

Creation Research Society Quarterly

25TH ANNIVERSARY

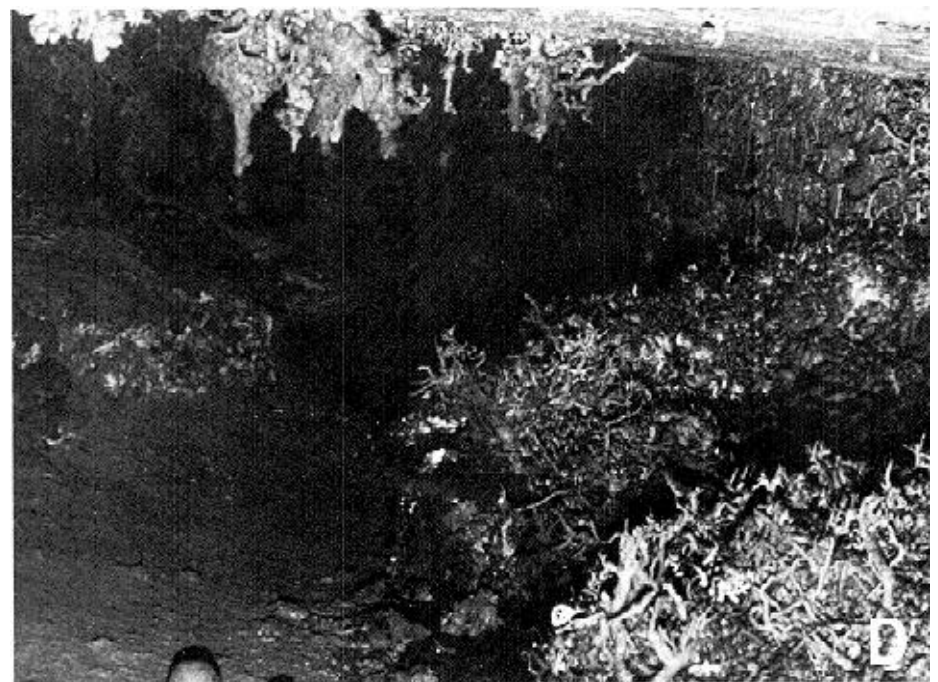
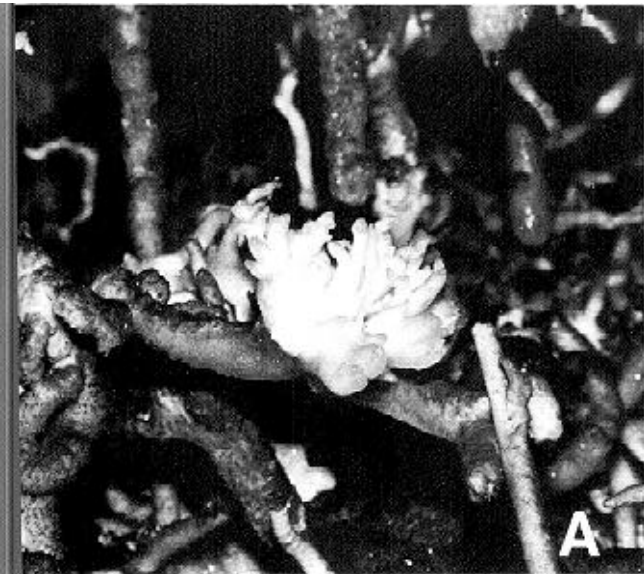
Haec credimus:

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

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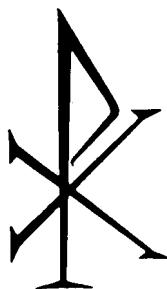
SEPTEMBER 1988

NUMBER 2

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Errata

CRSQ 24:

Page 211, first column line 1 should be Romoser (1973)
instead of Fomoser (1973).

Page 212, References should include the following:
Callahan, P. S. 1972. The evolution of insects. Holi-
day House. New York.
Salmon, F. T. 1964. An index to the Collembola.
Royal Society of New Zealand Bulletin. 1(7).

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Cover Photographs

Various formations in New Fern Cave, Jackson
County, Alabama photographed by Robert A. Merrill
in January 1988.

- A. A gypsum flower among helictites
- B. A stalactite joining a stalagmite forming a column
- C. Various stalactites
- D. Stalactites and helictite bushes.

Researchers have discussed how such beautiful struc-
tures can form rapidly in active caves such as the one
where these photographs were taken. Yet cave guides
often grossly exaggerate the age and formation rate of
stalactites and stalagmites when conducting tours in
caverns. See pp. 81-2 for further discussion.

Emmett L. Williams

MEMBERSHIP/SUBSCRIPTION APPLICATION FORM

CREATION RESEARCH SOCIETY

See the current *CRSQ* for membership information

ORDER BLANK FOR PAST PUBLICATIONS

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CREATION RESEARCH SOCIETY

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

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

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

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

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

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

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

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

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Editorial Comments

Paul DuBois continues his study on the australopithecines. David Kaufmann relates the connection between the concepts of feminism and evolutionism. Wendell Bird finishes his series on the arguments presented to defend evolutionism, woefully lacking as they are. Three more papers are presented in the minisymposium on the speed of light. Barry Setterfield has been invited to respond in the December Quar-

terly. Dr. Dudley J. Benton discusses the implications of the special theory of relativity in the minisymposium.

Several shorter selections will be of interest to you. Many suggested research ideas as well as informational notes are included. I will be glad to hear from you on any subject covered in recent Quarterlies.

Emmett L. Williams

Remarks by the President

Frequently we hear from well-meaning but usually somewhat ill-informed evolutionists some expressions of doubt that creationists carry on meaningful empirical scientific research in ways which could falsify prior creationist conclusions. An answer to these charges are the some 12 dozen field and laboratory papers published in the *Creation Research Society Quarterly (CRSQ)* since its inception in 1964. Many of these reports have involved differences of opinion among the creationist investigators based on their observations. But the CRSQ is only one of the many publications which have reported empirical information from creationist investigators.

A sample case involves "human" footprints in rock. In 1971 W. H. Rusch surveyed this subject in his paper, "Human Footprints in Rocks" [CRSQ 7:201-13]. Some of these footprints were important primarily because they suggested contemporaneity of humans and dinosaurs. Further studies, as reported in various creationist publications primarily in and since 1986, have raised serious doubts that humans made many of the well-publicized Paluxy River tracks in Texas. As a consequence of these studies which have involved creationists, the film *Footprints in Stone* (produced by

Films for Christ) has been withdrawn from the market as has creationist John D. Morris' 1980 book supporting the Texas tracks as human. Also, the Institute for Creation Research in California closed for revision its museum display of the Texas Paluxy prints. Both organizations have suffered financially because of their decisions, but they feel that questions regarding their previous interpretation cannot be ignored. So they urge creationists not to use evidence from the Paluxy as an anti-evolutionary argument until more is known. However, some creationists have maintained research efforts on this subject, and still feel that there is evidence at the Paluxy that dinosaurs existed together with humans.

Even though there may not be a completely uniform position among creationists regarding the evidence for humans and dinosaurs in the same time frame, there is ample evidence that creationists are basing their positions and performance upon data which they have observed in nature. Studies of "the footprints" are continuing, as are many other creationist scientific investigations.

Wayne Frair

CREATIONIST EVALUATION OF AUSTRALOPITHECUS AFARENSIS

PAUL DUBOIS*

Received 14 December 1987 Revised 25 January 1988

Abstract

Anti-creationist Leon Albert believes that certain problems with creationist treatment of the australopithecines, particularly 'Lucy,' invalidate the creationist position that 'Lucy' is not a prehuman ancestor. This is not necessarily so but the argument on which that position is based is incomplete. The difficulties raised by Albert are discussed with a view to resolving them by completing the argument.

Introduction

In "Lucy' Out of Context," Leon Albert (1985) discusses what he considers out of context quoting by creationists in relation to the australopithecines. As discussed by Albert, these include *Australopithecus africanus* and *A. robustus*, which have been known for a number of years, as well as *A. afarensis*, which was discovered more recently (1974) and includes the specimen known as 'Lucy.' His analysis operates primarily within the context of the evolutionary tree proposed by Johanson and White (1979) placing *afarensis* at the base of both the australopithecine and human lineages (Figure 1). Albert focuses particularly

on creationist use of the works of Sir Solly Zuckerman and Charles Oxnard, two investigators who have studied australopithecine fossils in detail.

Creationist Treatment of Australopithecines

Before *afarensis* was discovered, most evolutionist investigators considered *Australopithecus (africanus, robustus)* ancestral to man. A few lone voices were raised in dissent, notably those of Zuckerman and Oxnard. Each author examined *africanus* and *robustus*, concluding that the degree of humanlikeness attributed to them was overstated and that they were probably not human ancestors. Creationists agreed with these criticisms and extensively quoted them to dispute the supposed prehuman status of the australopithecines.

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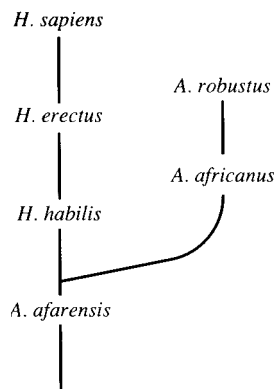


Figure 1. Evolution relationships as proposed by Johanson and White (1979).

When the *afarensis* material was discovered, it was alleged to form another distinct group of fossils and again emphasis was laid on its “prehuman” characteristics. Creationists, unimpressed in the past by similar treatment of other australopithecines, remained skeptical. Some responded to the nomination of *afarensis* as the newest member in the parade of missing links by repeating the criticisms of Zuckerman and Oxnard.

Albert takes exception to this for at least three reasons. (i) One of the most frequently quoted sources—Zuckerman’s *Beyond the Ivory Tower*—dates from 1970, before any *afarensis* fossils were discovered, so Zuckerman could hardly have said anything about them then. (ii) Oxnard’s “Human Fossils: New Views of Old Bones” (1979), another source Albert discusses as a creationist favorite, mentions the *afarensis* fossils only briefly and Oxnard does not specifically include them within the scope of his criticisms. (iii) Neither Zuckerman nor Oxnard had studied the *afarensis* material personally. The following examples are among those on which Albert bases his objections.

Since neither Zuckerman nor Oxnard had studied the *afarensis* material themselves, Albert maintains that creationist claims they had done so are false. He gives the following quote (p. 367), from a debate transcript, as an instance:

Henry Morris . . . claimed, with explicit reference to the “Lucy” fossil, that both Zuckerman and Oxnard “have analyzed this material in detail, measured it in the laboratory . . . and they have taken all of the australopithecine dentition, the skull structure that has been found, the limb bones, the knee bones, and so forth, and they have come to the conclusion that . . . it looks like an ape” (first ellipsis mine, emphasis Albert’s).

A 1981 article by creationists Duane Gish and Richard Bliss quotes Zuckerman (1970) to support the contention that there is no fossil evidence of a transition between ape and man. Albert observes (p. 368) that the article was published several years after the discovery of ‘Lucy’ (1974) but Gish and Bliss nevertheless choose to refer to a statement made well before then, in effect ignoring or denying the possibility that Zuckerman might have revised his conclusion after the *afarensis* fossils were unearthed.

According to a report of a debate between Duane Gish and Steven Shore in *Acts and Facts* (1983), when Shore claimed that ‘Lucy’ was prehuman, Gish “reiterated Zuckerman’s and Oxnard’s conclusions concerning the australopithecines” that they “did not walk upright and were not intermediate between ape and man.” Albert’s position is that since neither Zuckerman nor Oxnard were commenting on the *afarensis* material, their statements do not constitute a rebuttal to claims made about ‘Lucy.’

These examples are persuasive to varying degrees. Albert says creationist claims that Zuckerman or Oxnard had studied the *afarensis* fossils are false. It is true that to whatever extent, if any, Morris intended to include the *afarensis* fossils among the material he mentions, he would be claiming that Zuckerman and Oxnard had studied them, which would be incorrect. But the intent of Morris’ remark is equivocal in the example Albert gives, since he does not reproduce the “explicit reference” to ‘Lucy.’ Morris himself maintains that the scope of the fossil material listed in the quote is too broad for him to have been referring simply to one partial skeleton (personal correspondence).

Albert’s contention that it is questionable to quote Zuckerman’s book in relation to *afarensis* has greater merit. Zuckerman was not specifically writing about *afarensis* and Gish and Bliss do not actually show that Zuckerman’s earlier skepticism about the human-like character of australopithecine fossils extends to *afarensis*.

However, the form of Albert’s argument is no different from that to which he objects. He implies that Zuckerman might have changed his mind when the *afarensis* fossils were discovered, but does not show that Zuckerman would agree with that either. Zuckerman’s remarks could be used more carefully by creationists, but given his antipathy in the ‘60’s and ‘70’s to the usual conclusions reached by other investigators about australopithecines, I doubt very much whether anyone would exactly be astounded if he were also skeptical about *afarensis*.

Significance of Albert’s Criticisms

Despite flaws in several of Albert’s examples of alleged abuses, it would be imprudent to discard his analysis entirely. It is pointless to deny that when considering the question of whether *afarensis* is a prehuman ancestor, creationists have taken statements by Zuckerman and Oxnard that were not made with reference to *afarensis* and generalized them to include that species. Albert correctly observes that little or no basis for doing so has been given. What is the source of this problem?

Albert seems to believe that creationists either simply did not recognize that use of Zuckerman’s and Oxnard’s comments in this manner requires a certain degree of extrapolation or else realized these remarks did not apply directly to *afarensis* but went ahead and used them anyway. I suppose Albert favors the latter possibility for he seems to think that after ‘Lucy’ was discovered, creationists decided to make the best of a bad situation by dredging up anything they could find in an effort to discredit the *afarensis* fossils. He paints a picture in which creationists suddenly start quoting Zuckerman and Oxnard after a description of the

afarensis material was published in 1979 but does not indicate (or know, perhaps?) that their work had been referred to by creationists previously (Bowden, 1977; Gish, 1975).

Apparently Albert never considered a third alternative. It is perfectly conceivable that creationists realize the comments of Zuckerman and Oxnard are not explicitly about *afarensis* but judge the similarities between different species (e.g., *afarensis* and *africanus*) sufficient to warrant the generalization. Even if that is so, however, criticisms such as those by Zuckerman and Oxnard of *africanus* (perfectly proper and surely cogent within the domain for which they were originally intended) have limited applicability to *afarensis* unless a reasonable justification for using them that way is demonstrated.

Objections such as Albert's arise precisely because such a rationale has not been very clearly articulated in creationist literature. The value of his paper is that it identifies this disturbing gap in creationist reasoning, although he has incorrectly evaluated its significance. For Albert, creationist arguments are indefensible because conclusions about *afarensis* that are derived by generalization from other australopithecines are undeniably specious. From his mocking tone and the smugness of his report he evidently considers this point quite beyond dispute.

Nevertheless, extrapolation of statements from one species to another is not always unwarranted. After all, if species did not sometimes have similarities, there would be no generalities (i.e., enlargement of the scope of statements) and therefore no systematics. The supposition that creationist arguments are wrong (rather than simply incomplete) because they extend the scope of a set of statements is best justified by demonstrating the extension to be implausible—something Albert never does.

Albert objects that creationist arguments "blur over the distinctions between the various forms of *Australopithecus*" (p. 368). That is true but misses the point. The problem is not combining of groups, it is the lack of a reasonable justification for doing so. The practice of lumping groups together is after all hardly unique to creationists. Indeed, Oxnard himself sometimes derives conclusions from results of studies that pool together—"blur" in Albert's terms—different australopithecine forms. For example, in assessing affinities of the shoulder and pelvis of groups of primates, Oxnard (1975, pp. 41-42) pools certain australopithecine forms, justifying his procedure as follows:

When we recognize the extraordinary variability that exists within living primates at the generic level, we realize that this a reasonable exercise. For the differences that are known to exist among the individual fossils (for instance, between the robust and gracile forms) are not nearly as large as those found within some living genera. Comparisons may be made for instance, with the genus *Homo*, who displays continuous variations in height within living man that greatly exceed any that are known to have existed among the australopithecines . . . morphologically they tend to fall rather close to one another. It is true that there are considerable differences among the various skull fragments, but certainly not much more than can be seen among

the various macaques, for instance, and certainly less than that existing between the sexes of chimpanzees and of gorillas, for example.

Though not written with reference to *afarensis*, this passage shows the invalidity of blanket objections to combining groups for analysis. The problem remains, however, that while Oxnard takes the trouble to justify his procedure, *creationists do not*. Were this not so, Albert's criticisms would have no force. Do creationists refer to the remarks just quoted (or anything similar) as support for combining groups of australopithecines? I submit that the answer is no. Do creationists address the question of whether the similarities between *afarensis* and other australopithecines really are sufficient to justify generalizing Zuckerman's and Oxnard's statements? Rarely, if ever.

If creationists are going to use sources such as Zuckerman's *Beyond the Ivory Tower* and Oxnard's "New Views" to press the point that *afarensis* should be referred only to *Australopithecus* and is unrelated to *Homo*, the rationale for doing so must be clearly specified. Otherwise, creationist analysis will continue to be undermined by failure to explain why conclusions about *africanus* should be extended to *afarensis*.

An Approach to Solving the Problem

The foregoing is not a counsel of despair. Although evaluation of *afarensis* must of course rest most heavily on analysis of the *afarensis* fossils themselves, studies concerning *africanus* are far from irrelevant. Because of this, I would characterize the way creationists use quotations from Zuckerman and Oxnard simply as inappropriate or unsatisfactorily supported, though Albert describes these uses as "dishonest" or "scholarly felonies" (p. 369). Such terms carry a lot of emotional baggage that obscures the issue. Indeed, though I believe Albert's report should not be entirely dismissed, his own arguments are often without any greater foundation than those he criticises as I have elsewhere discussed (DuBois, 1987).

Albert's failure to consider possibilities other than "scholarly felonies" stems from his definition of out of context quoting: "the use of an authority's published words against evidence that was *not even available* at the time of the publication of those words" (p. 364). This sounds reasonable at first but actually means that all publications apply only retroactively and that anyone who quotes a previously published report to interpret new evidence is using it out of context. At the extreme, it implies that under no circumstances should work on *africanus* published prior to the discovery of 'Lucy' be cited with respect to *afarensis*.

I disagree. It is an entirely orthodox principle of scientific investigation that new evidence routinely is and should be interpreted by reference to older work. The manner in which this principle may be applied to the matter at hand is evident: statements made about, say, *africanus* are legitimately open to extension to *afarensis* to the extent that affinities exist between the two forms.

Consider Oxnard's work on the *africanus* shoulder (1968), from which he was led to suggest tree-dwelling behaviors. His work has recently been complemented by that of Stern and Susman (1983), who find similar structural characteristics in the *afarensis* shoulder. The

similarity between the two forms leads one naturally to suppose that *afarensis* also may have engaged in arboreal activity. The relevance for creationists is that since arboreality and bipedality are often viewed as unlikely to both occur in well-developed form in the same organism, indications of arboreality in *afarensis* tend to mitigate the effectiveness of attempts to assert its prehuman status that stress its potential for bipedality.

Perhaps Albert, as Oxnard's defender against the wiles of crafty creationists, would object to such reasoning, for instance on the grounds that Stern and Susman would very likely disagree with it. (They are among those who believe that arboreality and bipedality may both have been significant in *afarensis* locomotion.) But the fact is that the connection between Oxnard's *africanus* findings and the results of Stern and Susman on *afarensis* was pointed out by Oxnard himself (1983b, pp. i-iii) and taken as evidence against prehuman status for *afarensis*.

Or perhaps Albert would object that this single example is insufficient to establish any general argument against bipedality in *afarensis*. Quite true, but Oxnard also discusses other parallels between *africanus* and *afarensis*, e.g., for the hand, arm, pelvis and foot. That lends additional weight to the argument and illustrates how links may be drawn between *afarensis* and other australopithecine forms and shows—Albert's definition of out of context quoting notwithstanding—that work on *africanus* is highly relevant to consideration of the *afarensis* fossils. Because these affinities exist, statements about *africanus* are not automatically invalid when applied to *afarensis*. Taken with reasonable caution, studies of other species of australopithecines form an important part of the evaluation of *afarensis*.

Ironically, Albert's analysis suffers a lack of justification parallel to that found in creationist arguments. Creationist claims rely implicitly on the validity of pooling various australopithecine forms but Albert's argument depends just as much on the validity of the distinction between them and he, like creationists, fails to justify his premise. The whole of his rationale for accepting the distinction is that 'Lucy'-like fossils are "generally recognized as a distinct form of early hominid . . . sufficiently different from any previously named category to place them in their own taxon" (p. 365). That is simply an argument from authority, entirely unconvincing except to those who already believe it and who therefore need no convincing anyway.

Nowhere does Albert even hint at the considerations weighing against acceptance of the distinction between allegedly different forms—perhaps because were he to acknowledge them, much of the basis for his objections would be obviated: statements made about other australopithecines would be applicable to *afarensis*, and the creationist position might be strengthened as a result. Horrors! The present classification may be endorsed by several investigators, but there are reasons for questioning it.

McHenry (1983, p. 187) points out that *afarensis* and *africanus* are very close post-cranially—"strikingly similar." 'Lucy's' pelvis is in his opinion "astoundingly similar" to some specimens of *africanus* (p. 196).

Suzman (1982) too says that aspects of the pelvises of *africanus* and 'Lucy' are "strikingly similar." He finds that the differences between them are within the range of variation found in human samples and entertains the possibility that 'Lucy' may be *africanus*.

The history of the australopithecines is littered with shifts of genus and species attribution and coalescence of "different" species into one. The *afarensis* fossils have not been immune to this kind of taxonomic flux: currently referred to *Australopithecus*, some of them were initially identified as *Homo*. Given this change in genus, it is hardly wild speculation to suggest that time may also bring to *afarensis* a change of species, e.g., to *africanus*.

Oxnard (1975, p. 41) points out that studies estimating the similarities or differences of the australopithecines in relation to man or the apes typically fail to compare these estimates to known variability in existing groups of primates. The fact that certain groups display marked variation among their members is an observation of considerable importance. Species distinctions based on morphological variation between fossils which is exceeded in extent within some living species may well be spurious.

Some of what difference there is between *afarensis* and other australopithecines may be due to sexual dimorphism (sex-related structural differences). It is noteworthy that although the general differences between *africanus* and *robustus* may well be greater than those between *africanus* and *afarensis*, it has been suggested from time to time that even *africanus* and *robustus* should be classified as a single species on the grounds that they constitute sexually dimorphic segments of a single group. If *africanus* and *robustus* may be conspecific, what of *africanus* and *afarensis*?

Furthermore, the possibility of sexual dimorphism is not easily ruled out as it is not a simple property. Oxnard finds among living primate groups seven distinct types of sexual dimorphism (1983a; Oxnard *et al.*, 1985) and cautions against assuming that all dimorphisms are of the same type, since that may impede accurate fossil assessment. Significantly, Oxnard (1983a, p. 20) names Johanson—'Lucy's' discoverer and one of those responsible for the current classification of *afarensis*—as one making this erroneous assumption. To rule out dimorphic variation between *afarensis* and *africanus*, one must establish that the fossils do not fall into any of these seven types. Even if that were shown, it remains a possibility that they are of yet another type. Since each living hominoid genus has its own unique pattern of differences, according to Oxnard, it is not unlikely that extinct groups will also.

The basis for a species distinction between *afarensis* and *africanus* rests partially upon a difference in geological time between the two sets of material. The dating has been a matter of some controversy. For instance, Boaz *et al.* suggest that the time gap between *afarensis* and *africanus* be narrowed and note that this has "implications for interpretation of the early hominid fossil record" (1982, p. 633). One of these implications is discussed, somewhat impatiently, by Rak (1985, p. 281): "Some authors [he names Boaz *et al.*] have carried their argument so far as to imply that with the diminishing of the time gap, there remains no obstacle

to lumping the *A. africanus* and the Hadar hominid samples together." Rak takes the position that the distinction is legitimate; his tone shows that a shrinking time gap represents a challenge to that position.

The basis for taxonomic differentiation of *afarensis* from other australopithecines may stand the test of time but if so, it will not be without challenge. Considerations such as those above illustrate that until the issue is settled, Albert's alleged broad consensus about the distinctiveness of *afarensis* and its prehuman status remains questionable.

Conclusion

Albert's article is valuable in that it brings up an issue that deserves to be treated with greater care by creationists, i.e., the basis for drawing conclusions about *afarensis* from the results of investigations done on other australopithecine forms. Albert is nonetheless incorrect in supposing that because this basis has not been made explicit one does not exist and my sympathies with his objections do not extend so far as to agree that creationist conclusions about *afarensis* (not prehuman; may not even be a distinct species) are essentially incorrect.

It is sometimes reasonable to link *non-afarensis* studies to *afarensis* but the evidence suggesting the connection needs to be pointed out. Creationist analysis has left unspecified part of the chain of reasoning leading to the conclusion that *afarensis* is not prehuman and has been correspondingly weak at that point. The preceding discussion shows that this is unnecessary, suggests briefly how work on *africanus* pertains to evaluation of *afarensis* and strengthens the applicability of criticisms such as those of Zuckerman and Oxnard.

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AJP—American Journal of Primatology
AJPA—American Journal of Physical Anthropology

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Editor's Note: For those readers interested in one of the more recent Oxnard books see Oxnard, C. E. 1987. *Fossils, teeth and sex: perspectives on human evolution*. University of Washington Press. Seattle.

