dotted hypothetical ‘lineages’ are therefore useless as evidence for the alleged transformations, as they are speculative. The real culprit is the fossil record which has failed to document the reptile to mammal lineage, just as it fails to document any evolutionary lineage, plant or animal! So disappointing is this that Ridley (1981, pp. 830-1) wrote—“(People) think that the main evidence for evolution is the gradual descent of one species from another in the fossil record . . . (however) the fossil record is useless for testing between evolution and special creation.”

Of the fossil evidence in general, no less an authority than T. Kemp, Curator of Mammals at Oxford University Museum was quite frank. In a personal communication to me dated October 20, 1982, Dr. Kemp told me re: the lack of fossil evidence, that his view was that the fossil record is incomplete. He said that the other view, based on creationism, is that no intermediates ever existed and he continued—“Unfortunately, the absence of data (the missing transitionals), being negative evidence, does not permit us to distinguish between these two explanations.” Readers can draw their own conclusions!

### The Big Cop-Out

Since the main support for evolution, the fossil record, has proved such a failure, most evolutionist authorities resort to special pleading—the well-worn and unjustifiable concept of parallel evolution. This concept which includes convergence and the idea of polyphyletic origins is invoked incessantly to try and overcome the *hostility of the fossils*. The hostile aspect is of course expressed in a negative manner—the virtual total and systematic lack of transitional lineages and the existence of innumerable missing links, which should not exist were evolution true. As Kemp has already admitted, the *consistent failure* to find ancestral species leading to the development of new groups means that the paleontologists must rely on educated guesses, wishful thinking and speculation to establish ‘lineages’ which exist only on paper in the textbooks.

Working almost solely on skimpy skeletal features, they pick and choose their preferred lineages as Olson has to admit. Referring to the welter of reptile fossil specimens of the Triassic period (pp. 125-6), he conceded:

There is almost no chance of making even reasonable guesses as to which of the known forms were close to lines that were eventually successful . . . because of the confusion of the ancestral stocks *it is difficult or impossible* to be sure that the genera placed in a single lineage did actually come from the same ancestor.

Further on the same pages Olson admits:

The phenomenon of polyphyletic origins of groups *above the species level* appears to be quite common. Much is made of polyphyletic origins in evolution but mostly (it is) the result of *ignorance* on the one hand and *our own constructions of classifications* on the other.

Note how he shifts the blame from the fossils to the classification system, which as I will show is not at fault.

On page 186 Olson has to concede a *major weakness* of paleontology—“. . . evolutionary transformations *must* have gone on. We determine the nature of these transformations by tracing them, but how they are traced *depends on the particular lines of descent that we envisage and these are often speculative.*”

Now the various phantom lineages that exist on paper in the mammal-like reptiles and early mammals have their foundations in comparative anatomy—that is, the paleontologists select the most likely candidate in a lineage on the features which most closely resemble the ‘preceding’ fossil. They forget that *lateral variability* within a type can often be so great that members of the same species could easily be wrongly classified if they were found only as fossils—such as dogs, pigeons, horses and many others.

The appeals to parallel/convergent evolution and polyphyletic origins are so great that it is nothing short of scandalous, and even experienced evolutionists such as Gribbin and Cherfas (1981, p. 520), have publicly admitted that parallel evolution is a cop-out. The experts complain about the imperfection of the geologic record yet they do not hesitate to use that same imperfect record to construct lines of parallel evolution!

### More Problems

The net result is that we cannot be sure at all that any of the published phylogenies in the reptile/mammal field is genuine and the stubborn fact remains that no fossil mammal-like reptile can be shown as being ancestral to any mammal. This is a farcical situation aggravated by the lack of soft parts in the fossils where the *major differences* between reptiles and mammals would be clearly shown, such as mode of reproduction, mammary glands, method of breathing, hair or fur, etc. The monotreme platypus for example lays an amniote-type egg but this does not make it a transitional form between a reptile and a placental mammal. Luckily, this interesting creature is not extinct and only the fact that we can examine it and see that it is a unique type of

mammal prevents the evolutionists from drawing up some sort of ‘lineage’ and placing it between say the cynodonts and the marsupials or placentals. Olson (pp. 194-5) has published a chart showing the monotremes as branching off from a separate *hypothetical* mammal-like reptile far distant from the other mammals. He shows about six groups of mammal-like reptiles as having crossed what he describes as the ‘mammal threshold zone.’ As fossil monotremes are only known from the Miocene, some *100 million years later* than Olson’s hypothetical ancestor, his reliance on speculation is clearly shown!

Olson himself is a persistent offender as on p. 123 he shows a very impressive looking family tree of 16 reptile groups but we find on p. 124 that he admits the phylogeny is misleading, due to the widely scattered nature of the fossils in time and space, and the “great many gaps in the record.” He says—“If we are willing to jump over these gaps *broad patterns* of evolution do emerge . . .” Creationists of course reject the invitation to jump over the gaps!

Despite all the means at their disposal, the evolutionists cannot explain the origins of any mammal orders, whether monotreme, marsupial or placental, so they take the easier way out as M. R. Dawson did as previously mentioned, and rely on speculation *which is not science*. One of Australia’s leading experts on monotremes, Gordon Lyne, does not hesitate to evade the problem by claiming that—“. . . the monotremes are *not closely related* to the marsupials and higher mammals, and they evolved *from a distinct group* of reptiles.” (1967, p. 5) How does he ‘know’ this when there are no fossils to prove it?

Virtually all paleontologists believe that all the major mammal groups including the marsupials evolved independently, in parallel from different reptilian ancestors. A good example is the famous Tasmanian marsupial wolf, the Thylacine which bears many remarkable resemblances to the placental wolf, yet they are not related, and once again we have the usual appeal to parallel/convergent evolution. Both allegedly evolved from *separate ancestors* somewhere in the mysterious Mesozoic era, up to 180 mya and by amazing coincidence and chance mutations with natural selection they finished as look alikes. What sort of thinking is this? Both animals are *unique* types fully deserving their own separate classifications and there is no need to appeal to parallelism or evolution at all. They can just as easily be assigned to created types. Noted British evolutionist Francis Hitching is openly skeptical. (1982, pp. 176-80) Writing about alleged parallel/convergent evolution, Hitching writes—“. . . the current explanation *explains nothing* . . . it seems highly unlikely that Natural Selection of chance mutations could have arrived at the same near-identical solution of adaptation.”

Because of this idea of parallel/convergent evolution and polyphyletic origins we thus have no way of telling whether the pelycosaur, therapsid and cynodont reptiles were in fact related evolution-wise or whether they were created, unique and separate groups. The evidence of the fossils (no convincing transitions) therefore is at least a strong *prima facie* case in favor of creation.

### Taxonomy

The lack of transitional forms and of common ancestors and the heavy reliance on parallel/convergent evolution has created an enormous problem for taxonomy and classification. This problem is not due to the fact of evolution causing blurring between the classes, *but is due entirely to the assumption* that various organisms have evolved into others.

The classification system which originated long before Darwin was quite adequate until it became *loaded down by the unnecessary and unjustified concept* of evolution. The case of the reptile/mammal concept is a good illustration.

The diagnostics of class mammalia run to about a dozen main characteristics—1. A single lower jaw bone, the dentary. 2. A squamosal-dentary jaw joint. 3. Three bones in the middle ear (stapes, incus and malleus). 4. The organ of Corti (ear). 5. Teeth highly differentiated into canines, incisors and molars with complex cusps. 6. The mammalian diaphragm. 7. A number of post-cranial osteological features. 8. Live birth of the young (except monotremes). 9. Supply of milk to the young via mammary glands. 10. Hair or fur. 11. Self regulating heat control system, and a few others mainly connected with the excretory system.

What then are we to make of the monotremes which are egg-layers, have no teeth and have a different excretory system from placentals? Or the marsupials which give birth in the embryonic stage? Evolutionists *interpret* these things as being evidence that the monotremes and marsupials are 'primitive' in comparison with placental mammals and exhibit signs of reptilian ancestry (i.e. the egg laying of the monotremes).

But is this true? Of the earliest (Cretaceous) fossil marsupial, the opossum, Michael Denton (1985, p. 181) writes—". . . it was *already* at the level of living, primitive mammals such as the insectivores or the Virginia opossum." The Australian zoologist T. J. Dawson, (1983, pp. 1-2) wrote that the currently held theory that monotremes and marsupials represent the earliest stages of evolution and being on the way towards placentals, is *simplistic and now known to be misleading*. He further wrote on p. 51 that marsupials are *not inferior* to placentals.

Therefore there is no reason except for evolutionary preconceptions for believing the monotremes and marsupials to be primitive ancestral-like types for the higher placentals. They are not primitive and they hold their own in competition with placentals in every respect—defense and attack, temperature control, intelligence, adaptability etc. The only reason, for instance, which caused the fierce marsupial thylacine 'wolves' to become extinct was because of man—Australian farmers literally destroyed them to protect their herds of sheep, cattle, chickens etc.

### Other Oddities

Once we question the status of monotremes and marsupials, we might also ask about bipedal dinosaurs (reptiles), toothless turtles (reptiles), marine reptiles with fins like fish, and the extinct flying reptiles. We may ask are penguins evolving towards 'marine birds'?—or feathered fish? Evolutionists of course, would scoff at such suggestions yet they accept the

monotremes and marsupials as being types of semi-evolved mammals! Even Archaeopteryx has now been abandoned as a transitional between reptile and bird because the mere possession of some characteristics from two or more other groups is not sufficient to establish it as a transitional. Eldredge and Gould (1977, p. 147) dismiss the unique bird Archaeopteryx as a transitional and describe it as a curious mosaic. The reason is that this curious creature, although possessing apparently true reptile and true bird features, *does not possess any individual characteristics which themselves are in a state of transition*.

### Clades, Cladistics

Due to the problems mentioned above, many modern paleontologists have moved away from the standard phylogenies and have embraced a form of classification called cladism which simply shows, in line form, which creatures resemble other creatures and the relationships in clades do not imply evolutionary connections. Some leaders in this field are in the British Natural History Museum and a prominent figure is Colin Patterson, the senior paleontologist who once admitted that he could not name a single, proven transitional in any branch of paleontology (1981).

Needless to say, the cladists have aroused the ire of the hardline evolutionists who continue to insist on belief in evolution despite the evidence, but Patterson's case as published in an issue of *New Scientist* (1982) is well worth study, and has proven to be an annoying thorn in the side of the orthodox transformists. Patterson writes on p. 305 that a cladogram reconstructed from the evolutionary viewpoint disagrees with a cladogram based on character analysis. He points out that the latest studies from the molecular field, particularly amino-acid sequences of RNA and DNA and other proteins line up far better with a non-evolutionary cladogram than with the predictions of standard evolutionary phylogenies. In fact the evolutionary predictions fail, which is because, in my opinion, the evolutionist phylogenies do not match the naturalness of a cladogram. Once again we see that there is nothing wrong with our classification system, as long as we do not burden it with the assumption of evolution. There is now powerful empirical molecular evidence which matches the natural classification system and disagrees strongly with any evolutionary approach to taxonomy.

Patterson (p. 306) states that cladistics call into question much of conventional evolutionary history and offers a new approach to comparative biology which has a *coherent, theoretical* base that is not necessarily tied to evolutionary theory and does not depend on Darwinian or neo-Darwinian presuppositions. Creationists need have no objections to cladistics as this system fits in very well with the natural taxonomy of creationist scientist of two centuries ago.

### A More Detailed Examination

Since the evolutionary case is lacking in broad terms, it is time to take a closer look at the actual fossil evidence cited for the transition and see how it is interpreted.

One of the best technical works on the subject is by Alfred S. Romer whose famous textbook (1966, pp. 173-209) covers in detail, most of the reasons for evolutionists' acceptance of the cynodont (late Therapsid) group as being ancestral to the early mammals. The so-called advanced therapsids are described in considerable detail by Romer and include a number of sub-orders and genera such as *Cynognathus*, *Thrinaxodon*, the Tritylodonts and the ictidosaurian *Diarthrognathus*, mostly from the late Triassic. The number of fossil specimens and variations in their structure make it almost impossible to accurately determine relationships between and within these groups and, to make matters worse, much of the fossil material is fragmented and disarticulated.

When one studies the various authorities one finds many different opinions on the classification and meaning of the advanced therapsids and the early mammals. Instead of locating and identifying actual ancestors, a process of generalization takes over which bogs down in parallelisms as the experts pick and choose various features from various reptiles and 'new' orders, families and even genera are 'created' from the available material. Then lineages are constructed which are to put it bluntly, very speculative.

For instance, some of the 'later' therapsids have a dentary larger than those found in 'earlier' specimens, and less pronounced lower jaw bones such as the surangular, the angular and the articular. Evolutionists call these smaller bones 'reduced,' i.e., in the progression from reptile to mammal, the dentary becomes larger and the other bones reduce in size almost to insignificance. However, what possible selection advantage such a slow and gradual series of steps would have for a reptile is difficult to comprehend—especially when each step was supposedly brought about by random mutations, not just in one line of reptiles, but in several lines independently and in parallel! We are truly being invited to believe in a miraculous series of blind events which had no purpose at any stage.

#### Some Cynodont Features

In McGowan's summary of reptilian, mammalian and 'intermediate' features of the various cynodonts, (1984, p. 138), it is notable that he allocates the lower jaw arrangement of cynodonts to the reptilian condition and not the 'intermediate' position, despite the large dentary, which in my opinion is correct. He includes this in his list of five cynodont features which are reptilian. He also classes five other features as mammalian and four which he describes as intermediate. One mammalian feature he lists is the forward sloping ilium, however, as he admits (p. 133) this feature also belonged to some reptiles such as certain dinosaurs and the pterosaurs and its status is therefore questionable.

The key points about the cynodonts are those four which McGowan describes as intermediate *in themselves*—

1. Cheek teeth with cusps which are not complex.
2. The jaw joint is formed between a hollow in the lower jaw and a flat surface in the skull (instead of a rounded knob). This 'intermediate' feature is found in the genus *Probainognathus* of the mid Triassic.

3. Prominent ribs confined to chest region, but short ribs in front of pelvis.
4. Legs not splayed like most reptiles but not quite vertically beneath body either.

Let us examine these so-called intermediate conditions—

#### 1. *Differentiation of teeth.*

While it is true that some cynodonts have specialized teeth such as incisors, canines and molars with simple cusps, we must remember that even some marine mammals such as dolphins and some species of whales have no differentiation and all their teeth are little more than sharp pegs—they have no incisors, no canines and no molars. Some mammals such as the monotremes have no teeth at all! In addition, there are reptiles with teeth and reptiles without them, such as turtles. This 'intermediate' condition is therefore inconclusive and cannot be used as definite evidence of a transitional stage.

#### 2. *The Probainognathian hollow on the lower jaw.*

I am a little puzzled by McGowan because the hollow in the lower is a reptilian feature. How can anyone describe a 'flat surface' on the skull as a protoglenoid into which the lower jaw fits? Only mammals have the hollow (glenoid) *in the skull* (not the jaw!), along with the scroll-like knob on the lower jaw which fits into the glenoid. To claim that the 'proto-glenoid' of *Probainognathus* is in the skull while the hollow itself is in the lower jaw makes one wonder at McGowan's objectivity.

Of the much touted claim that *Probainognathus* had a double articulation (the reptilian quadrato-articular and the mammalian squamosal-dentary), Kemp (1982c, p. 271) stated: "A second much quoted feature of *Probainognathus* . . . is the secondary contact between the dentary and squamosal. In fact there is some doubt whether there is actual contact between these bones . . ." This 'intermediate' condition must therefore also be questionable.

#### 3. *Prominent ribs confined to chest region, but short ribs in front of pelvis.*

It is true that in mammals, the ribs are confined to the upper abdominal area and the lumbar region just in front of the pelvis is free of ribs. However, in the cynodont genus *Massetognathus* there are shorter ribs in front of the pelvis. It is purely a matter of opinion whether these shorter ribs are in fact a sign of intermediacy. It must also be remembered that in *all* mammal-like reptiles the neck region also carries ribs whereas in mammals there are none. This 'intermediate' condition must therefore also be suspect.

#### 4. *Cynodont legs not splayed like other reptiles but also are not vertically placed beneath the body as in mammals.*

McGowan has forgotten that not only mammals have their legs vertically placed underneath the body. As he correctly mentioned (p. 133), many quadrupedal and bipedal dinosaurs had this feature and they are all reptiles. Therefore his claim that the cynodont condition is an intermediate sign is not clearly demonstrated.

It must be further remembered that even among the cynodont group, *not one single genus* has all the

mammalian, reptilian and 'intermediate' features as described by McGowan. This is why no actual ancestral group can be identified. It is no good picking about and taking one or two features from various families and orders and putting them together as a composite. Anyone can play that game and this is the reason why T. J. Dawson (1983, p.2) is forced to admit—"Good transitional stages *are not known* but some of the features . . . *probably* evolved within these advanced mammal-like reptiles."

#### Re-evaluation of McGowan's Comparison

It can now be seen that none of the four alleged intermediate features listed by McGowan can properly stand up to scrutiny and all four are therefore either questionable or irrelevant. The importance of a specimen which possesses features which are common to its own and another class, as evidence for a transition, is not of great importance. It is the *individual characteristics* of a candidate for a transitional form which count. It is true that some forms exist which display features common to another class, viz—

Archaeopteryx (bird)	Long bony tail, abdominal ribs (reptilian features)
Monotremes (mammal)	Egg laying of reptilian type
Cynodont reptiles	Large dentary, temporal opening high on skull (mammal features)
Dolphins (mammals)	Fish-like fins, undifferentiated teeth (reptilian feature)

It is noticeable that McGowan, when listing the bird and reptilian characteristics of *Archaeopteryx* (p. 117), is totally unable to show a single feature which *in itself* is transitional in form. Also, of *Archaeopteryx*, Stebbins and Ayala (1985, p. 54) noted: "In spite of the intermediate position of *Archaeopteryx* as a species, its *individual traits* are not transitional in character." This is the reason Eldredge and Gould (1977, p. 147) had to reject *Archaeopteryx* as an intermediate between bird and reptile. They wrote—"Curious mosaics such as *Archaeopteryx* do not count (as intermediates)."

Michael Denton (P. 109) said:

Undoubtedly, if the various anatomical and physiological systems of the lungfish and the monotremes were all strictly transitional between fish and amphibian and between reptiles and mammals respectively, then the case for them being genuine transitional types would be far clearer.

Denton (p. 110) wrote about the little caterpillar-like organism called *Peripatus* which some evolutionists see as being transitional between the annelid worms and the arthropods. He stated that although *Peripatus* possessed some typical arthropod features and a typical annelid feature, it lacks transitory *individual features*. Denton says—"These forms) provide little evidence for believing that one type of organism was ever gradually converted into another."

Characteristics common to two classes can be as equally due to creative design as to evolution and only if a characteristic is *clearly transitional in itself* would that be evidence for evolution.

#### Some Major Problems

Although one can see reasons for evolutionist reliance on forms such as the cynodonts, the experts often overlook many other major hurdles—

1. Reptiles have no diaphragm and therefore do not breathe like mammals. Their thorax is not a closed box and is not alternately expanded and contracted. In mammals the thorax is capable of expansion. The development, by rare chance mutations of the revolutionary fibro-muscular organ, the diaphragm, simply defies explanation as there is nothing like it in the whole reptile class. It is also required that each stage of the transition be advantageous to give the animal an adaptive edge over its rivals. What advantage which could accrue *in each step* defies explanation.
2. Who can explain the origin and development by the same process of such a complex and wondrously designed feature of mammal hearing—the organ of Corti? No fossil reptile displays any comparable complexity.
3. Who can explain, by rare mutations, the change from the amniote egg to live, placental birth in mammals?
4. Where did mammalian self regulatory temperature control originate?
5. How did reptile scales turn into mammalian hair or fur?

All these and many other macro changes would have had to take place more or less together by chance genetic mistakes over millions of years. It is not sufficient for evolutionists to call on parallel evolution or on mosaic evolution. For all these changes to occur *independently* several times is just asking too much and evolutionist biologists will have to develop something much more plausible if the theory is to survive!

#### The Jaw Joint and the Mammalian Ear

The strongest claim in the evolutionist arsenal is the alleged double jaw articulation in some of the later cynodonts. Two of the best candidates are *Morganucodon* (also known as *Eozostrodon*) and *Kuehneotherium*, both from the mid Triassic. It is generally claimed that not only did these creatures possess a squamosal-dentary (s-d mammal) jaw joint, but also a quadrate-articular (q-a reptilian) joint. However in fact both possessed a full complement of reptilian bones in the lower jaw and furthermore there was no reduction in the functional importance of the reptilian q-a joint (Gish, 1985, pp. 96-8).

Even though these creatures are sometimes considered as primitive mammals, (Gish, p. 97) the most striking characteristic of the accessory jaw bones is their *cynodont* (reptilian) character. The actual reptilian jaw joint itself was *relatively as powerful* in the mammal (!) *Morganucodon*, as it was in the reptile *Cynognathus* and that this was quite unexpected.

Although there is no doubt that *Morganucodon* and *Kuehneotherium* both had powerful and standard reptilian jaw joints, did they have a contact point between the large dentary and the squamosal and if so, was this an incipient mammalian type jaw joint? Unfortunately the pitifully small, fragmented and disarticulated fossil material makes this claim unresolvable and any claims are based on inference.

One fragment of jaw however was found with the quadrate bone *still in contact with the articular* (Gish,

p. 98), but there are *no fossils* showing the mammalian condition, i.e., the dentary in contact with the squamosal of the skull and it is therefore also a matter of inference only. It is extremely difficult to conceive how, if there was such a contact and if it was effective, an animal with two effective but very different types of musculature, with different types of arrangement of blood vessels and nerves etc. could function when it came to opening and closing of the mouth and chewing. It is again very hard to see what selectionist advantage would accrue to such a messy arrangement.

However, similar claims concerning two other reptiles with alleged double jaw joints have been questioned. Concerning the cynodont *Probainognathus* and the ictidosaurian *Diarthrognathus*, Gish (p. 99) quotes comments by Kemp of the Oxford University Museum and C. E. Gow. Kemp is quoted as stating that there is now some doubt as to whether there is an actual s-d contact in *Probainognathus*, and Gow is quoted as pointing out that several previous conclusions in regard to the morphology of the lower jaw of *Diarthrognathus* and its articulation with the skull were wrong.

All this confusion and the drawing of inferences is due to the small and very fragmented nature of the fossil material and it demonstrates again the dangers of making firm conclusions on such slender fossil evidence. To emphasize the paucity of the evidence for the alleged transition from reptile to mammal, Columbia University's Richard Goldschmidt wrote in 1952 (pp. 84-98) that the evidence was meager and that evolution had hardly an indication of intermediate forms in the so-called sequence.

#### Hearing—Auditory Bones

McGowan (p. 139) makes an attempt to visualize how the mammalian ear bone, the malleus, began as part of the lower jaw in the embryonic stage of mammals, and then articulated with the incus (the mammalian middle ear bone). He describes this as an 'embryonic' jaw-joint. From this he concludes that these two bones represented the articular and quadrate bones of reptilian ancestry. Such a conclusion once again is highly speculative because in not a single fossil is such a stage even suggested. Even though some lower reptilian jaw bones are very small, in every fossil case where the jaw was recovered intact, the quadrate and articular were firmly attached to the dentary, as in *Morganucodon*.

McGowan's claim that it is easy to visualize the linking of the articular and quadrate to the stapes to form the mammalian ear ossicles is an incredible statement. Reptiles have only one ear bone—the stapes, and *this applies to every mammal-like reptile* including *Morganucodon* and *Kuehneotherium* which are reptiles despite some claims to the contrary. In contrast to this, every mammal, living or fossil, has three middle ear bones—the stapes, the incus and the malleus. Gish correctly points out the sheer absurdity of the notion that the quadrate and the articular jaw bone of the reptile could work their way into the reptilian ear to eventually become the mammalian incus and malleus respectively. He writes (p. 100):

This would have required that the stapes (columella) of the reptile became free from its attachment

to the ear drum, and the retrarticular process of the articular gained an attachment to the ear drum (because the articular bone of the reptile supposedly became the malleus of the mammal, which is attached to the ear drum). Somehow the reptilian quadrate must gain its freedom, move into the middle ear *and insert itself between* the stapes and the malleus.

This revolutionary biological engineering feat would truly have required a miracle to occur. Each bone involved would have had to be refashioned precisely and linked perfectly into a new exquisitely complex hearing system totally different from the reptilian type. The animals concerned would have had to achieve some selection advantage *at every stage* and at the same time at least maintain adequate hearing and chewing abilities. Does the evolutionist seriously believe such fairy-tale science, if indeed it can be called science? To add to the evolutionist's problems, I notice that McGowan fails to mention one other major mammalian middle-ear hearing apparatus—the organ of Corti. This is an essential instrument of hearing in mammals and it is incredible in its complexity. In humans it is a spiral three millimeter diameter ridge of cells, containing about 20,000 rods and more than 30,000 nerve endings. As Francis Hitching admits (1982, pp. 92-3), nothing remotely as complicated can be found in the ear of any reptile, living or fossil, and he finds it hard to see how any transitional forms leading to it could have made the mammal ear slightly more perfect. I am not really surprised at Dr. McGowan's silence on this point.

Not only did new species of mammal-like reptile suddenly appear which were not preceded by a species directly ancestral, but as a consequence we find new families and orders similarly appearing without known ancestry. Boyce Rensberger (1982, p. 42) writes—“There should be lots of intermediate forms linking major groups. If, for example, reptiles evolved into mammals, there should be many gradational fossils between the two types. *Instead*, the paleontologists found more gaps than continua.”

#### The 'Early' Mammals

Romer is understandably cautious in his chapter on the so-called early mammals (pp. 197-206), due to the extreme scarcity and fragmented nature of fossils in the Triassic-Jurassic periods of the Mesozoic. He admits (p. 197) that the monotremes are almost unknown as fossils and that the marsupials and placentals appear *only at the end of the Cretaceous*, some 120 million years *after* their supposed evolution back in the Triassic. Of the Triassic and Jurassic periods, Romer says (p. 197):

Mammals *presumably* came into existence towards the end of the Triassic, but *we know extremely little* about their history during almost the entire span of the Mesozoic . . . almost all the early Mesozoic remains consist of *isolated* teeth or at the most, jaws; prior to the late Cretaceous we have *not one* satisfactory skeleton and *very little* skull material.

On p. 198 his frustration is noticeable when he complains that:

Many of the diagnostic features of the class Mammalia relate to soft anatomy and physiology and hence *cannot be determined* from fragmentary skeletal remains . . . some of the supposed Mesozoic mammals may still be in, technically, a therapsid or semi-therapsid stage . . . the class Mammalia may be *polyphyletic* in origin.

He also admits to the dangers of drawing too positive conclusions from the Mesozoic fossils. (I only wish he and others had heeded this sound advice.) Orders allocated on this skimpy material to Mesozoic mammals are Triconodonts, Pantotheres, Symmetrodonts and the Multituberculates.

Romer says (p. 201)—“In the order Symmetrodonts we reach, *for the first time* in our recital of Mesozoic types, a group which *may* be related to the ancestry of the higher therian (placental) mammals.” Such a statement surely reflects the uncertainty over the interpretation of the admittedly skimpy material and should be compared to the dogmatic statements made in other places by Romer himself and other vertebrate paleontologists. The Symmetrodonts are from the late Jurassic, still 40 million years after the alleged transition took place from the cynodont reptiles. Romer *speculates* about the possible interpretations of the evidence and the possible relationships between themselves and ‘later’ mammals, (pp. 199-201), but it is not until the late Cretaceous that he at last finds representatives of the two greatest living groups of mammals—the marsupials and the placentals. (p. 202-3) This is truly wondrous—over 100 million years of alleged evolution without conclusive evidence! I often wonder if evolutionists really think about how long is a million years let alone 100 million and consider the implications of such scanty evidence over such an immense amount of supposed time.

Of marsupial fossils in the late Cretaceous, Romer (p. 200-2) says—“it has often been *assumed* that the marsupials are the ancestors of the placentals . . . yet indeed we know primitive placentals *fully as old* as the earliest opossum-like (marsupial) forms.” He speculates further—“Surely both the marsupials and the placentals have arisen from a common ancestral stock of which, as yet, we know little of early Cretaceous age.” Unfortunately for Romer and other evolutionists, the placentals and the marsupials are *clearly divided* from their first appearance! Such are the ‘facts’ of Mesozoic history!

