Creation Research Society Quarterly

Haec credimus:

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

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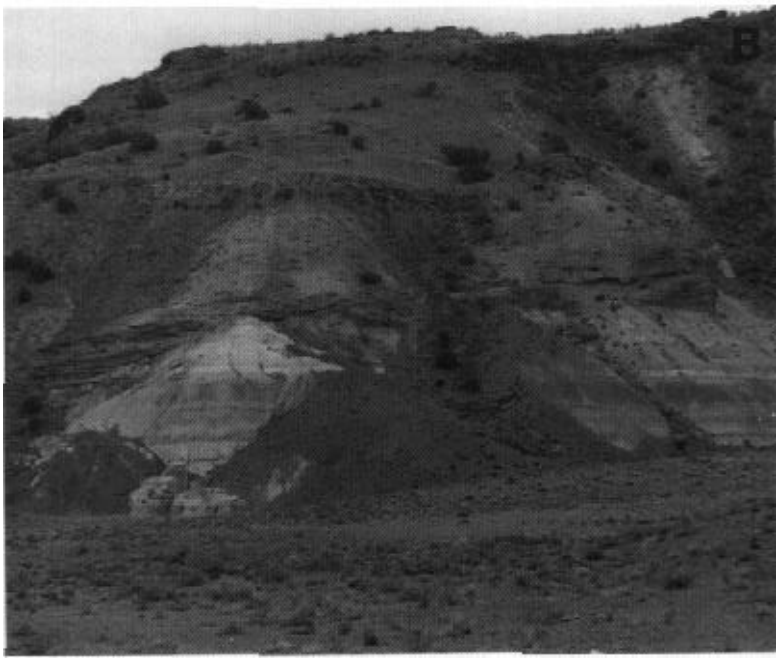

CREATION RESEARCH SOCIETY

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DEPARTMENTS

Cover Photographs

A. Section of Bidahochi Formation at town of Bidahochi, Arizona.
B. Section of Bidahochi Formation at intersection of Navajo Highways 6 and 15 north of town of Bidahochi, Arizona.
C and D. Sections of Bidahochi Formation (Pueblo Colorado Wash) near Greasewood, Arizona.

The lower member of this formation indicates that a vast lake once existed in this part of Arizona. See *CRSQ* 29:18-24. All photographs by Glen W. Wolfrom.

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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.

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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 humans, 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 a historical 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 Savior for all people. Therefore, salvation can come only through accepting Jesus Christ as our Savior.

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Editor's Comments

It is my pleasure as editor to first read the manuscripts for upcoming articles in the Quarterly. We have featured some excellent articles in recent issues and I express my gratitude to the authors. Likewise the peer reviewers deserve a vote of thanks for their work on the papers. Each of you could help the Society by submitting technical notes, letters to the editor and reviewing some books for us. As always, article-length manuscripts are welcome.

All back issues of the Quarterly are available now. Encourage libraries in your area to subscribe to the Quarterly. Let them know that back issues are available in full-sized bound issues or in microform. Your help is needed to increase our circulation. May each of you have a joyful Christmas and successful new year.

Don B. DeYoung

CREATIONIST RESEARCH
(1964-1988)

By Duane T. Gish

Research done by creationists and reported in the ***Creation Research Society Quarterly*** is abstracted. Geological research, genetic research, natural selection studies, taxonomy, other biological research, thermodynamic efforts as well as other physical science research are covered. Creationists have performed excellent laboratory and field work without any support by government funds.

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DEDICATION TO DUANE T. GISH

It is a pleasure to announce that Dr. Duane T. Gish has been elected a Fellow of the Creation Research Society. Dr. Gish was on the original *Team of Ten* who corresponded with Dr. Walter E. Lammerts, concerning the organization of the Society. Dr. Gish also attended a meeting in June 1963 at the home of the late Dr. John Grebe at Midland, Michigan where the general guiding principles of the Society were determined.* He has been a member of the Board of Directors since 1964 and has faithfully attended all Board meetings since that time. Our new Fellow is one of the best known creationists in the world today. Besides his work for the Society, he has spoken all over the globe in defense of creationism.

Dr. Gish received a B.S. in chemistry from U.C.L.A. in 1949 and a Ph.D. in biochemistry from the University of California, Berkeley in 1953. Because of this background, one of his strongest attacks on the evolutionary hypothesis in defense of the creation model has been in the area of the origin of life. He has spoken and written frequently on this topic and he is likely the foremost creationist authority on the subject. During his academic years, he was elected to the Phi Beta Kappa (U.C.L.A.) and Phi Sigma Epsilon honorary societies.