FEMINISM, HUMANISM AND EVOLUTION

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Abstract

Feminism is an ideology based on the anti-Christian philosophy of humanism. Evolution, through genetic, embryological and DNA evidence along with the bizarre technological possibility of male pregnancy and childbirth, has become the scientific framework on which feminism rests. This has resulted in the individual replacing the family as the functional unit in secular society. In contrast, the doctrine of creation establishes the divine order of the sexes with the family as the functional unit in a Christian society.

Introduction

When history of the U.S.A. is finally recorded, it will be said that the decades of 1971-80 and 1981-90 were the decades of feminism. It is during these time spans that females have gained much equality with males in the family, the church and society. A comprehensive definition of feminism is given by Schlafly (1985):

Feminism is an ideology which teaches that women have been mistreated since time began, and that even in America women are discriminated against by an oppressive male-dominated society.

Feminism is a political movement which teaches that a just society must mandate identical treatment for men and women in every phase of our lives, no matter how reasonable it is to treat them differently; and that gender must never be used as the criterion for any decision.

Feminism is an economic movement which teaches that true fulfillment and 'liberation' for women are in a paying job rather than in the confining repetitious drudgery of the home, and that childcare must not be allowed to interfere with a woman's career. Feminism is a psychological outlook on life which is basically negative; it teaches women that the odds are stacked so

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severely against them that they probably cannot succeed in whatever they attempt.

Although most people do not realize it, feminism and Biblical Christianity are antithetical belief systems. According to Naomi Goldenberg (1979, p. 22), a leading spokeswoman for feminism:

Jesus Christ cannot symbolize a liberation of women. A culture that maintains a masculine image for its highest divinity cannot allow its women to experience themselves as equals to men. In order to develop a theology of women's liberation, feminists have to leave Christ and the Bible behind them.

Actually feminism is an ideology based on the anti-Christian philosophy of humanism. The eleventh principle of the *Humanist Manifesto II* (Kurtz, 1973) claims that society should eliminate all discrimination based on sex:

This means equality of opportunity and recognition of talent and merit. We believe in equal rights for both men and women to fulfill their unique careers and potentialities as they see fit, free of invidious discrimination.

This principle has led Gloria Steinem (1985) to profess her famous antireligion quote: "By the year 2000 we will, I hope, raise our children to believe in human potential, not God." Theodore Letis (1982-3, p. 187) has summed up feminist theology in his essay "Feminine Spirituality" by stating:

It is not faulty exegesis, nor sexist translations that are oppressors of women—it is the religious systems of Judaism and Christianity . . . Scripture, as well as the Christian God are considered not just irrelevant for women, but are regarded as primeval enemies of all women and true spirituality.

Feminists claim that Biblical creationism is at the root of sexual abuse. Thorkelson (1986) quotes Rev. Mary Potter as saying: "The church teachings about creation, suffering and Christology all contribute to oppression of women and children."

Evolution: Feminism's Scientific Foundation

Since feminism is in conflict with Biblical Christianity and has made phenomenal advancement not only in our society, but in most Christian churches, what has been the stimulus for this significant growth? I contend that the validity and promotion of feminism is substantiated by the doctrine of neodarwinian evolution.

Genetic Evidence

Creation argues that man was created first (Genesis 2:7, 21-25) and woman was formed out of the side of the first man, to be an helpmate to him. This is the reason that the New Testament teaches that the husband is the head of the family (I Timothy 2:11-15; Ephesians 5:22-24). Feminism rejects this order in the family and claims that women are equal to men in every sense, including ability. It insists that a woman can do anything a man can do and vice versa. Sometimes it even claims that women are essentially superior to men, and of course, being humanistic, it must stand on the bulwark of secular science, evolution. In order to

proclaim an original equality of the sexes, evolutionists expound the ultimate unity of the male and female genders eons ago when they speculate about the origin of the first sexual signs. According to Margulis and Sagan (1986, p. 201):

When meiotic sex first evolved, isogyny (identical gametes) was the rule. Sexual partners were identical in appearance, as many sexual protists (unicellular and multicellular eukaryotic microorganisms) still are today. (parentheses added)

Superiority of the Female Chromosome

Chromosomes entirely determine the genetically transmitted sex in vertebrate animals, and hormones trigger the actual specific functioning of sex. An XY chromosome contributes to becoming a male while an XX chromosome contributes to becoming a female. According to Nowak (1980, pp. 24-5):

The X chromosome is far more vigorous than the Y chromosome. Occasionally, chromosomes may be lost (or even gained) amid the complex events surrounding conception. If an X chromosome is lost from an XX pair or a Y chromosome from an XY pair, the fertilized X cell still survives and multiplies, eventually producing a female body, though one lacking in fertility. But the loss of an X chromosome from an XY combination is lethal. No human has ever been found with a Y as the sole sex chromosome . . . The X chromosome, as you can see, is basic and indispensable; the Y chromosome simply an embellishment upon nature's original theme. The female form is first; the male a modification. The female-base principle thus makes its debut.

In line with this reasoning of female superiority, she further argues that primitive gonads in the embryo are basically feminine. They normally become ovaries, and in order to depart from natural development they must be programmed to become testes. She continued:

For the first six weeks, the embryo, whether XX or XY, coasts along in sexual ambiguity. But at the end of this time, by some mechanism as yet unknown, the Y chromosome of the male embryo will trigger the gonads into developing as testicles. The XX embryo, on the other hand, will yet remain for a time as primitive, all-purpose organs . . . 'Actually the gonad is an ovary, not because it has that structure, but rather because it is not a testis,' said J. Gillman of the Carnegie Institution of Washington, and many other researchers concur. Gradually over the second six weeks of the XX individual's life, these gonads, these 'presumptive ovaries,' as they have been referred to, do indeed develop into true ovaries complete with lifetime supply of eggs with which a female is normally born.

Because of this new evidence, which is interpreted by humanistic pre-suppositions, she argues that intelligent Christians should correct Scripture to agree with this scientific discovery:

God looked at Eve and said: 'It is not good for her to be alone.' Therefore with his almighty power he concocted the magic of the Y chromo-

some and of testosterone. Man is really a variant upon the original first model, woman (Eve). Therefore, the primordial fetus was female. If one wants to be mythologically symbolic, we would have to say Eve preceded Adam and that the Biblical story is a reflection on ancient male chauvinism.

According to her, the female is nature's original model and has triggered mankind's evolutionary heritage. Nature, not culture has given women a superiority over men. Of course, the creationist reply to this logic would be: the XY male and XX female configurations do not require that the female was formed first. The Creator could have modified the male cellular material (XY) by eliminating the Y and duplicating the X. Whatever result this causes in embryology (female-ness) would then come second or after the creation of man. The fact that male and female development runs somewhat (but by no means entirely) parallel during the early weeks of pregnancy does not demand that the female condition was "primitive."

DNA Evidence

Schmeck (1986, pp. 7B, 8B) reports the work of Wilson, Cann and Stone whose studies of the DNA from the placentas of 147 humans from the U.S.A., Europe, Africa, Australia and Asia trace the entire human race back to one primordial female. They analyzed the changes of DNA, the substance of genes, from the oxidative organelles, mitochondria, that are inherited only through the mother. Their calculations, based on evolutionary presuppositions, of the slow changes of DNA from humans from the five populous continents indicate that everyone alive today may be a descendant of a single female ancestor who lived in Africa 140,000 to 280,000 years ago. This argument attempts to refute the claim in Genesis that man was created first. Creationists, however embrace this concept of monogenesis. Biblically, all people except Adam and Eve have been engendered by Eve. The DNA evidence fits neatly into the creationist model of origins.

Can Man Have Babies?

In an attempt to prove total equality between men and women, technology has developed a way to enable men to give birth. Kent (1986) reported that male pregnancy would involve fertilizing a donated egg with sperm outside the body. The embryo would be implanted into the bowel area, where it could attach itself either to the kidney or large intestine. The fetus would be delivered by Cesarean section. The author quotes Dr. John Parsons, senior registrar and lecturer in obstetrics at King's College Hospital: "it can be done, and undoubtedly, someone will do it." In true anti-Christian fashion the article suggests that candidates for male pregnancy might be homosexuals, transsexuals or men whose wives are infertile.

Were All Evolutionists Feminists?

As shown, the latest evolutionary thinking and research has firmly established a scientific basis for total equality between the sexes. But this has not always been the case. Hrdy (1981, p. 12) reports that Herbert Spencer, a 19th century English evolutionist, thought

"females never had been inherently equal to males and could never be; subordination of women was not only natural but, in his view, desirable."

This earlier teaching of evolution has led to some unfair treatment of females and to the advocacy of immoral activities of males against females. Burton (1976, p. 156) has stated:

The double moral standard which punishes an adulteress severely while often condoning the man can be defended on biological grounds. It increases a man's reproductive potential and it might be added that those who indulge in extra-marital activities are those who are the 'fittest' and most deserving to be biological fathers as they must possess a high degree of cunning and initiative, and often physical agility.

Creation: The Divine Order of the Sexes

In certain senses men and women are equal. The Creator created both the first man and the first woman in His image (Genesis 1:26, 27). The Creator loves both men and women. Men and women should be treated with equal justice under the civil law and should receive equal pay for work of equal value.

What is wrong is the feminist principle that men and women have equal potential in all areas, and therefore it is desirable to interchange their roles. Their rule is identical treatment in all aspects of life. The Creator created men and women with certain different abilities and designed them for different roles within the family structure (husband/father; wife/mother). In spite of the bizarre attempt by medical technology to enable men to give birth, no man can be a natural mother. No woman can be a natural father.

What is true and good for the family is also true and good for the church and society. Unfortunately in our modern American culture the individual has been deified as the basic unit in our secular society. But the Creator desires that the basic unit should be the family. The church, according to Scripture, must always work to strengthen the family, never to weaken it (Ephesians 5:21-33; I Timothy 5:3-16; I Peter 3:1-7).

The difference in size, strength, attitudes and hormone levels equip men and women for different roles in the family, church and society. The Creator's intention is that these differences between the sexes should serve to benefit men, women and children as they adjust to the responsibilities and pressures in life.

Men are not better than women; women are not better than men. But they are uniquely different in certain essential responsibilities and functions. Men are better equipped for certain roles while women are better equipped for other roles. The differences in these role functions should not be used in any harmful way against the other sex. Selfish domination, competition, jealousy and usurpation of another's authority are all wrongful misuses of these differences.

It should be pointed out that the responsibility of the headship of the family and the governing of the church belongs to the men. There is some indication that many have been irresponsible and abused women on occasion. Since men are bigger, stronger and faster than women, they should use these differences to protect women. Machoism has no place in the divine order of the sexes or creationist philosophy. It is antimasculine

for a man to use physical force in any way against women or children unless in self defense. Likewise, it is wrong and immoral for men to attempt to sexually seduce women. According to creationist tenets, the sexual act between men and women is restricted to the marriage framework with one's spouse.

Conclusion

Feminism is an ideological corollary of the doctrine of humanism. The doctrine of evolution tries to scientifically support feminism. Feminism is antiception and anti-Christian. There can be no such thing as Christian feminism. They are conceptual contradictions like capitalistic communism and evangelical lesbianism. The divine order of the sexes is a corollary doctrine of the order of creation. It establishes the family as the natural unit within society and the church. Men and women are similar in some abilities but are designed to have certain different positions and functions within the family and church. The husband/father is given the headship in the family and the church with the wife/mother as an assistant and chief educator of the children. The husband/father is not always right or should not always get his way, but in the last analysis he is always responsible in family/church decisions.

Feminism claims women are equal to men in every way. It claims order, position, responsibility and function in life are determined by one's abilities which should be developed to one's highest potential. The individual is supreme and order in society, family and church should be determined by the "survival of the fittest" with equal opportunity for either sex to survive. Feminism, instead of solving the conflict between the genders, really makes the situation worse as it not only

"fans the flames" of abuse and harm, but it destroys the order in the family and church that the Creator commands (Genesis 2 and 3, I Corinthians 11 and 14, Ephesians 5 and I Timothy 2). Evolution, operating from naturalistic presuppositions controlled by random probabilities, supports the anti-feminine philosophy of feminism. In contrast, creationism is pro-feminine. Feminine means emphasizing the womanly attributes that allow women to be delightfully different from men. The feminine and creationist woman enjoys being a woman uniquely different from men. She has a positive view of herself and a benevolent outlook on life. She knows she is a person with her own identity in God's order of creation.

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THE POSTULATED EVIDENCE FOR MACROEVOLUTION AND DARWINISM: DARWINIAN ARGUMENTS AND THE DISINTEGRATING NEO-DARWINIAN SYNTHESIS—PART II

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Abstract

Part I discussed three of the eight primary lines of evidence offered for macroevolution and Darwinian mechanisms. Part II addresses the remaining arguments for macroevolution and Darwinian mechanisms. Evolutionists are cited who suggest that (1) the "facts of comparative anatomy provide no evidence for evolution," while the "attempt to find homologous genes has been given up as hopeless"; (2) 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"; (3) 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"; (4) 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 (5) 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 unless otherwise identified. All emphases in the quotes is the author's.

Comparative Anatomy and Physiology Argument and Difficulties

The comparative anatomy argument for macroevolution, which focuses on comparing anatomical structure (Stansfield, 1977 p. 113) is based on the similarity of structure of various organisms. Most evolutionists

interpret some, but not all, similar structure to show common ancestry. The comparative anatomy evidence that is anomalous, in the sense of indicating the unrelatedness or distinct ancestry of organisms, is treated fully elsewhere (Bird, 1987, pp. 94-8). The comparative anatomy and physiology argument is constructed on a problematic foundation that can be viewed causally and historically.

This basic problem is that similar anatomy or physiology does not necessarily indicate common ancestry and evolutionary descent. First, nearly all evolutionist

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scientists acknowledge that "there exists no *a priori* relation between the appearance of two structures and their relatedness," as Schwabe and Warr (1984, p. 468) state along with Cracraft (1981, p. 32), Denton (1985, p. 178), Patterson (1981, p. 217), and Macbeth (1971, pp. 13, 16). Boyden adds that:

such general resemblances in protoplasmic systems as we find in *all* living organisms *do not necessarily mean genetic relationship*. Rather, such resemblances may be *fundamentally convergent*, and represent only the minimum requirements or "conditions of existence" which all living systems must possess. (1973, p. 27).

Yet T. H. Morgan (1923, p. 246) acknowledged the fatal consequences if homology did not necessarily indicate ancestry:

If, then, it can be established beyond dispute that similarity or even identity of the same character in different species is *not always* to be interpreted to mean that both have arisen from a *common ancestor*, the *whole argument from comparative anatomy seems to tumble in ruins*.

There are two types of similarities that indisputably do not mean common ancestry: convergences and parallelisms, and homologous structures with nonhomologous genes.

Second, the convergences and parallelisms to which Boyden referred (anatomical or physiological similarities that do not reflect common ancestry) are strong arguments that similar anatomies or physiologies "do not imply any close biological relationship," as Denton (1985, p. 178) notes:

Then there is the problem of convergence. *Nature abounds in examples of convergence . . .* In all the above cases the similarities, although very striking, *do not imply any close biological relationship*.

Numerous acknowledgements were cited in Bird (1987, pp. 94-8) of "massive convergence"—"the bugbear of the taxonomist"—as Dawkins (1986, p. 269) describes it. So widespread are convergences and parallelisms that, Ross (1981, p. 2153) concludes, "[m]ore often than not functional comparisons turned up phylogenetic paradoxes instead of parallels."

In fact, third, the assumption that similar anatomy indicates common ancestry is generally wrong, because the genes that produce the similar anatomical structures are generally not sufficiently similar (homologous), as de Beer (1971, p. 15) points out:

It is now clear that the pride with which it was assumed that the inheritance of homologous structures from a common ancestor explained homology was misplaced; for such inheritance cannot be ascribed to identity of genes. *The attempt to find homologous genes, except in closely related species, has been given up as hopeless . . .* It is useless to speculate on any explanation in the absence of facts.

Thus Zuckerman (1970, p. 64) agreed that "relationships which are inferred on the basis of comparative anatomy may not necessarily correspond to true ge-

netic relationships," and "are in the final analysis speculations." For similar reasons, transformed cladists generally reject the alleged relation between similar anatomy and common ancestry, even though they classify organisms by some similar anatomical features (synapomorphies), because such cladists argue that similar features "need have no evolutionary implications." (Patterson, 1981, p. 208; 1982, p. 364).

Furthermore, an equally basic difficulty is that this alleged relation to common ancestry is contradicted by the history of comparative anatomy and physiology. Linnaeus, "the father of systematic biology" and a creationist scientist, established the classification system by grouping organisms that "resemble one another in body structure" (anatomy), but "did not interpret this propinquity as a consequence of common descent," Dobzhansky (1982, p. 734) observed. The anti-Darwinian scientist Owen then developed the basis of comparative anatomy, while disagreeing that it supports evolution:

The central concept of comparative anatomy is *homology*, a term introduced by Sir Richard Owen, the first director of the British Museum (Natural History), London, and a powerful anti-evolutionist (Patterson, 1978, pp. 121-2).

Therefore, the comparative anatomy argument "provides no evidence for evolution," in Denton's words (1985, p. 155):

In the last analysis the *facts of comparative anatomy provide no evidence for evolution* in the way conceived by Darwin, and even if we were to construe with the eye of faith some "evidence" in the pattern of diversity for the Darwinian model of evolution this could only be seen, at best, as indirect or circumstantial. . . .

The same deep homologous resemblance which serves to link all the members of one class together into a natural group also serves to distinguish that class unambiguously from all other classes. Similarly, the same hierarchic pattern which may be explained in terms of a theory of common descent, also, by its very nature, implies the existence of deep divisions in the order of nature. *The same facts of comparative anatomy which proclaim unity also proclaim division*; while resemblance suggests evolution, *division*, especially where it appears profound, is *counter-evidence against the whole notion of transmutation*.

Similarly, the comparative physiology argument has "not contributed much" to knowledge about macro-evolutionary relationships, in Ross's assessment (1981, pp. 2153-4):

It is an unspoken assumption that the data of comparative physiology must have implications for phylogeny. Is this realistic? . . .

Without multiplying the examples, one can safely say that the *data of comparative physiology have not contributed much to our views about phylogenetic relationships*. That is not to say that these data are irrelevant for theories of evolutionary processes . . .

It seems that *most of the stated aims and general objectives of comparative physiology in all its*

aspects have been unfulfilled. “Illusory” may be too strong a word to describe these objectives but, as I hope to show, it is not just that these objectives have been unrealized but *probably that they are unrealizable.*

The basic difficulties with the comparative anatomy argument are that similarity “may not correspond to the genetic relationships” (Denton, 1985, p. 178), and the “attempt to find ‘homologous’ genes has been given up as hopeless.” (de Beer, 1971, p. 15). In this area, the biological theories of macroevolution and Darwinism are not compelling.

Comparative Embryology Argument and Difficulties

The comparative embryology argument for macroevolution, which focuses on the embryology or development of tissues and organs in a particular organism, (Stansfield, 1977, p. 103), arises from the similarity of development of organs and tissues in various organisms. Most evolutionists view that similar development reflects common ancestry and descent. This argument too faces serious difficulties that many evolutionists acknowledge.

First, the “biogenetic law” was the center of the embryology argument for early Darwinists, and stated that “the embryological development (ontogeny) of an organism repeats (recapitulates) the evolutionary history (phylogeny) of its species.” (Stansfield, 1977, p. 104). That biogenetic law has “been demonstrated to be wrong by numerous subsequent scholars.” according to Bock (1969, p. 684):

the biogenetic law was widely accepted by biologists and served as the basis for the surge of embryological research that continues unabated to this day. Moreover, the biogenetic law has become so deeply rooted in biological thought that it cannot be weeded out in spite of its having been demonstrated to be wrong by numerous subsequent scholars. Even today both subtle and overt uses of the biogenetic law are frequently encountered in the general biological literature as well as in more specialized evolutionary and systematic studies.

Raup and Stanley (1978, p. 354) call the biogenetic law “largely in error,” Ehrlich and Holm (1963, p. 66) note its “shortcomings” and yet its place in “biological mythology”; Danson (1971, p. 35) says that it is “intellectually barren”; de Beer (1965, p. 331) refers to the “evidence against the ‘biogenetic law’ or recapitulation in Haeckel’s sense”; Bonner (1961, p. 242) calls it “probably nonsense”; Robinson (1976, p. 610) calls it “in error”; and even Mayr (1982, p. 215) describes the biogenetic law as “invalid.” In fact, Haeckel, the formulator of the “biogenetic law,” supported it with fraudulent drawings (Rager, 1986, p. 449; Singer, 1931, p. 487).

Second, the modern embryological argument shares the same foundational problem as the comparative anatomy argument, with which it is “very closely related,” (Stansfield, 1977, p. 113): that similar embryological development does not necessarily indicate common ancestry and evolutionary descent (Bird, 1987, pp. 193-6). Oldroyd (1986, p. 154) describes one aspect of the problem:

Anatomically homologous parts in different related organisms appear to have quite different embryonic origins. This is almost *impossible to reconcile with orthodox Darwinian or neo-Darwinian theory*, and it is by no means evident at the time of writing how such problems may be overcome.

Third, the vestigial organ aspect of this embryology argument (that modern organs with no use or reduced usefulness are vestiges of evolutionary stages with former usefulness) is “invalid,” as zoologist Scadding (1981, pp. 173, 175, 176) notes:

An analysis of the difficulties in unambiguously identifying functionless structures and an analysis of the nature of the argument, leads to the conclusion that ‘*vestigial organs*’ provide no evidence for evolutionary theory. . . .

I would suggest that the *entire argument* that vestigial organs provide evidence for evolution is *invalid on two grounds*, one practical, the other more theoretical. The practical problem is that of unambiguously identifying vestigial organs, i.e., those that have no function. The analysis of Wiedersheim’s list of vestigial organs points out the difficulties. As *our knowledge has increased the list of vestigial structures has decreased*. Wiedersheim could list about one hundred in humans; recent authors usually list four or five. Even the current short list of vestigial structures in humans is questionable

Similarly, for other ‘vestigial organs’ there is reasonable ground for supposing that they are functional albeit in a minor way. . . .

The other major objections to citing vestigial organs as evidence of evolution is a more theoretical one based on the nature of the argument. The ‘vestigial organ’ argument uses as a *premise* the assertion that the organ in question has no function. There is *no way however, in which this negative assertion can be arrived at scientifically.*

Since it is not possible to unambiguously identify useless structures, and since the structure of the argument used is not scientifically valid, I conclude that ‘vestigial organs, provide *no special evidence for the theory of evolution.*

The alleged vestigial organs—such as human “gill slits” (which do not exist), (Langman, 1975, p. 262), male nipples, Wolffian and Mullerian ducts, tonsils, appendix, coccyx (“tail bone”), thymus, and facial muscles—almost all have been found to have functions and not to be vestigial:

A fourth category of *vestigial organs* would be those that are the *vestiges of the reproductive structures of the opposite sex*, e.g., nipples in men, vestiges (in the female) of the Wolffian duct, and (in the male) of the Mullerian ducts. These structures, however, clearly reflect the embryonic development of a sexually dimorphic organism which begins its development in a sexually indifferent condition with structures characteristic of both sexes. They certainly do *not reflect phylogenetic development*. No one supposes males evolved from females or vice versa. On the basis of

this analysis, I would suggest that *Wiedersheim was largely in error in compiling his long list of vestigial organs. Most of them do have at least a minor function at some point in life.* (Scadding, 1981, pp. 173-6).

Thus, the comparative embryology argument for biological macroevolution and Darwinism also offers little support, in the view of many evolutionists. In the embryology area, the biological theories of macroevolution and Darwinism are not compellingly established.

Comparative Biochemistry Argument and Difficulties

The comparative biochemistry argument for macroevolution centers around "homologies at the molecular level" that evolutionists believe reflect "the degree of genetic relationship [in which] evolutionary lineages are optimally expressed" (Stansfield, 1977, p. 125). Those molecular comparisons involve the similarity of hemoglobin, cytochrome *c*, insulin, and other molecules in various organisms (Stansfield, 1977, pp. 125-7). The argument from some molecular similarities to macroevolution (common ancestry) and to Darwinian mechanisms involves several difficulties that numerous evolutionists acknowledge. First, the comparative biochemistry evidence that is anomalous, in the sense of indicating significant differences between allegedly closely related organisms, is so widespread that it may instead better support the unrelatedness or distinct ancestry of natural groups of organisms (Bird, 1987, pp. 98-103).

Those widespread anomalies lead Denton (1985, p. 291), a molecular biologist, to conclude that comparative biochemistry contradicts macroevolution:

This new era of *comparative biology* illustrates just how erroneous is the assumption that advances in biological knowledge are continually confirming the traditional evolutionary story. There is no avoiding the *serious nature of the challenge to the whole evolutionary framework* implicit in these findings.

For example, a study of cytochrome *c* by Jukes and Holmquist (1972, p. 530) reveals "anomalies" showing amphibians and reptiles to be more distant in an evolutionary sense than a bird and a fish, a mammal and a fish, or a mammal and an insect:

In either case, certain anomalies appear in certain vertebrates with respect to the magnitude of these changes and their relationship to time. Such anomalies show up on "phylogenetic trees" as apparently negative rates of evolutionary divergence, or incorrect taxonomic placement of an organism in the wrong family However, the difference *between turtle and rattlesnake* of 21 amino acid residues per 100 codons is *notably larger than many differences* between representatives of widely separated classes, for example, 17 between chicken and lamprey, or 16 between horse and dogfish, or even 15 between dog and screw worm fly in two different phyla.