As stated earlier some evolutionists believe that the reptile *Morganucodon* is actually a primitive mammal and possibly ancestral to monotremes, but T. J. Dawson (p. 4) admits:

It is perhaps paradoxical that of the two basic types of early mammals, the Morganucodontidae were the most common and widespread, and yet the details of their relationship to their supposed descendants, the monotremes, are *still largely a mystery*.

Now the alleged time gap between *Morganucodon* of the late Triassic/early Jurassic and the *first* monotreme fossil remains of the Miocene amounts to over 150 million years. (T. J. Dawson, p. 7). With the scrappy evidence and the supposed enormous time gap it is no wonder that evolutionists find themselves in

such confusion as to which bone or tooth belongs to which type of animal or alleged lineage. With regard to the sometimes claimed transitional nature of marsupials as being the in-between stage from monotreme to placental, T. J. Dawson (p. 51) is candid enough to admit that this assumption of inferiority of marsupials, especially in reproduction, is not correct.

### The Biochemical Evidence

It is not only creation scientists who are attacking the modern evolutionary scenario. Even from within the general orthodox establishment, a number of authorities have recently stated their disillusionment with the state of the theory such as William Fix (1984) and Francis Hitching of the Royal Society (1982). A noted non-creationist Australian microbiologist, Michael Denton has now made a devastating attack (1985) based not only on the hostility of the fossils, but also on the molecular evidence.

Many people do not know that there are two powerful evidences against evolution. One is a negative factor—the virtual total lack of major, sound transitionals which are *absolutely required* to prove evolution beyond reasonable doubt. The other is a positive factor—the hostility of the comparative molecular structures which show unequivocally that late biochemical studies clearly reinforce the absolute division and separateness of each of the major classifications—families, orders, classes and phyla. This evidence I summarize from Denton’s work. (pp. 274-306).

The amino acid sequence of a protein from two different organisms can now be readily compared, the differences between them can be quantified exactly and these results provide a novel approach to measuring the differences between species. Different types of protein exhibit different degrees of interspecies variation. Using the protein Cytochrome C, haemoglobin and other proteins, Denton found that organisms which were close in terms of their haemoglobin sequences were also close in terms of their cytochromes, *and the same was true of all other proteins examined*. He also found that the differences between organisms at the molecular level *corresponded with their differences at the morphological* (taxonomic) level. Thus biology now is armed with a strictly quantitative means of measuring the distance between two species and of determining the biological relationship.

It is now possible to use the cytochrome sequences to classify species into groups and these groups correspond precisely with the major groups arrived at on traditional morphological grounds.

After listing the findings (pp. 278-9), Denton states that the most striking feature is that each identifiable sub-class of sequences is *isolated and distinct* and that *no sequence or group of sequences* can be designated as intermediate with respect to other groups. Transitional or intermediate groups are *completely absent*. Using 33 comparisons between the bacterial cytochrome C of *Rhodospirillum rubrum* and non-bacterial eukaryotic cytochromes from man, lamprey (jawless fish), fruit fly, wheat and yeast, *all exhibit* a sequence divergence of between 64 and 67 percent! Dr. Denton correctly states that this is one of the most astonishing findings of modern science because it means that no

eukaryotic cytochrome is intermediate between the bacterial cytochrome and the others; as far as the bacterium is concerned, all the eukaryotics are *equally distant*.

The bacterial kingdom has no neighbor in any of the diverse eukaryotic types and as Denton says, the links are well and truly missing. Exactly the same pattern is observed in all the higher groups and Denton found that when comparisons are made between insects and vertebrate groups, no group is found to be primitive or in any sense a link between the phyla Arthropoda and Vertebrata; each group is isolated and unique.

Exactly the same result was found *within* the phylum Chordata (vertebrates). Not only are the jawless fish (cyclostomes) separated cleanly from other vertebrates, the others are also *cleanly separated from each other*; i.e. bony fish, amphibia, reptiles and mammals. Denton says:

There is *not a trace* at the molecular level of the traditional evolutionary series cyclostome to fish to amphibia to reptile to mammal! Amphibia, always considered intermediate between fish and other terrestrial vertebrates are in molecular terms *as far from fish as any group of reptiles or mammals*. This is astonishing. (p. 284)

Obviously the mammal-like reptiles cannot be compared as they are all extinct but as Denton works down through classes to orders and even down to families, *exactly the same* result is apparent. For instance, monkeys are totally distinct from the gibbons, apes and humans as a group and in turn, these themselves are just as equally isolated from each other!

Denton comments (p. 290) "Thus the molecules, like fossils, have failed to provide the elusive intermediates . . ." On p. 292 he states—". . . none of *the types* deemed intermediate or primitive (lamprey, opossums, monotremes) by generations of evolutionary biologists shows any sign of their supposed primitive status."

Of course Denton is well aware of the claim that the biochemistry of ancient forms may have changed from their modern descendants in respect of the so-called molecular clock hypothesis, but his response (p. 291) is simple: "The only way to save evolution . . . is to make the ad hoc assumption that the degree of biochemical isolation of the major groups was far less in the past . . . There is however absolutely *no objective evidence* that this assumption is correct." Further, the molecular structure would have had to alter dramatically *without corresponding major changes* in the morphology of the so-called living fossils.

Denton goes into much detail on this subject and impressively answers possible objections and he delights in referring to the living fossils, especially lungfish. The proteins of lungfish are just as far from the lamprey as any other fish, amphibian or mammal group! He points out that the lungfish is morphologically very close to its Devonian relatives while one of its closest relatives has undergone profound physiological and morphological changes which culminated allegedly in mammals and man himself! It is very difficult to understand why a protein functioning in the basically unchanging physiological environment of the lungfishes' red cell should have undergone *precisely* the same number of beneficial mutations as a related

protein evolving in a line subject to such global adaptational changes.

Denton (p. 306) sums up his 32 page chapter by observing—" . . . in the face of this extraordinary discovery, the biological community seems content to offer explanations which are no more than *apologetic tautologies*."

### Summary and Conclusions

It seems virtually certain that if we had living cynodonts and other therapsid types to examine, we would quickly see that these groups were unique animals within class Reptilia and would stand in relationship to each other and to mammals in somewhat the same way as marsupials and monotremes stand within class Mammalia to each other and to other types. When all the evidence is carefully considered, we find there is no need whatever to postulate evolutionary relationships between any of the major groups and in fact the separateness and uniqueness of all the higher taxa indicates a created origin for them with considerable variability *within* each group. A creationist explanation fits the evidence better than the transformist hypothesis.

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## HISTORICAL PERSPECTIVES

## NATURALISTIC OUTLOOK

CLARENCE B. CARSON\*

## Abstract

*The ascendance of naturalism over supernaturalism in American intellectual circles is traced. The effect of the acceptance of the evolutionary hypothesis in American literature, economics and sociology is reviewed.*

## Introduction

A new wave of ideas swept over America in the latter part of the 19th century. They were not entirely new ideas—there are few enough of those, in any case—, but many of them were certainly given new twists. The one idea which entranced, gripped, and served as a kind of illumination for, many intellectuals was the idea of evolution. Not simply biological evolution—though that was central—but evolution applied in every direction, for many thinkers came to see everything through evolutionary lenses, so to speak. Undoubtedly, some thinkers were questing for a natural explanation for the world and all that in it is, and evolutionary ideas and theories appeared to provide an answer for them.

Once again, the formulations of these ideas were done mostly in Western Europe and were taken up in America afterward. Not only were these ideas naturalistic in character but they were also generally opposed to supernatural and older philosophical and metaphysical ideas. Science, or *scientism*, was replacing philosophy, man replacing God at the center in men's minds, and history replacing metaphysics. Romanticism, by exalting feeling and insight, tended to downgrade reason. As reasoned philosophy lost its disciplinary hold on thinking, thinkers turned more and more to explanations with a single idea at the base (ideology). This tendency was further reinforced by the increasingly monistic character of thought. Monism is the belief that there is only a single level of reality, that it is material or physical, for example. This is in contrast with earlier views of reality that it is dual or multiple, i.e., physical and metaphysical or material, mental, and spiritual. Thus, philosophers had usually provided much more complex explanations of reality. But with the breakdown in philosophy and the tendency toward monism, such systems of thought as appeared were often based upon a single idea.

The 19th century has sometimes been referred to as an Age of Ideology. Certainly, many ideas and doctrines were brought forth in Europe in that century, and some of them have had a powerful impact on the world in the 20th century. An ideology may be defined as a system or doctrine based on a single root idea. They quite often have an "ism" suffix, as in socialism, though it has become so common to use this suffix on words that they do not always signify an ideology. The appearance of some of these "isms" is indicated by this description in an European history:

So far as is known the word 'liberalism' first appeared in the English language in 1819, 'radicalism' in 1820, 'socialism' in 1832, 'conservatism' in

1835. The 1830's first saw 'individualism,' 'Constitutionalism,' 'humanitarianism,' and 'monarchism.' 'Nationalism' and 'communism' date from the 1840's. Not until the 1850's did the English-speaking world use the word 'capitalism' . . . (Palmer and Colton, 1958, p. 431).

Such words and systems continued to pour forth in the ensuing years, such words as "Darwinism" and "Marxism." And, as the historians say, "Without the 'isms' created in the thirty-odd years after the Peace of Vienna it is impossible to understand or even talk about the history of the world . . ." (Palmer and Colton, 1958, p. 431).

From the mid-19th century onward there was, if anything, more of an ideological bent to the thought systems that were brought forth. Moreover, the idea of evolution became a galvanizing agent for a variety of ideologies, giving them a thrust and vigor they did not have before that.

## Theories of Evolution

Evolution was in the intellectual wind for most of the 19th century. This was so while the idea of biological evolution was still only a quaint theory which some person here or there had advanced. Romanticism provided the setting by shifting the focus of thought from the enduring features of things (their natures) toward that in which they were unique, different, and individual. It emphasized change and growth. Undoubtedly, too, such developments as rapid population growth and increasing technological change reinforced the sense of a prevalent growth and change. In any case, thinkers began casting about for explanations of change, seeking for the laws of development and change, and even for methods of predicting the course of changes to come.

The German philosopher G. W. F. Hegel developed a full-fledged theory of how change takes place. The method is called the dialectic. Change, Hegel held, results from the ideas that men hold. These change dialectically, that is, out of the contest between two conflicting ideas. First, there is the thesis (the proposition, idea, or theory), then its opposite, the antithesis. Out of the contest over these, there comes the synthesis, the resolution of these opposing views which contains elements of both of them. The synthesis, in turn, becomes a new thesis, and the process goes on and on, not around in a circle, however, but upward in progressive improvement. Hegel was the major German philosopher of the first half of the 19th century, and, when German philosophy became an influence on America in the second half, he had followers in this country.

The French sociologist Auguste Comte set forth a scheme that attempted to explain the development of mind and society through three successive stages. The

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first stage he called theological, when thinkers explained things in terms of religion. The second stage was metaphysical, when explanations were in terms of abstractions, and the third—final and highest—stage was the scientific, by which he seems to have meant mainly the factual stage. The scientific stage he also referred to as the Positive stage, and, in connection with it, he set forth what might be loosely called a religion or Positivism. It might better be called a religion of the worship of humanity or *humanis*, to use one of his words, for he said:

Towards Humanity, who is for us the only true Great Being, we, the conscious elements of whom she is composed, shall henceforth direct every aspect of our life, individual and collective. Our thoughts will be devoted to the knowledge of Humanity, our affections to her love, our actions to her service. (van Baumer, 1967, p. 491)

Although these ideas were important ingredients in the New (secular) Humanism which arose, the central point here is that he was explaining change in terms of successive stages of development.

But it was Herbert Spencer, the English synthetic philosopher of the mid-19th century, who set forth a scheme of universal evolution for the English speaking world. Everything is undergoing change, Spencer held not simply random change, but change which is moving in the direction of fulfillment and perfection. All this was supposed to be occurring according to the law of change and progress. He described the mode of the change in this way:

Evolution . . . is a change from a less coherent to a more coherent form. This is the universal process through which sensible existences, individually and as a whole, pass during the ascending halves of their histories. (Schoenwald, 1965, p. 151).

The end toward which this "universal process" moves, according to Spencer, is progress:

Progress, therefore, is not an accident, but a necessity. Instead of civilization being artificial, it is a part of nature; all of a piece with the development of the embryo or the unfolding of a flower. The modifications mankind have undergone, and are still undergoing, result from a law underlying the whole organic creation; and provided the human race continues . . ., those modifications must end in completeness . . . (van Baumer, 1967, p. 505).

Spencer focused entirely upon the changing, ignoring the enduring and reducing the eternal to a remote Unknowable. Even so, his works enjoyed a wider circulation and greater popularity than had those of any thinker of his depth before him. He wrote many volumes, and the sales of his books in America from the early 1860s to 1903 amounted to 368,755 copies. Henry Holt, the publisher, said, "Probably no other philosopher ever had such a vogue as Spencer had from 1870 to 1890." (Hofstadter, 1959, p. 34). At least one general theory of evolution had been thoroughly publicized.

### Biological Evolution

It was Charles Darwin's theory of biological evolution, however, presented in his book, *The Origin of*

*Species*, which set the intellectual world afire after 1859. Up to that time, "the development theory," as it was then called, had not made great headway, though it was widely known. Even Spencer's great vogue came mostly after the publication of *The Origin of Species*.

Charles Darwin was hardly the first person to propose that species of plants and animals had evolved and new ones emerged in the course of time. Indeed, the idea had been advanced among the ancient Greeks, but was generally rejected during the classic age of Greek thought. It was revived in the late 18th century by French thinkers, most notably by Jean Baptiste Lamarck. Lamarck believed that higher and more complex forms of life had developed from simpler forms by natural processes. He thought this might come about through the inheritance of acquired characteristics. However, this theory was never widely accepted. Charles Darwin's grandfather, Erasmus Darwin, also studied animal life extensively and advanced the idea that all the higher forms of life could have developed from a single simple beginning.

Herbert Spencer, too, put forth the idea that new species arise naturally by way of development, several years before Darwin did. Indeed, Spencer sent Darwin a copy of one of his books dealing with the subject, to which Darwin replied:

Your remarks on the general argument of the so-called development theory seem to me admirable. I am at present preparing an Abstract . . . on the change of the species; but I treat the subject as a naturalist, and not from a general point of view, otherwise, in my opinion, your argument could not have been improved on, and might have been quoted by me with great advantage. (Schoenwald, 1965, p. 121).

Moreover, another Englishman, Alfred Russell Wallace, arrived at virtually the same conclusions as Charles Darwin before *The Origin of Species* was published. In 1858, he sent a paper to Darwin which explained his theory. Darwin was astounded. "I never saw a more striking coincidence," he wrote Sir Charles Lyell, "if Wallace had my Ms. sketch written out in 1842, he could not have made a better short abstract! Even his terms now stand as heads to my chapters . . ." (Irvine, 1955, p. 42). Darwin got up a short abstract of his ideas so that they could be presented alongside those of Wallace.

In any case, Darwin's *Origin of Species* made the great impact for evolution. Indeed, the concept of evolution became more or less synonymous with Darwinism after the publication of his book. Theories of biological evolution had generally been rejected or ignored before Darwin's work was published. Thereafter, it soon became the dominant theory and bade fair to replace all others. Three decades after Darwin's work appeared, Alfred Russell Wallace declared, with not any great exaggeration:

The whole scientific and literary world, even the whole educated public, accepts as a matter of common knowledge, the origin of the species from other allied species by the ordinary process of natural birth. [Moreover, he continued,] . . . we claim for Darwin that he is the Newton of natural history, and that . . . Darwin, by his discovery of the

law of natural selection . . . [has] not only thrown a flood of light on the process of development of the whole organic world, but also established a firm foundation for all future study of nature. [Brackets added.] (van Baumer, 1967, p. 533)

Darwin focused his attention almost from the outset of his studies upon varieties of plants and animals within species. He came to believe that some varieties developed away from the original species over long periods of time until eventually they emerged as a new species. The process of development of superior varieties or breeds had long been well known among animal breeders. They select the hardier specimens, or those with the most desired characteristics, generation after generation, and thus are able to develop distinct breeds (as in horses, cows, and other domestic animals). If this process were carried on long enough, Darwin thought, perhaps tens or hundreds of thousands of years, a new species could emerge. But human selection could hardly account for the process by which species had originated; it had to occur in nature if all plants and animals (including man) had developed in this fashion.

The key Darwin hit upon was *natural selection*. He borrowed the idea of *struggle for survival* from Malthus, and the idea of *survival of the fittest* from Spencer. Darwin noted, as have others, that plants and animals reproduce in prodigious quantities; they multiply much more rapidly than does the means for their survival. In consequence, a struggle for survival goes on in nature, especially among those of the same species. Variations, which become the basis of varieties, enable some to survive while others die out. These "fittest" which survive in the struggle for life develop along paths which may eventually lead to new species, Darwin held. He also believed that *sexual selection* might have played a role, at least among the higher animals. (Thus, if gentlemen do indeed prefer blondes, blonde would presumably become the dominant hair trait.)

If Darwin had contented himself with merely stating his theory, or more correctly, hypothesis, it might have fared little better than others which had preceded it. But he did much more than that. He did present his hypothesis, indeed, gave it first place in the book. He considered it important, too, that he gave much thought to the objections that would be raised and dealt with them at some length. Beyond that, however, and probably much more important for the acceptance of his hypothesis as a valid theory, he summarized a vast amount of material which he submitted as evidence for his case. This gave to the work as a whole the appearance of scientific (or factual) support, something which greatly impressed many of his contemporaries. Indeed, Darwin had been collecting geologic, botanic, and zoological evidence for 25 years before he published the *Origin*. His five-year voyage on the *Beagle* enabled him to collect a vast assortment of information from other places in the world. After returning to England, he spent many more years collecting and studying all sorts of anecdotes, specimens, plants and domestic and wild animals. In his book, he brought this tremendous array of information to bear, or arranged it in such a way that it gave support to, his thesis on natural selection and biological evolution. Darwin had a well-established reputation as a

careful observer and faithful reporter before he published his most impressive work. When he assembled this information behind such a broad and comprehensive thesis as biological evolution, he accomplished a *tour de force*. It commanded attention.

Darwin did not leave it entirely to chance, however, that his book would have an impact. Many books are published; few change men's minds to any extent. Not only had he already established a reputation as a naturalist by publishing books and articles, but he also cultivated others in the field to bring them around gradually toward his view for years before he published the *Origin*. He corresponded with and conversed much with Sir Charles Lyell, who was a leader in his field. He corresponded with Asa Gray at Harvard, who became his champion in America. Above all, he had almost persuaded T. H. Huxley before his book appeared, and afterward Huxley became a one-man publicity manager for Darwin's explanation (though he harbored some misgivings about the mutability of the species). Most likely, Huxley was more concerned with seeing to it that a natural explanation get a fair hearing than with Darwin's particular hypothesis, but he served Darwin well nonetheless.

Darwin believed that he had hit upon an explanation, and a grand one at that, for the development of all life forms. He described it this way:

thus, from the war of nature, from famine and death, the most exalted object which we are capable of conceiving, namely, the production of higher animals, directly follows. There is grandeur in this view of life, with its several powers . . . that . . . from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved. (Irvine, 1955, p. 96)

If there had been any doubt that Darwin believed that man evolved from lower animals, he removed it with the publication of *The Descent of Man* in 1871. In this exceedingly long treatise, Darwin maintained that man had evolved from some ancestor of the ape, or at least that man had common ancestors with these creatures. He attempted to explain how consciousness, conscience, moral sentiments, and man's more or less peculiar features might have evolved. Much attention was devoted to describing attributes in lower animals which bear resemblance to those much more highly developed in man. Ultimately, he maintained, the differences between man and the lower animals are differences of degree, not of kind.

#### **Critique of Darwinian Evolution**

Almost from the beginning, the belief in the natural origin of the species—and, more broadly, of the natural development of all things—has been a kind of faith. The faith can be called Darwinism, naturalism, or evolutionism, or something on that order. It is an essential ingredient in the faith that is nowadays often referred to as secular humanism.

Evolutionism is held to as a faith, however, not because names have been applied to the believers but because they exhibit an attitude of faith toward it rather than submitting it to logical and evidential tests. To demonstrate that this is the case, it may be helpful to apply some of these tests to it here.

The theory of natural evolution of the species is fraught with difficulties. In the first place, despite the claims made for its scientific validity, it is basically an historical proposition, not scientific validity, it deals with events and developments that are supposed to have taken place in the course of time. Very precisely, it is natural history, not human history, such as we ordinarily encounter. Even so, the rules of evidence that apply in history generally are the ones that basically apply to it. Moreover, the crucial events alleged—namely, that new species emerged naturally—are hypothetical. There are no witnesses to the events, and such evidence as there is that they ever occurred is negative. There is evidence, of sorts, that some species appeared later in time than others, but it is negative, i.e., no remains have been found of particular species in earlier deposits of remains. The absence of evidence does not prove anything. If the crucial events had been proved, then Darwin's explanation might be correctly described as a theory of evolution. As matters have stood, since the crucial events are hypothetical, Darwin's explanation is at best a theory to explain a hypothesis. Darwin's evolutionary hypothesis can be stated this way: *If* new species occurred in the process of natural development, the process *might* have taken place much as Darwin imagined it.

The scientific difficulties with the Darwinian hypothesis are, if anything, even greater than the historical ones. One of the most striking facts in nature is the tenacious persistence of species. A species is most readily distinguished from other species by the fact that males and females within it may mate and produce potent offspring. Simply stated, like begets like, and offspring do likewise in an apparently endless chain. Under man's guidance there has been some breeding across the apparent line between species. The offspring are hybrids, which are either sterile or unpredictable. The classic example of a hybrid is the mule. The mule is the predictable result of breeding a donkey and a horse. But the mule is sterile, i.e., cannot normally produce offspring. Every mule is the end of the line, normally.

Darwin tried to get around these various difficulties by positing the development of new species which diverged farther and farther from the parent species over a vast span of time. In short, the change would occur so gradually that the emergence of a new species would involve only infinitesimal changes over hundreds of years, say. Looked at in this way, there never would be anything which an historian might call an event in the emergence of a new species. That sort of disposes of one problem, but it gave rise to another. Namely, there would need to be numerous gradations of beings in the gaps between species. To get from monkey to man, for example, proof would require evidence of creatures who became more and more manlike and less and less monkeylike. Darwin was quite aware that he did not have these, so to get around the difficulty he posited "missing links," beings which *must* have existed at one time because they are necessary to the proof of his theory. Contrary to what has been widely believed, there is not simply one "missing link" but innumerable ones that would fill in all the gaps between species.

The mountainous evidence accumulated by Darwin provided abundant proofs of the development of varieties, strains, and breeds *within* species. That is, he proved many times over what nobody much doubted in the first place. Domestic plants and animals have long been subject to selective planting and breeding to produce plants and animals with the desired characteristics. It may be, too, that Darwin's (and later accumulations) points to a natural process whereby hardy varieties are developed and sustained. That is, it may be that Darwin contributed to our understanding of an evolution *within* species. But he did not prove the evolution of the species, nor establish as fact the method by which it occurred. Those who believe this take it on faith, not because it has been shown to be true. Undoubtedly, those who believe that God created man in His image, that He created the other species and gave man dominion over them, accept this on faith also. The latter are aware of and avow their faith; the former conceal theirs under a scientific gloss.

### The Impact of Evolution

Darwinism sent shock waves into all areas of thought, shock waves which have not yet spent themselves. Darwinism was brought forth in a framework in which the idea of evolution as a natural explanation of all sorts of developments was gaining sway. When its claims were accepted, they provided confirmation of evolution in a most vital area.

Both Darwinism and the general idea of evolution had as great an impact upon the United States as upon England, if not greater. The popularity of Spencer has already been noted. His leading disciple in America was William Graham Sumner, but there were many others. The contacts between Darwin and Asa Gray at Harvard have already been noted, and Gray became a leading exponent of Darwin's ideas in the United States. Louis Agassiz, also of Harvard, was a vigorous opponent of Darwin's theories, but the theory of biological evolution gained ground rapidly in this country nonetheless. John Fiske, historian and philosopher, made evolution much more congenial for theists by describing it as being the way God works in the world. Far from being overwhelmed by any notion of man as simply a littler higher animal, Fiske declared that "the whole creation has been groaning and travailing together in order to bring forth that last consummate specimen of God's handiwork, the Human Soul." (Commager, 1954, p. 85). But if Darwinism was to serve as the basis of philosophy or ideology, which it certainly did, there were other directions in which it could and was pointed.

The most general impact of the idea of evolution was to focus attention on the changing and mutable features of reality and to downgrade or ignore the enduring, the fixed, and the eternal. Indeed, to a thoroughgoing evolutionist it often seemed as if there were no fixed or enduring features to reality. Looked at broadly, all was in a state of flux, alteration, adaptation, and adjustment. Everything seemed to be relative to time and place and to everything else. The idea of *relativism* was given great impetus by Darwinism, and early in the 20th century Albert Einstein made public his general theory of relativity, bringing the whole universe under its rule. Fixed points and enduring laws tended to recede into the background or fade out of mind.

God was, for Herbert Spencer, the Unknowable, but for many contemporary intellects, He was most apt to be the Unknown. As a youth, Charles Darwin had begun studies which would lead to a career in the church, but he abandoned that for science. In the course of his life, he drifted away from earlier religious beliefs, though he usually took pains to avoid religious controversy. Not so, T. H. Huxley, an agnostic—a word which he invented to indicate that he did not know whether or not there is a God—, for he tangled with the clergy whenever the occasion arose in his career. Adam Sedgwick, a geologist, declared of Darwin's theory of natural selection that it was a "dish of rank materialism cleverly cooked and served up merely to make us independent of a Creator." (Barzun, 1958, p. 37). Undoubtedly, Darwinism caught on because it offered a natural explanation, and, while it might not dispose of the need for some sort of Beginner, if not Creator, it certainly required no more than a most remote God.

Indeed, the German philosopher, Friedrich Nietzsche, proclaimed that God is dead. His meaning, we may suppose, is that the belief in God is no longer supportable. If that were the case, it certainly portended great changes to come, for without God, much would surely be different. But

the event itself is far too great [Nietzsche said,] . . . for even the report of it to have reached . . . many people . . ., to say nothing of their capacity for knowing what is really involved and what must all collapse, now that this belief has been undermined . . . [He foresaw a] prolonged excess and continuation of demolition, ruin and overthrow which is now impending . . . [Brackets added]. (Weber, 1959, p. 672)

The whole system of morality would collapse, he thought, and much that had restrained men in times past. "Man has one terrible and fundamental wish," Nietzsche declared; "he desires power, and this impulse, which is called freedom, must be the longest restrained." (Black, 1964, p. 472). While Nietzsche professed to greet the coming era without God as a new and "rosy" dawn, he did correctly foresee the destruction that might follow when the will to power was released from restraints and exercised by tyrants, as has been the case in many lands in the 20th century.

Strangely, man without God cannot acquire knowledge. He can acquire reams upon reams of more or less factual information, of course, as men have busied themselves at doing ever more vigorously since the latter part of the 19th century (and devised ever more effective means to spread it), but it does not add up to knowledge or truth. Without God, we lack a first and final premise for knowledge, a Knower in whose information is knowledge, a fixed point from which to proceed to get knowledge. That is the ultimate source of the relativism of this age, of which evolutionist relativism is a reflex. None of this is meant to suggest that men have ceased entirely to believe in God generally since that time. That is hardly the case universally. What has happened, however, is that belief in God has become increasingly unsprung from intellectual endeavor, resulting in deep wounds both to religious belief and to intellectual endeavor.

One other general impact of evolution and Darwinism needs to be discussed before turning to some particular applications of them. The thrust of this revolution in thought was for history to replace philosophy (and theology). To put it another way, the study of virtually everything tended to become a study of its history. (Notable exceptions were chemistry, physics, and mathematics, though there have been strenuous efforts to place mathematics and physics into a relativistic framework, e.g., the "new mathematics.") Thus, philosophy tended to become the history of philosophy, literature the history of literature, political science the history of political development, biology the history of the evolution of plants and animals, theology the history of religions, economics the history of economic institutions, and so on. The focus everywhere tended to be on how things had evolved, whether the subject was animals, monotheism, or government.