His professional career prepared him for his later role as a creationist advocate. Dr. Gish was a Lilly postdoctoral Fellow in the natural sciences at Cornell University Medical College from 1953-1955 and he was an Assistant Professor of Biochemistry at the same school from 1955-1956. Then he served as Assistant Research Associate in the Virus Laboratory at the University of California, Berkeley (1956-1960). Later he was employed by the Upjohn Co. as a Research Associate in the Department of Hypersensitivity Diseases (1960-1971). From 1971 to the present, he has had a rewarding and eventful career as Associate Director and Vice President of the Institute for Creation Research in Santee, California. He is listed in Who's Who in the West and in American Men of Science.

Dr. Gish has authored or coauthored well over 50 technical papers, monographs and books in his professional career. Topics covered include his field of biochemistry, origin of life studies, and the fossil record. Two of his most popular books are *Evolution: The Fossils Say No* and *Evolution: The Challenge of the Fossil Record.* He has participated in scores of debates with evolutionists. Generally large crowds are present when he skillfully defends the creation model

*For an interesting history of the Society, see Appendix G in Rusch, Sr., W. H. 1991. *Origins: What is at Stake?* Creation Research Society Books. Kansas City, MO.

as the best approach to origins. Even though he has been attacked viciously and unfairly by spokesmen for the evolutionary hypothesis, Dr. Gish remains calm and unruffled. His temperament and sense of humor are well adjusted for the give and take of debate and the controversy that accompanies such activities. He never displays any personal animosity toward his opponents.

Dr. Gish always has been interested in creationist research. He wrote "A Decade of Creationist Research." a review dealing with the field and laboratory work that had been reported in the Quarterly in the first 10 volumes. In 1988 he wrote a pair of articles on the reported research in the Quarterly for the next 14 volumes. These three articles have been published in booklet form *Creationist Research (1964-1988).* Dr. Gish served on the Research Committee of the Society for many years offering suggestions on the various research projects the Society had undertaken.

It is a pleasure to dedicate this issue of the Quarterly to a good friend, a guiding member in the founding of the Society, and a faithful worker for creationism, Dr. Daune T. Gish. May he continue to devote his superb talents to the cause!

Emmett L. Williams

QUOTE

It must be significant that nearly all the evolution stories I learned as a student from *Trueman's Ostrea/ Gryphaea* to *Carruther's Zaphrentis Delanovei,* have now been debunked, similarly, my own experience of more than 20 years looking for evolution among the Mesozoic Brachiopods has proved them equally elusive.

Ager, Derek. March 5, 1976. Presidential Address. *Proceedings of Geological Association.*

A DETERMINATION OF THE SPEED OF LIGHT IN THE SEVENTEENTH CENTURY

EUGENE F. CHAFFIN*

Received 3 March 1992; Revised 15 May 1992

Abstract

A careful computer analysis of data taken primarily by Roemer in the 1670's is performed to determine the speed of light at that date. Data taken personally by the author during 1988-1991 are used as a control, along with data from the Harvard College Observatory taken during 1887-1880. The result is a value for the speed of light in the seventeenth century that was within 0.4% of the modern value.

Introduction

The major moons of Jupiter: Io, Europa, Ganymede, and Callisto, occasionally move into the cone of shadow cast by Jupiter, which is 55 million miles in length. It was noticed by astronomers in the seventeenth century that accurate timings of these eclipses would provide data that could be used to measure longitude. According to historical records (Mirsky, 1970; Anderson, 1956, p. 22) at least one explorer, one who traversed the Canadian northwest, used this method. This apparently was one reason why Cassini, Roemer, and other astronomers at the Paris observatory of Louis XIV kept the accurate records of these eclipses. This paper will not be concerned with longitude measurement, but rather the use of these eclipse timings to determine what the speed of light was in the 1600's.

The eclipse times [the times when Io moves into the shadow (called an ingress) or out of the shadow (called an egress)] provide accurate reference points which, together with computer modelling, enable a person to calculate the speed of light. The observation of these eclipses is possible with even a very small telescope. Galileo was probably the first person to use a telescope to view these moons. This occurred in the early 1600's, shortly after telescopes became widely known in Europe. The Paris astronomers had a long lasting argument about whether the speed of light was actually infinite or just very large. Ole Roemer announced in 1676 that his study of the eclipses indicated that light took 11 minutes to cross the radius of the Earth's orbit. The Cassini family, which controlled the Paris observatory, argued that the irregularities in eclipse times noted by Roemer were to be explained in other ways, and that the speed of light was infinite. While the resolution of this argument had to wait for independent measurements made by Bradley in the 1700's, it did cause a very careful record to be kept.