Macroevolution and Darwinism, by contrast, involve an evolutionary sequence from invertebrates (including insects) to vertebrate fish, to amphibians, to rep-

tiles, to birds and mammals. Studies of relaxin by biochemists Schwabe and Warr (1984, p. 471) similarly "do not fit the evolutionary clock model" for comparative biochemistry:

Thus the conclusion to be drawn from the *relaxin sequence data* is that they *do not fit the evolutionary clock model*. The alternative models, those based on positive selection, depend on spurts of mutation fixation or a hypothetical gene duplication whenever a molecule does not fit into a monophyletic evolutionary tree. In fact, the neo-Darwinism of molecular evolution is used exclusively to fit molecular data into paleontologically derived evolutionary trees which in themselves are controversial. That is, the hypothesis is *not based on independent scientific reasoning* that would allow predictions to be made and tested.

Studies of DNA by Vawter and Brown (1986, p. 194) yield such anomalies that the authors call for throwing out the molecular clock hypothesis entirely:

[The] disparity in relative rates of mitochondrial and nuclear DNA divergence suggests that the controls and constraints under which the mitochondrial and nuclear genomes operate are evolving independently, and provides evidence that is independent of fossil dating for a *robust rejection of a generalized molecular clock hypothesis of DNA evolution*.

Many other anomalies, which also "do not fit the evolutionary clock model," were given elsewhere (Bird, 1987, pp. 98-103).

Second, the "molecular clock" of comparative biochemistry is conceptually flawed, as Denton (1985, p. 305) describes:

The difficulties associated with attempting to explain how a family of homologous proteins could have evolved at constant rates has created chaos in evolutionary thought. The evolutionary community has divided into two camps—those still adhering to the selectionist position, and those rejecting it in favour of the neutralist. The devastating aspect of this controversy is that neither side can adequately account for the constancy of the rate of molecular evolution, yet each side fatally weakens the other. The selectionists wound the neutralists' position by pointing to the disparity in the rates of mutation per unit time, while the neutralists destroy the selectionist position by showing how ludicrous it is to believe that selection would have caused equal rates of divergence in "junk" proteins or along phylogenetic lines so dissimilar as those of man and carp. Both sides win valid points, but in the process the credibility of the *molecular clock hypothesis* is severely strained and with it the whole paradigm of evolution itself is endangered.

There is simply *no way of explaining how a uniform rate of evolution could have occurred in any family of homologous proteins* by either chance or selection; and, even if we could advance an explanation for one particular protein family, we would still be left with the *mystifying problem*

of explaining why other protein families should have evolved at different rates.

In fact, the common explanation of inconvenient anomalies, as the result of different rates of molecular evolution used, for example, by Jukes and Holmquist (1972, p. 530) in connection with the cytochrome c results, is simply an "ad hoc argument" that resets the molecular clock whenever it is convenient to macroevolution, as Schwabe (1986, p. 280) notes:

Consider species A suddenly divided into A1, A2 and A3 by insurmountable obstacles. . .

If instead of the expected equal distribution of differences one were to observe that the insulins of A1 and A2 differ by four residues whereas the insulin of A3 differs by 25 residues from both A1 and A2 then one would have discovered *an exception to the neo-darwinian hypothesis*. There are virtually no degrees of freedom in this scenario so that *contradiction can be smoothed over only by ad hoc arguments* such as faster rates of evolution, lateral gene migration or gross errors committed by paleontologists in determining the time of branching of A1, A2 and A3.

Third, the accuracy of the "molecular clock" is unsatisfactorily poor, because the techniques for measuring molecular distance are all unsatisfactory, according to Farris (1981, p. 22):

It seems that the only general conclusion one can draw is that *nothing about present techniques for analysing molecular distance data is satisfactory*. The distance Wagner method seems to be the best available method for arriving at genealogies efficiently—certainly it is far more effective than Prager and Wilson imagined—but, at least for minimizing WSD, further improvement seems possible. But using any method that fits branch lengths to a distance matrix *presupposes* that the distances are suitable for this sort of analysis. That supposition seems *unjustified* for any distance measure now in use.

None of the known measures of genetic distance seems able to provide a logically defensible method, and it appears that some altogether different approach will have to be adopted for analysing electrophoretic data . . .

Sequence data have sometimes been analysed by way of distances, although perhaps more often they have been treated by character analysis methods (for example Goodman et al., 1979). While sequence differences do not seem to offer the severe problems of interpretability of branch lengths that plague genetic distances, *neither is there any good reason to rely on distance techniques*. Reducing character data to distances, once again, simply wastes evidence on kinship.

Immunological distances, like genetic distances, cannot be truly clocklike, nor can they be analysed by branch length fitting, but there is no recourse to underlying character data in this case. . .

Farris is more qualified than almost anyone to analyze those techniques, because he developed one of the most used techniques for measuring molecular distance.

Fourth, the comparative biochemistry argument, like the other comparative arguments, in any event is based on the problematic assumption that similarities between organisms reflect common ancestry rather than engineering design or basic conditions for life:

It could be argued that the universality of much of biochemistry is *merely consistent* with the concept of a common ancestral population but does not in any sense prove it since *the same basic reaction patterns may be required for all life* (Kenyon and Steinman, 1969, p. 2154).

Therefore, the comparative biochemistry argument for macroevolution and Darwinism involves a "molecular clock" that does not work and is simply an "apologetic tautology," according to Denton (1985, p. 306):

The hold of the evolutionary paradigm is so powerful that an idea which is more like a principle of medieval astrology than a serious twentieth-century scientific theory has become a reality for evolutionary biologists.

Here is, perhaps, the most dramatic example of the principle that wherever we find *significant empirical discontinuities* in nature we invariably face great, if not insurmountable, conceptual problems in envisaging how the gaps could have been bridged in terms of gradual random processes. We saw this in the fossil record, we saw it in the case of the feather, in the case of the avian lung and in the case of the wing of the bat. We saw it again in the case of the origin of life and we see it here in this new area of *comparative biochemistry*.

What has been revealed as a result of the sequential comparisons of homologous proteins is an order as emphatic as that of the periodic table. Yet in the face of this extraordinary discovery the biological community seems content to offer explanations which are no more than *apologetic tautologies*.

Comparative biochemical information is simply "at odds" with neo-Darwinism, in the assessment of Schwabe (1986, p. 282): "One might ask why the neo-Darwinian paradigm does not weaken or disappear if it is at odds with critical factual information." Thus, at least in the area of comparative biochemistry or molecular biology, the theories of macroevolution and Darwinism are not compellingly established, in the view of many evolutionists.

Population Genetics Argument and Difficulties

The population genetics argument for macroevolution and Darwinism, which focuses on gene changes within a population (Roughgarden, 1979, p. 5), is founded on extrapolation of small changes in gene frequencies in a population to major transformations from a common ancestor to evolutionary descendants. It is "[p]erhaps the most important source of evidence in support of the theory of evolution," according to Stansfield (1977, p. 129) and "contains the core mechanisms of neo-Darwinian evolutionary theory," in the words of Ruse (1982, pp. 112-3). Yet that extrapolation is questionable, and the argument has failed, in the view of many evolutionist as well as nonevolutionist scientists.

First, population genetic changes (microevolution) *tell nothing about macroevolution* or even the origin of species, as Lewontin (1974, p. 159) observes:

It is an irony of *evolutionary genetics* that, although it is a fusion of Mendelism and Darwinism, it has made *no direct contribution to what Darwin obviously saw as the fundamental problem: the origin of species.*

and Grassé (1977, p. 170) notes:

To assert that population dynamics gives a picture of evolution in action is an *unfounded opinion*, or rather a postulate, that relies on *not a single proved fact showing that transformations* in the two kingdoms have been essentially *linked to changes in the balance of genes in a population. . . . [A]s for seeking in it proof of the formation of new species, there is no such hope.*

Mayr (1980, p. 11) said that "the most cogent objection against population genetics raised by Grassé" was that he "could not see any connection between changes in gene frequencies. . . and the evolutionary events. . . of species and higher taxa." Rosen (1978, p. 372) concurs that population genetics "cannot, and does not purport to, explain the origin . . . of new codons" (parts of new genes). Spiess (1977, p. x) concedes that "we are a long way from describing the origin of species . . . with methods of experimental population genetics." Saunders and Ho (1982 p. 185) add that population genetics simply "is not, however, a theory of evolution." Kempthorne (1983, p. 120), while wishfully assuming that after nearly a century "population genetics theory is in its infancy," cautions that a "real danger is that the theory is generated primarily by the mathematics we can do, rather than by the biological processes that should be included." Lambert (1984, p. 125) suggests that, because "evolution is . . . most especially not a population genetics problem," but "a problem of the origin of form," then "population genetics is not a tool to investigate evolution. Similar observations are made indirectly by all the evolutionist scientists who object to extrapolation from microevolution to macroevolution. Stanley (1975, pp. 648, 650; Bird 1987, pp. 54-64).

Second, population genetics comes perilously close to *explaining nothing*, as Lewontin (1974, pp. 11-2, 189) concludes:

The theory *explains nothing* because it explains everything. It is my contention that a good deal of the structure of evolutionary genetics comes perilously close to being of this sort. . . .

For many years population genetics was an immensely rich and powerful theory with virtually no suitable facts on which to operate. It was like a complex and exquisite machine, designed to process a raw material that no one had succeeded in mining. Occasionally some unusually clever or lucky prospector would come upon a natural outcrop of high-grade ore, and part of the machinery would be started up to prove to its backers that it really would work. But for the most part the machine was left to the engineers, forever tinkering, forever making improvements, in anticipation

of the day when it would be called upon to carry out full production.

Quite suddenly the situation has changed. The mother-lode has been tapped and facts in profusion have been poured into the hoppers of this theory machine. And from the other end has issued — *nothing*. It is not that the machinery does not work, for a great clashing of gears is clearly audible, if not deafening, but it somehow cannot transform into a finished product the great volume of raw material that has been provided. *The entire relationship between the theory and the facts needs to be reconsidered.*

Roughgarden (1979, p. 5) concurs that the assumption that by population genetics "longterm evolutionary phenomena would be explained" needs to be reassessed. And Saiff concludes:

The leading workers in this field have confessed, more or less reluctantly, that *population genetics contributes very little to evolutionary theory*. We will cite three such leaders. . . .

If the leading authorities on population genetics confess to this dismal lack of achievement and even chuckle about it, *it is altogether fitting and proper for the rank and file to take them at their word*. Therefore it seems to follow that *there is no need to teach population genetics in introductory courses on evolution*, although advanced courses may include it as a matter of history. (Saiff, 1983, pp. 71-2; cf. Hewitt, 1983, p. 73)

Finally, and related to its failure, population genetics embodies *circular reasoning* when it is used to support evolution, as Forey (1982, p. 124) argues:

The hope of many of the authors of trees is that the synthetic approach (Bock 1974) will combine the analysis of pattern with the theories of the population biologists/geneticists in mutual support. The problem is that both are ultimately derived from the theory of evolution: the fossil record being interpreted in the light of *population biology* theory and expressed as ancestor-descendent relationships (Simpson 1953). One might remark that, far from being mutually supportive, *this is merely "the blind leading the blind."*

Because the "idea of differential reproduction to explain evolutionary change formed the basis for what today is the field of population genetics" (Rosen, 1978, p. 371), the problems of natural selection in terms of macroevolutionary effect and evidence, and in terms of tautology and nonexplanatory nature. (Bird, 1987, pp. 158-65) apply equally to population genetics. In this area, macroevolution and Darwinian evolution are not compelling, in the view of many leading researchers.

Artificial Selection Argument and Difficulties

The artificial selection argument for macroevolution is based on extrapolation from breeding experiments (artificial selection) to natural selection and then to macroevolution. In fact, "more than almost any other single factor, domestic breeding has been used as an argument for evolution," according to Eiseley (1958, p. 223) as well as Dobzhansky *et al.* (1977, p. 97). The second extrapolation, which even many evolutionists

question, has been discussed elsewhere (Bird, 1987, pp. 155-78). The first extrapolation entails serious problems.

First, artificial selection is not *relevant* to natural selection, and thus is not a persuasive argument for it, according to Wassermann (1978, p. 235):

Artificial selection is often considered as a means for testing population genetic theories (cf. Lewontin [1974], p. 250). But there remain, in my opinion, *serious doubts* about the role that artificial selection could or should play. Dobzhansky ([1970], p. 201) argued that Darwin used artificial selection as a model of the natural process; a mathematical theory of selection must almost necessarily be derived from experiments on artificial selection. This belief, however (and its *doubtful* conclusion), rests on the implicit hypothesis that artificial selection necessarily simulates some natural selection process. *One could only confirm this hypothesis by studying first the natural selection process* extensively and then examine how well artificial selection simulates it. *But if one could study the natural process in the first place, then one would not need any simulation*, unless the simulation process could be better controlled (and provided the controls do not change the effects of the simulation process much from those of the natural process).

Other aspects of the irrelevance of artificial selection to natural selection are identified by Macbeth (1986, p. 194):

At the same time, however, *Darwin fell into the traps* that Fischer warns against. First, he was so enchanted with the similarities that *he paid little attention to the obvious dissimilarities* (presence of a guiding intelligence in artificial selection, plus the breeders' concentration on micro changes rather than on the big gaps). Second, he offered the analogy as a proof: see pages 14-15 and Chapter 4 of *The Origin*.

Although the analogy had nobly performed its function in stimulating Darwin's imagination, *it furnished no evidence of the correctness of Natural Selection*. It has historical interest, but it was not essential to the understanding or proof of Natural Selection. Alfred Russel Wallace did not need it to reach the same conclusion as Darwin; to the contrary, McKinney (1972, pp. 144-5) shows that he rejected the analogy . . .

Second, artificial selection is not just an irrelevant proof, but it is an irrelevant analogy, for macroevolution and Darwinism, as Russell (1962, p. 124) states:

It is important to note that in all these processes there is *no "selection"* in the proper meaning of the word. It is unfortunate that Darwin ever introduced the term "natural selection," for it has given rise to much confusion of thought. He did so, of course, because he arrived at his theory through studying the effects of selection as practised by man in the breeding of domesticated animals and cultivated plants. Here the use of the word is entirely legitimate. But the *action of man in selective breeding is not analogous to the action of "natural selection,"* but *almost its direct opposite,*

as Woltereck (1931) in particular has pointed out. Man has an aim or an end in view; "natural selection" can have none. Man picks out the individuals he wishes to cross, choosing them by the characters he seeks to perpetuate or enhance. He protects them and their issue by all means in his power, guarding them thus from the operation of natural selection, which would speedily eliminate many freaks; he continues his active and purposeful selection from generation to generation until he reaches, if possible, his goal. *Nothing of this kind happens, or can happen, through the blind process of differential elimination and differential survival* which we miscall "natural selection."

Even Dobzhansky *et al.* (1977, p. 97) join in the argument that artificial selection is too different from natural selection to be relevant. Just as human ability to construct bridges does not establish that bridges evolved without engineering, human ability to breed cows does not help establish that cows arose through macroevolution.

Third, artificial selection supports not the possibility of macroevolution but the genetic limits on viable microevolution. Those "more or less fixed limitations," and "limits to the development possible," were noted by Burbank (1939, pp. 97-9. Deevey (1967, p. 636) concurs that artificial selection faces a "species barrier." Eiseley (1958, p. 223) acknowledges that variations "cannot be selectively pushed beyond a certain point." Falconer (1960, p. 186) points out that, for breeding experiments, "only the fact that domesticated plants and animals do not live under natural conditions has allowed these improvements." Those genetic limits on viable change are analyzed (Bird, 1987, pp. 84-9).

Here, the biological theories of macroevolution and Darwinian evolution are not compellingly established, many evolutionists admit.

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QUOTE

Hopkins labored to construct an all-encompassing ethical system. God, because of the infinite goodness of his character, deserved infinite honor. Men, as creatures made in the image of God, deserved to be treated with the great respect accorded by this high honor. Hopkins' famous ethical catchword, "disinterested benevolence," was a description of the way in which individuals should act toward men—benevolently, because of the high order of divinely created humanity, and disinterestedly, in consideration of the overarching imperative to honor the other person's inherent worth.

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PANORAMA OF SCIENCE

Formation and Rate of Growth of Stalactites

The process of stalactite formation is probably not quite as simple as the generally accepted explanation would suggest. If the deposition of the calcareous matter depended entirely upon the influence of the carbon dioxide that rain water derives from the air, it is unlikely that, when the water drips from the roof of a cave, the gas would be given off to such an extent as to result in the formation of important calcareous deposits. Emeritus Professor C. Thompson, of Cardiff, has suggested to me that, when passing through the soil, in which carbon dioxide is much more abundant than in the air, water takes up a quantity of that gas greatly in excess of what it can hold under atmospheric conditions: this increases its capacity to dissolve calcium carbonate, and, on reaching a cavity such as a cave in which the composition of the air is probably very similar to that of the outer

air, the excess of carbon dioxide is given off with comparative rapidity, with a correspondingly rapid deposition of calcium carbonate . . .

Very little is known concerning the rate of growth of stalactites. The North Bridge, Edinburgh, was erected in 1772, and after a century, stalactites an inch and a half in diameter, with the characteristic crystalline structure were found pendant from it, while in the Ingleborough Cave, Boyd Dawkins found the rate of growth to be .2946 inches per annum or nearly 30 inches in a century. This was believed to be an exceptionally rapid rate of growth ["On the Action of Carbonic Acid on Limestone," *Report British Association* (1880), p. 573]. In any case the present rate of growth in a cave like those at Cheddar or in Derbyshire is not necessarily an indication of the length of time that the pendant columns have taken to reach their present dimensions, for at times when the local water level was higher than it

is now, the amount of mineral-charged water passing through the fissures may have been greater. (North, p. 452).

One of the generally accepted mechanisms of rapid dripstone and/or cave formation is the supply of water available (Williams and Herdtklotz, 1977, p. 197; 1978, p. 88). Water is able to dissolve large quantities of CO₂ as it percolates through soil (Williams and Herdtklotz, 1977, pp. 192-3; 1978, pp. 88-9). The CO₂-laden water would dissolve considerable CaCO₃ as it passes through jointed limestone (cave formation) and then redeposits the CaCO₃ in the form of dripstone in a previously-formed ventilated cave (Williams and Herdtklotz, 1978, p. 88). Thus very rapid dripstone formation is possible under the proper conditions. Rapid formation and growth of CaCO₃ structures in a laboratory situation have been reported. See Williams, House and Herdtklotz, 1980, pp. 205-8, 226 including the references. Vast periods of time are not needed to produce large quantities of dripstone.

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Contributed by Emmett L. Williams

Replacing Creationism with "Self-Created" Man (Roche)

If so unpromising a god as unrelieved narcissism, the Me, can form the basis for a mass religion, it is indeed wisely said that everyone must believe in some god. By "god," I mean whatever pre-emptive authority we set over the mystery of our being. The authority, of course, can be virtually anything, secular or divine, from the Me, or the sun, or cocaine, to the Tao of the East, the one God of Islam, and the Holy and Undivided Trinity of Christianity. To a son of the Enlightenment, the god of all men is Man, our collective being transcending the individual, and thus apotheosized. Men may die, but Man will live forever, under the care of the heavenly host: Science, Progress, Enlightenment, Evolution, Revolution, and all the angels on high. Man, made priest of creation through Nature's selection, shall rule the very cosmos. Men "shall be as gods."

This is most curious. We see we are, as it were, god-ridden or god-fueled. The most resolute efforts by Rational Atheist Societies and Militant Atheist Leagues succeed only in producing weird new faiths of their own. It appears literally impossible for us to believe in nothing. Without an absolute standard of Good, we have no way to discern between better and worse; our life loses all direction; we can't think at all. Our minds just don't work that way. Yet if we are, as the anti-hero insists, purely natural creatures in a purely natural universe, how could a god concept have possibly

arisen at all? Nothing like this is observable in Nature, nor could it ever be in a purely material cosmos. When we look at Nature with the eyes of beasts, we see only Nature. Only when we look at her with the eyes of men, with reverence and wonder and intelligence, do we see spirit. And we see the workings of the divine. The god concept is common to us all, and nags us all unmercifully. Where did it come from, if not from our Creator? How else could it be that our minds will not function without a god figure? Surely it is clear we carry within us a divine spark, else we could not conceive these matters, much less long to be "as gods." One is reminded of the first moment we did see ourselves with the eyes of men, as related in the creation story in *Genesis*. There occurs one of the most striking passages in mythology—and as we'll see, I do not use "mythology" to mean "false." (We may think of *Genesis* as fanciful, but next to all other creation myths it is as prosaic as a newspaper report.) When Adam and Eve had eaten the forbidden fruit, "the eyes of them both were opened, and they knew that they were naked." (my emphasis) Their first act "as gods" was to get dressed—imagine! All people of all times have worn clothes, and it is one of those maddening details about this strange beast, man, that defies materialist theories. Here, allegorically, at the beginning of our career in the god business, what we saw was our fall and our shame. No evolution myth can give us half so good an account of ourselves.

And that is a prime clue to our nature, this everlasting temptation "to be as gods, knowing good and evil." Is it not just this that impels the anti-hero to dethrone God and deify himself? Anti-heroism centers in rebellion against all religion, and against Christianity particularly. Yet anti-heroes, as all other men, must have a god concept. The philosophical inquirer will always find it rewarding to ask what god a man will believe in, if he won't believe in the Creator. It is ever instructive to see what the anti-hero *will* believe, in order to disbelieve in God. We therefore ask here: With what god does the anti-hero displace the Almighty? To what faith or faiths does he cling?

The investigator would find many. There is a pantheism, made to look new and modern whereas it is as old as man, dressed in the conceit that it is more spiritual and refined than the old faiths, when it is merely so diluted and empty as to be no faith at all. What Professor Voegelin terms gnosticism, which in simplest terms is the reduction of complex reality to gnosis, formulas, with neither reverence nor understanding, is a permanent lure for the anti-heroic mind. But it is a trap, egotism posing as knowledge, and its fruits are bitter. There is "alienism," too, in Joseph Sobran's coinage, a sort of holy rejection of the normal and enduring foundations of civilization, a rejection of the center in favor of the abnormal, the outsiders, the minorities and supposed victims. And many more. We need not examine all the curious faiths of the anti-hero. A close look at a few will do. But it may be said: Wherever the doctrine that "religion is irrelevant," or worse, is voiced or implied, you have found the influence of anti-heroic faith.

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NCSE Meeting

On 13 February 1988 hundreds of scientists gathered to choose what lectures to attend from among the scores presented at the American Association for the Advancement of Science (AAAS) meetings in Boston, Massachusetts. Some of these scientists allotted time to participate in a non-AAAS function in the same hotel. They joined about 125 people, mostly educators, at the annual meeting of the National Center for Science Education (NCSE).

The NCSE is the national coordinating organization for the Committees of Correspondence. The latter were conceived in response to what the founders considered the "threat" of creationists undermining the public's understanding of evolutionary theory. Currently, the Committees are a series of small-staffed groups organized on a statewide or comparable level. Each group tries to counter creationist articles in local periodicals and answer claims made by local creationists. They try to monitor local creationist conventions and lectures and offer the general public alternatives to the teachings of the creationists. A number of the Committees also publish a newsletter. Beginning in 1980 the Committees of Correspondence formed the National Center for Science Education. The NCSE publishes the bi-monthly *Creation/Evolution Newsletter*. It is also attempting to develop a repository of anti-creationist material for use by the Committees of Correspondence in critiquing creationists.

Even though Eugene Scott, Executive Director of the NCSE, challenged the membership in Boston to "find out where creationism is being taught and oppose it," the whole NCSE agenda is not as straightforward as this. It appears that in an attempt to avoid being considered anti-creationist or anti-anti-evolutionist, the NCSE has adopted a purpose which differs from the original purpose of the Committees of Correspondence. The official purpose of the NCSE is to "improve science education." Clearly a strong thrust of the organization is pro-evolution and anti-creation; yet the organization's stated purpose attracts such people as the authors, who do not have such an interest.

It is necessary to be aware of this slight paradox of history, because it allows one better to understand the mixed nature of the presentations at a meeting of the NCSE. There were lectures designed to attract potential members to the meetings, as well as presentations intended to distribute information to those interested in achieving the organization's goals. The latter presentations are of two types—one that presents anti-creationist material, and another that presents material to improve science education. The 1988 annual meeting of the NCSE offered examples of each of these.

The annual business meeting revealed that the NCSE has had trouble raising money. The Carnegie Corporation of New York granted them \$150,000 and the Esther A. and Joseph Klingenstein Fund, Inc. \$25,000. Monies have come from other sources including the Lounsbery Foundation and the Deer Creek Foundation, but they apparently lack a good base of support from either members or the general public.

Also part of the 1988 NCSE meeting was a panel discussion purposing to address problems of the Committees of Correspondence. Here, we gained the distinct impression of despair. Although they felt capable

of mobilizing in the event of a creationist incursion or threat, the Committees have had trouble maintaining momentum among members between "crises." They seemed discouraged that they were not able to actively campaign in *preparation* for creationist insurgences, rather than in *response* to them.