The quest was on, too, for the "laws" of historical development or evolution. A major shift occurred in the meaning and significance of natural law, so far as thinkers continued to believe in it at all. At the time of the founding of the United States, people had usually thought of natural law as principles of regularity imbedded in things; they were metaphysical, that is, underlying the physical. These laws were conceived of as the framework within which actions and events occur, potentialities until someone or something had acted. Thus, natural laws determined effects, but were not causes. In the historical framework which had come to prevail in intellectual circles in the latter part of the 19th century, metaphysics had been largely abandoned. Natural laws were now thought of as forces, causes, if you will, which explained the course of development. Natural-law-as-force was the cause of things happening, not the result of human and other behavior. Thus, thinkers spoke of the forces which produced change. The evolution of all things came to be widely thought of as the result of natural forces at work in the world.

Man, too, was in this forceful stream of causation of natural development. It could hardly be otherwise for those who believed in evolution as a natural process and in man as a product of natural evolution. It was an easy step from this to the belief that human behavior was *determined* by these causative forces. The mind and will were not free; they were in a stream of causation which determined them. Thus, thinkers and writers cast about and came up with theories of determinism. Biological determinism lay ready at hand as an explanation for how behavior was determined. Those who emphasized this would focus upon heredity as a primal cause of human behavior and development. After all, heredity must surely be the main causative factor in biological evolution. Ominous racial theories grew out of these beliefs. But environment also was often conceived as playing a large role, and environmentalism was another determinism that gained currency. John B. Watson, an American psychologist, developed a thoroughgoing mechanical view of the role of the environment with his stimulus and response theory. Deterministic theories tended not only to cut away any belief in the freedom of action or choice of man but also any personal responsibility for acts.

Probably, the other most prominent determinism was economic determinism. Karl Marx was the most vigorous proponent of this view. He held that control over the instruments of production determined social organization, and that "it is not the consciousness of men that determines their existence, but, on the contrary, their social existence determines their consciousness." Hillquit, 1909, p. 63).

It should be noted, too, that as more and more things came to be viewed historically, what had been thought of as history lost much of its meaning. History becomes largely the story of how things got to be the way they are, plus some attempt to discover trends that would show the way they were going. Some historians boldly proclaimed in the early 20th century that there were no lessons to be learned from history. Historian J. H. Robinson (1912, pp. 17-8) said:

It is true that it has long been held that certain lessons could be derived from the past . . . But there is a growing suspicion . . . that this type of usefulness is purely illusory . . . Their value rests on the assumption that conditions remain sufficiently uniform to give precedents a perpetual value, while, as a matter of fact, conditions . . . are so rapidly altering that for the most part it would be dangerous indeed to attempt to apply past experience to the solution of current problems.

H. E. Barnes (1925, p. 589) thought the very idea of seeking truth from the past was hilarious:

Not even a Texas Methodist Kleagle, [he argued] would think of taking his car to Moses, Joshua, Luther, or George Washington to have the carburetor adjusted or the valves ground, yet we assure ourselves . . . that we ought to continue to attempt to solve our contemporary problems of society, politics and conduct on the basis of . . . information which in many cases far antedates Moses. [Brackets added.]

If all is indeed changing, as many evolutionists came to believe, if there is only history, the ironic truth seems to be that history does not matter much.

### Naturalism in Literature

The 19th century was the age of the novel in literature. Poetry had been revived considerably during the surge of romanticism in the first half of the 19th century, but it succumbed once more to the prosaic character of the times after the Civil War. Walt Whitman lived for many years after the war, but he no longer brought forth thunderous poetry to match his earlier *Leaves of Grass*. Newspapers increased in number and even more impressively in circulation between the Civil War and World War I, but the journalistic mode had not yet come to dominate as a literary form. The essay was an important means of expression, and there were a number of quality magazines of opinion and information, such as *The Atlantic Monthly* and *Harper's*. By the 1890s popular magazines, such as *Ladies' Home Journal* and *Collier's*, were making an impact with their stories and articles.

But the novel had come into its own as the most important vehicle of literary expression. It focused on the individual, gave scope for the full development of

the rise and fall of individuals, in an era when individualism was highly prized as a way of life by Americans. All sorts of novels were published, ranging from romances, to poor-boy-makes-good-in-the-big-city stories of Horatio Alger, to utopian scenarios, to realistic ones which depicted the details of life and living with great exactness. Some of the most enduring of the literature produced during the period is often described as "local color." This refers to short stories and novels mainly which are based on some particular locale in the country and try to capture its particular flavor and character. Edward Eggleston, who wrote about life in the Midwest, explained what moved him to do local color for his region this way:

It used to be a matter of no little jealousy with us . . . that the manners, customs, thoughts, and feelings of New England country people filled so large a place in books, while our life, not less interesting, not less romantic . . . had no place in literature. It was as though we were shut out of society. (Williams, Current and Freidel, 1959, p. 80).

His best known book was *A Hoosier Schoolmaster*. George Washington Cable wrote stories of Louisiana, Sarah Orne Jewett of New England, and Joel Chandler Harris captured the flavor of Black stories and dialect in Georgia in his account of *Uncle Remus*.

Mark Twain (born Samuel Clemens) was much too versatile in his writings to place him in a single category. He was a humorist, a satirist, a writer of local color, and an accomplished teller of tall tales in the American vein. In *Roughing It*, he described life on the frontier, and *Innocents Abroad* captured the contrast between European and American ways. But he endeared himself to generations, especially of the young, with *The Adventures of Tom Sawyer* and *The Adventures of Huckleberry Finn*. Once read, who can ever forget Tom's attending his own funeral, or beguiling other boys into whitewashing Aunt Polly's fence, or Nigger Jim and Huck on their journey down the Mississippi? Henry James was the studied master of the realistic novel, and William Dean Howells was the leading literary critic of the period.

It is the naturalistic writers, however, that fit most nearly into the theme of this chapter. It might be supposed that naturalism in literature is closely akin to realism, but that is only the case, if at all, in a perverse sort of way. The naturalists tended to conceive of man as a part of nature, devoid of heroism, idealism, and having only a veneer of civilization. "Animalism" might capture the thrust of naturalism better, for naturalistic writers focused on man as a barely tamed animal. They could fully exploit by way of imaginative novels some of the conclusions that seemed to follow from the theory of evolution and Darwinism. Some had read or studied the evolutionists and were quite carried away with the ideas. "To give up Spencer," Jack London had one of his characters say, "would be equivalent to a navigator throwing the compass and chronometer overboard." (Cowley, 1956, p. 304). Theodore Dreiser read Huxley and Spencer, and they had a fateful impact on his writing. Until he had read Huxley, he said, he had at least a lingering belief in Christianity, but afterward he concluded that the Old and New Testaments were "not compendiums of revealed truth

but mere records of religious experiences, and very erroneous ones at that . . .” From Spencer, Dreiser discovered all:

I deemed substantial—man’s place in nature, his importance in the universe, this too, too, solid earth, man’s very identity save as an infinitesimal speck of energy or a “suspended equation’ drawn or blown here and there by larger forces in which he moved quite unconsciously as an atom . . . (Cowley, 1956, p. 303).

But wherever they picked up the ideas, whether by reading original evolutionists or getting their ideas second or third hand, the novelists embodied them in the stories of their characters. For Jack London, who wrote such novels as *The Sea Wolf*, *The Call of the Wild*, and *White Fang*, man was apt to revert at any time to his animal nature:

Civilization has spread a veneer over the surface of the soft shelled animal known as man. It is a very thin veneer . . . Starve him, let him miss six meals, and see gape through the veneer the hungry maw of the animal beneath . . . Touch his silly vanity, which he exalts into high-sounding pride, call him a liar, and behold the red animal in him that makes a hand clutching that is quick like the tensing of a tiger’s claw, or an eagle’s talon, incarnate with the desire to rip and destroy. (Commager, 1954, pp. 110-1).

Frank Norris, the author of *The Octopus* and *The Pit* described one of his characters as afflicted by a “foul stream of hereditary evil.” The greater emphasis, however, was usually on the role of the environment in shaping men’s lives. Stephen Crane, author of *Maggie*, *A Girl of the Streets*, said that the novel shows “that environment is a tremendous thing and often shapes lives regardlessly.” (Cowley, 1956, p. 315). Theodore Dreiser, in a spate of novels from *Sister Carrie* to *An American Tragedy*, depicted characters caught in the grip of forces which they could not withstand or overcome.

Naturalistic novelists could and did give impact to the idea that man’s behavior is determined by forces—instinctual, hereditary, environmental, and social—beyond his control. They could give imaginary flesh and blood to a dubious theory. Moreover, if man’s behavior is determined in this way, he is not responsible for it or to blame for the consequences of his acts. This was a powerful idea, corrosive both to morality and to traditional ways of apportioning responsibility. It pointed, too, toward the conclusion that the individual acting alone was powerless to deal with life and thus gave impetus to collectivism in the 20th century.

### Conservative Darwinism

Professor Richard Hofstadter called his book on the social and economic application of evolutionary ideas *Social Darwinism in America*. The term “Social Darwinism” has been widely used, following his lead, to applications in more than one direction. As historian Eric Goldman has pointed out, it is somewhat less confusing to refer to one application as “Conservative Darwinism.” While it is somewhat doubtful that any thoroughgoing application of evolution would be espe-

cially conservative, the distinction that he makes is an important one, and will be followed here.

In any case, the initial impact of evolutionary ideas only served to reinforce some of the prevailing ideas, and that could be called conservative. They gave added support to an already widely accepted belief in progress. If the fittest survive in the struggle, then here is a clear case both for believing that the latest is the best and that free competition among individuals is the way to achieve it. Moreover, Spencer and his disciples generally believed in free enterprise and opposed government regulation or intervention in the economy. For example, Spencer (1865, p. 334) said:

Fortunately it is now needless to enforce the doctrine of commercial freedom by any considerations of policy. After making continual attempts to improve upon the laws of trade, from the time of Solon downwards, men are at length beginning to see that such attempts are worse than useless. Political economy has shown us in this matter—what indeed it is its chief mission to show—that our wisest plan is to let things take their own course.

More broadly, it could be argued that any attempt to change the course of development by human activity would be to short circuit the benevolent process of progress.

The Spencerian idea of the survival of the fittest (incorporated into Darwin’s biological evolution as well) suited well the outlook of many successful businessmen. James J. Hill proclaimed that the “fortunes of railroad companies are determined by the law of the survival of the fittest.” (Hofstadter, 1959, p. 45).

John D. Rockefeller seconded these views enthusiastically:

The growth of a large business is merely a survival of the fittest . . . [In the process, many small businesses fall by the way. But that, Rockefeller thought, is the way of natural development.] The American Beauty rose can be produced in the splendor and fragrance which bring cheer to its beholder only by sacrificing the early buds which grow up around it. This is not an evil tendency in business. It is merely the working out of a law of nature and a law of God. [Brackets added.] (Hofstadter, 1959, p. 45).

Andrew Carnegie said that as a result of reading Darwin and Spencer, the “light came as in a flood and all was clear. Not only had I got rid of theology and the supernatural, but I found the truth of evolution.” And the truth, he thought, was that in a natural order progress took place onward and onward toward perfection. As for those who found fault with this struggle for survival, he had these words of counsel:

It is here; we cannot evade it; no substitutes for it have been found; and while the law may sometimes be hard for the individual, it is best for the race, because it insures the survival of the fittest. (Hofstadter, 1959, pp. 45-6).

William Graham Sumner put it bluntly when he said: “The millionaires are a product of natural selection, acting on the whole body of men to pick out those who can meet the requirement of certain work to be done

. . ." (Hofstadter, 1959, p. 46). The above views are sometimes referred to as "rugged individualism."

Most important, this concept of a natural order of the survival of the fittest which produced progress was a weighty argument against any reformist or revolutionary effort to change the political and economic system. Talk of reform, utopian visions, and socialist ideas were widespread in the last two or three decades of the 19th century. Conservative Darwinism provided a forceful argument against these. Of the people who presented such notions, Sumner (1954, p. 73) said:

These persons, vexed with the intricacies of social problems and revolting against the facts of the social order, take upon themselves the task of inventing a new and better world. They brush away all which troubles us men and create a world free from annoying limitations and conditions—in their imagination.

Such visions ignore the stage of civilization and the course of evolution, Sumner thought. Evolution had brought man to the industrial stage, he held; everyone is within this framework and unable to alter it. In Sumner's own vigorous words (1954, p. 94):

It controls us all because we are all in it. It creates the conditions of our existence, sets the limits of our social activity, regulates the bonds of our social relations, determines our conceptions of good and evil, suggests our life-philosophy . . .

In short, "the industrial organization" exercises an "all pervading control over human life." In an even more dramatic mood, Sumner (1954, p. 104) maintained that:

The great stream of time and earthly things will sweep on just the same in spite of us . . . It is only in imagination that we stand by and look at and criticize it and plan to change it. Every one of us is a child of his age and cannot get out of it. He is in the stream and swept along with it.

Such ideas had a considerable impact. Henry George, a man with a determined reformist bent himself, listened to a friend decry the ills besetting New York City in his day. George asked the man what he proposed to do about it. "Nothing," he replied, "you and I can do nothing at all . . . Perhaps in four or five thousand years evolution may have carried men beyond this state of things." (Goldman, 1956, p. 66).

Even so, Conservative Darwinism, if that is the right phrase for it, was a shortlived philosophy, so far as much popular following was concerned. There may have been some elements of truth in it, but Spencer and Sumner's evolutionary ideas provided highly unstable grounds for the defense of free enterprise, individual liberty, or American institutions. It attempted to ground the defense in a changing rather than an enduring order. Moreover, Sumner's view was so thoroughly deterministic that it did not appear to leave room for any significant human freedom. As for his defense of private property—which he believed was an invaluable institution—he thought that it "may give way at a future time to some other institution which will grow up by imperceptible stages out of the efforts of men to contend successfully with existing evils . . ." (Sumner, 1954, p. 82). In addition, Sumner repudiated

the natural rights doctrine which undergirds the Declaration of Independence and the United States Constitution. "There are," he said, "no rights against Nature except to get out of her whatever we can, which is only the fact of the struggle for existence stated over again." (Hofstadter, 1959, p. 59)

### Reform Darwinism

In any case, reformers did not wait long to claim Darwinism, the idea of evolution, and the idea of progress through gradual development, for their own. The idea of stages of development had been advanced by Saint Simon and Comte, reform-minded men, even before Spencer's or Darwin's ideas had made their impact. In that context, Darwinism has only served to cut the ground from under another very important fixity—that of the species. Reformers wanted to make fundamental changes, and the focus upon change turned out to be grist for their mill.

The American who is most often credited with having shifted the evolutionary argument in the direction of reform was an obscure sociologist by the name of Lester Frank Ward. Ward maintained that a new stage in evolution had been emerging for a long time. What made this stage possible, he claimed, was the appearance and development of the mind of man in the course of evolution. (The mind was to be greatly aided now, he thought, by the development of a science of sociology.) It was, Ward said, the "advent with man of the thinking, knowing, foreseeing, calculating, designing, inventing and constructing faculty, which is wanting in lower creatures . . ." This development repealed "the law of nature and enacted in its stead the psychological law, or law of mind." (Commager, 1954, p. 206).

This development having occurred, or so Ward alleged, it had now become possible to take over the development and direction of society. In the past, the development of society had occurred more or less naturally, without any clear line of control or planning. But now it could be taken over and directed. By whom? Undoubtedly, Ward would have nominated sociologists to do the social planning, or "social invention," as he sometimes called it. There should be no doubt, however, that what he had in mind was for government to control the process of social development. He wanted to set about "the improvement of social conditions through cold calculation . . ." The aim should be not "merely to alleviate present suffering," but "to create conditions under which no suffering can exist." (Ward, 1920a, p. 468). This would be accomplished through legislation. "Legislation," Ward said, "is nothing else but social invention. It is an effort so to control the forces of a state as to secure the greatest benefits to its people." (1920b, p. 36). He admitted that governments had usually made a mess with their interventions in the past, but that was due, he thought, to the ignorance of those who made the laws. The science of sociology would change all this:

Before progressive legislation can become a success, every legislature must become . . . a laboratory of philosophical research into the laws of society and of human nature. No legislator is qualified to propose or vote on measures . . . until he masters all that is known of the science of society. Every true legislator must be a sociologist . . . (Ward, 1920b, p. 37).

Ward was at least somewhat aware that massive government efforts to alter the ways of people would meet with resistance. This was where "social invention" would come in, he thought. "Social invention consists in making such adjustments as will induce men to act in the manner most advantageous to society." He hoped that most of those who opposed these changes would not

require to have their liberty restricted, since they, too, have wants, and the social inventor should devise means by which such wants shall be spontaneously satisfied through . . . socially beneficial action. (Ward, 1909, pp. 569-70).

The greatest social problem, he declared, was redistribution of goods, and he proposed to solve this problem collectively by the use of government. (Not to put too fine a point on it, he proposed to use the force of government to take goods from those who owned them and distribute them to others.)

This is an exclusively social problem, [Ward (1909, p. 571) said,] and can only be solved by social action. It is today the most important of all social problems, because its complete solution would accomplish nothing less than the abolition of poverty and want from society. Brackets added.

The most important point here, however, is that Ward turned the argument of Darwinians against reform and revolution into an evolutionary argument for reform. He began the process by reformers of laying claim to the latest stage of evolution as being favorable to reform, and progressive as well. He did not prove, of course, that such government-initiated reforms as he favored would achieve the results that he sought, or even that some new stage in evolution had taken place. But his position set the stage for the

gradualist movement toward socialism in America, and he made it appear that all this would be progressive.

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## PANORAMA OF SCIENCE

### The Geographical Distribution of Animals and Plants

#### Introduction

John C. Willis' propounded a theory some years ago on the geographical distribution of plants, in which he claimed that the geological age of any genus of plants is directly proportional to the area of the accumulated geographical ranges of the first 10 members of the genus. In the early 1940's, seminars in genetics and botany, held in various parts of the country, enthusiastically espoused the claims of the theory. There were some prominent dissenters, however, including C. A. Arnold, professor of Paleobotany at the University of Michigan and author of a textbook in that field. They claimed that Willis was an armchair scientist who did not substantiate his views with sufficient research in the field. Interest in the theory, significantly, has diminished remarkably with the passing of time.

\*This theory was discussed in the Genetics Seminar at the University of Michigan in the spring of 1941. The seminar was presided over by J. T. Baldwin, visiting professor of genetics from William and Mary College. One of the graduate students reported on a genetics seminar, at another university, in which the theory was given considerable emphasis.

More recently, this question was brought to my attention in an interview that Don Chittick and I had with a seminary student who had been unsettled by the evolutionist teaching his professors had been giving. He had a long list of questions that we answered to his satisfaction, one of which was, "Are there any species found both in North America and Europe?" On further consideration of the question of biogeography it appears that the subject of geographical distribution should be re-investigated, although from a different angle. There is a dearth of published material in this field and yet such data can produce information of significance in the proper understanding of species distribution.

For many years it has been speculated that a land bridge once existed across the Bering Strait, where Alaska and Siberia are but a few miles apart. Soundings shown on contour maps reveal the shallowness of the waters here, in some places as little as 20' in depth. Strong evidence for this land bridge has recently appeared in the translation of Indian pictographs found in central Indiana about 160 years ago, written by the Leni-Lenape (Delaware) Indians. On translation, they revealed scenes parallel with Genesis, describing the creation of the world, the heavenly

bodies and living creatures, an evil spirit or personality and the Great Flood. After the Flood, life was difficult and they began migrations, feeling that "it would be good to live on the other side of the frozen water." Many chiefs led them ever southward and finally eastward. Despite hostile Iroquois who had preceded them, they moved on to the seaboard area where the English settlers first found them. (Hill, 1981/82)

### The Mammals

This study especially emphasizes the large mammals in North America and their counterparts in Europe and Asia, with a few comments on other continents and areas. Other studies include similar distribution of birds, flowering plants and ferns. It is not the intent of this study to cover all the other plant and animal forms.

Firsthand information and photographs were obtained by traveling to zoological gardens in Zurich, Basel, Berne, Stockholm, Amsterdam, London, Chicago, New York City, Philadelphia and Washington, D.C. In telephone conversation with the director of the London Zoo, the latter recommended several zoos to visit. The Skansen Zoo in Stockholm seemed to stand out, and our decision to visit it seemed providential, as their collection of native European fauna was far and away the best we found in Europe, although we did not get to the zoo in West Berlin.



Figure 1. The wolf (*Canis lupus*) has the widest geographical distribution of any land animal.

### Large Mammals

*Homo sapiens*, of course, has the greatest geographical distribution of any created being. The variations recognized as being within this species are great, including height, body frame, facial bone structure, body hair, texture of head hair, thickness of lips, height of cheek bones, skin, eye and hair color, flaring of the nose, to name a few. It is important to remember that the tall, sturdy, blond Swede and the tiny black African pygmy are both classified as the same species. Next to man, the wolf (*Canis lupus*) has the greatest distribution of any land animal. It was fashionable to call one who had loose morals a "wolf." This is an insult . . . to the wolf! The wolf mates for life, kills only to sustain life and does not plunder the resources of his countryside. The red fox (*Vulpes vulpes*) is next to the wolf among carnivorous animals in distribution. The brown bear (*Ursus arctos*) is found in Europe, across Asia and in Alaska and Canada. The polar bear (*Thal-arctos maritimus*) is found in the polar regions of all



Figure 2. The European Brown bear (*Ursus Arctos*) is a race within the same species that includes the Kodiak bear of Alaska.

three continents. The arctic fox (*Alopex lagopus*) is found throughout these regions. Other carnivores on both continents are the wolverine (*Gulo gulo*) and the lynx (*Lynx lynx*). Among grazing animals the red deer and wapiti (races of *Cervis elephas*) are found in the widest distribution, from Scotland across Asia and North America. Other pairs that are now recognized as the same species are the moose and European elk (*Alces alces*); the reindeer (*Rangifer tatandus*) and the Greenland caribou (*Rangifer arcticus*); the American bison (*Bison bison bison*) and the European bison (*Bison bison bonasus*); North American bighorn sheep (*Ovis canadensis Stonei*) and the Siberian snow sheep (*Ovis canadensis nivicola*). This study revealed that there are two races of bison in North America and, until recently, two races in Europe. The race in the Caucasus Mountains is said to be extinct and North American plains bisons were released there to replace it. Correspondence with Canadian research scientists at Wood Buffalo National Park in northern Alberta reveals that their studies indicate that the plains buffalo (*Bison bison bison*) is closer in body structure to the European wood buffalo (*Bison bison bonasus*) than the latter is to the Canadian wood buffalo (*Bison bison athabascae*). They are all now considered to be merely races of the same species. The musk-ox (*Ovi-*



Figure 3. The rare European wolverine (*Gulo gulo*) is now recognized as identical to our North American wolverine.

*bos moschatus*) is found in Siberia in fossil form, as "the same species or a close relative" (Walker). The pronghorn (*Antilocapra americana*) is found only in North America but fossil forms in the "pleistocene" and "pliocene" are virtually identical. Grzimek (1970, IV, p. 157), in his monumental work on mammals, acknowledges their sudden appearance in geological strata stating, "Suddenly, during the glacial period (Pleistocene) in the Old World, the moose, roe, and reindeer appeared, and so far none of their ancestors are known from the Tertiary." In addition, the walrus and four species of seals are found in both Atlantic and Pacific waters, as are 31 species of Cetacea (12 of dolphins and 19 of whales), 14 of which are found in all temperate oceans or "world-wide." Of 39 similar mammals studied in North America, 27 are identical to European counterparts. Seven are considered probably identical and one warrants further study.

A few North American mammals were thought to have no very close counterpart, such as the fisher, the wildcat and the black bear, although further study of the Spanish lynx and the Asiatic black bear respectively might be warranted.

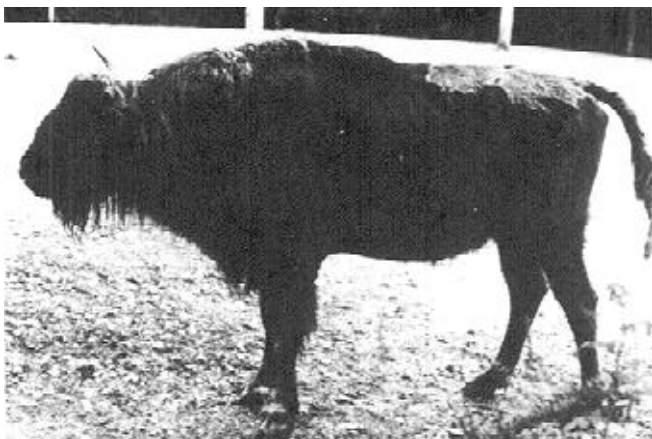


Figure 4. The European bison (*Bison bison bonasua*) is a race within the same species as our plains bison (*Bison bison bison*).

#### Smaller and Miscellaneous Animals

Among those which are recognized as the same species on both sides of the ocean are the beaver (*Castor canadensis*) with the European beaver (*Castor fiber*), arctic hare (*Lepus arcticus*) with blue hare (*Lepus timidus*), southern flying squirrel (*Glaucomys volans*) with European flying squirrel (*Pteromys volans*), brown hare (*Lepus capensis*), ground squirrel (*Spermophilus parryi*), short-tailed weasel (*Mustela erminea*) with European stoat (same sp.), least weasel (*Mustela riposa*) with weasel (*M. nivalis*), two long-tailed shrews (*Sorex cinereus* and *S. arcticus*), a vole (*Microtus oeconomus*), a true lemming (*Lemmus sibiricus*), a water vole (*Arvicola richardsoni*) with European water vole (*A. terrestris*) and sagebrush (steppe) vole (*Iagurus curtatus*). Species that are strikingly similar and should be considered the same in most cases are: the black-footed ferret (*Mustela nigripes*) with the steppe polecat (*Putorius putorius*) some of which have a "striking resemblance to the North American black-footed ferret" (Nowak and Paradiso, 1983, II, p. 992), marten (*Martes americana*) with pine



Figure 5. This Southern flying squirrel (*Glaucomys volans*) is identical to the European flying squirrel (*Pteromys volans*).

marten (*Martes martes*), north American mink (*Mustela vison*) with European mink (*Lutreola lutreola*), river otter (*Lutra canadensis*) with European otter (*Lutra lutra*), Richardson's ground squirrel (*Citellus richardsoni*) with European suslik (*Citellus citellus*), tassel-eared squirrel (*Sirius aberti*) with European red squirrel (*Sirius vulgaris*), North American porcupine (*Erethizon dorsatum*) with Eurasian and African porcupines (*Hystrix cristata*). This does not include many species of North American bats which are closely related to those in the same genera in Eurasia, as follows; *Myotis*, *pipistrellus*, *eptesicus*, *Nycticeius*, *Plecotus* and *Tadarida*.

#### Bird Life

The comparative study of birds reveals a number that are already recognized as identical on both sides of the Atlantic. These total 132 species. In addition, another eight species are so similar that they may well be eventually recognized as identical to comparable European races. Another 16 are classified as possibly identical, and eight showed sufficiently close similarity that they were considered as meriting further study. A fair number of these species studied were from coastal regions with another large group being the ducks, 20 being identical. There were nine such among the birds of prey, 13 sandpipers and curlews, 18 gulls and seven owls. There was a good sampling, however, of many other genera, e.g. including the great blue heron, horned lark, Bohemian waxwing, barn swallow, two chickadees, two crossbills, pine siskin, red poll, and winter wren. The area covered is eastern North America to the Rocky Mountains.



Figure 6. 132 species of birds in Eastern U.S. are recognized as the same as their European counterparts, including these European barnacle geese (*Branta leucopsis*) and white-fronted geese (*Anser albifrons*).

### Flowering Plants

A large number of flowering plants (474) in Europe and North America are identical. In addition, there are 32 species that are strikingly similar, some of which have been classified by some botanists as identical. A large number are coastal or alpine species, but there are many old friends that are not, such as yellow lady's slipper (*Cypripedium calceolus*), fireweed (*Epilobium angustifolium*) and broad-leaved cattail (*typha latifolia*). In addition, most of the common trees of Europe have very similar counterparts in this country. If the latitude of variation within the species that is now applied to man (*Homo sapiens*), and more recently to bird life, is applied to other plants and animals, the number of plants and animals on opposite sides of the ocean that may be recognized as identical, can increase considerably. The area covered: eastern North America south to Virginia.

### Ferns and Their Relatives

Among plants, the ferns have the highest percent of their species on both sides of the Atlantic. Thirty-four species have long been recognized as identical and another one possibly so. Nineteen of the fern relatives (other *Pteridophytes*) are also classified as the same. This includes the horsetails, pine mosses, quillworts and their relatives.