The bulk of these eclipse records were thought to have been lost during the French Revolution. Books written in the twentieth century even announced that it was so. But heroic efforts by J. H. Lieske (1986a, b), including a trip to the archival institutions in Paris, resulted in a rediscovery of the data. The thousands of eclipse records which he found are now published in the Supplement Series of Astronomy and Astrophysics.

Setterfield (1981) and Setterfield and Norman (1987) have circulated a report giving a statistical analysis of historical measurements of the speed of light. They conclude from the data that the speed of light must be decreasing with time, and give a discussion of biblical records of the creation in the light of this finding. Since the eclipses of Jupiter's moons provide the earliest known data relevant to this subject, the interpretation of these data is of crucial importance. This paper will attempt to show that the data are consistent with a constant value of the speed of light, and are not consistent with a value of the speed of light which was more than about one percent different from the present value. This does not rule out transient or episodic variations (Chaffin, 1990a), but it seems to provide evidence against the idea that the speed of light variation has a "tail" on the plot of speed of light, c, versus time, at least as large a tail as that favored by some recent creationist writers. Before treating the Roemer data in more detail, an example of the "tracking" phenomenon will be provided which is relevant to the speed of light data and the question of whether the speed of light has changed in the last 300 years.

*Eugene F. Chaffin, Ph. D., Bluefield College, Bluefield, VA 24605.

Tracking and Charge of the Electron Measurements

"Tracking" is the tendency for researchers to report an experimental result close to the results of their predecessors. Richard P. Feynman won the 1965 Nobel Prize in physics, along with Julian Schwinger and Sinitiro Tomonaga, for work on the theory of quantum electrodynamics. In one of his books (Hutchings, 1985), Feynman discussed what he called "cargo cult science," referring to a practice of South Sea islanders who used cargo boxes to simulate radios, airplanes, etc. which they had seen during World War II. The simulations were somewhat realistic, but the planes did not fly and the radios did not work. Feynman then launched a discussion of the history of measurements of the charge of the electron as an example of "tracking." R. A. Millikan won the Nobel prize in 1923, partially for his measurement of the charge of the electron.

Millikan's experiments involved measurement of the rate of rise and fall of a charged oil drop between two charged metal plates. Input included a value of the viscosity of air. While Millikan did his original experiments in the winter of 1909-1910, he repeated them in 1917. The absolute value found for the charge of the electron, e, was 4.77×10^{-10} esu (electrostatic units), whereas the modern value is 4.806×10^{-10} esu or 1.602×10^{-19} Coulombs (Millikan, 1917). The chief error in Millikan's experiment was a wrong value of the viscosity of air, as was discovered by Shiba (1932). Between 1917 and the discovery of this error in 1932 other experimental results were reported (Hull and Williams 1925, Wadlund 1928, Baecklin 1928, and Bearden 1931).

Hull and Williams based their results on the random emission of electrons from the cathode of a vacuum tube, known as the shot-effect. Theory related the charge of the electron to the mean square current in a tuned circuit. Wadlund, Baecklin, and Bearden based their results on X-rays. Using ruled diffraction gratings, they measured the precise wavelengths of sharp X-ray emission lines. The knowledge of the wavelengths then made possible the determination of spacings between layers of cubic crystals. This, coupled with other data, enabled a precise measurement of Avogadro's number, N. From the value of the Faraday, Ne, the charge of the electron, e, was obtained. None of these experiments used the erroneous value of the viscosity of air which Millikan used, since the properties of the air were not involved. Nevertheless, the reported results were close to Millikan's. Feynman (Hutchings 1985, p. 342) described it this way:

> If you plot them as a function of time, you find that one is a little bit bigger than Millikan's, the next one's a little bit bigger than that, and the next one's a little bit bigger than that, until finally they settle down to a number which is higher.
>
> Why didn't they discover that the new number was higher right away? It's a thing that scientists are ashamed of—this history—because it's apparent that people did things like this: When they got a number that was too high above Millikan's they thought something must be wrong—and they would look for and find a reason why something must be wrong. When they got a number closer to Millikan's value they didn't look so hard. And so they eliminated the numbers that were too far off, and did other things like that. We've learned those tricks nowadays, and now we don't have that kind of disease.