Stephen Jay Gould presented what might have been conceived as the "potential-member-attracting" lecture. If NCSE included it for this purpose, it was a dismal failure. Firstly, Gould's lecture did not seem to attract many more people than were already attending the NCSE meeting. Secondly, in his rather typical style, Gould did not engage the audience with warmth and enthusiasm. He entered the room moments before the lecture, read most of the lecture from the tear-sheets of an already-published *Natural History* article, and left immediately after some brief questions. Finally, Gould seemed to lose some of the NCSE members themselves when he stated that the recent Supreme Court Decision against creationism had destroyed the last remaining "creationist strategy." This statement had an air of naiveté in the light of previous NCSE presentations. There had been sharp and specific warnings from the previous speakers that the creationists in fact *possessed* another strategy and that the Committees of Correspondence would have to hone their swords and keep a sharp eye out for this new stratagem.

Several presentations were anti-creationist in content. The meeting started with a panel discussion on "Debating Creationists: Do's and Don'ts." Leading the discussion was Frederick Edwords, the Executive Director of the American Humanist Association of Amherst, New York and editor of *Creation/Evolution* followed by Kenneth Miller from the Department of Biology at Brown University—both with experience in debating creationists. The strongest "don't" was that one should *NOT* debate a creationist if it were possible to avoid it. Debates are not effective means of arriving at truth. A good debater, after all, can be very effective on either side of an issue, with or without substantive evidence, and with or without belief in the position he or she is defending. Clearly, though, it would not have been necessary for Edwords and Miller to take two hours to say that and they would have left out the "do's." In fact, they offered both their materials and themselves (as what they called "hired guns") to debate creationists. Edwords' presentation was very good, concentrating on debating methods—noting that what the audience remembers about a debate is more important than the substantiveness of the scientific evidences. Edwords has been very successful at suggesting ways of matching the very effective debating tactics of, for example, Duane Gish. Creationists should be very careful in choosing debaters in the light of the information and personnel that NCSE can produce against a creationist debater. Miller had a very different approach to the issue and a much less effective presentation. He concentrated on the scientific problems with some of the creationist arguments, such as the amount of dust on the moon, the decay of the earth's magnetic field, etc. Although some of his critiques were good, he failed on others, not being current on such things as the petrified forest

in Yellowstone National Park, and the Glen Rose Texas footprint controversy.

Rev. James W. Skehan, the Director of the Weston Observatory for Boston College, lectured on the interaction of science and the Bible. Picturing Darwin as essentially a religious man, he went on to serve what is a popular NCSE menu of evolution and Biblical criticism. Even though embellished by some beautiful slides, the presentation contained some questionable facts (for example, the number of species and there was an expressed lack of understanding of Genesis).

By far the best of the papers presented at the NCSE was by Craig Nelson from the Department of Zoology, Indiana University. His presentation was the only one which truly dealt with the stated aims of the NCSE itself—namely the improvement of science education. He summarized the salient features of a teaching method he has been using in his evolution course at Indiana University. He attempts to teach

students critical thinking. This is a controversial, but much-superior method to the “stuff-and-regurgitation” (KPW term: “stuff as much stuff into the head of a student as possible and have him regurgitate as much of it as possible on a test”) used in most classrooms today. Nelson claims that he has seen marked improvement in the reasoning abilities of his students as a result of his method of teaching. The drawbacks are that the students take some time to adjust and in fact object to a teaching method where they have to “work.” In addition, it takes much time and effort for a teacher to develop material and change the way they were taught themselves for the sake of the students.

We would like to see more material along the lines of Craig Nelson’s presentation from NCSE. This type of material is more honest to the stated aim of NCSE, and more useful to students nationwide. We all are interested in better science instruction.

Contributed by Kurt P. Wise and Wayne Frair

MINISYMPOSIUM ON THE SPEED OF LIGHT—PART II

THE SPEED OF LIGHT AND PULSARS

ROY D. HOLT*

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Abstract

The consistency of pulsar signals provides unique constraints on the hypothesis that the speed of light has decayed in the past. The model of decay in the speed of light proposed by Norman and Setterfield is found to be an inaccurate description of reality. The data and theory strongly suggest that if the speed of light has decayed, it has done so in a very obscure manner.

Introduction

Seeing light from distant stars in a short creation time frame is a well-known point of confrontation in creation/evolution discussions on cosmology. Creation explanations have generally been to suggest that the light was created in transit, the universe exists in Riemann space or that the speed of light was much faster in the past. It is the purpose of this paper to examine the last explanation and the model recently proposed by Norman and Setterfield (1987).

If the speed of light has changed, hopefully, there is some way we can detect it. A change might be found by examining the speed of light measurements over the last 300 years or by measurements of light from distant stars in the last two decades. An excellent review of speed of light measurements prior to 1941 was completed by Dorsey (1945). Dorsey’s conclusion was that the speed of light was a constant.

During the past decade Setterfield and others have advocated that the speed of light is not a constant and that it has decreased by many orders of magnitude. They support this position by attempting to document a small decrease in the speed of light from historical data (Norman and Setterfield, 1987). Other contributors to this symposium have shown that measurements during the last 300 years support a constant, not a decreasing, value for the speed of light. Though the speed of light has been constant in recent history, perhaps it was faster prior to 300 years ago.

Pulsars behave as very consistent clocks and may provide a unique way of measuring changes in the speed of light in the past. Any changes in the speed of light during the emission of pulsar signals or during the transit of the signals through space should affect the timing of these signals. If the speed of light is changing, the length of time between pulses should be increasing at the same rate that the speed of light is decreasing.

Pulsars

Pulsars are thought to be spinning neutron stars that are surrounded by a co-rotating magnetosphere by which they generate periodic signals. In 1984, 368 pulsars were known and more have been discovered.

Pulsar timing is characterized by its period P , period time derivative $\dot{P}$ (the rate of increase in the period), and stability. Pulsar stability is a measure of irregularities in pulsar timing after taking into account the period derivative $\dot{P}$.

All pulsars measured have periods between 0.0015 and 4.3 seconds. The average pulsar period is about 0.7 second. For 268 pulsars where $\dot{P}$ is known, $\dot{P}$ ranges from 10^{-12} to 10^{-18} s/s with an average at 3×10^{-15} s/s. Stability of pulsar timing is better than 10^{-10} (Hewish, 1980; Dewey *et al.*, 1984; Cordes and Downs, 1985). A notable exception is PSR 1937+21 (sometimes referred to as 1937+214) with a $\dot{P}$ at 10^{-20} s/s and a stability of better than 6×10^{-14} . The stability of this pulsar has been shown to rival that of atomic clocks (Rawley *et al.*, 1987).

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Pulsars are randomly distributed about a 10 kpc radius from our galactic center (Lyne, 1980). The low signal level of pulsars precludes their detection from extra-galactic sources. Velocity vectors of pulsars also appear to be random in direction and magnitude, with a mean velocity of 180 km/s and a maximum at about 500 km/s (Lyne, 1980).

Pulsar timing stability and consistency must be very good to measure any variation in the speed of light, preferably much better than any alleged variation. Period stabilities and consistencies of pulsars measured, as mentioned above, are better than the decay in the speed of light alleged by the Norman-Setterfield model. This stability and consistency of timing have strong implications for any model proposing a decay of the speed of light.

The Norman-Setterfield Model

The Norman-Setterfield model (1987) for the decay of the speed of light refers to two types of time or ways by which time is measured.

One is atomic time that is governed by the period taken for an electron to move around once in its orbit. In essence, it is electromagnetic in character. The other is dynamical time whose units are subdivisions of the period that the earth takes to make one complete orbit of the sun. Obviously, this clock is governed by gravitation. (p. 4)

Atomic time is measured with respect to atomic clocks and according to the model varies inversely proportional to the speed of light (p. 5). Atomic time will be designated by t .

Dynamic time is related only to earth's orbital period and not to its spin rotational period; that is, dynamic time is related to the length of earth's year and not the length of earth's day. This distinction is very important. Dynamic time will be designated by T .

In their model, Norman and Setterfield propose the following relationships:

$$m \propto 1/c(T)^2 \quad (1)$$

where m is mass and $c(T)$ is the speed of light as a function of dynamic time (p. 31) and

$$v \propto 1/c(T) \quad (2)$$

where v is the velocity of any mass (p. 33). Distance is time invariant in the Norman-Setterfield model (pp. 4, 45).

From equations 1 and 2 it is obvious that linear momentum (mv), and consequently angular momentum, varies as $1/c(T)$ and is not conserved in the Norman-Setterfield model. Momentum is not mentioned in the model and violation of this conservation law is never discussed.

Though not explicitly stated, the model appears to have been developed with the intent to conserve kinetic energy independent of the speed of light. Kinetic energy can be divided into two types, linear and rotational kinetic energy. Linear kinetic energy is the energy of an object associated with its instantaneous velocity, e.g., the energy of a falling rock or a moving automobile. Linear kinetic energy ($mv^2/2$) is conserved independent of the speed of light in the model, (p. 33).

Rotational kinetic energy is the energy of an object associated with its spin about its axis, e.g., the energy of a spinning top or a rotating planet. Norman and Setterfield did not directly address the conservation of rotational kinetic energy with a change in the speed of light and were silent about any implications this might have. To avoid a misreading of the model, the implications of conserving and not conserving rotational kinetic energy will be developed here.

For a sphere of uniform density with a mass m , radius R , and angular velocity, ω , the rotational kinetic energy is given by

$$KE = \frac{mR^2\omega^2}{5} \quad (3)$$

(In this article ω is used only to denote the spin angular velocity of an object and not its orbital angular velocity. For the earth, ω is related to the length of the day and not the length of the year.) Using equation 1 we may rewrite 3 as

$$KE = \frac{M_i R^2 \omega^2}{5c(T)^2} \quad (4)$$

where $m = M_i/c(T)^2$ and M_i is a constant independent of the speed of light. If we stop at this point, assuming R and ω are constants independent of the speed of light, we see that rotational kinetic energy is not conserved in the Norman-Setterfield model, but increases in time. This violation of the first law of thermodynamics would certainly have been noticed and has implications for pulsar timing.

Since ω is proportional to the instantaneous velocity of elements of a solid rotating system, one could infer from equation 2 that ω should vary proportional to $c(T)$ in the Norman-Setterfield model. If this is assumed, rotational kinetic energy is conserved and is independent of the speed of light. However conservation of rotational kinetic energy in the Norman-Setterfield model causes another very significant problem. If the rotating system is the earth and ω slows down in dynamic time proportional to $c(T)$, the length of a day will be constantly increasing. This increase in the day's length will be inversely proportional to the speed of light and very noticeable.

The implication of this for the Norman-Setterfield model is grave. About 6,000 years ago, when the speed of light was allegedly 10 million times its present value, the length of a day would be 0.0086 seconds. This would require 3.7×10^9 days in a dynamic year. Just 1,000 years ago when the speed of light was allegedly twice its present value, the day would be 12 hours and there would be 730.5 days in a dynamic year. Historical records do not allow for such an absurdity.

If rotational kinetic energy is conserved in the Norman-Setterfield model, we have an absurdity about the number of days in a year. If rotational kinetic energy is not conserved, the model violates the first law of thermodynamics. These inconsistencies show major errors in the Norman-Setterfield model.

Pulsars and the Speed of Light

Suppose there is some pulsar a distance from earth emitting periodic signals with a distance L_{mn} between wave fronts m and n (Figure 1). In dynamic time the

distance between two consecutive wave fronts m and n is given by

$$L_{mn} = \int_{T_m}^{T_n} c(T) dT \quad (5)$$

where T_m and T_n are the *emission* times in dynamic time of pulses m and n respectively. In atomic time the distance is given by

$$l_{mn} = \int_{t_m}^{t_n} \hat{c}(t) dt$$

where t_m and t_n are the measured arrival times in atomic time $\hat{c}(t)$ of pulses m and n respectively at earth and where $c(t)$ is the speed of light as a function of atomic time. (Corrections for the relative motion of the earth and pulsar having already been made.)

With distance as an invariant in the Norman-Setterfield model, the distance between wave fronts is the same in both dynamic and atomic time. (Distance need not be an invariant for the conclusions of this analysis to be valid as long as any transformation is linear in space and constant in time.) So we have

$$L_{mn} = l_{mn} \quad (7)$$

Using this relationship we may write, from 5 and 6,

$$\int_{T_m}^{T_n} c(T) dT = \int_{t_m}^{t_n} \hat{c}(t) dt. \quad (8)$$

This gives us a direct comparison between the speed of light in the present and the past.

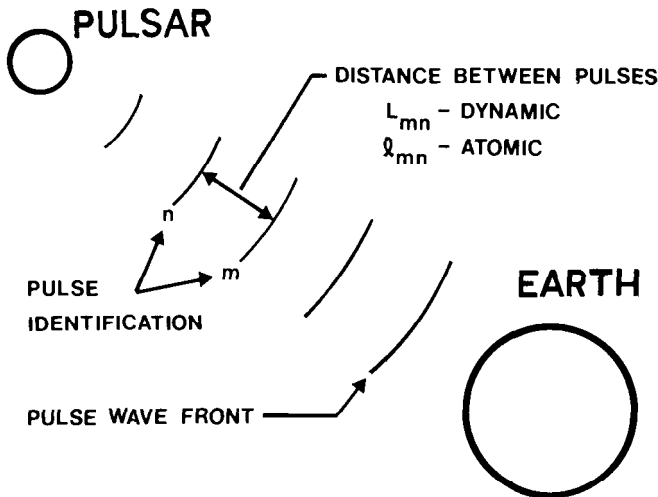


Figure 1. Pulsar signals traveling to earth. Distance variations between a pulsar's signals provide a way to measure any change in the speed of light since the time signals were emitted.

From this, two major problems for the Norman-Setterfield model will become apparent. One problem is the predicted pulsar timing time derivative $\dot{P}$ due to a change in the speed of light. The other problem is a direct implication for the consistency of the speed of light. A change in the speed of light while pulsar signals are being emitted will produce a change in the distance between pulses. This change in distance is

equivalent to a change in pulsar timing and can be given by

$$\dot{P} = \frac{L_{mn} - L_{mn'}}{1/2(L_{mn} + L_{mn'})}$$

Where L_{mn} and $L_{mn'}$ are the distances between consecutive pulses at the beginning and ending, respectively, of some observational time interval Δt . Though the derivation is involved, equation 9 can be reduced to

$$\dot{P} \approx \frac{2S(b + 4dT_m)}{a/T_m + b(T_m + S) + dT_m^6(T_n + 8S)} \quad (10)$$

if $T_m \gg S \gg \Delta t$,

where a , b and d are the constants defined by Norman and Setterfield (p. 55) and:

$T_m = T(t_m)$ and is the dynamic time since pulse m was emitted,

t_m = the number of atomic light-years it has taken pulse m to reach the earth,

and $S = T(t_m + \Delta t) - T(t_m)$ and is the dynamic time between the emission of pulse m and n .

To evaluate $\dot{P}$, $T(t)$ must be determined. By changing the limits of integration in equation 8 one can find that

$$t = T + \frac{bT^3}{3a} + \frac{dT^9}{9a} \quad (11)$$

as Aardsma (1988) has done. The Newton-Raphson method of iteration can be used to find T for specific values of t .

Evaluating 10 for all pulsars within 50 light-years of the earth and assuming a four month ($1/3$ year) observational period, one finds that $\dot{P}$ ranges from 6.1×10^{-6} to 6.6×10^{-4} . For PSR 1937+21 $\dot{P}$ is 4.4×10^{-5} . These values are six to 12 orders of magnitude higher than that actually observed for all pulsars and 15 orders of magnitude higher than that observed for PSR 1937+21. This is a huge difference between observation and prediction. (Allowing the rotation rate of the pulsar to slow in time makes this difference even larger.)

The other problem for the Norman-Setterfield model is with regard to a direct implication for the consistency in the speed of light. Since $\hat{c}(t)$ is a constant in atomic time, the integral on the right side of equation 8 becomes a measure of the period of atomic time between consecutive pulses. For all pulsars observed, excepting "noisy" glitches, this period is constant and stable to better than 10^{-10} .

For pulsar PSR 1937+21 the constant has tighter limits. Its:

... frequency stability is at least as good as 6×10^{-14} for averaging times longer than four months, and over the longest intervals the measurements appear to be limited by the stability of the reference atomic clocks. [In addition] The stable rotation and sharp radio pulses of PSR 1937+21 make this pulsar a clock whose long term stability may exceed that of the best atomic clocks (Rawley *et al.*, 1987).

With such stability of atomic time intervals and $\dot{P}$ values less than 10^{-12} , the right side of equation 8

becomes a constant to better than 10^{-10} for all measured pulsars and better than 6×10^{-14} for PSR 1937+21. We have then in dynamic time

$$\int_{T_m}^{T_n} c(T) dT = \text{constant to better than } 10^{-10}. \quad (12)$$

This represents a situation in which $c(T)$ would normally be labeled as a constant, but other interpretations are possible.

Option for Interpreting Pulsar Timing

There are three options available for interpreting equation 12 that allow a decay in the speed of light. They are:

- I. At the time of emission of all the pulses so far measured, the speed of light was constant in dynamic time. We do not know at what value it was constant or if it later changed.
- II. The difference between emission times or simply the pulse period of all the pulsars could have varied inversely proportional to $c(T)$ so as to keep each incremental integral from T_m to T_n a constant.
- III. It could be imagined that the velocity of pulsars relative to earth could be accelerating in dynamic time inversely proportional to $c(T)$ so as to keep the incremental integral constant.

Option III cannot be maintained in view of the random velocity vectors of the pulsars (Lyne, 1980; Cordes and Weisberg, 1984) and the absence of any corroborating evidence for such a selective distribution of pulsar acceleration. A discontinuous change (option I) would be expected to produce significant effects in stellar phenomena. These effects have not been observed. The only option remaining for a change in the speed of light is II. But is this possible, and what are the implications?

Implications of Pulsar Timing

A decay in the speed of light that is consistent with pulsar timing measurements requires the following proportions:

$$P(T) \propto 1/c(T), \quad (13)$$

and

$$\omega(T) \propto c(T) \quad (14)$$

where $P(T)$ is the pulse period (equivalent to a pulsar day) and $\omega(T)$ is the angular velocity of the pulsar. The pulse period would be 0.001 of its present value in dynamic time if the speed of light were 1,000 times faster, etc.

This relationship between the length of day, or period, and the speed of light produces the same inconsistency noted earlier in the Norman-Setterfield model. If the period of rotating objects, the earth or pulsars, varied with $1/c(T)$ to conform to pulsar timing measurements the length of a day in the past would have been absurdly short and the number of days in a year would have decreased enormously. If the period of rotating objects is independent of $c(T)$, pulsar timing measurements show the speed of light is a constant to better than 10^{-10} . The Norman-Setterfield model appears to have little, if any, relevance to the real world.

Conclusion

To assume that the Norman-Setterfield model of the decay of the speed of light is correct requires the negation of history or a violation of the first law of thermodynamics. If one assumes the first law does not hold, pulsar timing measurements indicate the speed of light has been a constant to 10^{-10} since the time light left the pulsars (the pulsars appear to be 30,000 light years and farther away). If one assumes the first law holds, 1,000 years ago a day would only be 12 dynamic hours and there would be 730.5 days in a dynamic year—an historical absurdity. In addition the Norman-Setterfield model gives pulsar $\dot{P}$ values that are six to 15 orders of magnitude different from what is observed. See Table I. These theoretical implications show the Norman-Setterfield model to be an inaccurate description of reality.

Table I. Norman-Setterfield Model Implications for Rotating Objects and the Length of Earth's Day.

Angular velocity	Norman-Setterfield model	Implications
$\omega = \text{Constant}$	Rotational KE $\propto 1/c(T)$ (Violation of first law of thermodynamics if $c(T)$ varies.) Predicted pulsar $\dot{P}$ is 10^6 to 10^{15} larger than observed.	Speed of light is constant to 10^{-10} according to pulsar measurements. Predicted pulsar $\dot{P}$ is 10^6 to 10^{15} in error. Day length is constant.
$\omega \propto c(T)$	Rotational KE conserved Predicted pulsar $\dot{P}$ is $> 10^6$ to 10^{15} larger than observed.	Day length $\propto 1/c(T)$ (Earth's day was 12 hrs., 1000 yrs. ago—historically absurd.) Predicted pulsar $\dot{P}$ is $> 10^6$ to 10^{15} in error.

The consistency of pulsar signals provides unique constraints on the hypothesis that the speed of light has decayed in the past. The testing times in the past can be greatly extended beyond the time of Roemer's measurements in 1675 by accurate timing measurements of pulsar signals. The constraints implied by pulsar signals will need to be addressed in any serious model that proposes a decay in the speed of light.

One could imagine everything changing with the speed of light in such a way that it would not be detectable, but there would be no way to prove this type of a decay. Such a model would only be speculation. If something cannot be detected and measured, it is not subject to the scientific method.

At this point pulsar data and theory strongly suggest that if the speed of light has decayed, it has done so in a manner that is very obscure. If a decay in the speed of light is the correct explanation for how we can see distant stars in a short creation time frame, perhaps further research will provide a workable model for a decay and identify a test that can verify or falsify such a model.

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THE SPECIAL THEORY OF RELATIVITY: ITS ASSUMPTIONS AND IMPLICATIONS

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Abstract

Since Albert Einstein proposed the Special Theory of Relativity in 1905, there has been much discussion, concern, and confusion. This theory is probably the most controversial concept within physics and has been the subject of no little controversy in the origins debate. It is not the intent of this paper to defend or refute the theory, rather to clarify what is and is not assumed and what is and is not implied by it. Thus, hopefully this will reduce the confusion and perhaps some of the unprofitable element of the controversy.

Introduction

There are two basic assumptions underlying Einstein's Special Theory of Relativity (STR): the speed of light in a vacuum appears the same to every observer regardless of their motion and the laws of physics appear the same in every inertial reference frame. To Einstein, the first assumption implied the absence of an ether and any physically measurable absolute reference frame. The second assumption does not per se prescribe what the laws of physics are, just that they be consistent to different observers. Once these two assumptions are made, one can, using calculus, derive the Lorentz contraction, time dilation, change in apparent mass, and the famous energy relationship. It is these derived relationships, and in some cases their misinterpretation, which give rise to the controversies surrounding the STR.

The Unstated Assumptions

Einstein made at least two unstated assumptions in developing the STR: orderliness and causality. Without the assumption of orderliness, there is no point in pursuing the study of physics. If phenomena do not occur in a regular manner, then experiments would not be repeatable and it would be absurd to attempt to apply logic and mathematics to increase our understanding of nature. Even if it is incorrect, the STR, if nothing else, is an attempt to develop mathematical expressions for certain relationships between causes and effects. The notion that the STR somehow assumes or even proves that the cosmos is chaotic and thus cannot be understood is false. On the contrary, in developing the STR, Einstein assumed the very opposite to be the case: an orderly, causal cosmos which could be understood by means of logic mathematics.

The Ether Question

The concept of a ubiquitous ether was the subject of much debate near the end of the nineteenth century. Today it is thought by most physicists to have been as thoroughly discredited by Michelson and Morley as the

concept of a flat Earth was discredited by Columbus and Magellan. This, however, is not the case. What Michelson and Morley did obtain was a null result for the motion of an ether with respect to the Earth. What they did not obtain was any result enabling one to distinguish between the at least three remaining logical alternatives: (1) there is no ether, (2) the ether moves with or is attached to the Earth, or (3) the ether, much like a viscous fluid, attaches to whatever body it contacts and is thus entrained or "dragged along" with the Earth.

Many physicists consider the annually varying aberration of fixed stars perpendicular to the Earth's orbit which was reported by Bradley in 1728 (Michelson p. 121) to be evidence that the ether is not entrained by or "dragged along" with the Earth; thus eliminating alternative (3) but not necessarily (2). Michelson investigated a number of tests related to Bradley's aberration beginning with Airy's problem in which the telescope was filled with water rather than air. Airy reasoned that if Bradley's aberration was to be explained by the motion of the telescope relative to the stellar light source as the light traveled through the telescope, then the magnitude of the aberration should depend on the refractive index of media inside the telescope. However, this experiment yielded a negative result which was subsequently explained by Fresnel, "that the luminiferous medium is carried along by the motion of the medium; not, however, by the full amount of this motion, but by a fraction . . ." (Michelson, p. 139). Michelson performed several tests of Fresnel's hypothesis. Michelson, however, did not consider this a satisfying proof as he continued his search for such an experimental test. Had Airy's test been positive without resorting to Fresnel's explanation and thus requiring yet another null hypothesis to be experimentally proven, Bradley's aberration would have been a much stronger test against alternative (3). That Michelson was not convinced by the positive results of Bradley's observation and subsequent negative results obtained by related experiments is illustrated by,

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It must be admitted, however, that these experiments are not sufficiently conclusive to justify the hypothesis of an ether which is entrained with the earth in its motion. But then how can the negative results be explained? (Michelson, p. 155.)

Michelson specifically stated in several places (e.g. pp. 162, 164, 166) that all three alternatives were equally supported by his experiments. What is rarely mentioned is why alternative (2) was rejected by most physicists: because it implies that the Earth has a very distinctive place within the cosmos. This latter notion is inseparably linked to the question of origins. Thus, to preclude or assert it from the outset amounts to an additional presupposition, which eventually may be called a logical foul involving circular reasoning if subsequent developments are used to discredit or confirm the notion.

It is quite possible that no optical experiment can be devised by which a clear determination can be made between the three alternatives. Michelson was unable to devise one, although he devoted many years to this pursuit. The space program may eventually provide an opportunity to test alternative (2). Thus, at this time the question as to the existence of an ether has not been conclusively answered. Moreover, these questions may not ever be answered by direct measurements involving light. To speak as if this were a closed case without the elusive experimental test of the hypothesis is presumptuous.