### Trees and Shrubs

There are at least 60 species of trees and shrubs in Europe that have close counterparts in North America. Five of these species are already considered identical; several others undoubtedly should be. If you planted American and European (Rowan) mountain ashes side by side, it would take a botanist with a tree guide to tell them apart. In addition, there are many Asiatic trees and shrubs that are strikingly similar to our North American species (such as hemlocks, bittersweet, *Torreya*, arborvitae, holly and Mahonia [Oregon grape] to name a few). All of these lists are being kept as checklists, and are available for anyone to utilize or to make additions.

### Conclusion

The number of years that have passed since some of the flora and fauna of Eurasia have migrated over to North America via land bridge and/or continental separation is a matter of conjecture. Macroevolutionists, of course, would claim the process required millions of years. It is evident, however, that the species did not evolve since that time, but have remained the same. This supports the concept that either very little time has elapsed since these species became isolated from each other (Meyer, 1985; Williams, 1986) or little evolution has occurred (even microevolution). Such a study also brings to light the intricacy, marvelous diversity and transcendent beauty of the handiwork of God.

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Contributed by G. Richard Gulp

### Can Fifty Million Frenchmen Be Wrong?

One of the arguments we hear raised so often against creation as an explanation of the variety of observed living things is that the overwhelming majority of scientists have rejected it and support instead evolution. Even those who recognize that scientific judgments are not reached by majority vote argue that opinions reached by a majority of unbiased scientists are more likely to be correct than those held by a small minority. Even more significant is the generally held opinion that if a theory or a hypothesis has substantial factual, observational support it must be true.

The January 1988 issue of *Smithsonian* carries an interesting column "Phenomena, comment and notes" with the subtitle "If such an eminent astronomer could have been so wrong 80 years ago, a physicist wonders, what do we 'know' now?" The author refers to a book published by the turn-of-the-century American astronomer, Percival Lowell, entitled *Mars as the Abode of Life*. Lowell builds on two of his previous books to establish the scientific "fact" that intelligent life and civilization exist on the red planet. The author, James Trefil, a physicist, refers to the fact that an Italian astronomer, Giovanni Schiaparelli, had seen straight lines on the Martian surface which he called "canali," which means "channels" in Italian. "Canali" was translated into English as "canals." Schiaparelli claimed to have seen 113 "canals" on Mars by the end of his life.

Lowell initiated a major Mars research program at his observatory and claimed to have identified no fewer than 522 canals. Indeed he drew a detailed map of their layout on the Martian surface. His 1908 book is full of carefully drawn figures showing the way the canals intersect, a table showing how they are distributed in latitude, one showing their longitude and descriptions of how the canals change over time, waxing and waning with the Martian seasons.

What Trefil finds "frightening" is that Lowell was obviously talking about observations made over a period of years by highly trained and competent observers. All of this elaborate collection of data, calculations and conclusion was assembled *in spite of the fact that there is not a single canal or straight line on the Martian surface!* (Trefil's emphasis.) Trefil thinks that the Lowell story is a good example of the

ability of the scientific process to correct its mistakes. But he also believes that if a competent scientist like Lowell could be taken in, much of what we do in modern science will someday join the canals of Mars as examples of human folly. He first applies this principle to the social sciences but then goes on to apply it to his own area. He asks whether the "big bang" theory should be placed in this category and then goes on to ask "what about quarks?"

It should be pointed out that there are a number of theories for which at one time there was thought to be ample observational evidence. What has happened to phlogiston? To ether? There was what was assumed to be ample observational evidence to support these

explanations, but today they are a part of the museum of discarded scientific theories.

Is it worth while applying these same questions to the theory of evolution? Can fifty million Frenchmen be wrong? The Lowell incident and others in the history of science clearly show that this is possible. The argument that evolution must be true because it is held by so many scientists and seems to be supported by so many observations is hardly an acceptable one. Lowell thought the evidence was there. He even had drawings and maps to support his explanation. Evolutionists too insist that there is ample evidence for their explanation, but they too can be wrong.

Contributed by John W. Klotz

## THE POSTULATED EVIDENCE FOR MACROEVOLUTION AND DARWINISM: DARWINIAN ARGUMENTS AND THE DISINTEGRATING NEO-DARWINIAN SYNTHESIS (PART I)

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### Abstract

*The eight primary lines of evidence offered for macroevolution and Darwinian mechanisms are discussed. Evolutionist scientists and writers are quoted who suggest that (1) the paleontology argument "contributed . . . nothing to evolutionary biology"; (2) the phylogeny argument has produced only a "meaningless waffle" and has been "another miserable failure"; (3) the classification argument "has nothing to say about evolution" and "ignorance concerning these relationships is still great"; and (4) the "facts of comparative anatomy provide no evidence for evolution," while the "attempt to find homologous genes has been given up as hopeless." Similarly, (5) the embryological argument used to center on a biogenetic "law" that has "been demonstrated to be wrong by numerous subsequent scholars" and now stands on the problem that "[a]natomically homologous parts in different related organisms appear to have quite different origins"; (6) the comparative biochemistry argument offers a "serious . . . challenge to the whole evolutionary framework" rather than support by widespread anomalies that require "a robust rejection of a generalized molecular clock hypothesis of DNA evolution"; (7) the population genetics argument has made "no direct contribution to what Darwin obviously saw as the fundamental problem: the origin of species," and "is merely the blind leading the blind"; and (8) the artificial selection argument overlooks that "selective breeding is not analogous to the action of 'natural selection'." All scientists mentioned in this article are evolutionists.*

### Introduction

The major arguments for macroevolution are described by Stansfield (1977, p. 98) as follows:

Two of the major lines of evidence supporting the evolutionary theory, namely *paleontology* and *biogeography*, were presented in the last chapter. This chapter presents further supporting evidence drawn from the disciplines of *taxonomy*, *comparative embryology*, *comparative anatomy*, *comparative biochemistry*, and *physiology*. (emphasis added)

Those "evidences for evolution" are similarly summarized by Good (1974, p. 3) and Gould (1987, pp. 65, 68). The arguments for Darwinian evolution are discussed as follows: (a) the paleontology argument, (b) the phylogeny argument, (c) the classification (taxonomy) argument, (d) the comparative anatomy and physiolo-

gy argument, (e) the comparative embryology argument, (f) the comparative biochemistry argument, (g) the population genetics argument, and (h) the artificial selection argument.

These postulated evidences for macroevolution are viewed with great cynicism by Grassé (1977, p.6):

Through use and abuse of hidden postulates, of *bold, often ill-founded extrapolations*, a *pseudoscience* has been created. It is taking root in the very heart of biology and is *leading astray* many biochemists and biologists, who sincerely believe that the accuracy of fundamental concepts has been demonstrated, which is not the case. (emphasis added)

The same skepticism is held by Kerkut (1960, p. vii) and many other nondiscontinuitist scientists:

The attempt to explain all living forms in terms of an *evolution from a unique source*, though a brave and valid attempt, is one that is premature and *not satisfactorily supported* by present-day evidence. (emphasis added)

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In fact, classification, comparative anatomy, comparative embryology, population genetics, and artificial selection data can "be accounted for on some different hypothesis" besides evolution, although paleontology allegedly cannot, according to Good (1974, p. 4).

#### Paleontology Argument and Difficulties

The paleontology argument for macroevolution is based on the fossil record; paleontology is "the study of fossils," Stansfield (1977, p. 67). "Direct study of macroevolutionary patterns is only possible in the fossil record," Valentine (1982, p. 517) and this fossil record "forms our most direct evidence . . . of the course of biological evolution," Dobzhansky *et al.* (1977, p. 314) conclude. Also see Good (1974, p. 4). Yet the fossil record is characterized by systematic abrupt appearances of groups of organisms and systematic gaps between groups, rather than by a generally progressive emergence of one group from another (Bird, 1987, pp. 46-64).

Consequently, even though the fossil record is the "most direct evidence" of macroevolution, Ridley (1981, p. 831) states that "no real evolutionist, whether gradualist or punctuationalist, uses the fossil record as evidence in favour of the theory of evolution as opposed to special creation." Thus far, according to Hoffman, paleontology has added nothing to evolutionary biology:

All the recent paleobiological debates have, in my opinion, contributed much to the conceptualization of paleobiological research itself, but nothing to evolutionary biology. I contend only that they have not done so . . . (1983, p. 241).

Patterson (1981b, p. 218) concludes that the concept that fossils are the best evidence for evolution is a myth:

I conclude that *instances of fossils overturning theories of relationship based on recent organisms are very rare, and may be nonexistent*. It follows that the *widespread belief that fossils are the only or best means of determining evolutionary relationships is a myth*. Tracing how this myth came to be an article of faith among biologists. . . should be an interesting study in the sociology of science; it seems to have followed, as an unquestioned corollary, from acceptance of evolution. (emphasis added)

(1) *Abrupt Appearance and Gaps*. The overwhelming problem that the fossil record poses for macroevolution thus is that the record is characterized by abrupt appearances and systematic gaps, as Gould acknowledges:

Increasing diversity and multiple transitions seem to reflect a determined and inexorable progression toward higher things. But the *paleontological record supports no such interpretation*. There has been *no steady progress* in the higher development of organic design. We have had, instead, vast stretches of *little or no change* and one evolutionary *burst* that created the entire system. (1976, p. 37, emphasis added)

*New species almost always appeared suddenly* in the fossil record with *no intermediate links* to

ancestors in older rocks of the same region. (1977, p. 12, emphasis added)

Gould (1977, p. 14) also states:

The *extreme rarity of transitional forms* in the fossil record persists as the *trade secret of paleontology*. The evolutionary trees that adorn our textbooks have *data only at the tips* and nodes of their branches; the rest is inference, however reasonable, not the evidence of fossils. (emphasis added)

That trade secret is being let out by more and more scientists, such as Grassé (1977, p. 31) and Boyden (1973, p. 27) for the origin of phyla, George (1960, p. 1) and Simpson (1953, p. 360) for the origin of genera and families, and many others quoted in Bird (1987, pp. 46-64).

The alleged evolutionary trees are not just mostly "inference" rather than "data," but are "highly conjectural" and either do not support or actually "deny" macroevolution. Patterson (1981, p. 216). Also see Rosen *et al.* (1981, p. 178). The alleged transitional forms are such that now "we have even fewer examples of evolutionary transition than we had in Darwin's time," and "there is not one such fossil for which one could make a watertight case," according to Raup (1979, p. 25). Also see, Valentine (1982, p. 516) and Patterson (1979, p.1). These few alleged transitional forms are not statistically significant enough to lend support to macroevolution or Darwinian mechanisms, as Denton (1985, p. 117) points out:

Even if a number of species were known to biology which were indeed perfectly intermediate, possessing organ systems that were unarguably transitional in the sense required by evolution, *this would certainly not be sufficient to validate the evolutionary model* of nature. To refute typology and securely validate evolutionary claims *would necessitate hundreds or even thousands of different species, all unambiguously intermediate* in terms of their overall biology and in the physiology and anatomy of all their organ systems.

That statistical insignificance is evident in view of the 250,000 fossil species that have been catalogued (Raup, 1979, p. 22) which represent thousands of orders, families, and genera; and the 1,071,500 animal species and 368,715 plant species alive today, Dobzhansky (1970, p. 24); Mayr (1969, pp. 11, 12). Moreover, many of the unbridged gaps are such that one cannot even "invent a reasonable sequence of intermediate forms," Gould (1977, p. 24) concedes. Also see Frazetta (1970, p. 55). The few alleged transitional fossils are discussed elsewhere Bird (1987, pp. 209-34).

These abrupt appearances and systematic gaps pose critical if not fatal problems for macroevolution. "[I]t should be possible to find some 'connecting links'," because "higher categories become distinct entities through extinction of intermediate related groups." Stansfield (1977, p. 511). Also see Huxley. (1967, p. 13) and Wald (1967, p. 19). Darwin (1859, p. 302) saw the "fatal" significance of abrupt appearances:

On the *sudden appearance* of whole groups of Allied Species . . . If numerous species, belonging

to the same genera or families, have really started into life all at once, the fact would be *fatal* to the theory of descent with slow modification through natural selection. (emphasis added)

Darwin (1859, p. 280) also recognized the fatal significance of the problem posed by systematic gaps:

Why then is not every geological formation and every stratum full of such *intermediate links*? Geology assuredly does not reveal any such finely graduated organic chain; and this, perhaps, is the *most obvious and gravest objection which can be urged against my theory*. The explanation lies, as I believe, in the extreme imperfection of the geological record. (emphasis added)

T. H. Huxley, probably his chief advocate, conceded that "if it could be shown that this fact had always existed, the fact would be fatal to the doctrine of evolution." (1882, p. 619). Biological macroevolutionists have proposed two major solutions.

(2) *Adequacy of the Fossil Record*. One defense raised by Darwin and many macroevolutionists is an "extreme imperfection of the geological record." (1859, p. 280). In fact, Darwin staked his case on imperfection being an accurate portrayal of the record: "He who rejects these views on the nature of the geological record, will rightly reject my whole theory." (1859, p. 342).

However, now that 200 million fossil specimens have been catalogued of over 250,000 fossil species (Raup, 1979, p. 22) the fossil record cannot really be called inadequate, Stanley (1979, p. 1) argues:

In part, the role of paleontology in evolutionary research has been defined narrowly because of a false belief, tracing back to Darwin and his early followers, that the fossil record is woefully incomplete. Actually, the record is of *sufficiently high quality* to allow us to undertake certain kinds of analysis meaningfully at the level of the species. (emphasis added)

The fossil "data for genera and families are numerous enough to allow for meaningful analysis," Raup and Stanley (1978, p. 303) state, and for mammals the "geological history is comparatively well documented." (Russell, 1962, p. 130). The only sense in which the fossil record is inadequate may be that it does not contain the numerous "intermediate links" expected by Darwin:

As an aside, one is led to ask *to what degree the "imperfect" geological record was viewed as such by Darwin simply because there were no observed transitional links*. There can be no doubt that Darwin had empirical reason to believe in an imperfect record, but was his emphasis based partly on his biological expectations? In any case, there is no question that Darwin expected those finely graded transitions. He firmly believed that transitions must have existed and struggled to rationalize their absence . . .

Darwin's expectations and theorizations were clearly at variance with his observations of nature . . . (Raup, 1979, p. 14, emphasis added)

(3) *Insufficiency of Assumed "Ancestral Groups."* Another defensive approach taken by many macroevo-

lutionists is to identify purported common "ancestral groups," a fall-back position resulting from the absence of actual fossils of common ancestors or transitional forms. However, postulations about "such groups are inconsistent with evolutionary theory," according to Patterson (1981b, p. 207). There are several problems.

First, talk about ancestral groups is highly conjectural, and may be imagining a relationship between what may be totally unrelated groups, as Rosen *et al.* (1981, p. 178) note:

The search for fossils has produced *superficially acceptable sequences*, as it was bound to, for *few transformations, however fantastic, are forbidden by the Darwinian or neo-Darwinian picture of the evolutionary process*. Yet the sequences consist of nothing more than abstractions from paraphyletic groups such as rhipidistians, osteolepiforms, and labyrinthodonts. (emphasis added)

Those conjectures are no different from the pre-Darwinian conjectures about archetypes, Brady (1983, p. 265) adds:

Patterson (1982) reviews the Owen version of an archetype and notes that the real problem is the notion of general homology, by which "an idealization" is homologized with actual features through "abstract transformations." He then argues that the same strategy can be found in the contemporary practice of homologizing dissimilar organs on the basis of the homology of each with the primitive version of the organ in a hypothesized progenitor. Due to the use of such a strategy in contemporary works, Patterson concludes that "*archetypes are by no means extinct, but live on in the form of speculative progenitors*. These modern versions are Darwinian, of course, but however far they appear to be from Owen's idealistic schema, they result in the *same difficulties*. (emphasis added)

Such "speculations" are not persuasive even if the postulated sequence is somewhat similar, Denton (1985, pp. 194-5) points out:

It is possible to allude to a number of species and groups such as *Archeopteryx*, or the rhipidistian fish, which appear to be to some extent intermediate. But even if such were intermediate to some degree, there is no evidence that they are any more intermediate than groups such as the living lungfish or monotremes which, as we have seen, are not only tremendously isolated from their nearest cousins, but which have individual organ systems that are not strictly transitional at all. As evidence for the existence of natural links between the great divisions of nature, *they are only convincing to someone already convinced of the reality of organic evolution*. (emphasis added)

For example, some rocks resemble each other and can be lined up in an evolutionary sequence, but that does not prove that one evolved into the others, Hull (1983, pp. 333-5). Also see Patterson (1981b, p. 218). "Fossils may tell us many things, but one thing they can never disclose is whether they were ancestors of anything else," as Patterson (1978, p. 133), Oldroyd (1986, p. 154)

and Good (1974, p. 383) emphasize. Second, Brady attacks the concept of "the hypothetical ancestor":

A similar problem arises with regard to the hypothetical ancestor, for *this invention is equally without empirical foundation*, and occupies a space in our reflections that should be reserved for actual observation. If we can invent one ancestral form, *we can invent many*, and lacking the evidence for which they substitute, there is no way to determine which form, if any, is correct.

We must remember, while reviewing these problems, that the mediating form was hypothesized because the data was inconclusive without it. It becomes, therefore, an interpretive tool by which the data can be given definition. The known forms are now interpreted by the hypothesized ancestor (or intermediate), and what the existent organisms do or do not possess will now depend upon what the hypothesized ancestor (or intermediate) says they possess. *If the empirical forms must submit to interpretation by the hypothesis, they cannot be used to test that hypothesis.* For these reasons Patterson calls general homology "vacuous," and I must concur. *Speculative construction of general forms and the paths of transformation that they suggest may hold a certain fascination for the imagination, but the practice is merely guesswork and can tell us nothing about the actual paths of transformation.* 'General homology' is not actual homology. (1983, p. 266, emphasis added.)

Third, statistically there should be a large number of actual fossils of common ancestors or intermediate forms, given the quarter billion catalogued fossils of a quarter million fossil species, as discussed above. Darwin and Huxley conceded that must be the case if macroevolution were true, and that should be the case even if macroevolution occurs in punctuated bursts (Bird, 1987, pp. 172-3).

Fourth, even if the search for actual ancestors or intermediates is abandoned for a search for roughly ancestral groups, there is the problem that not even rough ancestral groups exist for most major categories. There are no such ancestral groups for any of the 25 phyla of the animal kingdom, because there are "no extinct fossil groups known that are the common ancestors of two or more living phyla" and there are absences of intermediate forms between "all" 25 phyla, according to Ayala and Valentine (1979, p. 258). The same is true for many other higher categories and for every taxon where an abrupt appearance occurs (Bird, 1987, pp. 46-56). Transformed cladists, such as Patterson (1980, p. 217) criticize "the textbook stories [that] are replete with phantoms—extinct, uncharacterizable groups giving rise one to another." It is not enough for a small handful of rough ancestral groups to be found, as Denton (1985, p. 177) stresses:

To demonstrate that the great divisions of nature were really bridged by transitional forms in the past, *it is not sufficient to find in the fossil record one or two types of organisms of doubtful affinity which might be placed on skeletal grounds in a relatively intermediate position between other groups* . . . (emphasis added)

A number of rejoinders by defenders of the "ancestral group" approach are addressed by Patterson (1981b, p. 207):

Evolutionary paleontologists have offered three responses to criticisms of ancestral groups. First, the grouping by phenetic similarity (as in forming paraphyletic groups) reflects genotypic similarity, and is therefore a valid reflection of evolutionary relationships. This argument has been refuted above. Second, that ancestral groups are approximations, "with the admission that evidence available at present is insufficient to make a more specific statement" . . . ; the rank assigned to such groups "symbolizes the degree of confidence which one has in a hypothesis about phylogeny" . . . This response seems to reflect only the inductivist view so long prevalent among paleontologists—"we need more fossils before we shall know the answer." Third, paraphyletic groups are "adaptively unified" . . . While this may be true of some such groups (as some may be "phenetically unified"), it cannot serve as a general justification. No one can argue that Invertebrata (a paraphyletic group) are adaptively unified.

For ancestral groups as for adaptations, "a plausible story is not necessarily a true one." Gould (1977, p. 28).

Thus, the paleontology argument for biological macroevolution and Darwinism is weak if not subversive, in the view of many Darwinians as well as nonevolutionist scientists. That is why Ridley (1981, p. 831) states that "no real evolutionist, whether gradualist or punctuationalist, uses the fossil record as evidence in favour of the theory of evolution as opposed to special creation."

#### Phylogeny Argument and Difficulties

The phylogeny argument for macroevolution is based on "phylogenetic trees" of assumed evolutionary descent. Raup and Stanley (1978, pp. 131-2). This phylogeny field of systematics "is the very cornerstone of evolutionary analysis." (Cracraft, 1983, p. 177). Yet it is a weak cornerstone, according to a number of evolutionists as well as discontinuist biologists.

One problem with the phylogeny argument is that reconstruction of phylogenies is *impossible* in the professional opinion of many nondiscontinuists such as Hull (1983, pp. 333-5):

The chief question with respect to phylogenetic reconstruction has always been its feasibility. Throughout the history of "Systematic Zoology," a *series of objections* have been raised to it, starting with Blackwelder, Bigelow, Boyden, and Borgmeier, continuing with the pheneticists, and culminating with at least some cladists. (emphasis added)

For example, the cladist objection to phylogenies begins with the point that "common ancestral species are necessarily hypothetical, and . . . will forever remain unknown and unknowable in a directly empirical sense," (Nelson, 1972, p. 368). Consequently, rough ancestral groups must be used instead, and they are "flawed artefacts" with questionable assumptions, Patterson (1982, p. 306) contends:

Because of all these problems, it is *rare to find palaeontologists offering ancestral species*, or doing so with any conviction. Instead, they usually propose “ancestral groups,” as approximations to the truth, with the claim that the true ancestor, if found, would fall within the group . . . Yet these *flawed artefacts* play a *central role in phylogenies* — accounts of the evolutionary descent of lineages. This raises yet another problem, for *groups cannot evolve* — species are the largest units capable of change. Thus *cladistics calls into question much of conventional evolutionary history*. (emphasis added)

Also see Ball (1983, p. 446). The rejection of phylogenies—and of macroevolution itself—by many pheneticists and cladists is discussed elsewhere (Bird, 1987, pp. 152, 190-3).

Another serious difficulty, related to the first, is that phylogenies are almost *totally speculative*. Bonner (1961, p. 242) says that those in textbooks “are, as a rule, a festering mass of unsupported assertions.” Ghiselin (1972, p. 131) concludes that many “read like imaginative literature” and “fill gaps in the data with speculations.” Boyden (1973, p. 117) observes that “phylogenies grew up like weeds” for “even organisms with no fossils available,” and Good (1974, p. 8) notes that such “studies in plant evolution are generally no more than the raw materials of phylogenetic speculation.” Charig (1982b, pp. 411-2) calls some phylogenies as “meaningless waffle.” Turrill (1963, p. 213) calls most “extremely doubtful approximations,” and Sokal and Sneath (1963, p. 104) call many “utter rubbish.” Also see Rosen and Schuh (1975, p. 505). The gross subjectivity of postulated phylogenies is evident in the treatment of the fossil record as inadequate when the fossils do not fit the phylogenetic hypotheses, but as adequate when the fossils can be interpreted to support the phylogenetic hypotheses:

But *when a new fossil is found and is inconsistent with the phylogeny (like Ichthyostega)*, the stratopheneticist’s answer is that this *test has shown that the fossil record was insufficiently dense and continuous* for the method to work. And *when a new fossil is consistent with the phylogeny*, the record was sufficiently *dense and continuous*. Plainly, the method is hard to fault, for the fossil record, rather than the phylogeny, is under test . . . Patterson (1980, p. 216, emphasis added).

Forey (1982, p. 143) concurs in that objection and Kitts (1974, p. 467) provides part of the reason in noting that “paleontological phylogeny construction has not been provided with a solid theoretical foundation.”

A further problem, closely related to the preceding ones, is that phylogenies are used to support macroevolution through *circular reasoning*, as Thomson (1982, pp. 529-30) notes:

Any reasonably graded series of forms can be thought to have legitimacy. In fact, there is *circularity* in the approach that first assumes some sort of evolutionary relatedness and then assembles a pattern of relations from which to argue that relatedness must be true. This interplay of data and interpretation is the *Achilles’ heel* of the second meaning of evolution.

Forey (1982, p. 124), Cartmill (1981, p. 90) and Kitts (1974, p. 467) reaffirm the presence of circular reasoning.

A crowning problem is that the quest for phylogenies has “*failed*” utterly, according to Saiff and Macbeth (1982, p. 12):

A. *The Commitment in Theory*. Darwinian theory asserts that physical descent with modification has been universal, which means that every modern species is the latest link in a phylogeny. There must therefore have been hundreds of thousands of phylogenies, and it was Darwin’s expectation that these would be found. His followers, sharing his expectations, felt a duty to seek and find the phylogenies . . .

B. *Another Miserable Failure*. The expectations were vain. The zeal came to naught. In the 125 years since the *Origin* was published, nothing has been accomplished. *No phylogenies have been established* and the pursuit of them has fallen into disrepute. (emphasis added)

Also see Patterson (1981b, p. 213) and Macbeth (1976, p. 495). Mayr (1982, p. 218) finds the “futile attempts” to be “depressing”:

The *futile attempts* to establish the relationship of the major phyla of animals induced at least one competent zoologist at the turn of the century to deny common descent. Fleischmann (1901) called the theory a beautiful myth not substantiated by any factual foundation. Kerkut, fifty years later, does not draw such an extreme conclusion but he is almost equally pessimistic about ever achieving an understanding of the relationship of the higher animal taxa. Honesty compels us to admit that our ignorance concerning these relationships is still great, not to say overwhelming . . . (emphasis added)

Some will question whether phylogeny construction has failed in light of the horse phylogeny or other phylogenies. Raup (1972, p. 25) responds that the number of arguably correct phylogenies is shrinking:

The record of evolution is still surprisingly jerky and, *ironically, we have even fewer examples of evolutionary transition than we had in Darwin’s time*. By this I mean that some of the classic cases of Darwinian change in the fossil record, such as the evolution of the *horse* in North America, have had to be discarded or modified as a result of more detailed information—what appeared to be a nice simple progression when relatively few data were available now appears to be much more complex and much less gradualistic. (emphasis added)

Denton (1985, p. 185) agrees:

Considering that the total number of known fossil species is nearly one hundred thousand, the fact that the only relatively convincing morphological sequences are a *handful of cases like the horse*, which do not involve a great deal of change, and which in many cases like the elephant may not even represent phylogenetic sequences at all, serves to emphasize the *remarkable lack of any direct*

evidence for major evolutionary transformations in the fossil record . . .

It is possible to view such series in a very different light and read the fossil evidence directly as it stands; and infer that what is exceptional about such sequences is not their preservation but rather the fact that they occurred. *They may be exceptions which prove a very different rule: that in general, nature cannot be arranged in terms of sequences and that where sequence does exist it is exceptional or relatively trivial.* (emphasis added)

The various specific phylogenies are discussed elsewhere (Bird, 1987, pp. 209-34).

Thus, the phylogeny argument for biological macroevolution is viewed by many nondiscontinuitists scientists as impossible (according to transformed cladists and others), totally speculative (compared to everything from "weeds" (Boyden, 1973, p. 117) to "rubbish" (Sokal and Sneath, 1963, p. 104), "circular" (Thomson, 1982, pp. 529-30) and a "failure" (Saiff and Macbeth, 1982, p. 12). In this important area, the biological theories of macroevolution and Darwinism are not compellingly established.

#### Classification Argument and Difficulties

The classification argument for macroevolution is based on the hierarchical structure in nature; classification is "the theory and practice of naming, describing, and classifying organisms." Stansfield (1977, p. 98). Also see Kluge (1977, p. 10). There are "three prevailing systems of classification, known as phenetic, cladistic, and evolutionary." Dobzhansky *et al.* (1977, p. 262). Also see Luria *et al.* (1981, p. 684).

The most basic problem with the classification argument is that half of one school (transformed cladists, [Mayr (1982, pp. 226-7); Hennig (1966)]) and much of another school (pheneticists, [Mayr (1982, pp. 221-3); Sokal and Sneath (1973)]) deny that classification supports or is even relevant to evolution. "In 'transformed cladistics,' . . . evolution is deliberately ignored, being considered unproven and possibly unprovable," as one of its critics, Charig (1982a, p. 121) protests. Also see Platnick (1979, p. 537). Patterson (1981a, p. 92) agrees:

As I understand it, cladistics is *theoretically neutral* so far as evolution is concerned. It *has nothing to say about evolution*. You don't even need to know about evolution, *or believe in it*, to do cladistic analysis. (emphasis added)

Also see Beatty (1982, p. 31). Patterson (1982, p. 304) suggests that the branching diagrams produced by cladists "can be seen not as evolutionary trees" and as not supporting "common ancestry."