Feynman did not publish the data or graph he was discussing but I have collected the results and the graph is shown in Figure 1. When compared to graphs of the speed of light versus time circulated by Setterfield and Norman (1987), there is a possible parallel. In both cases there seems to be a trend which may represent "tracking," but the trend might possibly also represent a change of a physical "constant" with time. In the case of the charge of the electron measurements, the revised value of the viscosity makes Millikan's e value larger, indicating that tracking did occur. In the case of the speed of light measurements, the results of this paper seem to indicate that similar data manipulation has taken place.

It is interesting that Herrick (1971, p. 309) found that speed of light data taken from 1927 to 1941 seemed to cluster about 299781 km/sec; a value smaller than the present value, while data taken from 1950 to 1958 clustered about 299791.1 km/see. The increase apparently was coupled to a change to new methods of increased accuracy which occurred about 1950. The standard deviations for the 1927 to 1941 method were about 10 km/see while for the 1950 to 1958 methods they were about 1 km/see.

Calculations Using the Roemer Method

Chaffin (1990b) reported on preliminary calculations of the speed of light at the Second International Conference on Creationism. Readers are referred to that paper for details of the method. An important part of the paper was the use of data taken personally by the author using time signals provided by National Institute of Standards radio station WWV and the author's six-inch Newtonian reflector telescope. This provided a control for comparison with the historical data. Additional data, taken since the 1989 deadline for submission to the conference, are reported in this paper in Table 1. The method of data analysis basically followed the procedure of Goldstein (1975). But since Lieske (1986a,b) published more data than was known to be available at the time of Goldstein's 1975 paper, programs were needed to calculate the positions of Jupiter and Earth for each of the respective eclipse events. Goldstein published such positions based on punched card output provided by P. K. Seidelman, but it was necessary to treat other points. Hence a Fortran program (SATURN.F77) was developed to provide the orbital elements of Jupiter at any given date, based on published results of Simon and Bretagnon (1975a, b; 1978a, b), taking into account the secular variations caused by the planets and the periodic variations caused by the position of Saturn. It was learned, after some troublesome trial and error, that it is absolutely necessary to take into account the Saturn perturbations. A second program, BRETAG.BAS, based on published results of Bretagnon (1980), was written to provide the orbital elements of Earth at any given time. Other programs, based in part on some published programs of Tattersfield (1981), were written to generate three-dimensional heliocentric coordinates of Jupiter and the Earth, together with Jupiter-Earth distances, latitudes of the antisolar point, Jupiter-Sun distances, half widths of Jupiter's shadow, and other necessary data. The programs were validated, to a certain extent by comparison with the

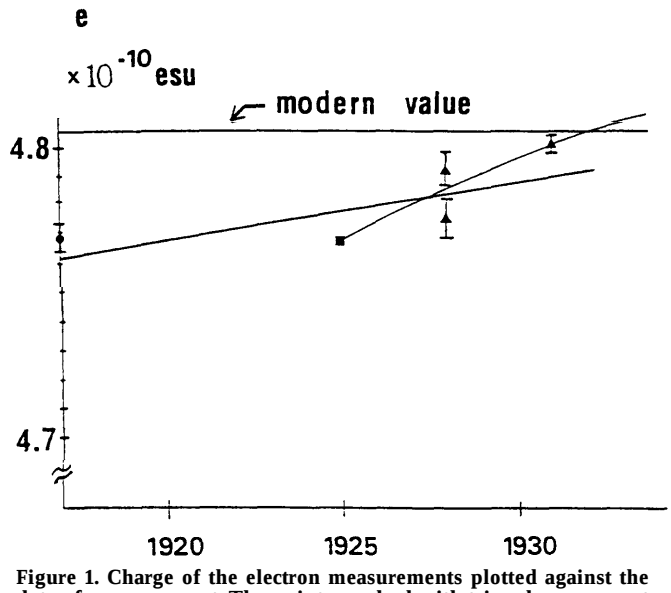

Figure 1. Charge of the electron measurements plotted against the date of measurement. The points marked with triangles represent values measured by the X-ray diffraction method, the square represents the value measured with the shot-effect method, and the circle represents Millikan's 1917 value measured with the oil drop method. The slanted line represents a linear least squares fit to the data. The curve represents a polynomial fit, omitting Millikan's point.