An Exchange Of Absolutes

The invariance of the speed of light in the absence of an ether has not been unquestionably established. On the other hand, it has not been unquestionably refuted either. Therefore, one may stipulate this as an assumption and continue in order to see if any useful developments follow—all the while being mindful that any subsequent developments rest upon this assumption. It is important to note that Einstein assumed that the speed of light was a universal physical absolute. Because of the STR corollary that there is no absolute frame of reference, it is often implied—if not directly stated—that the STR somehow assumes or even proves that there are no absolutes of any kind. Actually, the STR does not eliminate or deny absolute references, it exchanges them: an absolute frame for an absolute velocity. Einstein argued that the latter was to be preferred over the former, not that all absolute references were to be rejected.

Interestingly enough, this misconception about the STR and absolutes is sometimes extended from the physical to the metaphysical, so that it might be said by some, “even as Einstein proved that there is no absolute physical reference frame, so there is no absolute reference for morals.” Whether or not this thought entered into Einstein’s head, it is not consistent with the assumptions or the results. Arguably, the implications of the STR could be stated, “one cannot completely trust physical measurements without first comparing these to the standard: the speed of light.” If any extension of the STR were to be made from the physical to the metaphysical it should be more accurately stated, “one cannot completely trust an intuitive measure of the morality of a thing without first comparing this to the standard: the character of God.”

An Exchange of Principles

Next let us consider which principles Einstein preserved and which he forfeited in developing the STR. It is important to note that the second assumption stated the consistency, not the form, of the laws of physics from one inertial reference frame to another. Whether Einstein carefully weighed each principle and decided which to keep and which to reject, the result is the following: the STR preserves the conservation of momentum and energy, and forfeits the conservation of mass and the unbiased measurement of mass, length, and time. This amounts to an exchange, not a denial, of principles: spatial objectivity is exchanged for the invariance of the speed of light—to many a disadvantageous, even inconceivable exchange. It is at this point where many part company with Albert Einstein and his theory. Again, let me stipulate this bewildering exchange in order to proceed and see if any useful developments follow.

The Old and the New Physics

It can hardly be overstated that the STR revolutionized modern physics; but at the same time the STR is built upon the foundation of classical physics. The challenge of the STR is to revolutionize without destroying and thus undermining itself. Recall that the STR preserves the conservation of momentum and energy while forfeiting the conservation of mass and the unbiased measurement of mass, length and time. This immediately gives rise to the question that if classical measurement of mass, length and time were used to infer the principles of the conservation of momentum and energy from phenomena as being manifestations of nature, the forfeit of any one over another is self-contradictory. Were it not for the asymptotic behavior of the resulting relationships, such that at even seemingly great speeds, spatial objectivity holds, the STR would be hopelessly contra-logical; and logic cannot be forfeited or exchanged in such matters—as is the attempt with dialectics—for from what vantage point could one argue the logic of this exchange?

The Apparent vs. the Actual

Perhaps the most common misconception regarding the STR is that it implies that mass, length and time changes are a function of velocity. This is a misstatement. The STR implies that mass, length and time *APPEAR* to change with velocity. What the STR claims to provide is a set of mathematical formulae which relate mass, length and time as they appear to one observer to that which appears to another—the unifying factor being Einstein’s universal physical absolute: the speed of light. Recall that the second assumption upon which these relational formulae are based is that the laws of physics (viz. the conservation of momentum and energy) are consistent in every inertial reference frame.

The STR claims to describe the relationships between various measurements biased by velocity. The STR assumes that there is only one spatial absolute: the speed of light. What the STR cannot provide is any relationship between the apparent and the actual—if such a thing exists. Furthermore, Einstein concluded that there must not be any spatial actuality—that there is no absolute reference frame. This, of course, is

conjecture. Just because the STR is unable to provide any further relationships beyond the apparent, and even if the stipulated assumptions are correct, inability to relate does not imply nonexistence. The conclusion of the STR with respect to the actual could be stated: if there is an absolute reference frame, it cannot be found by means of physical measurements; and if one were within that frame, there would be no means of recognizing it as such.

Circular Inconclusiveness

The theoretical inability of the STR to distinguish between the absolute reference frame—if such exists—and any other inertial reference frame closes back on the question of an ether. Recall that Michelson admitted that his experiments equally supported alternatives (1) or (2). Therefore, if an absolute reference frame does exist and we are indeed within it, neither Michelson nor Einstein can offer any evidence to the contrary.

There is still the question that if there is no means of determining whether or not we are in the absolute reference frame or whether such a thing exists, does it matter? Whether or not something like this “matters” is a philosophical question and cannot be answered by physics. Philosophically, whether or not this is so matters greatly as to origins. Philosophically then, as stated previously, if there is an absolute reference frame and we are indeed within it, then the Earth has a very distinctive place in the cosmos.

What Then of the Theory?

There are arguable assumptions underlying the STR; and there is the exchange of absolutes; and there is the exchange of principles; but is this not the case with other theories—granted mostly to a lesser degree? If one is willing to do so, the STR can be evaluated on the basis of pragmatic contribution. Are there any phenomena which can be explained by the STR? The answer to this question is: yes. For instance, the STR explains how one can raise the energy of an electron above 0.51 meV. Does the STR explain all particle phenomena? The answer to this question is: no. For instance, the STR is incapable of explaining the existence of behavior of quanta. Are there any phenomena which cannot be explained by any theory other than the

STR? The answer to this question must be: we do not yet know. Alternate explanations of phenomena have been presented. See Barnes, 1984, for a recent bibliography and Slusher and Ramirez, 1984. What then of the STR? It does provide some useful results; and could be used with caution.

Conclusions

It has been presented that Einstein in developing the STR assumed both orderliness and causality. Thus, the STR cannot logically be used to refute either of these concepts. It has been presented that the STR rests upon an assumption that has neither been conclusively proven nor disproven. Thus, logically the STR is no stronger than its weakest assumption. It has been presented that the STR exchanges rather than denies absolutes. Thus, the STR cannot logically be used to argue against absolutes. It has been presented that the STR retains some principles, includes an additional one, and forfeits others. Thus, the STR must walk a logical tightrope in order to revolutionize without destroying its foundation. It has been presented that the STR only relates apparent quantities, but does not relate or even address actual quantities. It has been shown that the STR involves circular inconclusiveness. Thus, the STR predicts of itself that it cannot be conclusively confirmed or refuted using physical measurements. Also, it has been suggested that if there is pragmatic contribution of the STR, then it could be used with caution. Finally, it is suggested that, since the STR does assume orderliness, causality, and absolutes; it does preserve some principles; it relates only apparent quantities and it has some utility. It is incapable of addressing ultimate questions; and is therefore, in its basic assumptions, not necessarily supportive of either side in the origins debate.

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STATISTICAL ANALYSIS OF THE ATOMIC CONSTANTS, LIGHT AND TIME

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Abstract

A statistical analysis of the data presented in The Atomic Constants, Light and Time indicates that a significant change in the velocity of light has not occurred in recent years.

Introduction

The Atomic Constants, Light and Time represents an immense amount of literature search, mathematical analysis, and creative thought. The authors are to be commended for their contributions in each of these areas. The scientific community is indebted to them for their compilations of data and literature references. Many readers will be stimulated by the provocative views on cosmology presented in this report.

After reading the Foreword by Lambert T. Dolphin and the first few pages of introduction by the authors, I looked forward to finding, in succeeding pages, clear and statistically well-supported evidence for a continuing decrease in the propagation speed of electromagnetic radiation. My subsequent disappointment provides the motivation for preparing this review.

After writing the following paragraphs I read the supplement to *The Atomic Constants, Light and Time*, entitled "Geological Time and Scriptural Chronology" and written by Barry Setterfield. In this supplement it is apparent that Mr. Setterfield is motivated to produce a model in which the physical universe has been in existence for less than 10,000 years. Efforts to harmonize the data obtained from scientific investigation with the data contained in the Bible are commendable, since the testimony of a competent and dependable eyewitness provides a better basis for reconstructing a singular event than does only analysis of after-the-event consequences. But when an interpretation of testimony given by an eyewitness, by an external observer, (whether direct or through an intermediary) requires unreasonable or highly questionable interpretation of the after-the-event consequences, there is need for careful investigation of the possibility that the eyewitness, external-observer testimony has been misinterpreted. There is danger that some discerning minds will incorrectly conclude that the authority cited for eyewitness, external-observer testimony is neither competent nor dependable. These considerations have provided additional motivation for sharing this review.

Difficulties

My first difficulty came from an attempt to evaluate the two empirical equations for the speed of light given on p. 7. Not until p. 55 did I discover that t in the first is calendar years AD, t in the second is 1961 minus calendar years AD, and both yield reasonable values only over the relatively narrow range of time associated with the experimental determinations that have been made.

My second difficulty, which was magnified throughout subsequent pages, came on p. 8 where six determinations of the speed of light were claimed to provide 98.2% confidence for rejecting the hypothesis that the speed of light was constant from 1879.5 to 1926.5. The mean and standard error for these six determinations are 299,845.5 and 41.5 km/s. According to a Student's t -distribution, 5% of such data should lie beyond 83.5 units away from the mean either above or below. The 1926.5 value of 299,798 km/s is 47.5 units below the mean, only 57% of the distance to the 5% point; and the present value is 53.0 units below the mean, only 63% of the distance to the 5% point. It is true that these six measurements declined monotonously during the time interval over which they were made, but the maximum difference is only 112 and the observers claimed standard errors ranging from ± 15 to ± 90 km/s for their reported means. How can one claim 98.2% confidence that this set of data represents measurement of a steadily decreasing quantity, rather than the influence of subjective factors and normal error of measurement on the determination of a constant quantity?

On p. 9 a statement made in 1931 is quoted which affirms that new values for the speed of light are "invariably . . . lower than the last one obtained." In the *selected* listing on p. 26, 21 examples out of 56 succeeding measurements are greater than the preceding, five of which were made prior to 1931. On p. 11 the authors point out that Aslakson's determination of c in 1949 was *higher* than all measurements made between 1928 and 1940 (5 as listed on p. 26).

Statistical Analysis

Citing the Roemer-type (observational delays in the eclipse of Jupiter's satellite Io) determinations of c as tabulated on p. 12, the authors state (p. 11) "On this basis, the hypothesis that c has been constant . . . can be rejected at the 96.5% confidence interval." Using the following symbols

M = mean speed of light in km/s

S = square root of the variance, in km/s

N = number of samples in the data set

$T(.05)$ = T -value beyond which 5% of the data may be expected to lie, km/s from the mean; table $t(.05)$ value multiplied by S

D = difference between the mean and the most recently accepted value

the six high precision values (omitting the 1675 and 1693 "estimates") from Table 1, p. 12, may be summarized:

$M = 301,494$; $S = 1,697$; $N = 6$; $T(.05) = 3,419$; $D = +1,702$.

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One of these data is an increase over its predecessor, five are decreases, but the total characteristics of the set hardly seem to give 96.5% confidence for rejecting the hypothesis that c has been essentially constant from 1738 to 1983, since the significance of $T(.05)$ is that in a large number of similar sets of data 5% of the values may be expected to be more than $T(.05)$ units remote from their respective mean either above or below. $T(.05)$ as defined above and used throughout this review differs from the usage on which Norman and Setterfield's conclusions are based. They divide by $\text{SQRT}(N)$ to obtain a predictor for mean values. My objective is to evaluate their conclusions from the perspective of a broad view of each data set they use.

Data from Bradley-type (star aberration angle) determinations are claimed to provide 93.9% confidence for rejection of the hypothesis that c has been constant from 1740 to 1983 (p. 13). For the tabulation given on p. 14,

$M = 299,869$; $S = 386$; $N = 63$; $T(.05) = 635$; $D = +77$.

For the Pulkova observatory values only,

$M = 299,776$; $S = 282$; $N = 13$; $T(.05) = 503$; $D = -16$.

Figure 1 on p. 15 lists as Pulkova items two which are not so designated in Table 3 on p. 14. Is the error in Table 3 or in Figure 1?

Splitting the data of Table 3 into three segments of 50 year duration yields the following:

Date	M	S	N	T(.05)	D
1765 $\pm$ 25	300,555	134	2	846	763
1865 $\pm$ 25	299,942.5	424	16	743	150
1915 $\pm$ 25	299,812	348	45	585	20

Values greater than the two in the 1765 group are found in the 1865 and the 1915 groups. In my judgment, these data provide a stronger basis for rejecting the hypothesis that c has declined significantly from 1740 to 1983, than for rejecting the hypothesis that c has been essentially constant throughout this interval.

The 14 values for c as determined by the toothed-wheel method (Fizeau) tabulated on p. 16 yield the following summary analyses.

Selection	M	S	N	T(.05)	D
A	299,974	697	10	1,278	182
B	299,818	522	9	971	26
C	299,910	48	5	102	118
D	299,885	19	4	45	93

- A — Precision values 1872-1902.4 (1849.5-1855 omitted)
- B — Same as for A, but low precision 1880 value omitted
- C — Values considered by the authors as most reliable
- D — 1900.4-1902.4 highest precision values

The mean of selection D is 15 km/s below the value obtained in the final evaluation of Cornu's 1874.8 determinations, 1/3 the range to the 5% region of the D set. Furthermore, a standard error of ± 200 was assigned to the "Cornu" value of 299,900. Yet the authors claim "a confidence interval of 99.4% that c was not

constant at the current value during these experiments" (p. 17).

The nine values tabulated on p. 18 for c as determined by the rotating mirror (Foucault) method provide the following summary analyses.

Selection	M	S	N	T(.05)	D
A	299,661	632	9	1,176	-131
B	299,833	50	6	101	+41
C	299,845	46	5	98	+53

A — All values

B — Values considered by the authors as most reliable

C — Same as for B, but omitting 1932.5 short baseline determination (has lowest standard error among the B group and $D = -18$), as suggested by the authors

The authors claim significance for decreased values for c obtained in succeeding years with the same rotating mirror equipment [p. 19]. One example is an 1882.8 determination 57 km/s lower than an 1879.5 determination. The standard error reported for these measurements is ± 60 and ± 50 km/s, respectively. The other example is a 4 km/s lower value in 1926.5 than was obtained in 1924.6. The respective reported standard error values are ± 15 and ± 30 .

The authors state that "a decay is still apparent" (p. 19) in determinations made 1928-1940 using a Kerr Cell shutter. The data they quote on p. 20 yield

$M = 299,776$; $S = 7.07$; $N = 4$; $T(.05) = 16.64$; $D = -16$.

These data might be taken to provide over 90% confidence for a hypothesis that c increased between 1935 and 1983.

Unquestioned measurements of c made between 1947 and 1960 by cavity resonator, radar, geodimeter, radio interferometer, spectral line, and tellurometer techniques are tabulated on p. 21. These data yield the following summary analyses.

Selection	M	S	N	T(.05)	D
A	299,793.18	1.46	23	2.51	0.72
B	299,792.96	1.04	22	1.79	0.50

A — All values

B — Omitting 1947 preliminary 299,798 value

In both selections A and B only two of the items fall outside the 90% confidence range specified by $\pm T(.05)$, as predicted by statistical theory, but I cannot see in the positive values for D "a confidence level of 99.0% in the data showing c as higher than now during those 15 [14] years" (p. 19).

Determinations made from 1966 to 1983, mainly with lasers, are tabulated on p. 20, and may be summarized as

$M = 299,792.470$; $S = 0.033$; $N = 11$; $T(.05) = 0.060$; $D = 0.011$.

Regarding these measurements, the authors state:

The only conclusion to be drawn from these figures . . . is that any decay during this period would have occurred at a very low rate, perhaps may have ceased altogether, or c may have begun to increase at some time in this period (p. 22).

The speed of light may be determined from the ratio of electrostatic units to electromagnetic units for electric charge. Values obtained in this manner from 1856 to 1906 are tabulated on p. 24. These values yield the following summary analyses.

Selection	M	S	N	T(.05)	D
A	297,340	5,311	23	9,119	-2,452
B	297,383	5,432	22	9,348	-2,409
C	299,982	561	8	1,063	+190

A — Omitting the imprecise early determinations in 1856 and 1868

B — Omitting in addition an imprecise determination in 1883

C — Values considered most accurate

In group C, determinations made in 1890 are lower than those made in 1891 and 1897. Of the seven succeeding values in the series of eight, four are lower and three are higher than the immediate predecessor. With D only 18% of T(.05), there is little basis for confidence that these data “exhibit a decay trend” (p. 25).

Measurement of standing electromagnetic wave patterns on wires may yield values for c . A tabulation of determinations made in this way from 1891 to 1923 is given on p. 24. For these data

$M = 299,716$; $S = 1,626$; $N = 6$; $T(.05) = 3,276$; $D = -76$.

A refined summary of all data on the speed of light is tabulated on p. 26. This tabulation yields the following summary analyses.

Selection	M	S	N	T(.05)	D
A	299,845	150	57	247	53
B	299,803	32	50	52	11
C	299,791.2	6.5	41	10.9	-1.3
D	300,178	302	6	609	386
E	299,865	43	10	79	73
F	299,783	13	7	25	-9
G	299,793.15	1.44	24	2.47	0.69
H	299,792.473	0.033	10	0.060	0.014

A — All data from Table 11

B — Determinations since 1879 (Michelson)

C — Determinations since 1921 (Michelson higher precision)

D — 1740-1875

E — 1876-1923

F — 1924-1940

G — 1941-1966

H — 1967-1983

For all selections in these analyses the US NBS 1983 value is less than T(.05) away from the mean—less than about 1/3 of T(.05), except for selections D and E.

The fact that $D > 0$ in most of the preceding examples appears to support the hypothesis that c has been decreasing. For a generalized series of numbers $a(b + id)$, $i = 1, 2, 3, \dots, n$, scale factor a , displacement factor b , and increment factor d , the average value is $a\{b + [(n + 1)/2]d\}$, and the standard deviation is given by the expression $ad\{\text{SQRT}[(n^2 - 1)/12]\}$. The difference between an extreme value and the mean is $ad[(n - 1)/2]$. The ratio of this difference to the

standard deviation is $\text{SQRT}[3(n - 1)/(n + 1)]$. This expression ranges from 1.00 for one degree of freedom ($n = 2$) to 1.73 for infinite degrees of freedom ($n = \infty$). Multiplying by $t(.16)$ for a test value of greater statistical significance, the range goes from 1.4 for one degree of freedom to 1.7 for infinite degrees of freedom. [For infinite degrees of freedom $t(.16)$ is 1.00]. Practical measurements of a steadily changing quantity may be expected to yield D/S values that scatter above and below these values, but the average D/S for a large number of sample sets should be in the vicinity of 1.4-1.7 if the data represent a quantity that has changed continuously and significantly throughout the time over which measurements were made. If D is taken for a more remote point in time than that of the limiting value for the range over which the standard deviation is computed, the D/S ratio average for a large number of sample sets will be greater than 1.4-1.7.

In only six of the cases discussed above (paragraph five, laser data, and selections A, B, C, and H from the authors' refined summary in Table 11, p. 26) was D computed against the most recently determined member of the set. In all other cases it was computed against the US NBS 1983 value. For the six exceptional cases $D/S = 0.46$ with a standard error of ± 0.34 . Eliminating the 1.14 ratio for the paragraph five data, $D/S = 0.33 \pm 0.08$. For the 18 cases in which D was computed against the remote 1983 value the range of D/S is from 0.050 to 5.69, with a mean 1.35 ± 1.60 . Eliminating the 5.69 value for the $N = 2$ data in the 1765 $\pm$ 25 years range for the Bradley aberration method, the range of D/S is from 0.050 to 4.89, with a mean 1.10 ± 1.21 . Among these 17 cases only four have D/S greater than 1.3 — 4.89 for the Fizeau method data 1900.4-1902.4, with $N = 4$; 2.46 for the Fizeau method data selected by Norman and Setterfield as most reliable; 2.63 for the $N = 4$ Kerr Cell data, the mean of which is 16 km/s less than the 1983 value; and 1.70 for the 1876-1923 range of the summary data (Table 11, p. 26). These observations provide strong evidence that within the precision and accuracy of measurements made up to the middle of this century the speed of light has been essentially constant.

Norman and Setterfield base their confidence in the hypothesis that the speed of light has been monotonously declining over the last three centuries on regression analysis and statistical comparison of averages for small samples of data. Should success in fitting sets of data against time of acquisition, or subsequent reporting, to a line with negative slope override the basic statistical considerations on which the preceding analysis in this review has been developed? Investigation of physical constants which are related to the speed of light may provide some clarification. The last half of the monograph is devoted to such investigation.

From data tabulated on p. 30 the authors conclude that there is no experimental evidence for variation in the electrical charge carried by an electron. The data were collected between 1913 and 1973. Following the analysis pattern I have used for speed of light data, and using the 1973 determination as the reference for calculating D, the following summary analyses were obtained for the 37 items of Table 13, omitting three “pioneer results” considered of doubtful accuracy.

Selection	M	S	N	T(.05)	D	D/S
A	4.8042	0.0080	34	0.0135	0.0010	0.12
OD	4.8101	0.0170	7	0.0330	0.0069	0.41
OM	4.8036	(0.0048)	1	-----	0.0004	----
XR	4.8019	0.0007	4	0.0016	-0.0013	1.86
XM	4.8024	0.0013	7	0.0025	-0.0008	0.61

A — All 34 items
 OD — Oil drop method
 OM — Oil drop mean
 XR — X-ray method
 XM — X-ray mean

On the basis that the electron charge has been constant, the authors claim 99.17% confidence for the hypothesis that the product mc , electron mass times the speed of light, has varied from 1910 to 1973. Using the 1973 value for calculating D, the following analysis was obtained for the data in Table 14, p. 32, for e/mc .

Selection	M	S	N	T(.05)	D	D/S
A	1.7600	0.0027	33	0.0046	0.0012	0.44
CF	1.7613	0.0052	4	0.0122	0.0025	0.48
FS	1.7604	0.0033	8	0.0063	0.0016	0.48
ZE	1.7591	0.0020	4	0.0047	0.0003	0.15
DV	1.7600	0.0014	2	0.0088	0.0012	0.86
MD	1.7593	0.0005	3	0.0015	0.0005	1.00
XR	1.76006	(0.0004)	1	-----	0.0012	----

A — All data
 CF — Crossed field technique
 FS — Fine structure technique
 ZE — Zeeman effect
 DV — Direct velocity measurement
 MD — Magnetic deflection
 XR — X-ray refraction

On the basis of the D/S ratio data, I see no basis for confidence in a hypothesis that mc has been time dependent.

Constants Variable?

Planck's constant h is expected to increase with time if c decreases. For the data on h/e in Table 15A, p. 34, with D calculated on the latest (1973) value

M = 1.37724; S = 0.00238; N = 28; T(.05) = 0.00405;
 D = -0.0023; D/S = 0.97.

The authors state that "the data . . . does not negate the proposition" (p. 33). Although $D < 0$, with the 52 year time interval involved and $D/S = 0.97$ for $N = 28$, I would not expect $D = -0.0023$ to support the proposition any better than $D = +0.0023$.

Measurements of $2e/h$ from the a.c. Josephson effect are considered to give 96.2% confidence that h was not constant, assuming e to be constant (p. 35). Using the 1972.38 value for determining D, the following analyses were obtained for the composite data and for individual laboratories involved (Table 15B, p. 34). The time range for these data is *only two years*.

Selection	M	S	N	T(.05)	D	D/S
A	483593.798	0.235	11	0.426	0.192	0.82
NBS	483593.584	0.137	3	0.400	-0.022	0.16
NPL	483594.117	0.104	3	0.304	0.510	4.90
NSL	483593.791	0.054	3	0.156	0.185	3.43
PTB	483593.653	0.066	2	0.417	0.047	0.71

There is no justification from these data for a time dependency of h .

The quantized Hall resistance, h/e^2 , provides another approach to determining if h has been time dependent. The authors claim 92.9% confidence that h/e^2 has not been constant (p. 35). The data on which they base this conclusion is given in Table 15C, p. 36. Using the 1985 value for reference, the data collected over five years yield

M = 25812.8257; S = 0.0335; N = 6; T(.05) = 0.0675;
 D = -0.0212; D/S = 0.63.

All the data in this set fall within the 80% confidence range [T(.10) = 0.0494]. Of the four values which have an order of magnitude higher precision than the other two, the highest was determined in 1984.5, the second highest in 1983.5, the third highest in 1985.0, and the lowest in 1984.0. This pattern, together with the associated statistical analysis, provides no basis for asserting that h increased significantly over the five years of data collection.

Determinations of the Rydberg constant made between 1890 and 1981 are listed in Table 16 on p. 36. For these data

M = 109,737.310; S = 0.071; N = 24; T(.05) = 0.122;
 D = -0.005; D/S = 0.07

and all but one datum is within the 90% confidence range [T(.10) = 0.094]. I agree with the authors that "This strongly suggests that the Rydberg constant has not varied" (p. 37).

The authors claim 99.9% confidence that the gyromagnetic ratio of the proton has changed over the time interval 1949-1981. Their data list in Table 17, p. 38, yields

M = 26751.779; S = 0.803; N = 30; T(.05) = 1.364;
 D = 0.551; D/S = 0.69.

The 1981b value is only 40% of the T(.05) distance from the mean, and the 1962b, 1962c, 1966, and 1978 values are each lower than the 1981b value. Again I do not see evidence for a significant change over time.