Critics of transformed cladistics lambast its "non-evolutionary" or anti-evolutionary thinking, such as Oldroyd (1986, p. 154):

Indeed, as is well known, *much modern taxonomy has abandoned its Darwinian, historicist or genealogical approach*, and has adopted a positivistic methodology based simply on an examination of observable morphological similarities and differences, and excluding attempted reconstructions of

genealogies. This so-called cladistics is *fundamentally a non-evolutionary classification*. As such, it generates something *very like the nineteenth-century typologies* of authors such as Henri Milne-Edwards. *Cladistics*, which is, of course, an *anathema to neo-Darwinians*, is favoured by those who prefer not to transcend the observable data in their theorizing to "speculate" about genealogical relationships. (emphasis added)

Ball (1983, p. 446) also laments that transformed cladistics operates "in a non-evolutionary domain." Beatty (1982, pp. 29, 31) observes that, "[a]pparently in response to the perceived crisis in evolutionary biology, cladists have taken more or less extreme measures to sever their ties from that discipline" and are not merely "evolutionarily neutral" but "at odds with evolutionary thinking." Ridley (1983, p. 651) states that transformed cladists "are asserting that they can do cladism without evolution, and what they can do, they will." Benton (1984, p. 18) observes that they classify "without any prior assumptions that evolution has taken place."

Proponents of transformed cladistics agree that "cladistics calls into question much of conventional evolutionary history." (Patterson, 1982, p. 306). "Garth Nelson and Norman Platnick lodge a frontal assault on Darwinism in systematics by claiming it to be falsified," Cracraft (1985, p. 302) notes and they indeed do so by arguing that evolutionary phylogenies should be replaced with classifications that "include no ancestral taxa":

If phylogenies of one sort . . . are to pass away, is the notion of phylogeny doomed also? We judge not, for *there is an alternative notion, here simply termed classification* . . . Notions of this kind can be looked upon as phylogenies—as historical statements of ancestry and descent. But they are different in character. *They include no ancestral taxa. They deny the postulates of Darwinian systematics: that ancestral taxa have an objective identity independent of their descendants; that ancestral taxa can be discovered and identified as such; that ancestral taxa are under the constraints of empirical investigation.* (Nelson and Platnick, 1984, pp. 153-4, emphasis added)

The "phenetic schools refrain from taking any evidence from descent into account," Mayr (1982, p. 226) notes. Simpson concluded that phenetics brought "retrogression in taxonomic principle . . . a conscious revival of pre-evolutionary, 18th century, principles." (Mayr, 1982, p. 224). Thus, in the phenetics school, "some more radical numerical taxonomists have abandoned the goal of building a taxonomy that reflects evolutionary descent." (Luria *et al.*, 1981, p. 676). In the view of these specialists, the groupings in nature do not support Darwinian macroevolution but instead are at best irrelevant if not contrary to it.

A related problem is that essentially the same classification system was historically founded and developed by creationist scientists (Linnaeus and others), and was defended as more consistent with the theory of creation than with the theory of evolution by many scientists at the time of Darwin (Agassiz, Sedgwick, and others), as Stansfield (1977, p. 99), Dobzhansky

(1982, p. 734) and Patterson (1978, pp. 121-2) note. Darwin himself "took these relations to be established by previous biology," and "was clearly of the opinion that they were independent of that theory." (Brady, 1985, p. 113).

In fact, this preDarwinian classification system "has undergone surprisingly little change in the times following it, and such changes as have been made have depended only to a trifling extent on . . . phylogenetic relationships" proposed by macroevolution, Mayr (1942, p. 276) concedes:

The fact is that the classification of organisms that existed before the advent of evolutionary theories has undergone *surprisingly little change* in the times following it, and such changes as have been made have depended only to a trifling extent on the elucidation of the actual phylogenetic relationship through paleontological evidence. The phylogenetic interpretation has been simply superimposed on the existing classification; *a rejection of the former fails to do any violence to the latter*. The subdivisions of the animal and plant *kingdoms established by Linnaeus are, with few exceptions, retained in the modern classification*, and this despite the enormous number of new forms discovered since then. The new forms were either included in the Linnaean groups, or new groups have been created to accommodate them. There has been no necessity for a basic change in the classification. (emphasis added)

Simpson (1945, p. 4) acknowledges that, with the appearance of Darwin, classification continued to develop with no immediate evidence of revolution in principles." Blackwelder (1977, p. 134) agrees that, since that time, "[e]volutionary taxonomy never has been really different from classical taxonomy, in spite of all the fireworks." Thus, classification does not really provide an argument in favor of macroevolution.

Another weakness, of the evolutionary school of classification, is that the use of classification to support macroevolution involves circular reasoning because "[c]lassifications are designed primarily, we are so often told, to 'state,' or 'reflect,' or 'be consistent with,' evolutionary and genetic relationships," Kitts (1977, p. 190) points out. Also see Gould (1983, p. 355). Further, a "classification based on the idea of phylogenetic descent must at best remain highly speculative, for, save in a few fossil lineages, we do not and cannot know the actual course of events in the evolution of a group," as Huxley (1942, pp. 394-5) noted. Finally, the "two sources of information are often contradictory" that are used by the evolutionary school of classification, according to Luria, Gould, and Singer (1981, pp. 763-4).

A final difficulty of the classification argument, the one point on which all schools of classification generally would agree, is that it has not yielded much information about evolutionary relationships. Mayr (1982, pp. 217-8) summarizes the "uncertainty" and "ignorance":

It comes as rather a *surprise to most nontaxonomists how uncertain our understanding of degrees of relationship among organisms still is today*. For instance, it is still unknown for most orders of birds

which other order is a given order's nearest relative. The same is true for many mammalian families and genera, for instance the Lagomorpha, Tubulidentata, Xenarthra, and *Tupaia* . . .

Honesty compels us to admit that *our ignorance concerning these relationships is still great*, not to say overwhelming. This is a depressing state of affairs considering that more than one hundred years have passed since the great post - *Origin* period of phylogeny construction. The morphological and embryological *clues are simply not sufficient* for the task. (emphasis added)

Part of that uncertainty results from the "artificial" and rather arbitrary nature of the higher categories of classification themselves, which Stansfield (1977, pp. 510-1) describes:

[H]igher categories (genera, families, orders, classes, phyla, and even kingdoms), though they do represent greater levels of evolutionary divergence, are still artificial groupings made by humans for convenience in taxonomic work. They are not natural groups the way that species are.

Raup and Stanley (1978, p. 138) agree that higher categories have "no comparable objective basis" or division points. Richards (1970, p. 1477) suggests a view that:

Nearly all biologists must share, the species is the only taxonomic category that has at least in more favorable examples a completely objective basis. Higher categories are all more or less a matter of opinion.

Hence, there is no objective meaning of or agreement on what classifications mean or what the categories are.

Thus, the classification argument for biological macroevolution and Darwinism actually provides no support, because classification is not scientifically or historically supportive of macroevolution, according to the transformed cladistic school and part of the phenetic school of classification. Specifically, classification "has nothing to say about evolution," according to proponents of transformed cladistics such as Patterson (1981a, p. 92), and "is fundamentally a non-evolutionary classification," according to critics such as Oldroyd (1986, p. 154). The evolutionary school of classification only supports macroevolution through circular reasoning, and is speculative, according to Kitts (1977, p. 190) and Huxley (1942, pp. 394-5). Here, the biological theories of macroevolution and Darwinism do not have a compelling status, according to many of their scientific proponents.

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## QUOTE

The God of the Bible is the self-revealing infinite Spirit. He is the sovereign source of all other being. He is the authoritative and purposive ruler of nature and history, the specifier of a worthy way of life and ordainer of the community of faith. He stands at the commencement of a flawlessly created cosmos, one which at every stage of its origination He as its Maker declares to be good.

Henry, Carl F. H. 1986. The God of the Bible and moral foundations in Burke, Thomas J., editor, *The Christian vision: man and morality*. The Hillsdale College Press. Hillsdale, MI p. 2.

## MINISYMPOSIUM ON OF SPEED LIGHT—PART I

## HAS THE SPEED OF LIGHT DECAYED RECENTLY? — PAPER 1

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**Abstract**

*The hypothesis that the speed of light has decayed, presented by Trevor Norman and Barry Setterfield in *The Atomic Constants, Light, and Time* is shown to be unsupported by an objective analysis of the actual historic measurements of the speed of light given in that report. The implications of the hypothesis for radioactivity and radiocarbon dating are shown to be unacceptable.*

**Introduction**

This critique is the result of an intensive investigation into *The Atomic Constants, Light, and Time*, by Trevor Norman and Barry Setterfield (1987). In that report, the authors advance the hypothesis that the speed of light,  $c$ , has been decreasing in the past. This critique presents my reasons for rejecting the above hypothesis. Though the analysis provided here is not exhaustive, I believe it is sufficient to show that general support by the creationist community, of the decay of the speed of light hypothesis, is not warranted by the data upon which the hypothesis rests.

The supplement to the technical report, *Geological Time and Scriptural Chronology*, by Barry Setterfield is not critiqued. However, since this supplement is based on inferences from the technical report, rejection of the decay of the speed of light hypothesis leaves the supplement without a logical basis.

A superficial reading of the technical report and supplement is sufficient to produce an uneasy feeling about the decay of  $c$  hypothesis advocated by Norman and Setterfield for two reasons. First, creationists have historically argued that the fundamental physical constants show a remarkable degree of fine tuning and that such precision is clear evidence for special creation. For example, the hydrogen atom could not exist if the mass of the proton was just 0.2% greater (DeYoung, 1985). This historic position stands in sharp contrast to the hypothesis advanced by Norman and Setterfield, which claims that the speed of light has decreased by roughly a factor of 10 million since the creation of the universe.

Secondly, the proposed decay of  $c$  shows a very improbable behavior. According to Norman and Setterfield's theory, the rate of decay of  $c$  was greater than 1 km/s/year for the entire span of time from creation until about 1920 A.D. Experimental precision has been such that a decay rate of 1 km/s/year would have been easily and unquestionably verified from about 1950 A.D. onward. It seems a remarkable coincidence that direct experimental corroboration of the theory, open for thousands of years as a theoretical possibility, should have eluded us by just 30 years.

This coincidence, coupled with a knowledge of the normal behavior of physical measurements, strongly recommends serious consideration of the alternate hypothesis that  $c$  has been constant.

**Norman and Setterfield's Data Analysis**

The text, *Data Reduction and Error Analysis for the Physical Sciences*, by Philip R. Bevington begins:

It is a well-established rule of scientific investigation that the first time an experiment is performed the results bear all too little resemblance to the "truth" being sought. As the experiment is repeated, with successive refinements of technique and method, the results gradually and asymptotically approach what we may accept with some confidence to be a reliable description of events. (1969, p. 1)

Thus, any gradual and asymptotic approach of  $c$  to its present-day value, which Norman and Setterfield find in the data, needs to be carefully scrutinized to determine if the effect is due to real, physical changes in the structure of the universe, or if it is merely the result of the normal behavior of physical measurement.

It is also well known that a given body of data can be easily, inadvertently manipulated, due to subjective bias and statistical analysis misapplied in such a way as to yield unwarranted conclusions. The best way to avoid this problem in the current context is to treat the entire data set (consisting of 163 values for the speed of light derived from 16 different experimental methods spanning the past 300 years; tabulated in nine groups in *The Atomic Constants, Light, and Time*) as a whole. This minimizes the number of subjective decisions which must be made and enhances the possibility of discerning any real overall trend in the value of  $c$ , since it is very unlikely that 16 different experimental methods would all accidentally and independently conform to the same mathematical equation describing  $c$  decay, if  $c$  was constant.

Unfortunately, the authors of the report spend most of the time, which is devoted to  $c$  data considerations, discussing and analyzing the data in separate, small groups for any  $c$  decay trend within the group. They do, in one place, however, consider the whole body of data collectively. In this one instance, they use the standard, non-weighted least squares technique to fit the best straight line to the data, and conclude: "When all 163 values involving 16 different experimental methods are used, the linear fit to the data gives a decay of 38 km/s per year." (p. 25)

Actually, giving the computed decay rate without an estimate of the precision to which that rate has been determined is not very helpful. A decay rate of 38 km/s/year with an uncertainty of  $\pm 100$  km/s/year would not support the theory of  $c$  decay very well, whereas a decay rate of 38 km/s/year with an uncertainty of  $\pm 1$  km/s/year would seem quite convincing.

The least squares technique provides an objective estimate of the uncertainty in the decay rate determination. However, this number does not appear to be

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included in the report. I have repeated the calculation using the 163 data points and obtained the result,  $38 \pm 8$  km/s/year.

If this was the end of the matter it would provide powerful evidence in favor of the  $c$  decay hypothesis. Unfortunately, even a cursory glance at the data reveals that the above analysis is inappropriate for the given data set hence, Norman and Setterfield's conclusions are not valid.

**The Data Reanalyzed**

*The Atomic Constants, Light, and Time* tabulates 163 values for the speed of light derived from measurements spanning the past 300 years. A graph of this entire data set does not appear in the report, but is shown in Figure 1. This figure displays the percent difference between the measured value of  $c$  and the modern value for the speed of light given in the report of 299,792.458 km/s. The vertical lines on some of the data points are error bars which express the range of uncertainty in the measurement which was reported by the researcher. These are normally reported in such a way that there is roughly a 68% chance that the error bars encompass the true value, assuming there are no undetected systematic errors in the measurement.

The range of uncertainty was not reported for many of the earliest measurements, so some of the data points are plotted without error bars. Most of the data points after 1850 do have error bars but they are too small, in most cases, to be seen on the scale of the graph. The relatively few data points between 1850 and 1900 which have very large bars result from two indirect methods of measuring  $c$  which yield low-precision results.

In a non-weighted least squares fit, every data point has equal weight in determining where the best fit straight line should be drawn through the data. For a data set consisting of measurements having error bars of varying lengths, it is not appropriate to give every data point equal weight. It is standard practice to weight the data points in inverse proportion to the size of their error bars. That is, data points with large error bars (greater uncertainty) have less impact on where the best fit straight line would be drawn than do data points with small error bars. This is especially impor-

tant for the current data set since the reported error bars range from  $\pm 20,000$  km/s to  $\pm 0.0003$  km/s, spanning roughly eight orders of magnitude.

Before proceeding to the proper weighted computation of the straight line which best fits the data, it is necessary that all data points be supplied with error bars since the original researchers have not always supplied an error estimate. This can be done in a fairly accurate and objective fashion by calculating the standard deviation of a group of neighboring data points from the given data set which were obtained using the same experimental method of measurement. The absolute lengths of the error bars do not need to be known very accurately. Even if the estimates of the standard deviation are off from the true error bar length by a factor of two or three, the resultant weighted fit will be a vastly superior representation of the true nature of the data than was the unweighted fit which treated all data points equally, even though they differed in precision by orders of magnitude.

Only 28 of the 163 data points tabulated were given without error estimates. Of these, the first one needs to be dealt with separately, in detail, before the others are discussed briefly. This is the data point in the upper left hand corner of Figure 1. It is attributed to uncorrected observations of the Roemer type, by Cassini, in 1693. To obtain the speed of light by this method, the Earth's orbital radius is divided by the measured time of transit of that radius by light (about 8 minutes, 20 seconds, today). The following quote from Norman and Setterfield is illuminating:

Observations by Cassini (1693 and 1736) gave the orbit radius delay as 7 minutes 5 seconds. Roemer in 1675 gave it as 11 minutes from selected observations. Halley in 1694 noted that Roemer's 1675 figure for the time delay was too large while Cassini's was too small. (p. 11).

Since Roemer and Cassini's results used the same method, separated by only 18 years, it is appropriate to use these two time observations to calculate the sample standard deviation. The result is an uncertainty of  $\pm 166$  s in the two measurements. This yields error bars for Cassini's determination of  $c$  of  $\pm 138,000$  km/s (or about  $\pm 39\%$ ).

This is much larger than the error bars shown on Roemer's  $c$  determination shown at 1675, in Figure 1. The reason for this difference is that Norman and Setterfield have chosen to use a reworked or "corrected" value for Roemer's  $c$  determination and an uncorrected value for Cassini's. It is peculiar that Norman and Setterfield were content to use an uncorrected value for Cassini considering the comments by the eminent and talented Halley. It is also unfortunate\*, since this single, anomalous point is responsible for most of the apparent 38 km/s/year decay, which they report.

\*I say "unfortunate" because Norman and Setterfield have at this point, been misled into believing that the historical data strongly supports their hypothesis (i.e. shows a decay of  $38 \pm 8$  km/s/year) mainly because of this one data point. Had they removed this single data point and analyzed the remaining 162 data points in the same way, they would have found an apparent  $c$  decay rate of only  $11 \pm 6$  km/s/year. This result would have suggested that support for the hypothesis from the historical data is marginal, at best. (Of course, even this conclusion would not have been valid, since an unweighted fit is not appropriate for this data set.)

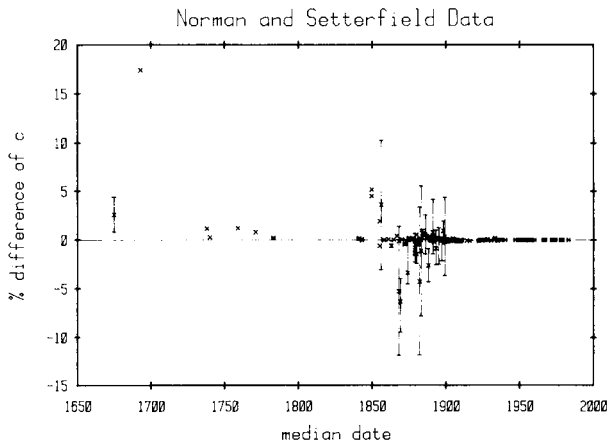


Figure 1. The 163 data points used by Norman and Setterfield shown as percent deviation of  $c$  from the modern value (i.e.  $\delta c = ((c - 299,792.458)/299,792.458) \times 100$ ) versus the median date of observation.

The 1740 determination attributed to Bradley was also a special case. For this data point, it was clearly most appropriate to calculate the standard deviation of the data from which it was obtained as an average (i.e., Table 2 of Norman and Setterfield less Busch's re-workings).

The remaining error bars were determined by calculating the standard deviation of a neighboring group of data points obtained using the same method of  $c$  measurement. These were calculated from the given values of  $c$  directly. This involved the use of three to eight neighboring data points, always spanning a time interval of less than 20 years.

When the entire data set of 163 points, complete with error bars was analyzed, using the standard, weighted, linear least squares method, the decay of  $c$  was determined to be:

$$\text{decay of } c = 0.0000140 \pm 0.0000596 \text{ km/s/year.}$$

This result shows plainly that there is no discernable decay trend in the data set presented by Norman and Setterfield.

Norman and Setterfield call into question the validity of the post 1972 data points in the technical report, as follows:

It is significant that the eight laser values from 1972 to 1983 were using the atomic time and frequency standards. It is therefore inevitable that the constancy of  $c$  in the atomic time frame will be reflected in the measurements. It is for this reason that no statistically significant trend was revealed in that period. (p. 45)

I have analyzed the data set exclusive of these suspect data points, since they could possibly obscure an earlier decay trend in the  $c$  data by their constancy and very high precision. Using the same weighted linear least squares technique, the result is:

$$\text{decay of } c = 0.0101 \pm 0.0068 \text{ km/s/year.}$$

Thus, there is still no discernible, statistically significant decay trend.

The conclusion is inescapable, that the  $c$  data upon which the  $c$  decay hypothesis rests, when analyzed objectively and in an appropriate fashion, does not support the hypothesis. This should be sufficient to cause the  $c$  decay hypothesis to be dropped from the repertoire of creationist answers to difficult questions. However, those who would like to preserve the hypothesis of  $c$  decay will probably point out that I have not shown the hypothesis to be false, but merely that it has no basis of support in the actual  $c$  measurements. Is it possible that the decay of  $c$  hypothesis advanced by Norman and Setterfield may still be right?

In my opinion, a look at the implications of the theory for radiocarbon dating alone is sufficient to eliminate this possibility.

### The Implications

Much of the impetus behind the decay of  $c$  hypothesis stems from its implications which are generally viewed as favorable to the young earth creationist position. In particular, the knotty problem of starlight from distant galaxies seems to be solved in a straightforward, naturalistic manner. The universe, however, is a very intricate machine and it is not possible to

modify the speed of light without a host of ensuing changes in other areas. Indeed, the implications of the theory have been extended far beyond the starlight problem to bring about an apparent coup for creationists in all areas. In the words of one creationist writer:

Can one simple scientific assumption radically alter the dimensions of the age of the universe, eradicate the 'Big Bang,' sink the Nebular hypothesis, undermine geological Uniformitarianism, and destroy Darwinism all at the same time? (Montgomery, 1987)

One of the implications of the decay of  $c$  hypothesis advocated by Norman and Setterfield is that of its effect on radioactive decay. According to Norman and Setterfield, the decay of  $c$  is just a symptom of a more fundamental change which has been occurring in the basic structure of the universe since creation. This change is that time as measured by atomic clocks has been different from time as measured astronomically (called dynamical time). Norman and Setterfield state it this way:

As a consequence, it would seem that the further back in the past we go, the more quickly the atomic clock ticked. It therefore registers a systematically old date when compared with the dynamical standard.

All forms of dating by the atomic clock are subject to this effect. This includes radiometric dating whether it be the uranium/lead, thorium/lead, lead/lead, rubidium/strontium, potassium/argon, carbon 14 or any other. (pp. 82, 83)

The overall effect is that 18 billion years of atomic time can be rescaled to fit within less than 10 thousand dynamical years. Apparent ages of billions of years, as deduced from radiometric decay dating techniques, are thus posited to be due to the more rapid rate of decay of atoms in the past.

It is apparent that a past, accelerated radiometric decay rate presents serious problems of excess energy release via radioactive decay. For example, a solid outer crust for the Earth would not be possible and no life would be able to survive the intense radiation fields resulting from the postulated increase in decay rate. The authors have anticipated this problem. They suggest that though the number of gamma ray photons resulting from radioactive decay would have been 10 times greater when  $c$  was 10 times larger and though each photon would have had the same energy as a photon from such a decay today, still: "... it requires 10 radioactive atoms to decay in unit time back then to give the same total flux,  $\dot{S}$ , or intensity, equal to that now." (p. 56)

The argument advanced by Norman and Setterfield to support this conclusion seems to me to be unnecessarily abstruse. Simply stated, all of the energy which is released from the radioactive decay of an atom must eventually show up somewhere if energy is to be conserved. For the decay of uranium in the earth, for example, the energy released by the decay of a single atom is ultimately all converted to heat energy in the Earth. If the decay rate was 10 times higher, the heat production in the Earth would also be 10 times higher. Similarly the energy deposited in living tissues by 10

times higher radiation fields would be 10 times higher. These conclusions are only dependent on the conservation of energy; the details of energy transport are irrelevant.

This is an enormous problem for the decay of  $c$  hypothesis, when it is recognized that the particular behavior of  $c$  which is advocated by Norman and Setterfield does not imply a rate of decay merely 10 times higher in the past, but over a million times higher for all of Earth history prior to about 3500 B.C. It is very difficult, if not impossible, to imagine how such a huge increase in the energy released by radioactive decay would be tolerable to life.

### Radiocarbon

The implications for radiocarbon dating have not been anticipated in the report, but are no less serious than the problems pointed out above. According to the report the following mathematical equation for the speed of light at any dynamical time,  $T$ , can be used:

$$c = a + b \times T^2 + d \times T^8 \quad (1)$$

where  $a = 299792 \text{ km/s}$ ,  $b = 0.01866 \text{ km/s year}^2$ ,  $d = 3.8 \times 10^{-19} \text{ km/s year}^8$  and  $T = (1961 - t) \text{ year}$  where  $t$  is the year A.D. Norman and Setterfield have no qualms about applying this equation to periods of time well outside the several-hundred-year span of the data from which it was derived, extrapolating it back to the origin of the universe. The extrapolation of this equation is much less severe and, hence, much more justifiable in the case of radiocarbon since we are dealing with relatively recent atomic times.

Equation 1 can be used to produce a formula for calculating atomic time,  $\tau$ , given the dynamical time,  $T$ , and vice versa, as follows:

$$\tau = \int_{T=0}^T \frac{c(T)}{c_0} dT, \quad c_0 = c(T=0) = a \quad (2)$$

thus,

$$\tau = T + \frac{b}{3a} T^3 + \frac{d}{9a} T^9 \quad (3)$$

(This equation is not given in the report but checks with the rescaled dates given by Setterfield in the supplement.)

Most creationists would agree that the results of radiocarbon dating are valid, at least back to the time of Christ. The Dead Sea Scrolls, for example, can be dated paleographically, and by inference from excavations at Khirbet Qumran to between 135 B.C. and 68 A.D.; cloth associated with the scrolls was radiocarbon dated to 20 B.C.  $\pm$  200 years (Yamauchi, 1975). According to Norman and Setterfield's theory, however, radiocarbon is not valid in this period since the decay constant was changing as  $c$  decreased. As a result of their theory, the above agreement between the different methods of dating the Dead Sea Scrolls must be regarded as fortuitous; the radiocarbon date, 20 B.C. (i.e.,  $\tau \approx 2000$  years), corresponds to a "true" dynamical date for the Dead Sea Scrolls of about 800 A.D. when rescaled according to equation 3.

The difficulties intensify further backwards in time. Consider the deck board obtained from the Egyptian funerary ship of Sesostri III. This specimen is dated archaeologically to between 1845 and 1831 B.C. and

yields an uncorrected radiocarbon date only a few percent younger than this (Save-Soderbergh and Olsson, 1970). If we accept the radiocarbon date as approximately correct, (i.e.  $\tau \approx 3700$  years) then the "true" archaeological age, according to Norman and Setterfield's theory is about 1370 years old, or 600 A.D. If, however, radiocarbon has somehow been disturbed so that it cannot be used for even approximate dating back to 1845 B.C. and the archaeological date is accepted as approximately correct (i.e.  $T \approx 3800$  years), then we have an even greater problem. A dynamic age of 3800 years yields an atomic age of about 23 million years. This corresponds to over 4000 half-lives for radiocarbon decay. Thus, there must have been greater than  $2^{4000}$  (or  $10^{1204}$ ) times more radiocarbon in this deck board when its tissues were living than is measured in it today. This condition cannot be met even if it is assumed that every atom in the whole board was originally radiocarbon!

### Conclusion

The hypothesis of a decaying speed of light developed by Norman and Setterfield in *The Atomic Constants, Light, and Time* is not supported by the experimental measurements of the speed of light for the past 300 years presented in that report. Furthermore, the implications of the theory are unacceptable for radioactive decay, in general, and radiocarbon dating, in particular. In the absence of positive, historical, experimental evidence for the decay of  $c$ , and in the face of the unacceptable implications of the theory, the hypothesis must be rejected.

N. Ernest Dorsey, writing on this same topic of  $c$  decay in 1944, noted that trends in the measurements prior to his writing were explained by many:

... as probably arising in large part from two all but universally acting causes: (1) the observer's exaggerated opinion of the accuracy of his own work, and (2) his inability to avoid being influenced in some measure by his preconceived opinion as to what he should find. (p. 2)

In my opinion, the problem of subjective bias in scientific endeavor is likely responsible for the current widespread scientific consensus in favor of macroevolution, despite the lack of support for that hypothesis in the actual data. But it will hardly do to point out slivers of this nature in the anti-creationists' eyes, if we are content to have logs in our own eyes.

### Acknowledgements

The author thanks Lambert T. Dolphin for aid in clarifying some aspects of Norman and Setterfield's technical report. The assistance of Institute for Creation Research graduate students Dave Rush (who obtained a host of library materials used in the production of this paper) and Max Chappell (who offered a helpful computation suggestion) is greatly appreciated. Mr. David Unfred provided a valuable, independent appraisal of the statistical analysis given in *The Atomic Constants, Light, and Time*.