Table I. 1988-199110 Eclipse Data

Date	Julian Date (244+...)	Coordinated Universal Time of Event (UTC)
Aug. 12, 1988	7385.90701 i	9:46:06
Sept. 20,1988	7424.84352 i	8:14:40
Sept. 27,1988	7431.92260 i	10:08:33
Sept. 29, 1988	7433.69260 i	4:37:21
Oct. 6, 1988	7440.77199 i	6:31:40
Oct. 15,1988	7449.62118 i	2:54:30
Oct. 29, 1988	7463.78024 i	6:43:33
Dec. 7, 1988	7502.80804 e	7:23:35
Dec. 25, 1988	7520.50786 e	0:11:19
Jan. 17, 1989	7543.51765 e	0:25:25
Feb. 9, 1989	7566.52816 e	0:40:33
Sept. 9, 1989	7778.82459 i	7:47:25
Jan. 27, 1990	7918.73553 e	5:39:10
Jan. 29, 1990	7920.50558 e	0:08:02
Dec. 6, 1990	8231.90461 i	9:42:38
Mar. 3, 1991	8318.72338 e	5:21:40
April 2, 1991	8348.81347 e	7:31:24

i denotes ingress, e denotes egress.

Astronomical Almanac, published jointly by U.S. and English governmental agencies. This was only possible for the 1988-1991 data. This output then provided the raw data for use with the Goldstein procedure. But improvement of this procedure was found to be necessary.

Improvement of the Goldstein Procedure

Goldstein (1975) used a least squares approach to model the perturbation of Io caused by Europa. The orbit of Io is approximately in the plane of Jupiter's equator; the inclination is only 2 arc minutes. Due to the difficulties involved in taking into account the precession of Io's orbit, the method adopted consisted of considering the 2 arc minutes to be negligible. The same basic method was used in this work. But Goldstein also used "empirically" determined values for the inclination of Jupiter's equator to the plane of Jupiter's orbit. Due to the availability of space probe data, this is no longer necessary.

Null (1976) used Pioneer 10 and Pioneer 11 data to accurately determine the direction in which Jupiter's pole is pointing. Following Lieske (1978, 1980), this work used a computer program (ANGLES.F77) to calculate the inclination and nodes of the plane of Jupiter's equator with respect to the plane of Jupiter's orbit. The program incorporates Lieske's formulas for the precession of Jupiter's pole. Input included the appropriate longitude of the ascending node and inclination of Jupiter's orbit with respect to the ecliptic plane (from SATURN.F77). The program used trigonometric formulas given in the Explanatory Supplement (1961, p. 332) and quoted by Lieske. If the time dependence of the program calculating Jupiter's orbital elements (SATURN.F77) is accurate, then this should represent an improvement. There must be a certain accuracy in this procedure, since the calculations of the eclipse times of Io reported here are successful, even without the empirical adjustments reported by Goldstein. The adjustments Goldstein reported included the values for the radius and eccentricity of Jupiter's disc. These adjustments were also not performed in this work.

Goldstein also allowed the period of Io to be a parameter determined by the least squares fitting. Since we are now interested in the speed of light as a possible variable, proper methodology requires that Io's period be held constant. Therefore, the least squares procedure was modified to hold Io's period constant. This left the amplitudes of the sine and cosine terms, L2 and L3, together with the initial longitude, L0, as free parameters determined by the least squares fitting (the so-called "normal" equations). Goldstein also allowed the perturbation frequency to be slightly adjusted to improve the fit. This frequency is twice the difference in the mean daily motions of Io and Europa. Hence, to conform to proper methodology, this frequency was held constant at the modern value.

These refusals to allow more adjustments to the parameters are a major reason why the standard deviation for the calculated minus actual eclipse times increased from Goldstein's value of 31 seconds to about 40 seconds. The data points are not the same ones, but that is a secondary factor. A 30-40 second standard deviation seems quite reasonable considering that time zones did not exist until the late 1800's, and the Paris astronomers needed considerable skill in adjusting their clocks.