Radioisotope Half-Lives

In harmony with their line of thinking, the authors take the position that radioisotope half-lives have increased over time. They tabulate data which they claim "support the contention of increasing half-lives by an almost two-thirds majority . . . The most pessimistic conclusion is that they do not invalidate the proposal" (p.41). Their Table 19 (p. 40) lists half-life values for 35 radioisotopes, as published in 1904, 1913, 1930, 1936, 1944, 1950, 1958, 1966, and 1978. Unfortunately, many of these values are only restatements of previously published values, and do not represent an additional independent determination. To reduce the hazard of using imprecise or inaccurate values which were determined before techniques and instrumentation were well developed, my analysis covers only the values published in 1930 and afterward. This selection eliminates consideration of Pb-214, Po-218, Ra-224, and Ac-228, for which the same half-life value is repeated from 1930 to 1978. Using the 1978 value for reference, the following analyses were obtained for the significant figures of the half-lives.

Isotope	M	S	N	T(.05)	D	D/S
Tl-207	4.754	0.032	7	0.062	-0.016	0.5
Tl-208	3.108	0.044	7	0.085	+0.055	1.25
Tl-210	1.314	0.010	7	0.019	+0.014	1.40
Pb-210	20.67	2.29	7	4.45	-1.63	0.71
Pb-211	36.07	0.04	7	0.08	-0.03	0.75
Pb-212	10.62	0.02	7	0.04	-0.02	1.00
Bi-210	4.97	0.06	7	0.12	-0.04	0.67
Bi-211	2.156	0.005	7	0.010	+0.006	1.20
Bi-212	60.57	0.11	7	0.21	-0.030	0.27
Bi-214	19.73	0.076	7	0.15	+0.03	0.39
Po-210	138.51	1.61	7	3.13	+0.13	0.08
Po-216	0.152	0.0075	7	0.015	+0.002	0.27
Rn-219	3.937	0.031	7	0.060	-0.023	0.74
Rn-220	54.73	0.43	7	0.84	-0.87	2.02
Rn-222	3.8235	0.0018	7	0.0035	0.000	0.00
Ra-223	11.34	0.19	7	0.37	-0.095	0.50
Ra-226	1603	17	7	33	+3	0.18
Ra-228	6.43	0.46	7	0.89	+0.67	1.46
Ac-227	21.11	0.86	6	1.73	-0.66	0.77
Th-227	18.72	0.27	7	0.52	+0.002	0.01
Th-228	1.905	0.006	7	0.012	-0.008	1.33
Th-230	7.97	0.29	7	0.56	-0.03	0.10
Th-231	24.99	0.52	7	1.01	-0.53	1.02
Th-232	1.47	0.12	7	0.23	+0.06	0.50
Th-234	24.17	0.25	7	0.49	+0.07	0.28
Pa-231	2.69	1.00	7	1.94	-0.59	0.59
Pa-234	6.70	0.03	5	0.06	-0.05	1.67
Pa-234m	1.156	0.018	7	0.035	-0.019	1.06
U-234	2.49	0.12	5	0.26	+0.04	0.33
U-235	7.08	0.035	5	0.075	+0.042	0.20
U-238	4.493	0.049	7	0.095	+0.025	0.51

There are 16 negative D, 14 positive D, and one zero D. For only one isotope, Rn-220, is D greater than the T(.05) value—0.87 versus 0.84. Since there seem to be only three independent values in the 1930-1978 citations for Rn-220, a correct value for T(.05) would be larger than 0.84. The apparent conclusion is that these data provide no evidence for a significant change in nuclear half-lives over the last 50 years.

Gravitational Constant

The authors and I are in agreement with our conclusions regarding the Newtonian gravitational constant—"invariant" (p. 44). From their Table 20, p. 42, covering data from 1798 to 1981 one can derive the following analyses, with D computed against the 1981 value.

Selection	M	S	N	T(.05)	D	D/S
A	6.657	0.053	25	0.091	-0.016	0.30
B	6.653	0.050	24	0.086	-0.020	0.40
C	6.674	0.027	20	0.047	+0.001	0.04

A — All values

B — Uncertain 1798 determination deleted

C — Low precision items deleted

Conclusion

The authors may be convinced that "The above data presentation indicates strongly that both light speed and atomic processes, including atomic time, are undergoing a uniform decay process" (p. 51) and I am certain that many of their readers will agree. To both those who agree with the authors and those who intuitively question their thesis and mode of defense, I recommend a thorough consideration of the analysis in this review.

ARTICLE REVIEW

Hot gas in the universe by Roger A. Chevalier and Craig L. Sarazin. 1987. *American Scientist*. 75:609-18.
Reviewed by Paul M. Steidl*

This article, by two experts in the field of interstellar and intergalactic matter, is a review of hot gas in the universe, from supernovae to superclusters. At each level they describe the hot gas observed and attempt to account for its properties. Their basic premise is that hot gas can be accounted for by matter ejected from supernovae on a small scale, and galactic infall on a large scale. Hot gas in this context refers to highly ionized gas at millions of degrees.

Their discussion of the causes of supernovae is of necessity highly theoretical, but the description of supernova ejecta was well-founded in observation. Highly ionized elements have been observed spectroscopically in both Type I and Type II supernovae. The problem occurs when they say that the even distribution of heavy elements in interstellar matter is a result of the mixing of supernova ejecta with the interstellar medium. This is because of their assumption that the interstellar medium started as pure hydrogen and helium, a consequence of the big bang theory. Without that assumption, neither mixing is required, nor are the large time scales over which the mixing must operate.

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Evidence for hot gas in the interstellar medium is not nearly as strong as for supernova ejecta. At present it rests primarily upon the observation of the ions C⁺³, Si⁺³ and N⁺⁴. Hot hydrogen, which must constitute the bulk of any hot gas, is not directly observable. Ionization by extragalactic radiation and by stars might account for at least some of these ions, meaning that there may not be a large amount of hot gas at all. This must be left to further observations. If it is not there, then eons of supernovae are not required to supply it. But even if it is there, the cooling time for million degree gas is 100 million years, so no continuing source is required to account for it in the creationist time frame.

Elliptical galaxies were long thought to be devoid of dust and gas. More recently it was discovered that they are strong x-ray sources, implying that they contain large amounts of very hot gas, around 10 million degrees. The amount, they say, is comparable to the amount of gas ejected by all the galaxies stars over the period of their lifetimes. If the heating had been done by supernovae, these energetic explosions would have blown the gas away from the galaxies entirely, so the heat source must lie elsewhere. One mechanism they suggest is the motions of the stars themselves, whose speeds reach 300 km/s. For a hydrogen atom, this corresponds to 10 million degrees,* so no other mechanism is required. Since the gas being added to the medium

*From setting $1/2 mv^2 = 3/2 kT$.

is the same temperature as the intracluster gas as a whole, they are in equilibrium and no time scale is implied.

Clusters of galaxies are the brightest x-ray sources known after quasars. Gas at 166 million degrees fills all the space between the galaxies in the cluster. The element distribution in the gas appears to be about the same as the sun. The total mass of the gas can be as great as all the stars in the galaxies combined. This presents a problem for the usual assumption that all interstellar gas was ejected by stars; there are not enough. They must assume that the supernova rate was very much greater in the past and that most of the stars with which the galaxies were born no longer exist. Since supernovae only occur in the most massive stars, this forces them to the conclusion that most stars were originally very massive. But where stellar distributions

are observable, primarily in our own galaxy, the vast majority of stars are at the low mass end. They also note that at the centers of many clusters is much cooler gas which they presume to be gas which has cooled and fallen inward to the center of the cluster. The problem is that there is far too little of it if this has been going on for the life of the cluster, which they take to be 10 billion years. They suggest that the rest has gone into galaxy formation, but there is no evidence for this as yet.

The study of hot gas in the universe can be very interesting in itself. However, that it all came from stellar winds or supernovae is an assumption, not an observation. Thus long periods of time are not necessarily needed to account for it. At present, far too little is really known about these phenomena to draw such conclusions.

BOOK REVIEWS

Francis Bacon's Philosophy of Science by Peter Urbach. 1987. Open Court Publishing Company. La Salle, IL. 209 pages. \$9.95.

Reviewed by Lawrence A. McGhee*

Peter Urbach authored this treatment of Francis Bacon's thought because he felt the so-called standard interpretation was wrong. That interpretation held that Bacon advocated starting with a mass of factual data which are submitted to simple rules which yield infallibly true knowledge. Urbach argues that the more accurate term for Bacon's method is hypothetico-inductive. This method starts with data, yielding middle axioms (not first principles) which are used to discover more relationships in the data, which yields new hypothetico-inductive axioms, etc.

Bacon's method is firmly based in observation and experimentation. Prior to Bacon, most science operated from either a Rational or Empiric stance. Aristotle was a leader in the Rational approach. This method begins with already formed rational explanations of phenomena. The practitioner merely bent experience to meet the rational preconception. The Empirics, on the other hand, made a few experiments and then fabricated a complete system from this cursory glance at experience.

Bacon's chief target in these "sciences" was "not the speculation itself, but the dogmatic defense of speculations and the tendency to regard them as infallible . . ." (p. 35).

Bacon called his own method "interpretation of Nature" and the common science of his day as "anticipation of Nature" which offered speculative hypotheses which fly

from the senses and particular to the most general axioms, and from these principles, *the truth of which it takes for settled and immovable*, proceeds to judgment and to the discovery of middle axioms. (p. 35).

Bacon felt that man should humble himself before the data of Creation with a mind containing as few

presuppositions as possible. In order to do this, Bacon called for the eradication of the idols of the mind. Bacon discerned five of these: 1. *The Idol of the Tribe* which supposes there is more "order and regularity in the world than the [human understanding] finds." (p. 86). 2. *The Idol of the Cave* is "where we stick to well-known and obvious effects in formulating and examining axioms, and ignore unusual and heterogeneous instances." (p. 89). 3. *The Idol of the Marketplace* leads "men to believe that their reason governs words; but it is also that words react on the understanding." (p. 92). 4. *The Idol of the Theatre* has already been discussed under the Rational and Empiric investigation. The final idol is that of 5. *Superstition*, "men observe when things hit and not when they miss; and commit to memory the one, and forget and pass over the other." (p. 99). Bacon would have us continue to look at the pattern which is not pleasing to our preconceptions.

With a mind cleansed of these Idols, Bacon suggested several positions on scientific questions of his day and ours: atomism, the existence of the vacuum, discovery of forms (the true purpose of science), astrology, alchemy, astronomy, biology and mathematics. (The last he terms an auxiliary handmaiden to science proper which must always ground itself in observation and experiment.)

Finally, we are treated to a careful examination of the role of experiment in science, both criticism and examples of its use in what Bacon called prerogative instances possessing "special rights and powers in the interpretation of nature." (p. 164). An example of instances which are specially revealing would be taking the pulse and testing the urine of a patient.

This book is scholarly and complete with an extensive bibliography of 17th century works related to Bacon with many modern studies also listed. The style is clear, focused on the college-educated population, especially those interested in history of science. The exposition is careful and full of well-chosen Baconian quotations. For those who have held the standard interpretation of Bacon's scientific philosophy, it will be a well-documented challenge.

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The Great Debate: Evolution vs. Creation (54 minute video tape) by Terry Mondy. Creative Media, 11 Circle Drive, Algonquin, IL 60102. \$49.95 + \$3.00 shipping.

Reviewed by Walter T. Brown, Jr.*

Debates on the creation/evolution issue continue to appeal to large audiences, especially students in high school and college. Such debates are often the first balanced exposure students have to this foundational topic. Unfortunately, the debaters' personalities frequently influence the audience more than they should. A debater may be labeled as insulting, arrogant, repetitious or boring. Such traits (real or imagined) sometimes become excuses for ignoring persuasive arguments for "the other side." For these reasons, debates have not communicated information as readily as one might wish.

Terry Mondy, a public high school science teacher with an M.S. from Purdue University, has for many years solved this problem in an innovative way. He has conducted a formal debate with himself. He played the roles of the moderator, "Professor Evolution" and "Doctor Creation." His students could not tell whether he was a creationist, an evolutionist or completely neutral. Nor would Mondy tell them until the academic year was over. Other teachers, evolutionists and creationists alike, would invite him to present the simulated debate to their classes. Soon Mondy was giving his "Great Debate" at conferences for science teachers—usually with standing ovations. The most frequent comment heard was "I did not realize there was scientific evidence for creation." A professionally done video tape of this debate has now been made before a live student audience.

The arguments and accusations in "The Great Debate" are typical of the more than a thousand debates that have been held in recent years. Professor Evolution protests that creation is thinly disguised religion while evolution is purely science. Dr. Creation shocks people by giving evidence for a young earth and universe. Each appeals briefly to various authorities and asks rhetorical questions. Each lays out some of the standard evidences: the peppered moth, the geologic column, the big bang, simple-to-complex fossils, experiments by Miller, Fox, and Oparin, radiometric dating, index fossils, the second law, polystrate fossils, Flood legends, moon dust, the complexity of a cell, fossil grave yards, design in nature and many others. Since the personalities of each debater are identical and only their clothes differ, the audience is much more apt to weigh the evidence. The novelty of this three-in-one approach also holds the viewer's attention, especially when "camera magic" has the two debaters and moderator on the screen at the same time.

How is this video tape best used? It is not especially appropriate for a Sunday school class, since Biblical topics are not mentioned. It would not have much appeal to a room full of scientists, since (1) the audience in the video consists of young-looking high school students and (2) the scientific technicalities are avoided. However, it would be an ideal introduction to a block of instruction on origins, especially at the

high school level. Perhaps the best use of this tape is by a parent who wants to encourage a science teacher to include some creationist evidences in the curriculum. "The Great Debate" is a simple way to make a constructive suggestion without getting into argumentative issues. Simply loan the teacher the video and ask for comments.

This video is currently being used in high schools and colleges throughout the United States. Its 54 minute length fits nicely into a one hour class. By deleting the last six minutes (the question and answer session), it could even fit into a 50 minute class period or allow time to administer a "before and after" questionnaire. This even-handed, low-keyed approach should offend no parent or school administrator. In fact, most will be pleased.

Darwinism: The Refutation of a Myth by Soren Lovtrup. Chapman Hall, New York, 1987. 469 pages, \$85.00.

Reviewed by Wilbert H. Rusch, Sr.*

The author identifies himself as a professor of embryology at the University of Umea, Sweden. Since he is approaching retirement, he considers this work a summary of his years of research in the fields of embryology and the history of evolutionary thought. At the outset, it should be stressed that Lovtrup, although quite a severe critic of Darwin, nevertheless is not a creationist. In that respect, his position resembles that of other critics of Darwin, such as Austin H. Clark, Michael Denton, Norman Macbeth, Otto Schindewolf and John C. Willis.

In his *Introduction*, Lovtrup raises two questions; a) is Darwin the founder of the theory of evolution?, and b) does his theory of natural selection give an acceptable explanation of the mechanism of evolution? If these two claims can be substantiated, then he holds that Darwin truly can be considered the Newton of Biology. However, it is Lovtrup's opinion that neither of these two claims is true. It follows that if his conclusion is correct, present-day biology, as he puts it, is in an awkward situation. As Lovtrup continues his analysis, he breaks down the whole of evolutionary thought into what he terms the microevolutionary theory and the macroevolutionary theory. As his studies continued, he realized five points: (1) that Lamarck and not Darwin is the true founder of the theory of evolution; (2) that the macromutation theory is older than the Darwinian theory (micromutation theory) having had supporters for over 150 years prior to Darwinism; (3) that in the last century hardly anyone, including Darwin himself, believed that natural selection could accomplish the events necessary for the occurrence of organic evolution, p. 4; (4) that the history of evolution as told today, includes a large number of mistakes and misrepresentations aimed at adulating Darwin and debunking his opponents and finally; (5) that many empirical observations have been made which may be used to test evolutionary theories. However, Lovtrup holds that the facts obstinately corroborate the macromutation theory and falsify the micromutation theory.

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Lovtrup presents the reader with a set of four theories which he requires to account for evolution. He lists them as follows, firstly *The Theory on the Reality of Evolution*. If the reality of evolution is accepted, then secondly *The Theory on the History of Evolution* follows. Thirdly is *Theories on the Mechanism of Evolution*. However, this latter must be divided into two radically different theories; *The Epigenetic Theory* and *The Ecological Theory*. According to Lovtrup, evolution involves innovations which have been accomplished through a major modification of the organism at some point in its embryonic stages. Lovtrup realizes that important mutations from an evolutionary viewpoint must have been drastic one-stroke changes of features, distinguishing the major taxa.

A major point to keep in mind is that using Lovtrup's book requires that his readers fully inform themselves as to the many ways that he uses the word *evolution*. It further requires that the reader have a more than casual knowledge of biological thought. Otherwise the cursory reader easily might misread Lovtrup at many points where he severely criticizes Darwin. In turn, this might cause some readers erroneously to believe that the author supports creation, when such is not really the case. It was in 1860 that Darwin presented his theory on the mechanism of evolution. Shortly thereafter Darwinism came to mean evolutionism, although many knew that Darwin was not the founder of evolution. Lovtrup time and time again shows conclusively that it just will not do to use the bare word 'evolution' when discussing the subject of origins in a detailed manner, but he all too often departs from the custom himself. There are far too many definitions of, and names for, the various forms of evolutionism for such a loose use of a word which has become almost undefinable and as a result often quite meaningless.

Among the interesting points Lovtrup makes is that although Huxley is considered by many as Darwin's greatest champion, yet he had many reservations and actually apparently supported Darwin only for his own reasons. On the other hand, Darwin made most of the changes in his many revisions on the basis of points made by the opposition rather than from criticisms by his friends. e.g., in the preface to the Sixth Edition, Darwin himself said that "Fleeming Jenkin has given me much trouble, but has been of more real use to me than any other essay or review" (see *Did Charles Darwin Become a Christian?*). Lovtrup points out the remarkable yet interesting fact that Darwin did not mention a single precursor in the early editions of his work. In fact, in the first edition of *On the Origin Of Species*, Darwin refers unabashedly to *my theory* 45 times, even when dealing with ideas found in Lamarck's and Wallace's works.

Incidentally, we find that even today Darwin is proclaimed, often in panegyric terms, as the founder of the notion of evolution in general and the theory of natural selection in particular, and the inspirer of all work on evolution during the last century (p. 402). In chapter 11, titled *The New-Mendelians*, Lovtrup draws attention to such as R. A. Fisher, J. B. S. Haldane, and Sewall Wright. Their appearance heralds the advent of mathematics into the field of evolution, particularly through population genetics. That natural selection could now be expressed in terms of increasingly

complex equations was hailed as a great advance in evolutionary thought.

An important message is the exposure of the humanness of many of the scientists that Lovtrup discusses. We so often encounter nonsense which holds that scientists are noble, objective, never self-serving, always impartial, and completely reliable, thus different from workers in other disciplines. This in turn is to insure the self-correcting aspect of evolutionary theory. However, chapter 15, titled *The Myth*, certainly shows that that view is particularly not valid in the case of many evolutionists whom Lovtrup has discussed. Lovtrup says that:

for almost two centuries, Empiricism has been the ruling orthodoxy in biology. Darwin's approach was to survey the literature for facts supporting his views, and he has often been lauded for his endeavors in this respect. But as we have seen, he never found any data corroborating his own theory, and therefore his main concern became . . . to explain away the lack of evidence while repeatedly stressing the greater plausibility of his theory over that of *special creation*. This may be Empiricism, but it is not good science. (p. 405)

Lovtrup's most definite rejection of Darwinism is: "nobody has ever been able to present empirical data which corroborates the Darwinian micromutation theory" (p. 351). Although he qualified this statement slightly later in the paragraph, he reiterates in the next paragraph "the micromutation theory is false." Further along, he says: "hence to all intents and purposes the theory has been falsified, so why has it not been abandoned?" His reply to this is that current evolutionists refuse to accept falsifying evidence. He also points out that the Modern Synthesis is not a theory but a range of opinions which each, in its own way, tries to overcome the difficulties presented by the world of facts. It is proper to ask what then is Lovtrup's theory that he presents as an alternative to Darwinism? He previously has stated his personal preference for the macromutation theory. In chapter 13 he stresses the importance of dealing with two aspects of evolution, i.e., the appearance of new forms and their survival. One of his criticisms of Darwin is that he lumped these two factors as being due not only to the same agent, but also into one process.

Lovtrup calls his theory the epigenetic theory. Over-simplified, it is composed of two parts, ontogenesis and epigenesis. In essence he proposes that it is in some stage of the ontogenesis of the individual organism that macromutation occurs that direct the formation of a new organism. Epigenesis then represents the mechanisms responsible for their occurrence in the adult. Lovtrup's explanation of this process in detail occupies the whole of chapter 13 dealing with theories on the mechanism of evolution.

It is true that Lovtrup's view sounds a bit reminiscent of Goldschmidt's *Hopeful Monster*. However, even more so it greatly resembles a concept proposed by a man whose writings Lovtrup possibly never encountered, since we find no mention of him by Lovtrup. Austin H. Clark promulgated his theory in his final work, *The New Evolution—Zoogenesis*, published in 1930. Another term that Clark used for his

theory was *eogenesis*. As with other workers, Clark proposed that new forms had their origins as macro-mutations in the early embryological stages. In conclusion Lovtrup says:

I believe that one day the Darwinian myth will be ranked as the greatest deceit in the history of science. When this happens, many people will pose the question: How did this ever happen? The present text surveys some of the answers which have been given, but there is no reason to believe that we have yet reached the final one (p. 422).

I urge that Lovtrup's book be recommended for purchase by every college library, both secular and parochial. It will serve as an extremely valuable reference work to provide a source of knowledge that will be of use to anyone writing a history or a discussion on the subject of the origin of species. It will also serve as a reference work for the history of evolutionary thought.

Carolina Bays, Mima Mounds, Submarine Canyons and Other Topographical Phenomena by William R. Corliss. 1988. The Sourcebook Project, P.O. Box 107, Glen Arm, MD 21057. 239 pages. \$17.95.

Reviewed by Emmett L. Williams*

When I first received this book, I started to scan its contents. Three hours later I forced myself to set it aside. To me the subject matter is fascinating. Corliss has catalogued many unusual topographical anomalies

(subject matter often not explainable by standard geological theory). This is his forte.

Many of the topics covered are of interest to creationists. The study of Carolina Bays could possibly be placed in a retreating Flood framework. Potholes, fossil reefs, submarine canyons, terraces along rivers, expanding Earth concepts, continental drift, eskers, wind gaps, apparently youthful rivers, Grand Canyon anomalies, elevated plains, and incised meanders either have been mentioned or interpreted within the framework of the creation model of science.

A bibliography is given at the end of each topic. If there is a particular subject of interest, a person could dig deeper into the phenomenon. Many of the anomalies offer research possibilities. Even if someone is not inclined to pursue a specific effect, the book is still pleasurable reading. Many features of the Earth are so little understood, and this appeals to the detective in all of us.

All topics are presented in a very readable style. If you enjoy mysteries, meteoritic bombardment theories, learning of strange collections of mud, dirt and rock debris, ice age postulations, unusual erosion effects or geology from a different viewpoint, you will appreciate this collection. Corliss' work demonstrates that scientists know very little about the present Earth. An obvious conclusion is that if we have trouble understanding the present topographical features of our planet, it should make us humble in approaching the subject of its origin.

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LETTERS TO THE EDITOR

Reptiles and Taxonomy

Many creationists question the reality of some taxonomic classifications used by evolutionists. In this brief essay I would like to consider the validity of the Class Reptilia which belongs to the sub-phylum Vertebrata (animals with backbones). I recently have completed a study of 12 different "reptiles," both extinct and extant, and have compared them both with each other and with other Classes such as the amphibians (toad), bony fish (teleosts), Aves and mammals (bat), in respect of 26 morphological features which can be used to determine classification status.

My study was triggered by a statement of Niles Eldredge of the American Museum of Natural History in an interview with Luther Sunderland (1984). Eldredge had suggested that the reptiles are not a natural group. An even stronger assertion was made by Colin Patterson, senior paleontologist of the British Museum (1982). Patterson wrote:

(the) Class Reptilia, necessitated or permitted by evolutionary classification, has one crippling disadvantage . . . (the) group is uncharacterisable; (it) has no characters of its own. The only statements that can be made about (it) are negative . . . the defining characters of a group like reptilia ARE NON-EXISTENT . . . (it) has no real existence; (it) is an artefact.

Patterson, a non-creationist, complained that the concept of ancestry, as part of evolutionary theory, had so captivated the systematists that they are trained to think only in terms of family trees, ancestry and descent. He further pointed out that such "flawed artefacts" still play a central role in phylogenies (evolutionary lineages). (p. 306)

These remarkable admissions by two of the worlds leading paleontologists, prompted me to make my own study of the subject. At random I chose the following representatives of the so-called reptilian class—EXTANT—snakes, crocodiles, turtles.

EXTINCT—plesiosaurs, ichthyosaurs, brontosaurus; synapsids including the mammal-like reptiles (the pelycosaur and cynodonts); the flying reptiles (the pterosaurs), the theropods and the parrot-beaked dinosaurs.