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## HAS THE SPEED OF LIGHT DECAYED RECENTLY? — PAPER 2

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### Abstract

*Because its historical research and statistical analyses have no depth, this book (Norman and Setterfield, 1987) fails to prove that the speed of light has decreased over the past three centuries. Its theoretical interpretations are flawed, and in some parts do not make sense.*

### Introduction

This monograph (Norman and Setterfield, 1987) is an invited research report prepared for SRI International. The principal author appears to be Mr. Setterfield, a non-degreed researcher who has been studying the velocity of light since 1980. I have been corresponding with him since 1985 when I reviewed the early versions of his manuscript. I know little about Mr. Norman who was not a co-author at that time. For clarity I divide the book into two parts and discuss them separately. I call the first half (chapters 1-3) Part One, a statistical analysis of 163 values for the speed of light measured experimentally since 1668 A.D. The second half (chapters 4-7), Part Two, discusses the theoretical implications.

Part One aims to prove a very controversial thesis: that the speed of light has declined by a few tenths of a percent over the last 300 years. Such a decline would be very significant to physicists because the speed of light,  $c$ , is involved in almost all phenomena. A decline would also be very important to creationists in particular because, extrapolated into the past, it could provide evidence that light traveled much faster in the early days of the cosmos. That would explain how Adam could see light from the stars within a few days of their creation [Genesis 1:14-18], and how light from distant galaxies could reach us in only thousands of years.

The alleged decline may actually have occurred but at this time it does not seem likely. In spite of the fact that I would welcome such a simple explanation for the speed-of-light problem, I have serious doubts about the monograph. I have questions about the depth of the historical research and the quality of the scholarship involved. These questions are extremely important in this kind of historical survey of experimental data because interpretation can be difficult. One needs to understand the details of a particular experiment fairly well in order to assess its reliability and accuracy. In this case, when a survey of hundreds of experiments over centuries of time is involved, it is vital for the reader to have confidence (A) that Norman and Setterfield have researched their subject in sufficient detail and (B) that there is no bias, conscious or unconscious, in their analysis or presentation. When I did my early reviews, I had naively assumed that these points did not need to be questioned. But when I received the finished book,

my confidence was undermined by disturbing details that kept emerging as I examined the work.

### A Disturbing Quote

The first of these unsettling details was the way Setterfield quoted some of my words. It was a minor incident but it turns out to be typical of a major problem with the whole monograph. Shortly after I began corresponding with Setterfield, I sent him a brief note on 24 May 1985 thanking him for the articles he had sent me and saying of the general goals of his research, "It is a good work that needs to be done." Then Setterfield sent me a preliminary draft of what later developed into his book asking me to review it for technical problems. On 8 September 1985 I wrote him:

... the most effective theme for a first article would be a rigorous statistical and analytical study of the historical measurements. It is very important that the essential point—the observed change in  $c$ —be established on rock-solid ground. I would leave out all theoretical arguments . . .

On 24 November 1985, I sent an even more specific letter, recommending "major surgery" for the article. In terms of the present book, what I had in mind was deleting Part Two (the theory) and expanding Part One (the empirical study), making a much deeper and more rigorous study, clearly explaining with diagrams the relevant history and experimental techniques. Setterfield added a little to Part One but nothing like what I had advised. As for Part Two, he modified his theory considerably but he ignored the essence of my advice, namely to drop the theory altogether.

With this background in mind, imagine my surprise when I found that, without asking permission Setterfield had spliced together parts of three of my personal letters and quoted them on the last page [p. 90]. And though he had not performed the "major surgery" I recommended, he included the sentence "It is a good work that needs to be done," from my May letter. Occurring among reviewer praises as they do, those words make it seem as though I approve of the book—even though I wrote the words before I ever saw the first draft! The quote seems to have deceived everyone who has seen it. To make myself perfectly clear: I did not think the book was a "good work" then, and I do not think so now.

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This misquote opened my eyes and shook my confidence in the good faith of the authors. If Setterfield could use my own words in a misleading way on a matter of relative unimportance, then could I trust him not to slant much more important points in his favor? I began scrutinizing the monograph much more carefully, checking references, quotes and calculations for myself. I found many discrepancies, the most important of which I have summarized below. The reader can judge for himself whether or not the monograph has the necessary quality of scholarship and scientific objectivity.

**The Roemer Affair**

On the question of depth of scholarship, an important example is how Mr. Setterfield has handled the data of a 17th-century astronomer, Ole Roemer, who was the first man to determine the speed of light. Roemer timed the eclipses of Io, a satellite of Jupiter, and noted the increasing delay as the earth moved in its orbit away from Jupiter, correctly ascribing the delay to the additional distance light had to travel. He accurately recorded (to the nearest second) the dates and times of over 50 eclipses from 1668 to 1678. Reckoning the speed of light from this data requires detailed calculations, and astronomers have obtained widely differing results from Roemer's data over the centuries. About 1973, an astronomer from the University of Virginia, Samuel Goldstein, began doing some careful analyses of the data, going back to photocopies of Roemer's original notebooks. At first he made a logical error in the analysis but when the mistake was discovered a few years ago, he corrected his work. The result was very important to Setterfield because a single accurate set of data from 300 years ago is more important to establishing the slope of the possible decay curve than dozens of experiments today (Figure 1). So one would expect that Setterfield would handle Roemer's data with particular care, perhaps going back to the notebook entries himself and repeating the analysis on his own. At the very least, one would expect him to correspond directly and frequently with Goldstein on the issue.

However, Setterfield apparently has not done this and he seems to have made a serious error as a result. On page 11 of the monograph, Setterfield quotes Goldstein as giving a speed of light 2.6% faster in Roemer's day than now, citing as his reference 21: "Goldstein, S. J., private communication, Feb. 25, 1986." I asked Setterfield for a copy of the Goldstein letter. Setterfield wrote in reply that the letter had not been sent to him and he did not have a copy of it. Instead, he had copied the quotation from a preprint of a new paper for *Ex Nihilo* by Vivian Bounds. When I wrote Goldstein for a copy of his letter to Bounds, he sent it. Also he added the information that he had stated his result ambiguously, apparently misleading both Bounds and Setterfield. What Goldstein had meant to say was the speed of light according to Roemer's data was 2.6% *slower* than it is now. Professor Goldstein has given me permission to quote the following from his 2 November 1987 letter to me: "The new result is that the velocity of light was slower in 1668 to 1678 by 2.6% than it is today. I do not think that the difference is significant, however." Here are the two values for the speed of light:

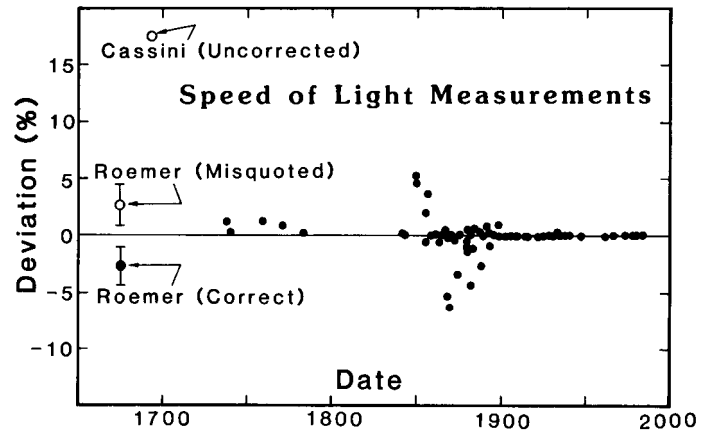


Figure 1. The 163 data points used by Norman and Setterfield, plus the correct Roemer point, showing the percentage deviation from today's value, 299,792 km/s. The two hollow dots represent incorrect data included in the book's 163-point fit (see text). If the two incorrect points are excluded and the correct Roemer point is included in a properly-weighted linear least-squares fit, no significant decay is seen. The book has no figure showing all 163 points. [Adapted from Gerald Aardsma, Institute for Creation Research].

Source	Date	Value (km/s)
Roemer/Goldstein	1668-1678	292,000
Recent measurements	1983	299,792

In other words, the Roemer data, when correctly analyzed, would flatten out the decay curve instead of making it tilt, thus working against the thesis of this book. Setterfield's mistaken value for the Roemer data was 307,600 km/s. He did not use the value in his analysis of the measurements using the Roemer-type method [p. 11], but he did include it in his analysis of all 163 values [p. 25]. Goldstein's revision was an important bit of knowledge. Setterfield misunderstood it because he seems to have violated several commonly-accepted practices among scholars: (1) Trace important data either back to its original sources or at least as far back as practically possible, (2) Never rely on an indirect quotation and (3) Ask for permission to publish private communications. The error bounds about the Roemer point, according to Goldstein, are a few percent. That is enough to include the possibility of no change since the 17th century but it casts doubt on the possibility of significant decay since then. More importantly, Setterfield's whole procedure in the Roemer case makes me doubt that he has really researched his subject sufficiently.

**The Cassini Data**

The next historical measurements the book cites were done by the astronomer Cassini in 1693, using Roemer's methods. Setterfield lists [p. 12] Cassini's value as 352,000 km/s, 20% higher than the corrected Roemer point, and 17% greater than points a few decades later, which are only a fraction of a percent greater than today's value. Since early estimates of c based on the Roemer data differed from the present value by similarly large amounts causing computational problems, the high Cassini value is probably an error of calculation. If an analysis like Goldstein's were done on Cassini's original data, a much more accurate value might be recovered. Setterfield cites [reference 14] some articles in French by Cassini, but he acknow-

ledged in a letter than he was actually depending on a secondary source's interpretation rather than on a direct analysis of his own. As in the case of the Roemer point, he did not include it in the analysis of the Roemer-type data, but did include it in the fit of all 163 data points. The Cassini point is so high and so long ago that it exerts a strong leverage on the fit (see Figure 1). Setterfield either should have tried a Goldstein-type analysis of Cassini's data or excluded the point from the fit since he acknowledges the value needs correction [p. 12, Table 1, note 2].

### The Missing Coefficient

Consider the question of conscious bias: have the authors knowingly withheld information detrimental to their hypothesis? One possible example of such bias is on page 13. There the authors withheld the *correlation coefficient* of a statistical analysis of 63 observations of stellar aberration angles (change of angle of starlight due to the earth's motion). The correlation coefficient is a statistical index which, in this case, indicates the degree of relationship between the speed of light and the date of measurement. A coefficient close to zero suggests little correlation; a coefficient near -1.0 implies a decrease in  $c$  as time goes on. For all but one of the data sets analyzed, the authors report correlation coefficients. Most are in the neighborhood of -0.95 to -0.98 which are the kind of numbers the authors want. In the case of the aberration data, they did a statistical analysis of all 63 points and got a decay rate (or slope) of -4.83 km/s per year. They reported the correlation coefficient on a subset of the data, 13 points, getting a coefficient of -0.947. However, they did not report the correlation coefficient for the whole set of 63 points. Why not? The correlation coefficient is -0.409, a rather low value. Is it just coincidence that the single unreported coefficient is the one least supportive of the authors' hypothesis?

### A Misleading Zero

Setterfield's Figure I on page 15 shows another example of possibly conscious bias, in this case a slanted presentation of data. The figure shows the "difference in  $c$  values" for a selected set of aberration observations from 1740 to 1940. However, it does not say what speed "zero" represents, i.e., it does not show what reference value of  $c$  the authors subtracted from the measured values to get the differences they plotted. Nowhere in the related text can I find a statement as to what the zero is for Figure I. In contrast to this absence, the very similar Figure II [p. 23] clearly states its reference point: "zero = 299,760 km/s," a value close to the one accepted today. Figures III and IV (the last figures in the book) show absolute values of  $c$  rather than differences but they each do have a dotted line showing today's value for reference. So all the book's other figures tend to lead the reader into believing that the zero of Figure I must be today's value of  $c$ . That was what I thought also, until I began to check the values plotted in Figure I against the values listed in Table 3 [p. 14]. Then I found that the zero in Figure I represents 299,500 km/s, an arbitrary number whose only significance seems to be that it coincides with the authors' fitted line on the right-hand side of the graph. The value is 292 km/s less than today's value, 299,792 km/s.

So why did the authors displace the zero and neglect to tell us its value, in contrast to what they did in Figure II? The values plotted in Figure II are all above today's value of  $c$ , in support of the book's hypothesis. But if you plot a line corresponding to the  $c$  of today in Figure I, you will find that *over half the points in Figure I are well below today's value*. In other words, the undeclared shift of Figure I's zero conceals some visual evidence against the  $c$ -decay hypothesis. Is it mere coincidence that this shift took place in the only figure where more candor would have worked against the book's main idea?

### Bias in the Analysis

In statistical studies a more subtle bias can occur, either deliberately or not, in the choice of analysis methods. A sentence on page 10 epitomizes an attitude which seriously unbalances the whole analysis: "Indeed, if  $c$  was constant, error theory indicates that there should have been a random scatter about a fixed value." Here the authors made a mistake about error theory. Many types of measurement or interpretation errors are indeed random. Such errors would cause the estimated value of  $c$  to fall randomly above and below the true value. However, other types of error are *systematic*, non-random errors introduced by the equipment or method. Systematic errors are usually unknown (otherwise the experimenter would work to eliminate them) and they can favor one side of a measurement. If there is some mechanism by which the errors to tend to fall systematically on the high side, then as experimental techniques improve with time, the one-sided errors would decrease and there would be a false downward trend in measured values, even if the true value of  $c$  were not to change. I did not think of this possibility myself until Gerald Aardsma of the Institute of Creation Research pointed it out to me.

This possibility of systematic high-side error explains one feature of the data that has bothered me for a long time—the steady, smooth flattening of the curve as it gets closer to our own time. Page 53 shows a curve which has  $c$  decreasing at over 10 km/s per year in the year 1740. By 1935 the rate is down to about 1 km/s per year and by 1979 there is essentially zero change. In other words,  $c$  has stopped changing for the first time in 300 years in our own decade, just when we can really measure it precisely. *Why should the rate of change of  $c$  diminish in direct proportion to our ability to measure  $c$  more accurately?* Setterfield nowhere answers this very natural question directly. Instead he keeps deferring it later and later into more and more intricate theory until both question and answer have receded from view in the mists and fog of Part Two. The simplest explanation would be that as measurement accuracy has increased, bias from systematic errors has decreased.

There are several ways to reduce the impact of such systematic errors. One is to do a *weighted* curve fit to the data, which gives measurements of high precision more emphasis. The idea is that if the estimatable random errors are low, then the unknown systematic errors are also likely to be low. However, Norman and Setterfield do not mention doing weighted fits and unweighted fits agree with their results wherever I did spot checks. So it appears that they did not make the

extra effort of protecting their analysis in this way. A second method of protection is to lump all the data from different methods together into one big analysis. The reasoning is that different experimental methods will have different systematic errors, so the systematic errors are less likely to affect the slope of the overall curve. Norman and Setterfield did do this but they included the erroneous Cassini point and the misquoted Roemer value, both of which were quite high. In all their other data fits, they isolated the data by type of measurement and treated each type separately, again making their analysis more vulnerable to systematic error. As other contributors to this mini-symposium have shown, *weighted* curve fits of *all* the data show no statistically significant decrease in the value of  $c$ .

Thus the whole book depends on the validity of the one implicit assumption Norman and Setterfield have made on page 10. However, it is very significant to me that when steps are taken in the analysis to reduce the effect of possible systematic error, the alleged decay of  $c$  disappears.

### A Suppressed Explanation

A few decades ago, a National Bureau of Standards scientist carefully examined the possibility of decay in  $c$  and suggested that systematic error could explain the trend [Dorsey, 1945]. Norman and Setterfield dismiss both the 110-page treatise and the suggestion with a few sentences:

However, *Dorsey did not address the main problem*. He failed to demonstrate why the measured values of  $c$  should show a systematic trend with the mutual unreliability of the equipment. . . Furthermore, in the seven instances where the same equipment was used in a later series of experiments, a lower  $c$  value has always resulted at the later date. *Dorsey had no satisfactory explanation for this phenomenon* [p. 10, emphasis mine].

Words such as these would certainly discourage the reader from examining Dorsey's article for an explanation of the trend. However, the reader would be deceived. In his introduction, Dorsey offers a very plausible explanation, one held by other experimental physicists experienced in  $c$  measurements:

As for the drift [the decrease] itself, they see it, prior to any critical study of several reports, as probably, in large part, a *psychological* phenomenon facilitated by the low precision of the earlier work, which not only obscured the effect of systematic errors, but made impossible the experimental discovery of such errors unless their effects were great. They see it as probably arising in large part from two all but universally acting causes: (1) the observer's exaggerated opinion of the accuracy of his own work, and (2) *his inability to avoid being influenced in some measure by his preconceived opinion as to what he should find*. . . An observer who thinks he knows approximately what he should find labors under a severe handicap. *His result is almost certain to err in such a direction as to approach the expected value*. The size of this unconsciously introduced error is, obviously, severely limited by the experimenter's data, by the

spread in his values. *The smaller the spread, the smaller, in general, will be this error* [Dorsey, p. 2, emphasis mine].

Dorsey explains, giving the example of Cornu in 1874, that most researchers introduce this bias honestly. There is always a wealth of reasons to reject any given datum.

Consider a hypothetical example, illustrated in Figure 2. In 1875 famous experimenter Alpha publishes his measurement of  $c$ :  $302 \pm 1$  Mm/s. Not being omniscient, Professor Alpha does not know (a) that the true speed of light is exactly 300 and (b) that his dog has chewed about 7 mm from the meter stick, thus shifting all his results upward by 2 Mm/s. Ten years later young Dr. Beta is pondering his own measurements. He is not omniscient either but fortunately he is using a correct meter stick. Also he has a more easily readable stopwatch, so his time measurements are more accurate. He has ten data points, three of which are below 300, a value which Beta considers ridiculously low. He is sure that the right value is somewhere at or above 301. He knows how meticulous Alpha was. Beta pours over his notes to find out what is wrong with those three low values. Aha! All three were taken right after he wound up the stopwatch; none of the others were. Let us see, a wound-up watch would run a bit faster, it would read longer times than it should and longer times would give lower speeds . . . that *must* be the reason! With joy he excludes the three offending points, writes his article (diligently explaining in a footnote why he excluded three bad measurements) and publishes his new value for  $c$ :  $301.0 \pm 0.5$  Mm/s. Years later, Beta's student Gamma wonders why published values of  $c$  have been steadily declining . . .

Thus an experimenter's natural reluctance to publish a value too different from preceding measurements introduces a systematic bias into his results.\* If the preceding values were too high, the bias will cause a tendency to be high also. But as methods improve with time, experimenters will be constrained by the precision of their results to numbers closer and closer to the true value. The curve would flatten out in exactly the way that Setterfield's curves do [Figures III and IV, pp. 43, 54]. This explanation by Dorsey answers my question as to why the speed of light should stop changing in our decade. If Norman and Setterfield were not satisfied with Dorsey's reasoning, they should have at least outlined it for the reader and explained the reasons for their dissatisfaction. Instead, they divert the reader from Dorsey's explanation and offer no clear alternative.

### Part Two: Physical Implications of Decay in $C$

The second half of the book is an attempt to interpret the effects of the presumed decay of  $c$  on electromagnetism, mechanics, gravitation, quantum physics, radioactive decay, time, paleontology, stars, galactic redshift and cosmology. The grandiose scale of this thinking suggests that the authors have succumbed to a common occupational disease among physicists, which I call the "Newton syndrome." I recognize the symptoms well, since I have suffered from them more than

\*Editor's note: This concept has been mentioned in a previous Quarterly. See DeYoung, D. B. 1976. The precision of nuclear decay rates, CRSQ 13:36-41.

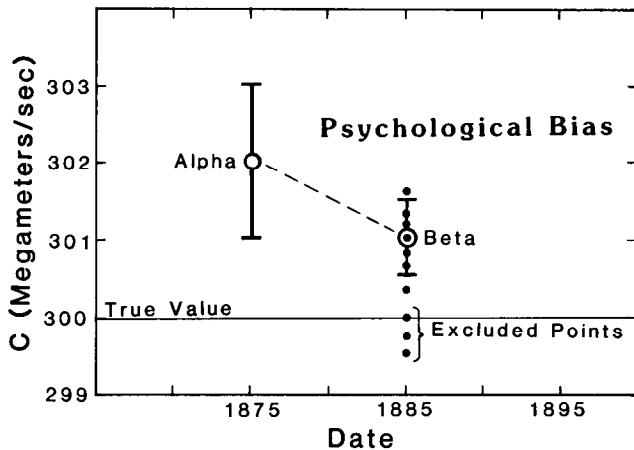


Figure 2. Hypothetical illustration of the effect of psychological bias on experimenters selecting which data to publish. Experimenter Beta influenced by experimenter Alpha's high published value of  $c$ , finds an excuse to reject three data points he considers too low. Beta's published average is below Alpha's, but still above the true value. Here depicted as 300 Mm/s: As methods get more accurate published values of  $c$  decline and asymptotically approach the true value.

once myself. Isaac Newton was the archetypal theoretical physicist and we all imitate him by trying to find the One Great Key which will unlock the riddle of the universe and enable us to make the Grand Unified Theory of Everything.

Unlike Newton's work, however, Setterfield's theory has neither a strong foundation nor sound construction. Starting from the shaky hypothesis that  $c$  really is decaying, Setterfield reasons approximately as follows: (1) He assumes implicitly that energy is conserved [pp. 29,33]. But in a cosmos where the speed of light is not constant, every physical law is likely to change, so this is an arbitrary assumption. Of course he must start somewhere, but he should discuss the assumption for the reader's benefit. (2) He assumes that distances remain constant, that is, measuring rods and even space itself are unaffected by the change in  $c$ . He only discusses this assumption in two sentences of the introduction [p. 4]. It needs more justification than that because General Relativity suggests that there is a deep connection between the speed of light and space. (3) Using the mass-energy equation  $E = mc^2$ , step one, and the  $c$ -decay hypothesis, Setterfield then deduces that mass is proportional to  $1/c^2$ , meaning that as  $c$  decreases, mass increases rapidly [p. 31].

Setterfield was at this stage when I informally reviewed the early versions of this book in 1985. Later I thought of an inconsistency and sent him a letter on 10 January 1987 containing the following simple example (Figure 3): Imagine a mechanical clock made of two equal masses tied together with a lightweight rod of fixed length, spinning freely in space about the center of mass. The rotation period is the basis of a clock by which we can measure the speed of light. It is easy to show that if steps 1, 2, and 3 are correct, then the rotation rate of the masses is directly proportional to  $c$ . This means that as  $c$  decreases, this particular mechanical clock slows in exact proportion. Setterfield had already deduced that atomic and nuclear process rates would decrease proportional to  $c$  [pp. 33-43]. I explained that the light-and-mirror clock used in relativity

textbooks would similarly slow down, and that probably the rates of all physical processes are proportional to  $c$ . I wrote:

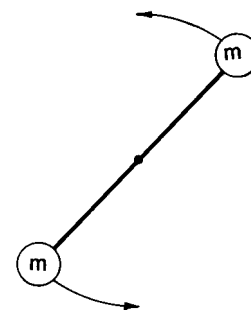
It looks like any clock ticks in lockstep with the speed of light, be the clock atomic, electromagnetic, mechanical, or gravitational. Here is the problem: if our measuring rods for distance remain constant in size, and if all our clocks have a rate proportional to  $c$ , then *how could we measure any change in the speed of light?*

There was silence from Australia for about the space of a month. Then I received a letter from Setterfield and within it was a new theory. I did not respond, and within six months the finished book was published, containing the new theory. Now Setterfield says that there are two types of time, "atomic" and "dynamic." Atomic time slows with  $c$ . Dynamic time, the periods of planets in their orbits, does not change. The reason it does not change, he says, is because he thinks gravitation is like magnetism, and he thinks that a change in the magnetic force constant (permeability) is responsible for the alleged change in  $c$ . This reasoning by analogy is tenuous and open to criticism, but without pausing he plunges onward. The Newtonian gravitational field strength,  $g$ , should be replaced by a magnetic-like field  $B^*$  which is the product of  $g$  and a new parameter, the gravitational analog of magnetic permeability. However, he does not explain exactly what  $B^*$  and the new parameter are. What are their units and values? How might we measure them? Without a definition of these new terms, his equations become meaningless.

This is not the end of Setterfield's revelations. There is also a new type of mass,  $M^*$ , which somehow differs from the mass he discussed in the previous pages,  $m$ . One would assume that  $m$  is the familiar mass that physicists all know, but it is not, says Setterfield: "In a dynamical context, we appear to be measuring mass in terms of  $M^*$  and acceleration in terms of  $B^* \dots$ " [p. 44]. So if the new mass and acceleration are what we have been measuring in physics then what are the old mass  $m$  and the old acceleration  $g$ ? Setterfield does not say.

Somehow the gravitational constant and masses change with  $c$  in such a way as to keep the period of

### Rotating-mass Clock



Period decreases with  $C$

Figure 3. Humphreys' counterexample to Setterfield's theory: a clock made of two masses tied together and rotating freely in space about the center of mass. Setterfield's early theory implies that this typical mechanical clock should slow down in exact proportion to the amount of slowing of the speed of light, thus making it impossible to detect any decrease in  $c$  with such clocks.

planets in their orbits constant. This construction of his mind makes a clock which ticks independently of changes in  $c$ , in contrast to electromagnetic clocks. Nowhere in Setterfield's letter or book does he say explicitly how all this applies to my example, which has nothing to do with gravitation. His foray into new realms of field theory is accomplished with stunning brevity upon a single page, 44. To the nonspecialist who does not speak the language of physics, all this may appear impressive. But speakers of the language can recognize that without clear and precise explanations of all the new terms, the theory is unintelligible: "So also you, unless you utter by the tongue speech that is clear, how will it be known what is spoken? For you will be speaking into the air" [1 Corinthians 14:9].

### General Remarks

There is much work behind this book; it took much time and effort to dig up the many details which it contains. Yet for all that, it seems incomplete and unpolished, as if done in haste. The list of 377 references is very redundant, with many entries repeated three or more times. For example, references 12, 26, and 96 are identical without so much as an *op. cit.* There are many other such redundancies, which tend to give the reader a mistaken impression that nearly 400 books and articles back up the monograph, instead of much fewer. The statistical analysis is very simple and does not appear to take advantage of the more advanced techniques now available. The exposition does not guide the reader as clearly through the wilderness of details as, for example, Dorsey's treatise does [Dorsey, 1945]. The scarcity of figures (four, as compared to Dorsey's twelve) obscures things. In particular, there is no graph such as my Figure 1, showing *all* the data from 1668 to now; the absence makes it difficult for the reader to form his own opinion about the alleged decay.

### Conclusion

Statistical analysis is a tricky art; it is easy to fool oneself and others with it. Consequently a study of this sort needs to apply the very best analytical techniques. In my opinion, the minimum acceptable analysis must do the following: (1) Use the correct Roemer point, (2) Correct or exclude the Cassini point, (3) Give each

point a weight proportional to its estimated accuracy and (4) Treat all the data from the different experimental methods together with as many points as possible. Norman and Setterfield have not done these things in their published work. When the minimum standards are applied, it appears that no statistically significant decay in the speed of light can be found.

The authors' practice of separating data into various groups seems to have made their analysis susceptible to distortion from systematic errors. One likely candidate for such error is the psychological bias each experimenter has when selecting data, as Dorsey pointed out. Such a bias would explain both the seeming downward trend within each group and the steady flattening out of the data curve as experimental accuracy improves. Norman and Setterfield offered no alternative explanation for the flattening trend, as far as I can tell.

The theory advanced by the authors starts with the *assumption* of a real decay in the speed of light, and the theory contains several more assumptions which are open to question. The lack of clear definitions of new terms on page 44 makes that part of the theory unintelligible.

The problem with this book is that it is misleading. If one reads it naively, assuming that the authors are presenting their case fairly and objectively, one gets the impression that the speed of light has decayed. However, if one examines it critically, carefully checking out the assumptions and calculations, one arrives at the opposite conclusion. This difference between the book's surface appearance and its deeper nature is already producing controversy among creationists. If it is any help to the reader, let him remember that I have been on both sides of the controversy. Also the reader should know that I actually favor the possibility that  $c$  has decayed sometime in the past, so that if anything I should welcome the book. But the underlying facts just do not support the hypothesis. The speed of light may well have decreased either during Creation week, after the Fall, or during the Flood. But after critically examining this book, I can find no statistically significant evidence that  $c$  has decayed during the last three centuries.

### References

Dorsey, N. E. 1945. The velocity of light. *Transactions of the American Philosophical Society*. 34:1-110.