Another outcome of this work is that Goldstein's conclusion that Io's period had changed now seems doubtful. Goldstein (1975) and Goldstein and Jacobs (1986) concluded that Io's mean daily motion increased from 203.48892 degrees / day in the 1670's to 203.488959 degrees / day in the twentieth century. Kieske (1987), working on a model for guidance of the Voyager spacecraft, concluded that Io's period decreased rather than increased. Lieske gave a rate of

$$\frac{1}{n_1} \times \frac{dn_1}{dt} = -0.74 \pm .87 \times 10^{-11}/\text{year}. \qquad (1)$$

Here n_1 is the period of Io. The amount of Lieske's decrease is so small as to be negligible for the purpose of this paper. If the period of 300 years ago is calculated, it may be used only if a value for the period of Europa is also given. But if we assume that the resonance between the motions of Io, Europa, and Ganymede was still holding at the earlier times (Yoder, 1979; Peale, Cassen, and Reynolds 1979), then some reasonable assumptions enable us to find Europa's period. The "resonance" refers to the fact that conjunctions of Io and Europa occur when Io is near its perijove (the nearest point to Jupiter in the orbit) and Europa is near its apojove (the farthest point from Jupiter in its orbit). See Figure 2. This resonance lock means that:

$$n_1 - 2n_2 + \frac{d\tilde{\omega}_1}{dt} = 0. \qquad (2)$$

where n_1 is the mean daily motion of Io, n_2 is the mean daily motion of Europa, and $\tilde{\omega}_1$ is the longitude of the perijove of Io's orbit. According to an equation of Tittemore (1990), the time rate of change of the longitude of the perijove is caused by the oblateness of Jupiter and will remain approximately the same if the percentage change in Io's period is small. Under these conditions, Equation (2) above may be used, together with Lieske's value for n, to find a value for Europa's mean daily motion, and hence a value for

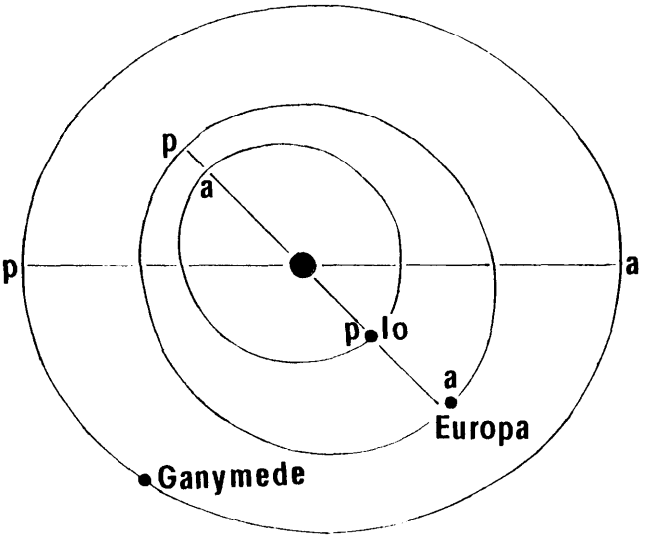

Figure 2. A representation of the elliptical orbits of Jupiter's moons Io, Europa and Ganymede. The eccentricity is greatly exaggerated. Due to the resonance lock between the three moons, Io is at its perijove (p) when Europa is near apojove (a), as shown. Conjunctions of Ganymede and Europa occur when Europa is near its perijove (p), but due to the advance of the perijove effect, Ganymede may be anywhere in its orbit.

the perturbation frequency. This procedure was followed and the speed of light was found to be modified by only 0.02 percent. Therefore, Lieske's small change in Io's period will not significantly alter the value for the speed of light from what is obtained for a constant period.

When dealing with the 1878-1880 Harvard College Observatory data, one must note that the data are photometric data giving the half-brightness times, not the last speck (for ingresses) or first speck (for egresses) times encountered with the other data. The Harvard telescope had a bigger aperture than for the other data, and visual photometry techniques were well developed at Harvard. To treat such data, some comments of Peters (1975) proved helpful. Instead of using the usual procedure for finding the half width of the shadow, given in Goldstein (1975), I used Peters' suggestion:

> The condition for mid eclipse, or the half-brightness point, occurs when light ray from the center of the Sun at t_1 is tangent to the planet or ring at t_2 passes through the center of the satellite at t_3.

Results

The procedure used two final programs. The first used normal equations, a "least squares" procedure, to find L0, L2, and L3, three parameters which specify the perturbation in Io's longitude caused by Europa. For each set of data, 1671-1673, 1676-1678, 1878-1889, and 1988-1990, the relevant values of these three parameters were found. These values were then inserted into a second program, along with an assumed value for the speed of light, to find the sum of the squares of the residuals between the calculated time and the actual measured time of eclipse for each data point. Thus the 1671-1673 data set included 43 points from Lieske's published list of the results, the 1676-1678 data set included 21 points, the 1878-1880 Harvard data set included 52 points and the 1988-1991 data taken personally by the author included 17 eclipse timings, given in Table I. Table II presents some of the parameters which were found. Figure 3 shows the speed of light versus time. The two points for the 1600's are 0.297% and 0.464% below the present value of the speed of light. While the point for 1878-1880 is 0.77% high at 302100 km/see, this cannot be a real effect since Michelson obtained 299910 km/see ± 50 km/see in an 1878 experiment. Part of the reason for the larger error for the 1878-1880 point may be connected with the ill-defined point of occurrence of the theoretical half-brightness point, as noted by Arlot, Morando, and Thuillot (1984). Even though those data were based on photometric methods, with a potential for great accuracy, there were no WWV signals or other accurate timings available in the 1870's. The 1988-1990 data are based on first and last speck times observed with a six-inch Newtonian reflector, so that even though the WWV signals were used, the result from the program is 0.297% low.