Among the 26 morphological features usually referred to in relation to reptiles are the following main items—possession of the quadrate-articular jaw joint, Poikilothermy/homoiothermy (cold-blooded/warm-blooded), single or multiboned middle ear; scales/armor/horny hide; skull openings behind the eye; locomotion; toes/claws; fins; wings; dentition; amniote egg; live birth; undivided heart or 2, 3 or 4 chambered heart; and the otic notch. Here is the problem—whereas the class Mammalia can be distinguished by the possession of certain exclusive features such as the squamosal/dentary jaw joint, mammary glands and

three bones in the middle ear (the ossicles), we find that reptiles possess not a single exclusive character. The birds have feathers and a wishbone but the reptiles, on close examination, have no such exclusive feature. The usual picture of a reptile is a slithering or crawling lizard, snake or crocodile, covered with scales or horny armored outer skin. But then tough scales or a horny armor were also possessed by certain ancient fish (early osteostraci and heterostraci) and by the modern toad which is an amphibian. Further, extinct flying reptiles (the pterosaurs) possessed a thick, coarse hairy-like covering on its body, much to the surprise of evolutionists. (Goodenough, 1981, p. 24).

Another favorite with evolutionists is the reptilian quadrate-articular jaw joint but we find this feature also in most fish, all amphibians and all birds. Modern reptiles are cold-blooded (poikilothermic) as are fish and amphibians. Self regulatory body temperature is possessed only by mammals and birds. What about the reptilian amniote egg? Many people may be surprised to know that all the modern birds lay this type of egg but an ancient, extinct marine reptile, the ichthyosaur, gave birth live, underwater. (Goodenough, 1981, p. 23). Mammals also have the three germ layers characteristics of amniotes. Not all reptiles have teeth (turtles, some dinosaurs such as *Struthiomimus* or ostrich dinosaur have none.) However, while teeth of most reptiles are undifferentiated, there were some extinct forms such as most therapsids and cynodonts, many of which possessed canines and incisors plus cusped molars. But then again there are mammals such as dolphins, which possess undifferentiated teeth, as do fish. It is confusing.

With regard to locomotion, we find that the reptiles are more diverse than any other Class. There are quadrupeds such as the crocodiles, the cynodonts and the brontosaurus; bipeds such as the extinct theropod dinosaurs; slitherers and swimmers such as land snakes, sea snakes and the extinct plesiosaurs; fliers such as the extinct Pterosaurs. There is an astonishing variety of two and three clawed theropod dinosaurs plus specimens with four and five toes and some with four claws. Some diapsids like the snakes and crocodiles have two openings in the skull behind the eyes; the anapsids (turtles, etc.) have none, while the euryapsids such as the plesiosaurus and ichthyosaurs had only one opening high on the skull. There were reptiles with fins (the extinct euryapsids), while most modern forms are land dwellers. Most reptiles have only a single condyle but some of the extinct cynodonts had two. Snakes and turtles have a three-chamber heart, crocodiles have either a two- or four-chambered heart and most dinosaurs had a two-chambered heart. However the brontosaurus and extinct marine reptiles had a two-chambered heart.

We have found the reasons for the previously mentioned comments by Patterson and Eldredge. The Class Reptilia, as such, does not in fact exist. There are no unifying features and the evolutionary concept which brought this taxon into its all-embracing coverage is, as Patterson says, a flawed artefact. Reptiles are, according to the phylogenetic viewpoint, all tetrapods which are not mammals, amphibians or birds! In its place, I suggest that creation scientists consider the uniqueness and separateness of the various sub-groups

at present lumped together under Class Reptilia and forget the alleged evolutionary relationships altogether.

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Embryology— Overlooked Facts You Should Know

Introduction

Some evolutionists have indicated that no reputable scientist since the 1920's or 30's would claim that the human embryo, in its development from the one-celled stage to the adult, passes through stages that parallel successive steps in the evolutionary development of that particular species. I have not found that to be the case. Rather it is still being used by evolutionists today.

When I took my medical training at what is now the University of Osteopathic Medicine and Health Sciences (Des Moines, Iowa), we studied embryology in our second year of studies in the basic sciences in 1959. Our professor had been previously engaged in research in embryology at Harvard University and his course was saturated with speculations concerning evolution. He was the most dogmatic evolutionist that I have ever had for an instructor. When he tried to explain why, e.g., certain blood vessels disintegrated while others developed to take over their function, he would say, "Nature decided [to do it]." How interesting attributing them to a personification capable of decision when he did not believe in a personal God? This was subtly supplanting the Creator with a form of folklore. Never once did he seem aware of disagreement among evolutionists on these points and our class members largely accepted his statements without question. As I took notes, I observed a number of flaws in his presentation which I would like to share with you.

In 1984, when I attended the debate at Purdue University between Gish and Prothero (Knox College in Illinois), Prothero attempted to use this "recapitulation theory" from embryology, asserting that the "gills" became the lungs later in life. Gish got him to admit, on questioning, that the lungs do not develop from them. We see, therefore, that some evolutionists are still using the theory when they feel it is advantageous to their cause.

The Protozoan Stage

Inasmuch as the male reproductive (sperm) cell is motile and actually swims to the egg cell, it is likened to Protozoa. As a matter of fact, the sperm has no

structural parallel in all of protozoology. The nucleus comprises well over 90 percent of its volume, whereas protozoan nuclei are relatively small. The "neck" and "tail" of the sperm cell is much more complex than that of protozoa. The flagella and cilia of rapidly motile protozoa appear to be solid (actually comprised of 10 pairs of internal microtubules) and hair-like with a very small diameter. The neck of the sperm is very rich (almost packed) with mitochondria and the long tail is filled with cytoplasm with a relatively short whip-like end. Physiologically, the sperm has only one function and that is to fertilize the egg and contribute its nuclear (and to some extent cytoplasmic) material to it. Unlike protozoa, it has no independent life, can neither assimilate food particles, reproduce itself nor carry on constructive metabolism (anabolism).

The Segmented Worm Stage

The similarity between somites, the symmetrical blocks of tissue which can be seen along the back (dorsal aspect) of the human embryo, and the segments in the adult earthworm are given as evidence that in man's ancestry he passed through a "worm stage." Important differences were observed, however. In the worm, segmentation is complete and both external and internal. In the vertebrate it is internal only and incomplete. The orientation of the embryo's somites is longitudinal and dorso-lateral within the embryo and is found only in the upper (dorsal) region, whereas the worm's segments (metameres) are composed of strong muscular masses which completely surround the internal organs. The bulk of these muscles run longitudinally, the circular muscles being much thinner. These are used for locomotion and bear little internal resemblance to somites of the embryo.

Perhaps more basic differences are the fact that the nephridia (simple kidney-like organs) of the earthworm are distinctly ventral (lower), while the nephritomes of the embryo are clearly dorsal (upper). The aorta is central in the embryo, but there are two main vessels in the earthworm, one dorsal and one ventral. Most significant is the *ventral* location of the worm's nerve cord, as in all *invertebrates*, whereas the neural tube of the *embryo* is always dorsal, in the position where it is found in all vertebrates. Thus the dissimilarity between the embryo and the worm parallels that between vertebrates and invertebrates in the *mature* state, a gulf which has always been an unanswerable stumbling block to the evolutionist.

The "Fish Stage"

This is the stage of development of the embryo that is most often referred to in evolutionist discussions. The pharyngeal pouches (outpocketings of embryonic tissue [endoderm]) in the pharynx or throat region resemble, in the minds of some evolutionists, the gills of a fish. However, these clefts (slit-like openings in the pharyngeal pouches) are always closed in the normal embryo, except for a very brief period of time. However, the fourth pharyngeal pouch, from the floor of which it was thought that the trachea, bronchi and lungs developed, normally remain *closed*. According to more recent findings, as cited by Balinsky, it is now conceded that the lungs come from ventral buds in the alimentary canal (foregut) and that none of the cellular

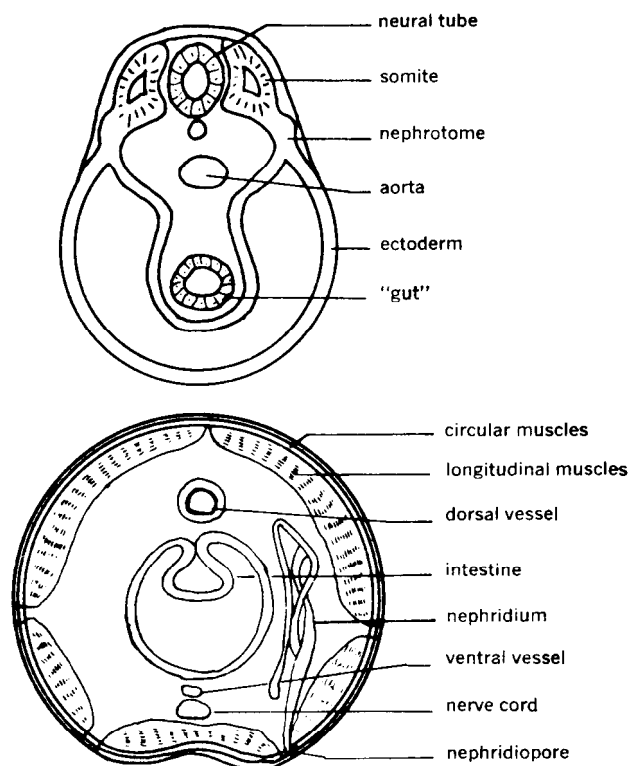


Figure 1. Cross sections of earthworm (top) and of human embryo.

tissue in the pharyngeal pouches develop into any part of the respiratory apparatus. The tonsils, thyroid, parathyroid, thymus and tongue develop from these pouches. "Its remnants are used up for the development of parts having nothing to do with respiratory function" (Balinsky, p. 522). Further, the gills of the fish are bright red due to the network of capillaries in the gills. This network provides a large surface area for the absorption of oxygen from the water, oxygen being present only in small quantities and necessitating a large area for rapid absorption of the gas. This enables the high energy level in the body movements of the fish. There is no such proliferation of capillaries in the pharyngeal pouches. There is no circulation between them as in gills. Further, they never function as *respiratory organs*. The embryo depends on the oxygen supplies via the umbilical cord until the newborn infant takes its first breath and starts breathing on its own. One can answer this argument in one sentence: *They are neither structurally slits nor functionally gills, nor do they form respiratory organs.*

The "Hair Stage"

The embryo is covered with very fine hair at about the seventh month of development of the embryo. The evolutionist claims this is evidence that men came from hairy mammals like the apes. However, these hairs are unlike the hair found on apes, as they are very small in diameter and always soft and unpigmented. This hair disappears from the body soon after birth. It is called lanugo and is quite unlike the permanent hair that grows on the human body and head. The lanugo on the body is followed by vellus hair which forms in the prepubertal years, some apparently from new

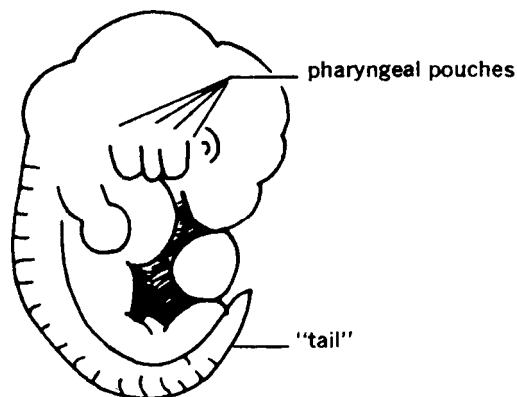


Figure 2. Vertebrate embryo of six weeks' gestation.

follicles. This persists on the face, neck and ears. Mature hair, beginning in puberty, is thicker and stronger than either of the previous forms.

The "Tail Stage"

This "stage" is found when many embryological structures appear distorted and "misplaced" for example; the body cavity actually invades the placenta for a time, and what will later be the coccygeal region extends slightly beyond the body, resembling a tail. It must be remembered, however, that the digestive cavity, or gut, actually forms a slight projection into the "tail." In all such animals with tails, such as most mammals including the primates such as monkeys, the tail depends for support on the vertebrae. In this stage of the embryo, however, no vertebrae are present but only the notochord, which does not become the vertebrae. The latter arise from *sclerotomes*, which migrate to and surround the notochord, which breaks up and the pieces become the nuclei (nuclei pulposi) of the disks between the vertebrae.

Because of the obvious speculation involved in this theory that "ontogeny recapitulates phylogeny," (Rusch, 1969, pp. 27-34) it has been discredited by many, if not most, embryologists today and is recognized among them as a quite vulnerable hypothesis.

As Waddington (p. 10) states:

The type of analogical thinking which leads to theories that development is based on the recapitulation of ancestral stages no longer seems at all convincing or even interesting to biologists. . . . Recapitulation appears nowadays as a series of problems for evolutionary theory to discuss rather than as an explanation of developmental processes.

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Reply to

"The Metaphysics of Modern Science"

The cosmic "original sin in the garden of science" is the erroneous injection by secularists of their philosophical-religious world view into the definition of science. Lawrence A. McGhee (1987, pp. 138-41) proposes that Christians should correct this gross error, not simply by excising the secularists' belief system, but by substituting in its place their own Christian world view. Thus he would substitute a new non-identical-twin "sin" for the former. He would have Christians committing basically the same error as the secularists.

McGhee would justify his philosophical biasing of the definition of science on two grounds: (1) that the Christian world view is right and the secularists' wrong, and (2) that the secular world view logically undermines the rationale and method of science whereas the Christian world view justifies them. These two correct judgments, however, are irrelevant for defining science. The injection of the Christian world view into the definition of science confounds science and philosophy of science, just as does the secularists' "original sin." It involves the same logical error which the secularists' distorted definition of science has done, i.e., it requires scientists to believe things which can never be either proved or disproved by empirical science.

I am accused of proposing a "philosophically neutral" definition of science which is in fact not neutral. However, I did not assert that the scientific enterprise has no philosophical implications. Rather, I asserted that "a correct definition of science is philosophically neutral at least to the extent that it has nothing to say about what a scientist believes or disbelieves" (Kofahl, 1986, p. 112). I also pointed out, however, that "the scientific enterprise has profound philosophical and religious implications" (p. 114). The practice of science involves certain assumptions, for example, that the natural order is (1) rational, (2) knowable, (3) reproducible and (4) that the laws of logic are valid. It is obvious that such assumptions are philosophically loaded, but just what the philosophical implications are will always be a matter of hot dispute among scholars of various philosophical persuasions. We as Christians contend that these assumptions follow logically from the Biblical Christian world view which is divinely revealed in the Bible. But shall we demand, as McGhee seems to suggest, that all scientists either espouse our faith world view and philosophy of science or withdraw from the scientific enterprise? Shall we further insist that science laboratories and journals be made a battlefield for the theological debates? I do not think so, but as I said in my article, along with secularist scientists:

Christians should expect the freedom in suitable technical journals openly to draw conclusions which explicitly relate their hypotheses and empirical data to their conceptual frameworks [which may contain elements of the supernatural]. (p. 114)

McGhee's implied restriction on the thinking of practitioners of science is analogous to a demand by a Christian basketball player that Larry Bird either begin playing for the glory of God or quit the game. For

Christians involved in science and other fields of scholarship to adopt any such stance would court disaster for the Christian witness in all fields of intellectual endeavor. We would be laughed out of the arena of rational discussion as mere religious enthusiasts and bigots—rightly so, in my opinion. Currently Christians are indeed excluded by the secularist mind-masters from full citizenship in the community of scholars, largely on the unjust ground of the distorted definition of science. We must not adopt a position which justifies our secularist persecutors. Let them be the bigots, not us.

McGhee (p. 139) asserts that my stricture that “nobody, including Christians, should seek to insert his personal belief system into the definition of science” defines science “as an impersonal pursuit. . . It does no such thing. McGhee is demanding that all scientists *think as he does* to preserve their status as scientists. But he is confusing the enterprise with the manner in which the individual participants function in it, including what they believe about it. I enumerated the rules of the scientific method in my paper. They determine how a scientist should conduct himself, but they say nothing about what the scientist must believe. Two equally effective scientists who function in accord with the rules can be poles apart in their beliefs and philosophy of science. And despite such disagreements, they can even work on the same research team, provided they both abide by the rules.

McGhee seems to hold (after Ferst, 1983) that the definition of science includes the supernatural as a mode of scientific explanation. It cannot be. Scientific explanations are expressed in the form of scientific hypotheses. Scientific hypotheses must deal only with the empirical world if they are to be empirically testable, i.e., falsifiable. How can a hypothesis asserting a supernatural, miraculous act of God be tested empirically? It cannot be tested, for the supernatural cannot be observed or measured, and God cannot be subjected to controlled experiments.

McGhee also seems to believe that the aim of science is to determine absolute truth. The only way he can make it so is to make faith in divine special revelation a part of the scientific method. But the fact is that science does not determine truth. Scientists, should by intelligent application of trial and error, by inductive and deductive logic, and by testing of hypotheses, seek continually to gain a more accurate understanding of the natural order. However, since any finding of science may tomorrow be replaced by more accurate knowledge, no scientific conclusion is truth. Truth is what God tells us in the Bible, the Word of God written. Scientific knowledge is knowledge obtained by the application of the method of empirical science. Knowledge which is divinely revealed in the Scriptures is *not* scientific knowledge! Revealed knowledge is truth which we embrace by faith and we expect that correct scientific knowledge will agree with divinely revealed truth. Consequently, we do not judge divine revelation by science but just the converse. Divine revelation gives us a conceptual and factual framework within which we Christians judge science and obtain guidance for our scientific endeavors as Christians. By revelation we also have true

knowledge about the natural order which science could never discover or prove.

All this, however, does not make faith in divine revelation a part of the definition of science or a rule of the scientific method, either for Christians or non-Christians. Christians in science, as we said above, possess true knowledge about the natural order and a perspective which comes not from science, but from God's Word. We believe that a basketball player can be a better one because of his faith in Jesus Christ and in divine revelation. We also believe that a Christian can be a better, more fruitful scientist because of his faith. We further believe that the scientific enterprise can become more fruitful and helpful to mankind if there are more Christians participating in science for the glory of God. None of these facts makes belief in God or in divinely revealed knowledge properly a part of the definition of science or of the rules of the method of science. Nevertheless, these elements of Biblical faith have much to do with the thinking, motivation, guidance and goals of those who are called to serve Christ through scientific enterprise.

In conclusion, McGhee is rightly concerned with the need to restrain the speculative scenarios manufactured by secular scientists engaged in the “historical sciences” such as geology, paleontology and cosmogony. I submit that the proper restraint is provided by the correct philosophically neutral definition of science. The requirement of empirical testability of all hypotheses is all the restraint that is needed. It goes without saying that historical speculations by Christians should be subject to the same restraint.

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Response to Kofahl

Modern evolutionary science is based on a philosophy with some distinct parameters: no use of teleology, no purpose, no supernatural Creator. None of these positions is neutral. Evolutionary science has made a reasoned decision to reject these parameters. They are not looking for a neutral position. It appears, however, creationists might hide in a proposed neutral position so as to be able to do science with those who reject creationism without conflict.

Why try to be neutral if, in fact, all thinkers must take a position either for an intelligent Creator or with chance, purposelessness and no intelligence as our origin. If indeed a neutral definition could be constructed—which I doubt—it would surely be discovered I am masquerading as neutral. Scientists are not neutral persons by nature, but members of a highly trained group for the most part dedicated to evolving the human race via technology and gadgetry of every kind.

My argument regarding the failure of Kofahl's neutral definition is clear and was not really addressed by him. Kofahl has no neutral words to use in discussing the issue because there are none. He picked his words of choice from the evolutionary camp: "effective scientists who function" (my italics) and "accurate understanding of the *natural order*" (my italics) (These phrases are from Dr. Kofahl's letter.) Why were "purpose" and "created" not used? This may sound picky. I think not. In 1965, when I wished to study biochemistry, I was told that I could not publish in any major science magazine in America if I used these two words. The situation is worse now. There is no neutral magazine on this issue because it is impossible to write about creation/evolution in a neutral manner.

Only two grand movements of the Universe are possible. One view of origins contends that all things happened from a Big Bang—a chaotic beginning which is claimed to be moving to greater and greater order under man's direction [with no purpose, however]. The final state will be an equilibrium of heat with all creative life dead. The motto is "gather ye rosebuds while ye may" (Herrick, 1956, p. 468).

The only competing view is diametrically opposed. All things began in intelligent order created by God and are now moving to greater and greater randomness. The motto is "live carefully for God will judge your life."

The only neutral position would be Fred Hoyle's steady-state—which he has not been able to maintain. Therefore, the universe is in process to one of two final positions—not both—or a little of both—or neither! It may be possible to have neutral positions on some lower level axioms of science—I think not on these major ones.

Science is human experience systematically extended (by intent, methodology, and instrumentation) for the purpose of learning more about the natural world and for the critical empirical testing and possible falsification of all ideas about the natural world. (Kofahl, 1986, p. 112)

When science proper is defined "neutrally" the word "purpose" appears. Can it be that we have an employee/employer distinction with science allowed purpose, but the scientist only function? I maintain that Kofahl's "neutral" position does take a position "about what a scientist believes or disbelieves." His "natural" world rather than "created" refuses to "name the name of the Creator," while "function" rejects "purpose" as we have noted. If the "supernatural, being personal, is not subject to investigation by the method of empirical science" (Kofahl, 1986, p. 114), what would be as close and empirical as a personal relationship with a personal God! Defining God out of science is a deceit hiding unbelief and apostasy regarding the most important question in the universe: Is there a Creator and can I communicate with Him?

Certainly all persons are not "systematic" in their thinking or living, nor are they intent on "extend[ing]" their experience—this is not a neutral pattern. Many presumptions appear in this "neutral" definition. (Kofahl, 1986, p. 112). My article sought to outline the state of the scientific foundations today—with an emphasis on the problems of evolution and the strengths of

creationists. Kofahl has a high view of scientific enterprise and the correct views respecting it, while he is not troubled at all by the obvious truth problem existing among the beliefs listed in "Number One" assertion under the freedoms of the scientist. "The scientist . . . is free to choose from among such disparate beliefs as atheistic materialism, pantheism, agnosticism, Eastern religions, pure idealism, liberal Christianity, materialistic evolution, theistic evolution, and Biblical special creation" (Kofahl, 1986, p. 112). What a devastating way of saying no one knows the truth about ultimates and science is more important and fun anyway!

In conclusion, I do maintain, the large paradigms and conclusions of science will be false apart from reference to The Authority of the Creator, Jesus Christ. Revelation of Christ removes the despair of modern science caused by the "apparent" triumph of death and entropy. We maintain that whether any man is walking toward an eternal "intelligent fellowship with the Invisible One, Jesus Christ, the Creator, or [into the fiery chaos called hell is according to] a mental attitude each man *must choose*." (Italics mine). (McGhee, 1987, p. 141). Good bedtime reading would be Gerard Manley Hopkins' *That Nature is a Heraclitean Fire and of the Comfort of the Resurrection*.

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A Comment on Patten's Views

Patten (1987) has asserted that mountains were caused by gravitational attraction between Mars and Earth. I must assume that Patten's precise, dogmatic statements concerning orogeny are somehow based on calculations which he has made concerning Roche's Limit. He says: "On this occasion, I believe Mars passed within about 15,000 miles (core to core) of the earth. (p. 62)

Are we talking about the center of the core or the perimeter of the core of the two planets? In any case, I stagger to hear that this postulated near miss would not have disintegrated the smaller body, Mars, and would be only "brushing" the Roche's limit of earth! Moreover, the statement that the phenomenon had occurred previously and that, on this previous encounter the distance separating the cores of the planets was approximately 17,000 miles, (p. 63) needs some technical support. Indeed, I would tentatively conclude that, had this event really occurred at this distance, Morton's thesis of an expanded earth would have received substantial internal support to the extent of the mass of Mars!

The postulated crustal tides with their "... upthrust, bending and breaking of the earth's crust in ranges, in arcuate ranges..." (p. 63) in no way can account for the types of diastrophism found there. Indeed, while the topographer notes a general curvature pattern along these "great circle arcs," the actual configuration is far more complex.

Even if we assume the survival of both celestial bodies in this postulated near collision, not nearly all of the mountain ranges in this crucial, hypothesized "uplifts" fit the predicted arc configuration. It looks far better on a world map. For examples, in Utah the Uinta Mountain Range is an uplift, the main axis of which lies east and west. Southeast of Glacier National Park the Little Belt Mountains similarly lie oriented about 90° from the main axis of the Northern Rockies. This latter range extends for hundreds of miles to the north and south. Is it a great uplift that is drawn into the circumference of a great circle arc? Quite the contrary, this system contains massive elements of lateral compression and of thrust FROM THE WEST. This utterly contradicts the uplift conformation required if flyby is the correct explanation for this series of ranges. Indeed, the presence of massive evidences of eastward trending, lateral thrusting, tilting, bending and overriding throughout major sections of the Western Rockies, the Northern Rockies and in the Canadian Rockies requires us to search for a more adequate mechanism to produce these mountains.

This physical evidence alone renders a "flyby" explanation highly suspect since it obviously accounts inadequately for the phenomena beyond the level of a surface consideration. Great fault blocks dip eastward and westward throughout these Rockies. These mountains also contain very extensive folding, overfolding and thrusting which so often is directed eastward. Did "gravitational attraction" pulling from another celestial body, thereby drawing these ranges into a long arc, and exerting the forces of diastrophism which moulded the Rockies? It is not likely. And any model which attempts to account for mountain building on a universal scale throughout the world must account for these factors.

Another area of incongruity in the roughly semi-circular configuration of mountains is the wide variety of radii to be found when a compass is swung on their circumferences. (As an example, set one leg of a compass on Bermuda and swing the other leg around the remarkable curvature of the Endless Mountains of Pennsylvania). Not only are these arcs found to vary enormously in their radii but the arcs themselves are found to be facing in many different directions. For arcs of mountains with their radii as small as 300 miles to be explained as the result of a "flyby," would require an impossibly near astral mass attraction for its production!