### QUOTE

The naturalists did nothing more than take the old theological concept of providence and degrade it beyond recognition and meaning. They took "predestination" and stripped it to "destination." They had to believe that we were destined to go where we were going because we were parts of that vast system of interlocking causalities and had behind us the irresistible pressure of all previous forces. But they reduced such facts to unintelligibility by rejecting the concepts of Aristotle's first and final causes. It should not be overlooked that the components of the term *providence* signify "prevision" or "foresight." The word thus contains the idea of seeing, of knowing, or recognizing a conscious purpose. But the determinism of the naturalists in literature, in philosophy, and in science is a blind process. The process does not know where it is going; neither does anything outside the process. A good example of this confusion is the statement by a representative modern biologist that the evidence in nature compels us to believe that nature has a purpose. But this is not a conscious purpose, he adds, nor can we ourselves define it, since any final end or purpose is inconceivable. His determination to keep out all religious or metaphysical ideas forces him to talk about purpose which is not purpose (since nothing is aware of it) and of a goal which is not a goal (since it infinitely recedes). The theologians who used to debate "fixed fate, free will, foreknowledge absolute" never got into muddles like this. This encourages the hope that from evolutionism and other forms of naturalism some good metaphysician may yet deliver us.

Curtis, III, George M. and James J. Thompson, Jr. (editors). 1987. Contemporary southern literature in *The Southern Essays of Richard M. Weaver*. Liberty Press. Indianapolis. p. 55-6.

## ARTICLE REVIEW

The origin of the Moon by S. R. Taylor. 1987. *American Scientist* 75:468-77.

Reviewed by Eugene F. Chaffin\*

In the last few years a new model for the origin of the Moon has gained prominence as more and more professional scientists have jumped on the bandwagon in support of it. The article reviewed here is heavily in favor of the new "impact model." Taylor considers the situation after the Apollo Lunar landings: samples of Moon rocks proved the absence of water and depletion in elements such as thallium, bismuth and potassium which are volatile below 1000 K. Evidence also accumulated that the Moon might be enriched, relative to the Earth, in refractory elements such as aluminum and uranium. When one considers unique features of the Earth-Moon system, such as the large mass ratio of satellite to planet and the large total angular momentum, Urey, Jastrow and other Apollo scientists were left without a completely satisfactory model for Lunar origin. This is the opposite of what they had anticipated before Apollo. Urey for instance felt less inclined to support the fission theory, a model originally put forward by Charles Darwin's son G. H. Darwin (and encouraged by mathematical studies by H. Poincaré) in the last quarter of the 1800's. In that model tidal pull of the sun, or in some versions rotational instability, results in the Moon splitting off from the Earth. Urey instead flip-flopped over to the capture models in which a Moon formed elsewhere is captured into Earth orbit.

The new model, the impact or collisional ejection model, was proposed in the mid-1970's to overcome the difficulties of earlier models. The model resorts to catastrophism to explain the Earth-Moon system as a likely consequence of a collision of the Earth with a Mars-sized body formed in the inner solar system at about the same time as the Earth, about 4.4 billion years ago according to the evolutionary time scale. The model supposes that iron cores of Earth and impactor had already formed, so that collision resulted in ejection of mantle type material from both objects. In order to have sufficient material into a stable orbit

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beyond the Roché limit, A. G. W. Cameron and W. R. Ward proposed a gaseous expansion mechanism, in which some of the vaporized matter produced by the collision is pushed out far enough to later form an "accretion disk."

The model has two clear advantages. First, the effects of different impact points, impact angles, relative masses, etc. can be studied by computer simulations. Then the most favorable case may be put forward as the one which actually occurred. Apparently no one was there to tell the proponents of the model that they were wrong and the "infallible" computer gives the answer! Secondly, the composition of the impactor may be *chosen* to give the present composition of the Moon, since most of the vaporized material demonstrably comes from the impactor rather than the Earth. Thus the formulators of the model felt bound by the laws of physics and chemistry, but felt free to choose the initial conditions in whatever way gave results, since the initial conditions are no longer accessible to observation.

In the article, Taylor points out how the Moon's composition is explained very well by this new model and that the impact variables may be chosen to give the high angular momentum of the Earth-Moon system. The lack of volatile elements in the Moon is recovered by these mechanisms.

In 1978 a creationist book by Whitcomb and DeYoung about the Moon also appeared. It did not discuss the collisional ejection model since it had not yet gained prominence. They considered the Genesis account and gave arguments supporting the view that the Moon was created after the Earth, possibly (in God's foresight) to discourage Moon worship. The Genesis account states that God made the Sun and Moon on the *fourth* day. It is tempting for a creationist to try to explain the low content of volatiles in the Moon by saying that God drew the Moon out of the Earth. But plants and dry land already existed on the *third* day, hence the impact theory seems to vaporize into a contrived explanation, which does not hold much merit for a creationist.

### Reference

Whitcomb, John C. and Donald B. DeYoung. 1978. *The Moon: its creation, form, and significance*. BMH Books. Winona Lake, IN.

## BOOK REVIEWS

*The Collapse of Evolution* by Scott M. Huse. 1983. Baker Book House, Grand Rapids, MI. 178 pages. \$7.95.

Reviewed by Robert L. Goette\*

The author states the purpose of the book on page 2 of the Foreword,

. . . first, to expose the scientific fallacies of the theory of organic evolution; second, to present scientific evidence for Biblical creationism; and third, to prove that evolution and Biblical crea-

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tionism are mutually exclusive and cannot be reconciled.

In the Introduction it is clearly stated that:

It is false to assume that evolution is an established fact of science; that evolution is often accepted not because it can be proven by scientific evidence but because the only alternative, special creation, is totally unacceptable to the non-Christian.

Theistic evolution, Dr. Huse stated, is a compromise position taken by some who claim to believe in God and the Bible while maintaining that all life has evolved from inorganic chemicals. His position is that the

theory of evolution is a blunder of science and that while neither evolution nor Biblical creation are accessible by the scientific method and must be accepted by faith, we cannot say that known scientific facts and observations are useless and to be ignored or discarded.

In the chapters which follow, the author presents reasons, drawn largely from writings of Dr. Henry Morris and his co-workers at the Institute for Creation Research, in the areas of geology, physics, mathematics, biology and anthropology which support Biblical creationism and are non-supportive of evolutionary theory. In Chapter 6, "proofs" commonly used to support evolution theory, such as archaeopteryx, fossil horse series, and the Miller-Urey origin of life experiments are critiqued.

Chapter 7 very briefly lists some scientists, living and dead, who believe in Biblical creationism. The eighth and last chapter lists some specific unreconcilable differences between the Bible and evolution. In concluding remarks the author stresses the point that belief in atheistic evolution has led many people to reject not only the Biblical account of creation but also the entire message of the Bible.

Appendix B contains a very helpful list, extensive though not exhaustive, of creationist organizations in various parts of the world and their addresses. Appendix C is a glossary which will be of help to a reader who has not had a scientific background. Useful Scripture, name and subject indexes are also included.

While the book is quite readable by the average person, it would not give sufficient detailed evidence for convincing the average evolutionist to rethink his position. References quoted in the text are listed in a table at the end of the book, but the form of these references is ambiguous. In many cases the number of pages in the book or pamphlet is given rather than the specific page(s) where the reference is to be found. Merely referring to a 479 page book is rather useless for someone wanting to examine the original author's statement! Many of the references used by the author are summaries rather than direct quotes from other authors. In all too many cases, references to statements of evolutionary writers only take one back to another creationist book where the statement is quoted. This makes it very difficult to find the context in which the original writer is speaking. When dealing with such a controversial matter as creation/evolution, it would be well to have specific detailed references to the original literature.

On page 16 the author states, "Such concepts as overthrusting are simply viewed by creationists as ploys to explain away the numerous contradictions encountered in the field," and then he indicates that this is from Whitcomb (sic), *The World That Perished*. Dr. Whitcomb does not make this statement, nor is the statement true. In the filmstrip and booklet, of the same title, *Fossils, Strata and Evolution* by John G. Read, pp. 12-17 (Scientific-Technical Presentations, Culver City, CA) it is clearly shown with excellent pictures that there are evidences of overthrusting in certain areas. However, in the case of the Lewis Overthrust of Montana and Alberta (which is one of two referred to by Dr. Whitcomb in his book) the characteristic layer of fractured rock found beneath an overthrust layer is completely missing, thus proving

that the Lewis Overthrust was not formed by overthrusting, but rather laid down by a worldwide catastrophe such as the Genesis Flood (more detail on this can be seen by reference to the filmstrip and booklet cited above).

On page 99 Dr. Huse, in referring to the problems of accepting Java Man as an Ape-Man says,

For instance, how is it possible to reconstruct so completely with such confidence from such scanty evidence? How can the 'experts' be so certain that all the pieces came from the same animal? How have these unpetrified bones managed to survive for so long without disintegrating?

According to both Bowden, *Ape-Man: Fact or Fal-lacy?* (second edition), Sovereign Publications, Kent, England (1977) pp. 139-40 and Cousins, *Fossil Man*, (revised edition), Creation-Science Movement, Portsmouth, England (1971), pp. 40, 42, the bones were fossilized. Since Dr. Huse gave no reference on this particular statement about unpetrified bones, it was not possible to trace the source of that information.

Care should be taken by all of us who believe strongly in the truth and reliability of Scripture as to how we speak in terms of those with whom we disagree. Therefore, such statements as the one found on page 128, "The collapse of evolution, then, is already a reality in the minds and hearts of the well-informed," while true, could better be stated in terms which do not sound so arrogant.

In spite of the difficulties mentioned above, *The Collapse of Evolution* would be of value to those wanting a quick overview of the creation/evolution problem that does not tell them, perhaps, more than they want to know. For others, this book will only be a beginning and other more detailed books will need to be consulted in which the matters mentioned by this author are developed in greater depth.

*Evolution: The Incredible Hoax* by Homer Duncan, 1982. 93 pages. \$3.00.

*Evolution: Fact or Fantasy* by Homer Duncan, 1986. 40 pages. \$1.00.

*Evolution: True or False* by Homer Duncan, 1986. Missionary Crusader. International Publications. Lubbock, TX 79411. 25 pages. \$1.00

Reviewed by Clifford L. Lillo\*

These three anti-evolution booklets by Homer Duncan are similar in scope and content. The first, *Evolution: The Incredible Hoax*, contains the greatest amount of information, but the smallest, *Evolution: True or False*, would make an excellent handout to give to friends.

In *Evolution: The Incredible Hoax* the author describes some well-known hoaxes, such as the selling of the Eiffel Tower by con man Victor Lustig, and then illustrates how evolution itself is also a hoax. The author provides a summary of the battle between creationists and evolutionists. He lists organizations he feels are responsible for turning the tide against evolution, including:

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The Creation Research Society  
Institute for Creation Research  
The Bible-Science Association

The remainder of the booklet consists of quotations from scientific literature and from the Bible to reinforce the author's premise that evolution is a hoax.

In *Evolution: Fact or Fantasy* Duncan quotes 12 statements from a brochure published by the National Academy of Sciences in 1984, *Science and Creationism*. It strongly opposes the teaching of creationism in the public schools. Duncan provides his response to each statement and quotes from a leading scientific authority to show errors made by the authors of *Science and Creationism*. He then lists 10 charges commonly heard about the creation/evolution controversy and answers each in turn, sometimes quoting other authorities to reinforce his own statements. In the last section, Duncan presents three reasons men believe in evolution and follows this with a "Four-Fold Appeal" 1) to evolutionists to investigate what he has written about evolution, 2) to evolutionists to consider the claims of Jesus Christ, 3) to Christian scientists and educators to come out of the woodwork and take a stand for Christ, and 4) to all Bible-believing Christians to join the battle against evolutionism.

The third pamphlet, *Evolution: True or False*, is, in the author's words, "a condensation of the larger booklet, *Evolution: Fact or Fantasy*. In this case, Duncan does not mention the National Academy of Sciences booklet, *Science and Creationism*, in his text but does footnote some of his quotes from that publication. At the end of Duncan's second and third pamphlets, there are lists of "Organizations Leading in the Fight Against Evolution," but for some reason the Creation Research Society has not been included.

*Origin Science: A Proposal for the Creation-Evolution Controversy* by Norman L. Geisler and J. Kerby Anderson. 1987. Baker Book House, Grand Rapids, MI. 198 pages. \$8.95.

Reviewed by D. R. Boylan\*

This book attempts to clarify the confusion in the use of terms in scenarios related to origins. The authors propose the terms "operational science" and "origin science" to distinguish between concepts that can be repeated, tested and validated and those concepts which cannot. With such distinction, the authors pose that "origin science" is a valid part of science. It deals with past singularities much like forensic science deals with events that occurred once in the past and cannot be repeated in the present. Just as the forensic scientist attempts to explain past events by analogy and causality, the origin scientist attempts to explain origins (the origin of the universe, the origin of life, the origin of new life forms) by analogy and causality. It is asserted that origin science deals with primary causes for singularities. In the case of the evolution/creation controversy this leads to the postulate of a supernatural cause.

The main argument of the book is that ". . . both 'evolutionist' and 'creationist' views on origin should be brought into the domain of singularity science

about the past and . . . each should be judged by the principles of that kind of science . . ." The authors, therefore, call science about the past "origin science." It differs from science about the present regularity of nature (called "operational science") in that ". . . the latter focuses on recurring patterns of events in the present against which . . . theories can be tested . . ." This is not true of singularity science.

Although science generally deals with repeatable, testable and regularly occurring events in the present, it can deal with events in the past, both regularities and singularities. The authors point out that when the ". . . object of study is a regularity it always has a secondary (natural) cause. Only when the object of scientific analysis is a singularity is it possible to have a direct primary cause." In this connection the authors caution the creationist that ". . . citing God's special intervention to explain regularly recurring events is to argue for a 'deus ex Machina' . . . an illegitimate God-of-the-gaps move." On the other hand, when a "constant conjunction" between an intelligent cause and a certain kind of event is known, ". . . it is possible to posit such a cause for a similar kind of past singularity."

The valuable contribution which this book makes to the evolution/creation controversy is in distinguishing between the philosophically different approach each has toward origins. The evolutionary approach is to search for secondary (natural) causes within the universe. The creationist approach is to posit a primary cause which may be beyond the universe and thus supernatural. That the creationist approach can be "scientific" is defended by its three major features: ". . . the principle of causality, the principle of uniformity (analogy), and circumstantial evidence . . ." These principles are accepted scientific principles and apply to regular as well as singular occurring events.

The reader will find in *Origin Science* an informative review of the naturalistic approach to origins in astronomy, geology and biology (chapters three and four) and a general outline of a creationist view of origins using "origin science" principles (chapter six).

The reader will also find interesting and helpful appendices. Appendix one is entitled "Paley's Updated Argument." This is a delightful treatment of an old theme and is worth the reader's special attention. Appendix two gives in outline form the major views of creationists and evolutionists in terms of primary and secondary cause considerations. Appendix three deals with miracles and causality and is helpful in considering the nature of miracles in the past and the question of miracles in the present. Appendix four should be required reading for all who are interested in the evolution/creation controversy. It is the dissenting opinion on the Louisiana creation-evolution law. Most newspaper accounts of this trial give only the majority opinion, and the interpretation is generally that the creationist views were totally without foundation. This dissenting view of seven circuit judges testifies to the opposite. It is a cogent argument for reversing the Court of Appeals decision. Appendix five gives a Greek versus a modern view of science, and Appendix six summarizes the evidence for creation as an explanation of origins.

*Origin Science* provides helpful insight to philosophical views of origins and suggests a scientific

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approach to the understanding of singularities, both present and past. For the creationist, the historical development of the current naturalistic explanation of origins should provide support for a creationistic view. For the evolutionist, the delineation of the philosophical differences should diminish the tendency to label the creationist "anti-scientific."

It is unfortunate that the authors include "macro-evolution" as a plausible secondary cause explanation of singularities in the past. This might encourage a theistic evolutionary position on the matter of origins. In any event *Origin Science* is a worthy contribution to the literature on the evolution/creation controversy.

*The Great Dinosaur Mystery and the Bible* by Paul S. Taylor. Master Books. El Cajon, CA. 63 pages. \$10.95

Reviewed by Emmett L. Williams\*

This is an excellent book for children, beautifully illustrated and well-written. It offers a refreshing antidote for the evolutionary propaganda about dinosaurs. The front and back covers, viewed together, and the title pages convey an important message in picture form, i.e., dinosaurs coexisted with animals such as the elephant and man in the past.

The content of the book will appeal to children since many of them seem to be fascinated by dinosaurs and they will find a considerable amount of interesting information about the animals. For instance, they may have been exclusively plant eaters. The Flood and the fossil record are discussed suggesting how most of these creatures became extinct. Many dragon legends are identified with post-Flood human encounters with dinosaurs. The possibility of some dinosaurs living today is suggested. The effect of man's sin is discussed and the plan of salvation is given.

I have one minor objection to a portion of the text in the section "Why did God send the Flood." Taylor states that the Flood was to punish man for his sin and says, "But God did remember the good people. There were only eight of them but He saved them." (p. 31) The Bible states that Noah found grace in the eyes of the Lord (Genesis 6:8) and when called Good Master (Matthew 19:16) the Lord replied, ". . . there is none good but one, that is God." (Matthew 19:17). However the book is not a theological treatise and this point is so slight that it does not detract from the main theme.

Give the book to the children you know, they will be enriched by reading it. So will many adults!

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*The Lie: Evolution*, by Kenneth A. Ham, 1987. Creation-Life Publishers. El Cajon, CA. 164 pages. \$12.95  
Reviewed by David A. Kaufmann\*

Ken Ham has written a short, but-to-the-point book which could easily be titled "How to Refute Evolution." In 11 brief chapters he outlines the simple Biblical and scientific arguments against evolution. In chapters 2 and 3 he argues that both creation and evolution are religious belief systems. Chapter 4 claims the root of the problem is that people do not want to accept creation because they would be accepting someone else (the Creator) to make the rules of life.

An evolutionistic humanist has a great selfish advantage—he can make his own rules.

Chapters 5 and 6 emphasize that sound Christian theology hinges on one's view of Genesis. Chapter 7 shows that "evolution (death and struggle over millions of years) destroys the foundation of the message of the cross." Chapter 8 relates atheistic evolution to abortion, communism, Nazism, drug-abuse and homosexual practices. Chapters 9 through 11 cover the impact of evolution and creation on evangelism and the last days.

Ham makes a strong point for creationists to minimize arguing from an "evidentialist approach—trying to use the evidence to convince students that it showed evolution wrong and creation true," and to maximize arguing from a "presuppositional approach—recognizing that it is primarily assumptions that determine one's conclusions and not logic alone.

The book contains two appendices: 20 Reasons Why Genesis and Evolution Do Not Mix and Why Did God Take Six Days? These handle many of the stumbling blocks in the creation-evolution conflict. The book is written for the average layperson. It is heavy on Scriptural sources but light on using scientific sources. I counted only 10 references to scientific articles.

It is a primer on creation-evolution with an Australian accent. Its suggested resources for study are narrowly limited to the Institute for Creation Research and its agencies and product lines. It does not mention the work of the Creation Research Society or the Bible-Science Association, two groups that have been promoting creationism even before the birth of the Institute for Creation Research. It does cite the *CRS Annual* (1969) once (p. 89).

*The Lie: Evolution* does the job without overburdening the reader with many words and complex presentations. It should help many laypeople, ministers and educators refute evolution and reaffirm their belief in the Genesis creation account, the foundation of Biblical doctrine.

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*The Three Universals*, by Gene Woods. 1987. Vantage Press. New York. 183 pages. \$13.95.

Reviewed by John R. Meyer\*

The author suggests, "Our real need is the adoption of an unshakable base on which we can, with confidence, build a rewarding life-structure." He continues by a thorough chastisement of modern science for its materialistic nature. Readers would soon be disappointed, however, if they had high expectations for a forthcoming suitable world and life view in the following pages. Instead, the writer launches into a thinly veiled appeal to Eastern mysticism in which he defines the three universals as energy, feeling and thought. In living organisms these three are thought to become one. Evolution is redefined in mystical, goal-oriented terms. He suggests:

Driven by an inborn urge to live, every surviving species has, through struggling trial and

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error directed toward surviving, been successful in drawing thought from the infinite to bring into being the changes it must have if it is to survive.

He develops this idea further by stating: "Through continued struggle each variety of life creates itself, drawing on the Unseen to change its body form and habits."

What of those forms of animals, such as microbes, which he believes have not evolved? He explains.

What happened to their calls on the infinite? Perhaps in many cases their living channel to the infinite atrophied. . . . Great changes in form, such as a new phylum, come from changes in a basic

thought. For example, before serpents evolved into birds, a basic thought for land-based life was necessity for solid support.

Just how far does the author take this cosmic connection to the infinite? Quite a way, one would judge from his assurance that ". . . plants do have an emotional life." This type of mystical approach to living systems supposedly finds its outworking in human freedom and feelings. Thus, it is not surprising that a significant portion of the book is given over to a pop, self-help psychology. In many places the book is poorly written and edited. It is a weak attempt to combine evolutionary thought with a rather pantheistic world view.

## LETTERS TO THE EDITOR

### The Big Bend of the Rio Grande

#### Introduction

The Rio Grande is one of the more famous and important rivers of our Southwest. The economic importance of the Rio Grande comes from its contribution to the irrigation of portions of its basin in both the United States as well as in Mexico. In addition it provides some superb scenery along portions of its length above Del Rio, Texas. The Rio Grande empties into the Gulf of Mexico at Boca Chica, just east of Brownsville, Texas and Matamoras, Chihuahua.



Figure 1. Approach to Santa Elena Canyon showing escarpment face. Sierra Ponce (Mexico) is on left and Mesa de Anguila (U.S.) is on right.

#### Location

Starting in the eastern portion of the San Juan Mountains of Colorado, the course of the Rio Grande is eastward into the San Luis Valley. Since the Sangre de Cristo Mountains form a barrier from north to south, the river's course in the valley is turned southward into New Mexico. It continues in this direction across New Mexico to El Paso, Texas, where it turns southeastward again. From El Paso on, the Rio Grande forms the international boundary between the United States and Mexico.

This whole section of the country composed of parts of Texas, New Mexico, Arizona, Nevada, and Utah is known as the basin and range physiographic province of the United States. Characteristic of this province are the many relatively small independent mountain ranges, separated by many intermontane basins. Biologically, this area is part of the Chihuahu Desert, one of the big three in the southwest United States. The other two being the Sonora and the Mojave.



Figure 2. View into Santa Elena Canyon showing narrowing of canyon.

Leaving El Paso the course of the Rio Grande is southeast and then after some miles, it turns northeast again to form a large bend before flowing southeast again. Much of this section has been incorporated into Big Bend National Park. Within the Park boundary, the Rio Grande flows through three spectacular and also quite different types of canyons. The three are the Santa Elena, the Mariscal and the Boquillas Canyons.

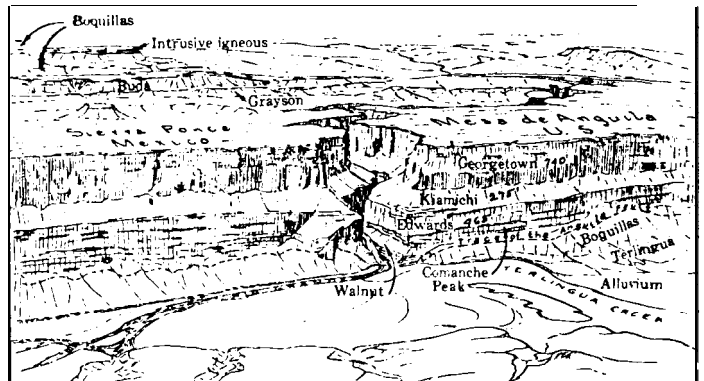


Figure 3. Schematic "Aerial" View of Santa Elena Canyon. The Anguila Fault in this figure is referred to as the Terlingua Fault in Maxwell, Ross A. 1968. *The Big Bend of the Rio Grande: A guide to the rocks, landscape, geologic history and settlers of the area of Big Bend National Park.* Bureau of Economic Geology Guidebook 7. The University of Texas at Austin. p. 89 (Figure 92).

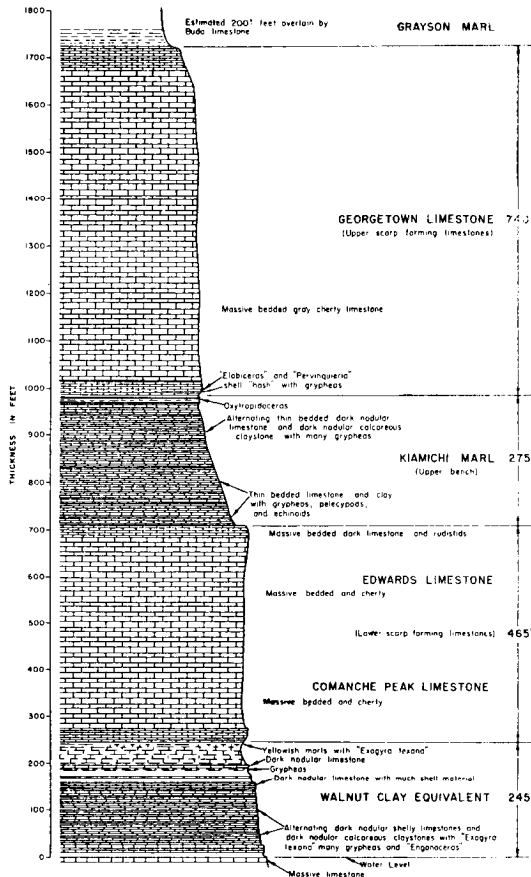


Figure 4. Geologic Section of Santa Elena Canyon.

**Santa Elena Canyon**

This particular letter will cover the Santa Elena specifically. The writer was privileged to experience his first field work in the Big Bend and adjacent territories. I worked under Dr. A. L. Lugin of the University of Nebraska. The field work was well documented by pictures, but they were taken almost 30 years ago.



Figure 5. Mouth of Santa Elena Canyon. Terlingua Creek flows from right to join the Rio Grande at center. The uppermost wall is Georgetown limestone, the bench is Kiamichi marl and the lower wall is Edwards and Comanche Peak limestones.

As one approaches the Santa Elena Canyon (Figure 1), one of the most spectacular views in the whole state of Texas is seen. The Canyon is approximately 18 miles in length, and begins near the northwest corner of the park at Lajitas. Only the lower seven mile stretch is narrow with steep walls. At places, the Canyon narrows to 30 feet, while the walls are about 1500 feet in height (Figure 2).

The Canyon seems to have been formed by the Rio Grande cutting through an upland, composed of Cretaceous limestone, with the resulting two sections known on the U.S. side as the Mesa De Anguila, and the Sierra Ponce on the Mexican side (Figure 3). When facing the mouth of the Canyon the formations are easily identified. Beginning at the upper scarp, they are the Georgetown limestone, 740 feet in thickness, followed by the first bench, formed of Kiamichi marl, 275 feet thick. This in turn overlays the Edwards limestone and the Comanche Peak limestone, which total 465 feet in thickness. Together they form the lower scarp. The latter finally overlays the Walnut Clay, 245 feet thick, which extends down to the water line (see Figures 4 and 5). Fossil pelecypod genera *Exogyra* and *Grypha* are found in the Walnut and the Kiamichi formations.



Figure 6. Close-up view of canyon walls showing apparent dip of beds to the northwest.

A short walk along the river westward up the U.S. side, will make it apparent that the Walnut dips to the west (Figure 6). If one can climb to the surface of the Sierra Ponce, one will encounter hills and ridges as remnants of the Del Rio clay and the Buda limestone.

The suggested sequence of events that produced this geological feature is usually a scenario of uplift, followed by erosion and fill in, in turn followed by downcutting after another uplift, followed by the final uplift or downdropping to form the front of the Canyon. The curves in the course of the Santa Elena would seem to preclude that the Canyon course was the result of following a fault.

**Observations and Speculations**

1. It is certainly strongly suggested that when one views the Canyon at its entrance, one is standing on a downdropped fault block and is facing the upthrown block. How this was produced, e.g. the west block moved up relative to the east, or both blocks moved cannot be determined.

2. It is interesting to note that the fossil zones lie separated from each other by many feet of unfossiliferous limestone.

3. The tilt to the west of the Walnut clay under the Comanche Peak indicates some other fault episode other than the one producing the dramatic first view of the Canyon.

4. The curves in the course of the Canyon would seem to preclude any theory that the river simply followed a pre-existing fault without the necessity of

cutting some 18 miles of channel. Rather it would strongly suggest that the river cut its channel down to the present level.

Hopefully creationists will direct attention to developing a model for the formation of canyons within the framework of the Flood and its aftereffects.