Table II. Results of the Computer Work

Data Set	L0	L2	L3	Speed of Light (km/see)	Percent Change
	----------(degrees)----------				
1671-1673	179.84098	.00127	-.51400	298900	-.297%
1676-1678	179.48756	.39856	-.12.348	298400	-.464%
1878-1880	179.89675	.20947	.59953	302100	+.770%
1988-1991	180.27384	-.27682	.52162	298900	-.297%

It is apparent that the results do not speak in favor of any variation of the speed of light over the last 300 years. Norman and Setterfield (1987) attributed a value of 3.076×10^8 m/see to Roemer's 1675 data point. This value is 2.6% higher than today's value, and is a result of a private communication with Goldstein. Only Goldstein et al. (1973) dealt with the possible variation of the speed of light, not the 1975 paper which was concerned with possible variation of Io's period. Since the 1975 paper included some improvements over the 1973 paper, it was necessary to communicate privately with Goldstein to obtain the 2.6% The results presented here show that the 2.6% was not real, and that any real effect that might be present must be smaller than about 1%, at most. This is a change from what Chaffin (1990b) said, but the improvements in the computer analysis forced a fresh analysis.

In the course of this work a procedure was tested in which the value of the speed of light was also altered in the original program for finding the parameters L0, L2, and L3 giving the perturbation of Io's longitude caused by Europa. This led to an iterative procedure in which the value of c "marched" away from the original value, finally settling down at values given in Table III. If we take the Table III values seriously, then the speed of light was smaller in the 1670's, not larger. But Table III does not seem to represent any real effect. Rather, if the object of the work was to see if the 1670's Roemer data were consistent with the present value of the speed of light, then the present value should be assumed in finding the parameters for the perturbation of Io's longitude caused by Europa. The fact that all of the speed of light values

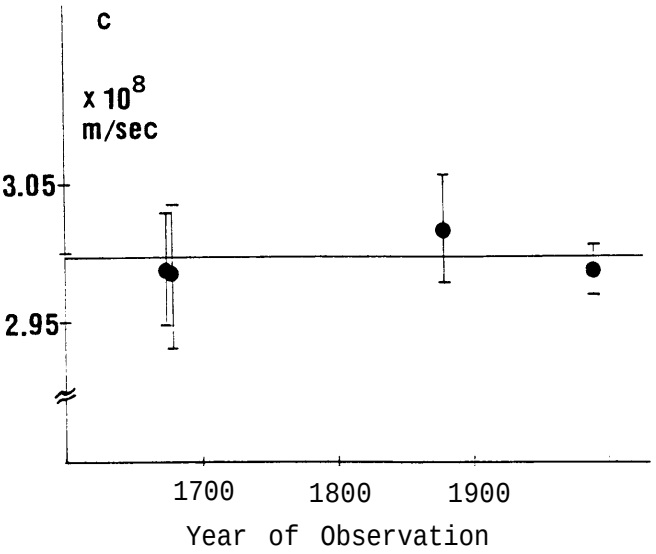

Figure 3. Results of the present calculations of the speed of light for the author's 1988-1991 data, the Harvard data of 1878-1880, and the Roemer method data for 1671-1673 and 1676-1678. The error bars represent the standard deviation found from the residual s for the actual versus calculated eclipse times. Since all of the error bars overlap the horizontal line, the results are consistent with a constant value for the speed of light over the last 300 years, and any systematic errors must be small.

came out within 0.77% of the present value then verified the correctness of this procedure. But, for completeness, Table III is presented.