Just as difficult to explain is the wide variety of directions that these arcs face. After all, how many successful "flybys" (without an occasional crash) occurred to provide the numerous mass attractions required for these to be produced? These and other factors suggest that the mechanism postulated is rather remote from the real cause of these somewhat arcuate patterns of mountain chains. It is an axiom worth heeding that "the more remote the hypothesis is from the truth, the more difficult its support, the less harmony found in examin-

ing details, and finally, the greater importance that dogmatism plays in its support."

Another significant area of disharmony between the celestial visitor harmonization model and the geological (and Biblical) evidences relates to its postulated role in producing the Noahic Flood. Patten long has taught that the ice which was stripped from the astral visitor into earth's proximity (or which WAS the visitor), fell through the atmosphere during the Flood to produce the glacial age. By this he appears to have reference to the Pleistocene ice epoch. This is a conclusion which is impossible in the light of the chronological clues left everywhere on his postulated "flyby" uplifts, the great mountain ranges. What is the sequence of events which is required by the actual physical evidence in the field? Wherever mountain and valley glaciers are found in these great ranges, they have a testimony concerning the chronology of the event series. I have been able to examine these as far south as southern Peru around Cuzco on the great Cordilleras of South America. I have seen their testimonies from the air in crossing the Alps repeatedly and in following their southern slopes. I have had much opportunity to read their message from coast to coast in the United States and in Canada and as far north as southern Alaska.

What do these mountains say about the timing of the icy catastrophe which scarred their peaks and canyons? First of all, it is clear that the vast, marine Paleozoic series was deposited by the Noahic Flood before the uplift of these great mountains. Broken remnants of those marine deposits are lifted and tilted, shattered and even left as pendant formations high in these mountains. The uplift is more recent than the Noahic Flood. There are multitudes of locations where one easily can recognize through field research that the Cordilleran Andes of South America, the Central Rockies, the Northern Rockies, the Sierras, the Coast Range of California, the Klamaths of Oregon, the Cascades of Washington, the Canadian Rockies, etc., all uplifted and fractured these great marine deposits of the Noahic Flood. Furthermore, the student of earth's physical history should recognize through field studies that there are no universal Flood strata which are younger than the Paleozoic deposit series anywhere in the world. More recent formations are filled with many clues which indicate that the land in many places had risen above sea level. Wind dunes, tidal waves and atmospheric volcanic extrusions are but a few signs which require the interpretation of the Paleozoic structures as the Noahic Flood year's deposits. The rupture of these layers by mountain uplift and by continental separation and movement is found to be younger than their chaotic movements. The mountain uplift which produced all of the world's great mountains is a post-Noahic Flood phenomenon. It cannot be made to precede it.

Secondly, succeeding formations, normally superimposed upon these near-universal marine formations, bear evidence of a sufficiently extended passage of time before that uplift, for the Paleozoic and Mesozoic formations to harden. They were slowly turning to stone. Furthermore, in many places the Paleozoic Hood materials were being covered by great Mesozoic aeolin deposits. These Mesozoic wind deposits still overlie the universal Noahic marine deposits in most

locations. Often these repeatedly alternate with massive tidal intrusions. These also testify by their distortion, their upwarping and by their sheared and eroded ends along both sides of the uplift, that they too are older than the uplift.

How can a model which insists that the visitor preceded the Noahic Flood yet caused these post-Flood uplifts be made to fly? Is the uplift being correctly located chronologically in the light of the physical evidence? No. The theory is utterly discordant with the physical facts when examined beyond the geographical map.

Thirdly, there is a very obvious chronological clue concerning the relationship of the ice epoch to the uplift and to the volcanoes which have built upon these uplifts. Throughout all of these mountain ranges in both hemispheres the glaciers obviously have developed subsequent to the erection of the mountain ranges. Their courses down their slopes and valleys are chronological clues that no one should overlook. The high pinnacle spires, the cirques which have been carved by the headward erosive forces of moving glaciers, the great "U-shaped" canyons refashioned by the great load of moving ice which pressed downward and outward in all direction, all testify to the icy tools which sculptured them. But, it must not be forgotten, they also testify concerning the order of events in earth's battered past. Their ice did not arrive before the Mesozoic/Cenozoic uplift of these mountains. And, lest their testimony should be misunderstood, precisely the same phenomenon continues to work its mountain shaping magic today wherever the ice age still endures. We must put the frosting on top of the cake, not under it!

What then must we conclude? Many years must separate the Noahic Flood and the so-called Pleistocene ice age which has left its scars across higher mountain elevations all across the world. But on the other hand, we must remain open to the possibility that the Noahic Flood did precipitate as snow in arctic regions as the canopy began to collapse at the beginning of the Flood. If there was ice associated with the Noahic Flood in polar regions (a distinct possibility that I long have examined), then that ice must be kept entirely separate from the Pleistocene ice as the product of a distinctly different historical event. Does this leave room for Patten's mechanism for an ice fall from a "flyby" to cause the precipitation of the Noahic Flood? I doubt it, since it seems impossible to call upon the mountain uplifts to testify of such a "flyby." Indeed, it appears far more likely that any Permian ice dump (at the close of the Paleozoic deposits) must be considered to be the result of the precipitation, not the cause.

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Response to Northrup

I notice that Northrup assesses my statements as "dogmatic." Copernicus wondered if the Earth really was the center of the solar system and later he was

dogmatic that the Sun in fact was the center. He knew. About 60 years later Kepler wondered if orbits were indeed circular, and later he dogmatically wrote that they were ellipses in every case. I shall take Northrup's comment about dogmatism for what it is, a left-handed compliment from a person who is himself unsure about the genesis of mountain cycles. I have long passed from the state of wondering (in the 1950's) to one of certainty, for the last 25 years. I think as a geographer and as an evangelical. I present four figures.

Figure 1 illustrates two cycles of mountain uplifting, the so-called Alpine-Himalayan Cycle (the horizontal series) and the so-called Circum-Pacific Cycle (the vertical series). I suspect that the Circum-Pacific series is not one, but two cycles of mountain uplifts which happen to toe into each other. These two mountain cycles, the greatest on our planet, are in "great circle alignments" which are identical with two (or three) flyby patterns.

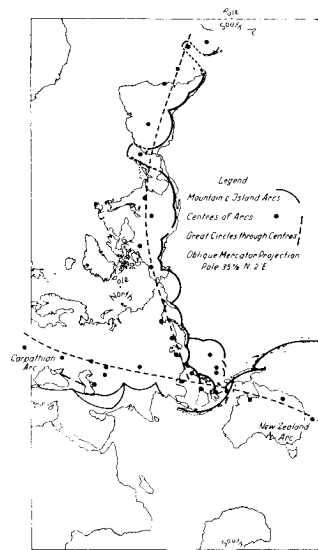


Figure 1. The mountain arcs which collectively compose the two zones of recent orogenic uplift, together with the epicenters of those arcs, and the great circle alignments of the epicenters. The two zones are the Alpine-Himalayan zone and the Circum-Pacific zones. (Wilson, J. T. 1954. *The Earth as a Planet*. University of Chicago Press. p. 153)

Figure 2 illustrates the mid-section of the Alpine-Himalayan Cycle. The front sector, to the east and not on the map, includes mountain ranges primarily lineal in pattern. Included are the island chains of the northern peninsula, North Island (New Zealand), New Caledonia, the New Hebrides, the Solomon Islands, New Britain and New Guinea. The ranges increase in size as one moves westward. In the Mars flyby theory, this sector was uplifted in about two hours, from east to west and Mars was between 30,000 and 20,000 miles distant.

The mid-section includes the Tibetan Plateau, surrounded by ranges that are primarily arcuate in pattern, not lineal. I account for the arcuate pattern as being caused by either the gravity of Mars plus the centrifugal force or spin of the Earth. This central zone includes such mountain ranges as the Himalayas, the Trans-Himalayas the Tien Shans, the Kun Luns, the

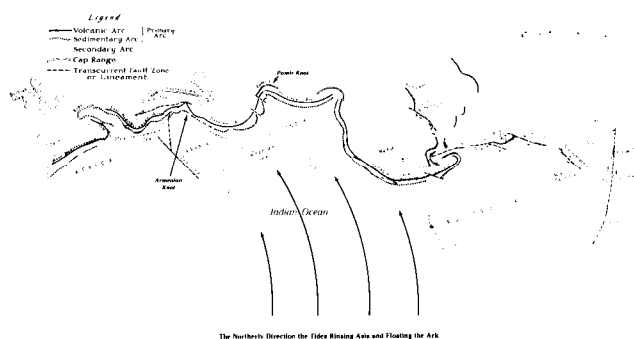


Figure 2. Primary and secondary arcs of southern Eurasia and Melanesia. (*The Earth as a Planet*. pp. 160,165)

Nan Shans, the Altai, the Hindu Kush, the Karakorums, the Pamirs and the Pamir Knot. These mountain ranges are over 1,000 miles wide (Himalayas to Nan Shan and Tien Shan). The Tibetan Plateau, rich in marine fossils, averages 13,000 feet high. These mountain ranges are the highest, widest and most handsome of our planet. In a Mars flyby theory, assuming an inside flyby, they were uplifted in less than two hours, during the closest of all of the many flybys (which probably numbered about 170).

To the west between 30° and 60° are other mountain ranges, not as wide, not as high, not as handsome, more lineal and less arcuate. These include the Elburz, the Caucasus, the Taurus and the Armenian Knot where the Ark of Noah was uplifted. In the Mars flyby theory, the Ark was uplifted on the same day as the Alpine-Himalayan cycle. The flyby perigee was about 11:00 A.M. (Ararat Time) over the Tibetan region, only 10,000 miles under the closest surface of Mars. The planets were about 16,000 miles distant core to core. This implies:

1. The ark was floated into the Armenian Knot region at about noon on the day of the closest of all flybys.
2. The ark was floated upward the same day on which the Alpine-Himalayan cycle was uplifted (Armenian Knot included).
3. The intensity of the oceanic tides reflects the intensity and the trajectory of the subcrustal tides.
4. The Indian Ocean was the origin of the tides which floated Noah's Ark.

To the west, and not on the map, is the back of the Alpine-Himalayan Cycle. Included are such ranges as the Alps, the Carpathians, the Pyrenees, the Apennines and the various Atlas series. Again, like in the front sector, these mountain ranges are more narrow in their spread, lower and more lineal than arcuate in alignment. The cycle ends with pincers, the Madeira Islands and the Canary Islands. The entire cycle, 12,500 miles long, must have been uplifted in about a 6.5 hour period because Mars was moving on an inside flyby, opposite to the direction of the Earth's rotation.

Figure 3 illustrates a similar series of mountain uplifts, the echelon of the East Asian arcs. This is the ending sector of another, earlier uplift cycle which involved the uplifting of the Kurile arc, the Aleutian arc, the Kamchatkas, the Brooks range, the Alaskan arc and other uplifts to the east and to the west. These mountain ranges are primarily in a marine environ-

ment, whereas the Alpine-Himalayan are in a terrestrial environment for the most part.

Figure 4 illustrates a very early zone of mountain uplifts, the western sector of the Appalachian-Herzian-Caledonian cycle. These also were uplifted in about 6-6.5 hours, not in 50 millions of years according to the Lyellian myth. The arcs are in series and reflect two simultaneous, and conflicting forces, the gravity of Mars, moving east-to-west and the centrifugal force of the Earth's spinning, moving west-to-east. The Appalachian-Herzian-Caledonian cycle was not only very early among the flybys, but was not as close in its perigee, perhaps 23,000 miles (core to core) versus the 16,000-mile distance of the Alpine-Himalayan flyby. Coal and limestone strata are particularly prominent amid this cycle of uplifting.

Northrup neither admits the possibility that these are flyby patterns nor does he account for them in any other way. If it is granted that these great circle patterns are also flyby patterns the next set of questions which Northrup has not addressed are these:

1. How long would a typical flyby pattern last? (6-6.5 hours)
2. What was the flyby planet in our celestial scene? (Mars)
3. Are there indicators in either the craters of Mars or the pitlets of Deimos and Phobos that Mars formerly had a different orbit?
4. What was the ancient, catastrophic orbit of Mars?
5. If the flyby or passover body was in orbit, did it also bring catastrophes in cycles of some kind?
6. If so, what were the kinds of cycles?
7. Did Mars cut the Earth's orbit once (non-coplanar) or twice (coplanar)?

Surrounding the need for good theory on orogeny or diastrophism, there is a series of additional ideas to be considered and questions to be asked. I have prepared a list of 120 such questions. These are in 10 categories as follows:

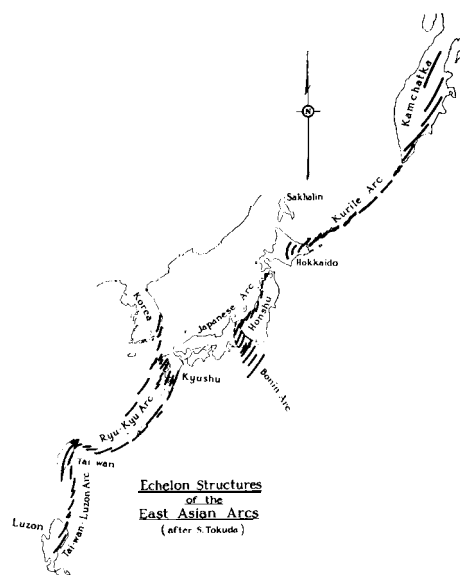


Figure 3. En echelon structures of folds and lines of volcanic craters along the East Asian arcs (after Tokuda and Bucher in *The Earth as a Planet*, p. 176)

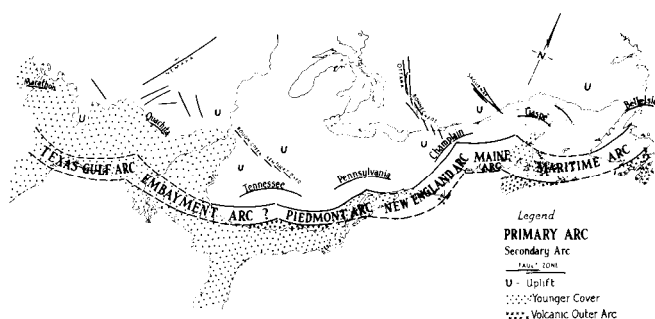


Figure 4. Diagram of primary and secondary arcs and fault zones of the inactive Appalachian Mountains (*The Earth as a Planet*, p. 174)

- A. Anniversaries of Catastrophes in the Bible
- B. Astronomy and the Bible
- C. Celestial Lighting in the Bible
- D. Cyclicism of Catastrophes in the Bible
- E. Geography of Mars
- F. Greek and Roman Literature of Catastrophism
- G. The Ancient 360-Day Year and its Implications
- H. The Origin of the Earth's Geomagnetic Field
- I. Mars and its 200+ Appearances in the Old Testament (10 Ways)
- J. Subcrustal Tidal Theory and Mars Flybys

A list of questions will be presented on only one category, due to space limitations. The other series of questions are obtainable, with their answers.

CATEGORY A. ANNIVERSARIES

1. Is there any indication in Hebrew literature (including Josephus and Talmudic material) that ancient catastrophes occurred on anniversaries of other catastrophes?
2. If so, are there one, two or more of these calendar dates of catastrophism, keeping in mind that a coplanar orbit of Mars would cross the Earth's orbit twice and theoretically they could have a close flyby at either crossing.
3. If they happen to be two in number, what are those two anniversaries of catastrophism? (Hint: Passover and the 17th day of the second month, civil calendar)/
4. If the catastrophes occurred on anniversaries, what kind of an astronomical model would that dictate? (Hint: Resonance Model).
5. Is there any indication in Roman literature that they had celestial fearidays (not holidays) on October 24 and on March 21? (Hint: armilustrium and tubulustrium).
6. If the Romans had memorial dates of catastrophes, which dates coincided with the Hebrew dates, what would that imply?
7. Did the Hebrews name any one of their 12 months after Mars and if so, what month? A flyby month?
8. Did the Romans name any of their months after Mars and if so, what month? A flyby month? A passover month?
9. If ancient catastrophies occurred on anniversaries in some kind of cyclicism, could that also be interpreted as occurring at two and only two locations in the Earth's orbit?

10. In our post-catastrophic era, does the Earth's perihelion occur almost precisely midway between the ancient flyby or passover occasions of October 24 and March 21? If so, is such by chance or design? If so, does such leave the Earth's orbital line of apsides perpendicular to that of Jupiter? Is that also by uniformitarian chance or by the design of the Lord of hosts?

Such is perhaps a five percent sample of the kinds of questions which need to be asked, some geological, some astronomical, some ancient historical. Northrop is to be congratulated that he has ventured to inquire into aspects of Earth history and their causes. But Northrop has not asked the key questions: Were the passovers flybys? Did the passovers such as in Isaiah 37 have energy sufficient to alter the Earth's orbit or tilt? Were the ancient catastrophies as recorded in the Bible cyclic as well as repetitive? What was that cycle in terms of years? Were there cycles, and periodically among them, megacycles? Did the ancients understand something of that cyclicism? (Hint: see II Samuel 24:15, Jonah 3:4, Judges 5:20, etc.) Why did the ancient Romans have such an awe and dread of Mars? What is the Hebrew word or words, if any, for Mars?

The Lord's question to Job, and to our century is thus:

Canst thou bind the sweet influences of *Khima*, or loose the bands of *Kheciyl*? Canst thou bring forth *Mazzaroth* in his season? or canst thou guide (*Ma*) *Ayish* with his sons?

Knowest thou the ordinances of heaven? canst thou set the dominion thereof in the earth? Job 38:31-33

... or who can stay the *nebel* of heaven ... Job 38:37

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Dog Canyon, Big Bend National Park, Texas

Introduction

The writer has always believed that the only proper study of geology is through field work, along with conventional class and laboratory experiments, not just by the reading of books. Hence during my teaching career, my physical and historical geology classes were required to make a one-day field trip. In addition, every spring a 12-14 day field trip to the Colorado Rockies was planned as a course in the geology program. In the course of these trips, and the field advance planning for them, as well as in private trips for leaves or other projects, I frequently observed what I was wont to consider 'geological oddities,' which I documented with slides.

West Texas

One such oddity was encountered in the basin and range country of West Texas. Traveling through the Texas region one is struck by the topography where between the numerous mountain ranges that occur, an almost flat basin lies between the ranges. As a result, roads usually follow the flat basins, going around

whatever range is encountered. Generally highways are not constructed through and over the ranges. Thus a range is often unexplored beyond its edges.

The drainage of such a region would be expected to follow the general dip of the region, with various stream patterns developing over the overall basin. Rainfall on the ranges would be expected to traverse down the slopes, to either form stream patterns across the basins or to fill the basins with lakes. Since current climates are at best semi-arid, streams and lakes would be intermittent.

Big Bend National Park

The oddity occurs when a stream is found that passes through the range, instead of going around it. One such instance can be observed in Big Bend National Park, Texas, about five miles south of Persimmon Gap park entrance. This is known as Dog Canyon, near the southern end of the Santiago Mountains. (Figure 1).

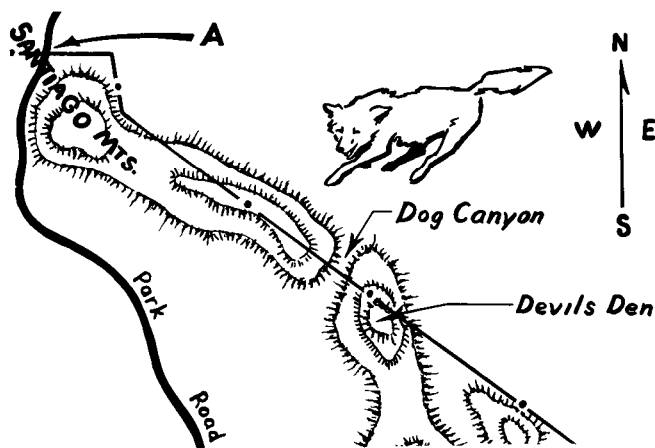


Figure 1. A schematic diagram of the location of Dog Canyon in relationship to the entrance of Big Bend National Park on U.S. Highway 385.

A — Entrance to Park (U.S. Highway 385).

Drawing by Justin Everson

The writer studied in this area under Dr. A. L. Luginbuhl of the University of Nebraska in 1959 as part of a graduate field work course. The series of photographs also date back to that time. These figures show that as one proceeds down the watercourse (eastward) and approaches the canyon, one finds horizontal beds ex-



Figure 2. Beginning of Dog Canyon. View eastward into canyon. Note horizontality of beds in south wall.



a.



b.

Figure 3. Two views further into the canyon. Note that beds in south wall (right side of b.) are still horizontal.

posed on each side. Then as one begins the penetration of the Canyon, the beds continue horizontally, until about level with the crest of the ridge. (Figures 2 and 3.) At this level in the canyon, the Lower Cretaceous limestone beds are tilted almost vertically upward (Figures 4 and 5) and a fault marks the point from beyond which they again are horizontal (Figure 6).

In trying to form a scenario for this peculiar topography, several possibilities exist:

a. The range rose slowly in the path of the stream, so slowly, that the uplift kept pace with the downward erosion of the stream, and thus the stream passed through the obstruction.

b. The whole area arose, with a stream draining it, followed by overall downward erosion, which exposed a buried range. As erosion continued, the stream was laid upon the range as it were, with the end result being that one has the anomaly of a stream apparently running through the obstruction.



Figure 4. Beds in south wall are now almost vertical. This is at the high point in the ridge.

c. That the stream could be following a previously developed transverse fault through the obstruction. If the stream's path followed a resultant series of angular bends, this might be a feasible explanation. However, since the course is a meandering one, this hypothesis would not seem viable.

A further complication is the steeply tilted beds which also require an explanation. This may be due to a compression of the beds. Then there is the sharp return to a horizontal configuration, probably marking the end of the fault. The whole region gives ample evidence of extensive faulting and folding. Finally, it is interesting to note that on the north side wall, relatively close to the bottom, there is a sequential deposition of alternatively fine grained deposits and coarse (conglomeritic) deposits (Figure 7). This area offers many research opportunities to creationists.



Figure 5. Beds in the north wall are almost vertical. Note the remnant of an apparent bend from the horizontal to the vertical. Tilted beds indicate considerable tectonic activity has occurred in this area with much faulting and folding.

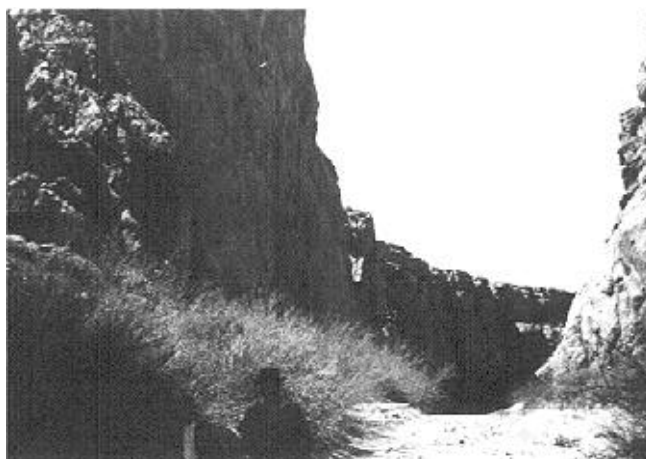


Figure 6. View eastward from the vertical beds. Note that the beds are now again horizontal.



Figure 7. Recent storm waters have cut into previously deposited beds revealing sequence of recent deposits in north wall. Note coarse to fine sequence. This phenomenon is quite common. The canyon had water flowing through it at various rates at various times in the past. This is responsible for the graded deposition. When water levels are high, then high velocities usually ensue. This in turn causes the carrying capacity of the stream to increase, as indicated by the increase in the size of the particles it can carry. Then as the velocity slows, the larger particles will be deposited first, forming the layer of coarse particles (future conglomerate?). As the velocity continues to decrease, smaller and smaller particles will be deposited. This can be demonstrated using a laboratory apparatus which has been particularly designed just for this purpose.

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QUOTE

Naturalism's inability to sustain fixed ethical principles is nowhere more apparent than in its many contradictory attempts since the Renaissance to fashion a humanistic ethics, and in its increasing tendency to justify and even to praise immorality.

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. 4.

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HELP US ESTABLISH THE CREATION MODEL OF SCIENCE

QUOTE

When we describe events and courses of events rather than personalities from the biblical creation perspective, we must zealously guard against the related evils of attributing determinative power to the events themselves or to chance. On the other hand, we must give due recognition to the fact, acknowledged by anyone who has studied history for any length of time, that the making of history is not exclusively in the hands of men, but rather often depends on the imponderable and unforeseeable — not “chance” or “fate” but, yes, God’s own sovereign hand at the helm of historical development. Yet man is genuinely responsible, and genuinely able to alter history, as is shown plainly in the biblical record of creation and the fall. Nothing was “just there” in the garden of Eden — God had placed the tree of the knowledge of good and evil there, as well as the serpent. But God did not cause Adam and Eve to eat of the forbidden tree; on the contrary, He did all He could to stop them from doing so, short of making them programmed automata or robots unable to disobey Him. Even so Caesar was not forced or “fated” to cross the Rubicon but did so by his own free will. Yet God used Caesar for His own purpose, foreseeing the Caesar Augustus, Julius Caesar’s heir and successor, under whom Jesus Christ would be born on earth 49 years later — and you and me. What an awesome, inspiring thing world history is when you see it from the perspective of our Creator God!

Myers, Ellen. 1987. Towards a Biblical Creation-based historiography. *Creation Social Science and Humanities Quarterly*. X(1):7.

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