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### Santa Elena Canyon—1986

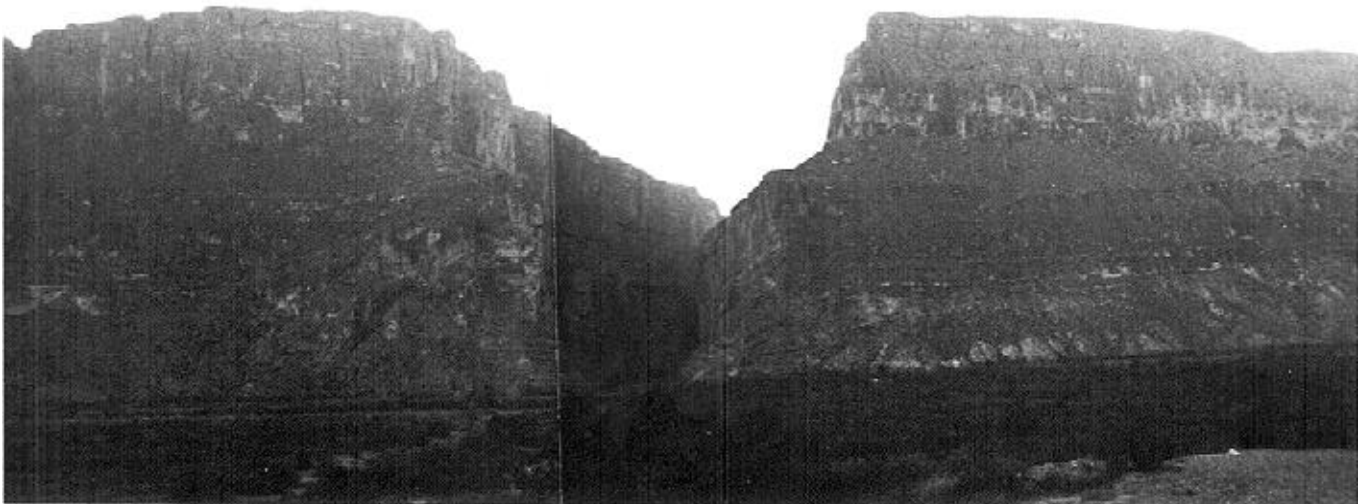


Figure 1. Santa Elena Canyon (1986). Sierra Ponce is on the left and Mesa de Anguila is on the right.

#### Introduction

The editor visited Big Bend National Park between Christmas and the New Year in 1986. More vegetation can be noted in my pictures of the Santa Elena Canyon area than those of Dr. Rusch. Much rain had fallen before my 1986 visit and the desert was more "lush" in appearance than when Dr. Rusch did his field work.

A composite panoramic view of the Canyon is given in Figure 1. A close-up view of the mouth of the Canyon is seen in Figure 2. The Sierra Ponce and Mesa de Anguila on either side of the canyon are given in Figures 3 and 4 respectively. The Mesa de Anguila can be observed downriver from the mouth of the Canyon in Figure 5.

#### Sedimentation Patterns

In studying Figures 1-5 many individual sedimentary layers of limestone can be distinguished. This layering appearance of sedimentary material caused either by deposition or subsequent erosion was observed in the sedimentary sand along the banks of the Rio Grande below where Terlingua Creek flows into it (Figure 6). After the unusually large amount of rainfall and the rising of the Rio Grande and Terlingua Creek, this sand was deposited below the mouth of Santa Elena Canyon and then exposed after the water subsided. Would it be possible to imagine the formation of some sedimentary layers during the Flood in this manner? Subsequent rapid hardening would be necessary to stabilize the layers against complete destruction by erosion. Sedimentation has been discussed

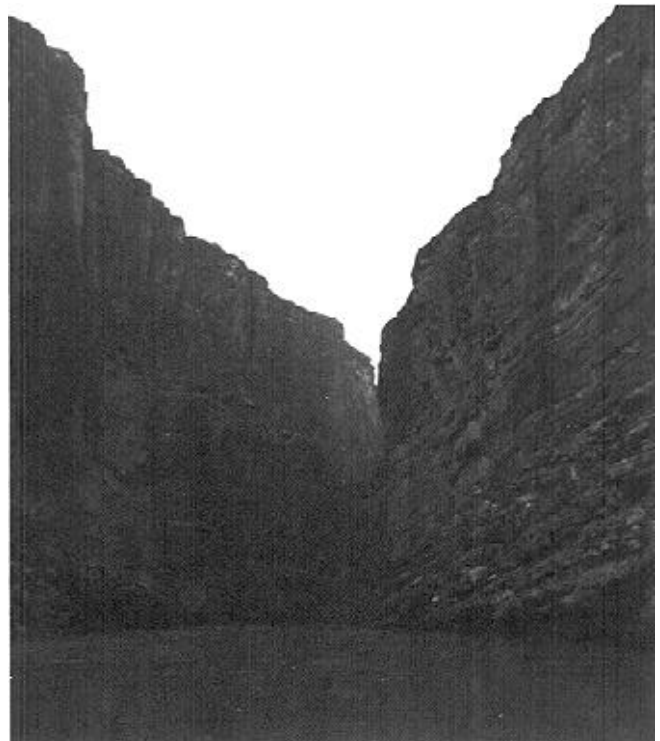


Figure 2. The "mouth" of Santa Elena Canyon. The Rio Grande flows out of the Canyon and Terlingua Creek flows into the Rio Grande from the right.

from a creationist standpoint in prior works. Examples are: Austin and Morris, 1986; Morris, 1966, 1967; Morton, 1984; Nevins, 1974; Peters, 1971, 1973; Whitcomb

and Morris, 1963, pp. 123, 144, 265, 271; Woodmorappe, 1978.



Figure 3. Sierra Ponce at the "mouth" of Santa Elena Canyon. Terlingua Creek is seen in the foreground.



Figure 4. Mesa de Anguila at the "mouth" of Santa Elena Canyon. Terlingua Creek joins the Rio Grande at the base of Mesa de Anguila.



Figure 5. Sierra Ponce downstream from the Santa Elena Canyon (C).



Figure 6. Sedimentary and erosion patterns in sand deposited along the Rio Grande below Santa Elena Canyon.

### Canyon Formation

Note the "miniature canyon" formed in the unstabilized sedimentary sand caused by a water flow after deposition (Figure 7). Would it be possible for canyons to develop rapidly in this manner in unhardened sedimentary material after the Flood? The Research Committee of the Society is studying canyon formation in various areas in the United States. Brief discussions of the formation of the Grand Canyon can be found in Burdick, 1974 and Whitcomb and Morris, 1963, p. 153.

### Conclusions

Rapid sedimentation and canyon formation appear to be areas where field study and laboratory experiments by creationists would be of value. You are invited to participate in these efforts.



Figure 7. Miniature "canyon" formed in sedimentary sand along Rio Grande below Santa Elena Canyon.

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Emmett L. Williams

### Missing Mineral Inclusions in Quartz Sand Grains

Uniformitarian geologists, and many creationist geologists as well, interpret sandstones as sedimentary or aeolian accumulations of sand grains. Quartz arenites, for example, are supposed to be detrital accumulations of quartz grains derived from pre-existing formations. The source rocks may have been either igneous or sedimentary, but ultimately most quartz grains in sediments must have an igneous parent rock, probably

granite, since this is the most abundant igneous rock on the continents. The quartz derived from granite is characterized by the presence of tiny mineral inclusions. Crystals of mica, rutile and tourmaline are common. In sandstones formed from granite-derived quartz grains, mineral inclusions should be evident, but they are actually rarely present in sandstones. (Blatt et al., 1980, p. 291)

Blatt et al. attempted to account for this anomaly by citing evidence that quartz grains containing mineral inclusions are less durable than quartz grains which are free of inclusions. Experiments indicate that mineral inclusions cause structural weaknesses in quartz grains, so it is inferred that such grains would be selectively destroyed during weathering and transportation. Thus, given vast periods of time during which multiple cycles of deposition and erosion could have occurred, uniformitarian geologists are confident that eventually the quartz grains in sandstones would tend to contain mostly grains free of mineral inclusions.

This explanation for the absence of mineral inclusions in the quartz grains of sandstones assumes a great age of the earth. In a creationist interpretation, such an explanation would be unworkable simply because there is not enough time for the selection process to occur. Weathering and transportation of quartz grains could only occur at the earth's surface at any given time. Sands that are now deeply buried must have been exposed at the surface for long periods, subsequently covered with sediments, the sediments uplifted again, and more sand deposited over and over again, for this kind of explanation to work. Certainly this is not a scenario with which a creationist is comfortable. The sheer volume of quartz sand in sedimentary basins strains the believability of this explanation even in a uniformitarian context. And because the mineral inclusions are very tiny, too small to be detected by the naked eye, the differential weakness of grains containing inclusions may be totally inadequate as an explanation for their absence in sandstones.

The absence of mineral inclusions in sand grains of quartz arenites is readily explained if these rocks were formed authigenically, as the writer has suggested (Cox, 1986). Perhaps these sandstones were not formed from granite-derived quartz at all, but were formed in place by precipitation of quartz from ascending pore fluids during compaction of the sediment pile at the time of the Flood. In an authigenic interpretation such as this, quartz grains in many quartz arenites would not contain mineral inclusions characteristic of an igneous environment.

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 Waterloo, Ontario

### Error in Helium Escape From Atmosphere

CRSQ 24:153, appendix IV has an error concerning the escape of helium from the atmosphere. Although at a given time only a few atoms are fast enough to escape, they are constantly re-equilibrating so that

other helium atoms are soon going fast enough to escape.

Using the Rubber Company tables without having had an introductory thermo course illustrates Keats' comment about a little learning.

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### Reply to Franklin

The theory that I used to obtain an indication of the helium escape from the Earth's atmosphere implicitly assumed continuous replenishment of those atoms which escape. Implied in the calculations was the assumption that this was an ongoing (not a one-time) process, in much the same way (although opposite in the direction of population change) as the statistic that at any given time 1 in 35 women are pregnant.

Because helium is continuously produced and very little of this escapes (only 1.4 in 10 million), one may infer that for practical purposes the helium now present in the atmosphere is essentially the accumulation of that which has been produced plus whatever original helium there may have been. This inference may depend on the past temperature of the upper atmosphere; but it does not depend on the rate at which helium has been produced in the past. This is why I chose to compute the escaping population fraction rather than the escaping flux (which could also be computed by the same Maxwellian theory) as this would require some assumption about the rate of production.

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## Philosophy of Science?

### Introduction

W. P. Riemen (*CRSQ* 23:141-5) expresses a proper concern for rigorous logical thinking in establishing a philosophical framework for the scientific enterprise. But his own article contains both confused thinking and fallacious arguments, mainly centered on the notorious difficulty of "proving that something is impossible." He suggests, and compares, two alternative assumptions or hypotheses, which can be expressed as follows:

- A. All phenomena in the material world have material causes (the "materialist hypothesis");
- B. Some material phenomena may have non-material causes (the "non-materialist hypothesis").

### Comparison of MH and NMH

It is surely obvious that, contrary to what Riemen states, hypothesis A (Riemen's MH), being the more restrictive, is more readily falsifiable than hypothesis B (NMH): a single phenomenon shown to have a non-material cause will suffice to disprove it. On the other hand, it is hard to see what would constitute falsification of the NMH: any number of "confirming instances" of the MH (phenomena shown to have material causes) cannot disprove or falsify the NMH.

Riemen then proceeds to examine various *particular* NMHs: 1) a NMH of origins (creationism and/or the First Cause argument); 2) a NMH of life (vitalism); 3) a NMH of sense perceptions. It is surely clear that the falsification of any of these, or indeed of any "particular" NMH, would not constitute falsification of the *general* NMH, which could still be true of other particular (perhaps yet unknown) phenomena.

It would however be misleading to suppose, as Riemen seems to do, that Popper's "falsification principle" means that "more easily falsifiable" is therefore "more scientific." This would lead to the paradoxical conclusion that the more demonstrably false a theory, the more it is "in the interest of science as a lover of truth"! The value of Popper's principle is to define which hypotheses can be subjected to scientific scrutiny. The NMH is intrinsically unfalsifiable, but is the remaining alternative if and when the MH is shown to be false.

Of course the real issue is the status of these affirmations: are they in fact assumptions, or hypotheses? As C. S. Lewis (1974) forcefully argues in *Miracles: A Preliminary Study*. William Collins, London. p. 7, if the materialist hypothesis is elevated to the status of an assumption, i.e. assumed to be true without argument, then no amount of evidence will suffice to show that, for example, a miracle (a material phenomenon resulting from a non-material cause) has happened, since any other explanation, however inherently unlikely, is more probable than what has been excluded *a priori* as impossible. If on the other hand the "general MH" is taken to be only a hypothesis (therefore subject to falsification under the ordinary rules of evidence and experimentation), it may not be difficult to prove it untrue.

Riemen affirms:

If MH is adopted for the creation of matter and energy, then science will have no means of proving that the creation of matter/energy from nothing is impossible by physical and chemical means alone . . . There will always be the suspicion that physico-chemical forces alone are insufficient to account fully for the material universe (p. 142).

There are several errors here:

1) If the MH is assumed for the origin (not "creation," which assumes a Creator) of matter/energy, it leads to almost insuperable difficulties. If the universe had a beginning, the "spontaneous generation" of matter/energy from nothing is assumed: this means discarding, not only the first law of thermodynamics, but the principle of cause and effect itself. But if everything that exists is ultimately an effect without a cause, there is no reason to suppose that any observable phenomenon necessarily has a cause, and science cannot exist. This being so, science indeed cannot "prove," but rather must assume, that spontaneous generation of matter/energy *ex nihilo* is impossible.

Riemen therefore understates the case here. The origin of the universe (matter and energy) obviously cannot be explained by physico-chemical forces, which are themselves expressions of matter and energy. (The only alternative for materialists is to assume an eternal material universe, which, as is well known, conflicts with the second law of thermodynamics. This

is of course the great weakness of the MH, and constitutes the well-known First Cause argument). The laboratory multiplication of matter/energy, on the other hand, would disprove the first law of thermodynamics and require a new theoretical framework for physics, but could not prove anything about origins.

2) Riemen continues: "In fact, [this] makes science state that it is inevitable that science will create matter/energy out of nothing." This is the first of many occasions in his article in which Riemen is guilty of an elementary fallacy. Because something cannot be shown to be intrinsically impossible, it does not follow that it is inevitable it will actually be done. Given that there is land all the way from Texas to the Straits of Magellan, it is not impossible that Riemen will travel there on a skateboard. That does not prove he will inevitably do so!

Going on to Riemen's second "particular NMH," we have the same confusion between "not intrinsically impossible" and "inevitable":

Not only will science inevitably be able to reproduce evolution and make animals and plants to specification, but be able to create life "de novo" in the laboratory . . . These possible future accomplishments would not be *inevitable* if NMH is adopted (emphasis mine).

Under a MH of the nature of life, they are not "inevitable," merely "not *a priori* impossible." The same applies further on, when he states (p. 144):

There is no doubt that science working under the MH must state unequivocally that *it is impossible not to produce* such machines [with true intelligence]. Science must state equally unequivocally that *it will produce* machines with emotions as well (emphasis mine).

This is simply nonsense: "*not impossible to*" does not equal, or imply, "*impossible not to*!" And "not impossible" is impossible to disprove.

A large amount of laboratory work has shown that no one has been able to make an existing species evolve into either another existing species or into a new species using . . . laboratory methods. Thus the findings of science *have shown that interspecies evolution and the creation of life do not seem possible* . . . (emphasis mine).

"Has not yet been achieved" is not the same as "has been shown to be impossible." Many unsuccessful attempts were made before 1953 to run a mile in four minutes, but that did not prove it to be impossible (it has now become quite commonplace).

Actually, Riemen's argument here is the old "God of the gaps" position: God is invoked to explain what cannot yet be explained in terms of material cause and effect. This argument should be used with extreme caution, since its effect historically has been to make theists or supernaturalists constantly retreat from positions which have become indefensible against the advancing materialist "enemy."

If, in the future, scientists were to succeed in reproducing living organisms or creating new ones in the laboratory (I am not of course saying that this is possible or will necessarily ever be achieved), it would

still be living beings (men) who were doing it, and the possibility of the spontaneous generation of life would not have been demonstrated. Vitalism would indeed have been proved false; but this would not have the effect of destroying the general NMH, for example as applying to origins (see above).

### Medical Science

Applying the NMH to biology and medicine, Riemen states: "Under MH it must be dogmatically stated that given time and money we should be able to 1) make the blind see and the deaf hear, 2) to cure all diseases, 3) to raise the dead and 4) to achieve immortality" (p. 143).

Objectives 1) and 3) are subject to difficulties of definition. Some causes of blindness (e.g. cataract) and deafness have, in fact, been cured by medical means. Also, modern resuscitation techniques have caused the definition of "life" and "death" to be reconsidered (and there is still no agreement as to where the boundary should be drawn). Under some definitions, then, both of these objectives have actually been achieved already! In practice, of course, what has happened is that "death" has been redefined in such a way as to admit that the person who has "come back to life" was "not really dead." As is well known, there is puzzlement and debate over the status of cases of prolonged "apparent death" followed by (spontaneous or medically produced) resuscitation: miracle, or natural phenomenon is not yet fully understood? It is possible, even likely, that the "boundary" between life and death will in the future be pushed still farther back. Therefore, it is extremely difficult to arrive at an agreed definition of what would constitute the achievement of these two objectives, let alone whether this would constitute a falsification of this "particular NMH."

Objective 2) is subject to the same difficulty, and objectives 2) and 4) also to logical difficulties. What is the definition of "disease" ("and health")? What would constitute evidence that all diseases have been cured? Even if remedies were found for all known diseases, how could we be sure that there did not remain other diseases as yet undiscovered? (The history of medicine is precisely that of both finding remedies for known diseases, and discovering both hitherto unrecognized and apparently "new" ones, e.g. AIDS.) There is therefore no means of telling whether this objective has been achieved. Similarly, what would constitute proof that immortality had been achieved? The only possible proof would be to show that someone *had already lived for an eternity*, which by definition can never be demonstrated. Therefore the rigorous, scientific falsification of this "particular NMH" is impossible.

### Sense Perception

The most questionable part of Riemen's article, however, is his adoption of a "non-material theory of sense perceptions." After describing the physical mechanism of sight, he states:

The brain . . . produces an internal image of the outside world. This picture appears in every aspect to the beholder as if he is looking directly into the outside world, but, in effect, *his brain fools him as he really is looking at an internal picture* . . . In order to accept such a conclusion, an observer has

to convince himself that what he sees, hears, tastes, touches and smells *is really not outside at all but really inside his brain* (emphasis mine).

Nonsense! What Riemen is describing here is not the “mediate theory of perception,” but solipsism! Of course “what I see, hear, taste, touch and smell” is not inside my brain but exists in a real world outside; what I am “looking at” is an external object and not an “internal picture.” To “see” or “look at” describes the whole process by which an external phenomenon produces an effect inside my brain by which I become aware of the phenomenon. Therefore there is no “infinite regress of pictures,” as Riemen supposes. My brain can grasp the “image” represented by impulses in the optic nerve (decoding it rather as a TV receiver decodes an electrical signal), precisely because it is inside the brain, whereas a chair or a house are not inside my brain! And, as is well known, the “three-dimensional representation” is produced by the brain’s simultaneous decoding of two different pictures (binocular vision), plus comparison with its stock of memorized data which help us to “interpret” what we see (certain “optical illusions” work by confusing this process).

“The mediate theory of perception can be refuted by . . . making a person to see with his eyes closed.” What about dreams? Dreams are, precisely, visual images which are not “fed into” the brain by the usual channel but generated (from the “data bank”) by an internal mechanism. The same is true of visual memories and imagination. Similarly, it is well known that amputees continue to “feel” in the limbs they no longer have. I suggest that Riemen do some more homework on the large volume of philosophical writings on the problem of sense perception.

Whatever theory of sense perception we choose to adopt, however, its falsification cannot demonstrate anything concerning the truth or otherwise of the “general NMH.”

### Conclusion

Personally, I see no reasons to exclude *a priori* that science may one day make the blind to see by direct stimulation of the brain. Should it do so—or, for that matter, succeed in making truly intelligent machines (though here, again, there are great problems in defining what is “intelligence”)—my belief in a Creator God (unlike Riemen’s?) will not be affected in the least.

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### Reply to Allen

It is a fact of good philosophy that it is impossible that God does not exist and He can create all things that are not logically impossible or repugnant to His goodness. Here are some examples of situations that God cannot create because they are intrinsically impossible. God cannot create a cubical sphere, a baseball that is both red and green all over at the same time and, if He creates one of a logically contradictory pair of situations, He cannot create the other at the same time. As an example of the last case, we can state that, with respect to the existing universe, perpetual motion

is either, in principle, possible or impossible to produce using only matter and physical energy (MPE) whether or not humans are clever enough to take advantage of the “possible” case.

The question immediately arises as to how modern science can decide which of these two cases is a proper scientific theory. Science can either opt for the theory that “perpetual motion is possible using only MPE” or “perpetual motion is impossible using only MPE.” I maintain that, if science opts for the first theory, any data that looks negative, which would ordinarily falsify a theory, will be ignored and treated as information on how not to accomplish perpetual motion. In fact, the “possible” form of the two contradictories lends itself to the material assumption which says that MPE is sufficient for all logically possible events. On the other hand, the “impossible” form implies the insufficiency of MPE and “outside” interference in limiting the action of MPE.

It is interesting that modern science has opted for the “impossible” form because of “the fact which has been tested by centuries of human experience, and repeatedly verified, viz that it is in no way possible, either by mechanical, thermal, chemical or other devices, to obtain perpetual motion . . .” (Planck, Max. 1945. *Treatise on Thermodynamics*. Dover Publications. New York. p. 40). In contrast, the optimistic positive form would ignore all those data. The positive form effectively says it is inevitable, at least in principle, to find a way to develop perpetual motion because it says the event in question is possible. The “impossible” theory is falsifiable by the production of a perpetual motion machine and, in the meantime, is strengthened by failure to produce such a machine. I conclude that the statement “it is not possible to produce perpetual motion” is the better scientific theory of the two.

Now let us examine the following list of contradictories in the light of these ideas.

1. It is possible/impossible to create MPE from nothing or to annihilate MPE to nothing using only MPE.
2. It is possible/impossible to prolong life indefinitely either free of all disease or potentially free of all disease because *all* disease, including the “disease” of death, can be cured using only MPE (disease is defined as absence of “normality” by medical textbooks).
3. It is possible/impossible to produce conscious, humanlike, intelligent machines complete with all senses with only MPE. (Please read the reference to R. J. Henle, *CRSQ* 23:45). It will be observed in all cases using the same reasoning as was done with perpetual motion machines that the assumption of MPE being self contained will lead to adopting the “possible” form and that the “impossible” form is the more scientific in that it accommodates the negative data of the “possible” form as positive data in its favor and is the only form that is falsifiable. I also submit that modern mainstream science endorses the “possible” form above and strongly implies it prefers the “possible” form in case number two. The “possible” form of case number one has even been proposed by the astronomer Hoyle.

4. It is possible/impossible to make a normal person with normal eyesight be able to see the external world when fully awake and with eyes tightly closed by direct brain stimulation. We are not talking about dreams, memories or hallucinations as these are pictures in one's head and are completely different from seeing external reality here and now. In addition, my description of mediate visual perception was not solipsism. Solipsism is the condition where a person is the cause of everything including "external" reality. Please read the introductory paragraph in my article where the distinguished Nobel prize winning scientist F. H. C. Crick states correctly the modern mainstream physiological position that what we see is "our internal picture of the external world." That is why it is called the mediate theory of perception as opposed to solipsism since it states that external reality exists independently of the observer and causes, with the use of only MPE, an internal picture of the external to be formed in the brain. In contrast to the mediate theory of perception, adopted by modern physiology, the immediate theory states that we have the power to extend ourselves immediately to directly grasp, in an immaterial manner, an object outside of ourselves. In other words, the tree that I see is neither actually in my head nor is it a picture in my head, but rather I am at the tree itself by immaterial extension. Please read the references to Crick and Coffey carefully (*CRSQ* 23:145). In any event, the "impossible" form is the more scientific theory for the same reasons given above.
5. It is possible/impossible to produce life from dead matter (shelf chemicals) and/or to produce at least some interspecies evolution using only MPE. This pair of contradictories is important in that, if mainstream modern science is correct when they say "evolution is a fact," the Bible, as the one and only true word of the necessarily existing God, would be seriously compromised. Geological history as depicted by evolutionists would not necessarily be proven correct if life and/or interspecies evolution could be produced in the laboratory, but such results would surely all but put to death any practical interest in the Biblical account of the origin of animals and plants and, consequently, the idea that the Bible is really the neverlyng God's true word. However, once again, the best scientific theory in this case is that "it is impossible to create life and produce interspecies evolution using only MPE."

In my article I was not trying to prove a particular theory but to make modern mainstream science be more scientific and less dogmatically arrogant in formulating "scientific" theories pertaining to the fundamental nature of our universe and the things that modern mainstream science can accomplish.

Finally, let us consider miracles as material phenomena resulting at least partly from non-material causes. Science could adapt either the theory that it is possible for miracles to occur or the theory that it is impossible for miracles to occur. The "impossible" theory effectively states that all is MPE and that any

"miracle" is an apparent miracle and that science will surely be able to reproduce it, at least in principle, because science has not investigated all the laws and properties of MPE. In other words, it is unfalsifiable using MPE alone. The other theory which states that miracles are possible is falsifiable in that, if science does duplicate an apparent miracle, then it is not a true miracle. The field is still open to find true miracles. Modern mainstream science would not like this theory as the shoe would be on the other foot.

In conclusion, modern mainstream science, if it wishes to be truly "scientific" in the philosophical meaning of the term, must pay much more attention to the nonmaterial than it obviously has.

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### QUOTE

On the other hand, although modern science is successful because it assumes a constant and reliably repeated order of physical events in the universe, this is not a beneficent rational order encompassing man as a physical and spiritual being, but a mechanical order utterly indifferent to human existence, even though it is susceptible to human control and purposes to some degree which yet remains to be determined. The universe of modern science is not a Socratic cosmos held together by the bonds of love and reason but an unimaginably vast aggregate of de-divinized, dead material bodies moving according to deterministic laws (according to pre-quantum mechanics physics) through empty space. This is not a universe to trust but machinery to observe and attempt to master. The paradox of modern science is that as man's power over nature has increased his existential power, his sense of having a place in the order of things has decreased.

Henry, M. 1986. Political hedonism and modern democracy. *The Intercollegiate Review* 21(3):57.

### QUOTE

The other direction from which the Old Right has been enriched is the use of anthropological and scientific evidence to corroborate its views of human nature and the social good. Here there is evidence of a generational gap, as seen, for example, in the correspondence between Thomas Molnar and Thomas Fleming of the Rockford Institute. Molnar, a Christian Aristotelian, takes exception with Fleming for his reliance on Darwinian biology in defending traditionalist values. Whereas Molnar identifies sociobiology with a despiritualized, morally nihilistic universe. Fleming hopes to use it to defeat the Left on its own alleged turf, modern science. Molnar may be justified in his fear, yet conservative sociobiologists have taken a useful and daring step in depriving the Left of its exclusive claim to science. A similar achievement is the work of Father Stanley Jaki in exploring the theological presuppositions of Western scientific discovery.

Gottfried, P. 1986. A view of contemporary conservatism. *The Intercollegiate Review* 21(3):21.

# REMEMBER

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### QUOTE

Far from being arrested or deterred, the "age of liberalism" has actually achieved an insidious triumph as its sophistic proclivities infiltrate every aspect of human thought and activity. This destructive process signals the advance of what Michael Polanyi calls a "positivistic empiricism," that is, "[the] idea of unlimited progress, intensified to perfectionism, [which] has combined with our sharpened skepticism to produce the perilous state of the modern mind." We can now discern a withering totalization of this advance as it absorbs and shapes both political and intellectual thought and opinion. Even traditionalist conservatism retreats in front of this peril, in fearful awe of its might. The paths of this retreat are strewn with surrenders, backslidings, defeats, and losses of unfathomable consequences. Not only a principled conservatism but also a spiritual conservatism has been debased. What we find in alarming amplitude is the gradual emergence of a conservatism susceptible to the centrifugal tendencies and aims that Polanyi designates.

Panichas, G. A. 1986. Conservatism and the life of the spirit. *The Intercollegiate Review* 21(3):22-3.

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