Table III. Speed of Light Determined by the "Marching" Procedure

Data Set	Speed of Light (km/see)	Percent Change
1671-1673	278500	-7.10%
1676-1678	289000	-3.60%
1878-1880	321000	+7.07%
1988-1991	291000	-2.90%

Comments on the Heat Flux from Io

Lieske's estimate given in equation 1 for the rate of change of Io's period now seems more credible than Goldstein's, since the improvements to the Goldstein procedure noted here seem to negate the validity of the former calculations. Lieske (1987) showed that the Goldstein (1975) and Greenberg, Goldstein, and Jacobs (1986) values were about a factor of 60 too big. Tidal friction generated inside Io is a major factor in maintaining the resonance lock of Io, Europa, and Ganymede noted before and in Figure 2. This tidal friction generates heat, and some would link this to the fact that the Voyager spacecraft found active volcanoes on Io. However, the measured infrared heat flux is too large by a factor of 10 to be accounted for on the basis of tidal friction (Lieske, 1987). Thus, on the basis of conventional cosmological and solar system models, crustal heat loss rates are anomalously high. On the other hand, this definitely provides evidence for recent creation models.

Acknowledgement

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Computer Programs

Anyone desiring a copy of the computer programs mentioned in this paper should send the author either a 5.25 or 3.5 inch disk (IBM compatible format) together with the request.

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VARVES — THE FIRST "ABSOLUTE" CHRONOLOGY
PART II —Varve Correlation and the Post-Glacial Time Scale

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Abstract

The varve correlation procedure is described and shown to depend excessively upon poorly constrained variables, to encounter too many difficulties, and to be theoretically unsound. Post-glacial "varves" from the Angermanalven River Valley in Central Sweden pose additional problems. Thus, varve chronology is not scientifically sound.

Introduction

In Part I (Oard, 1992) I discussed the historical development of the first "absolute" chronology. It was shown that the vital assumption of seasonal deposition for each of the two "varve" couplets is seriously open to question. Other mechanisms can deposit varve-like layers rapidly, and there is no unequivocal method of distinguishing between mechanisms in most circumstances. Part II continues with an analysis of the "varve" correlational procedure. The post-Ice Age "varves" that show the time since deglaciation as 9,000 years will be critically scrutinized.

Can Varve Sections Be Correlated?

The correlational procedure was briefly described in Part I. As a result of this procedure, De Geer and his colleagues originated a chronology that indicated the ice receded northward through Sweden for 4,000 years. In the same vein, Liden by correlation of the Angermanalven River rhythmites from the last deglaciation "varve" downstream to the modern delta, obtained 9,000 years of post-glacial time. Thus, the total time is 13,000 years. By similar varve correlations in New England, Antevs (1922) estimated the ice receded 400 km up the Connecticut River valley in about 4,000 years. How reliable are these correlations? This section will examine the deglaciation varve chronology, while the next section will analyze the post-glacial sequence.

A close examination reveals many problems with the correlation procedure. One problem is that each varve section actually represents an *average* of many individual varve profiles from the same locality (Fromm, 1970, p. 166). Antevs (1925a, p. 120) explains why this procedure is necessary:

> All individual curves were first matched and corrected for number of varves. If, for example, out of three measurements two agreed, but one had one varve less or more than the others, the exact location of the mistake was determined and the curve corrected by dividing one varve in two or uniting two varves in one, so that this curve agreed with the two others. Then the curves or such parts of them as included undisturbed varves of normal variation and thickness were selected for constructing the normal curve, and those curves were discarded that showed great difference in thickness from the majority or poor agreement in the shape of the curve.

The problem with this procedure is that it can easily be used to adjust the number of varves and the thickness of each couplet to enhance the correlation with other varve sections. One must remember that unconscious manipulation (or even conscious massaging) is probably a norm in science (Gould, 1978). A strong reinforcement syndrome acts to make data generally agree with either previous results or preconceived ideas (Oard, 1985, pp. 178, 179).

Once each normal or "type" section is constructed, all the sections are *visually* matched in the direction of ice recession. Distinct varves or unique varve sections aid the correlation. The upper portion of one section should agree with the lower portion of the next up-glacial section, which may be several miles north. In this manner a *floating* chronology is constructed for a large region, such as Sweden. This method is similar to the procedure that was used in constructing the bristlecone pine chronology in the southwest United States and the oak chronology in Europe. Figure 1 illustrates how early workers believed varves were deposited. Each couplet is evenly spread down the lake each year as the ice sheet slowly retreated northward. To correlate the varve sections, the top of exposure 1 is matched to the bottom of exposure 2, etc. until a year-to-year chronology for thousands of years is built.

Although De Geer expressed extreme confidence in his varve chronology constructed by correlation (see

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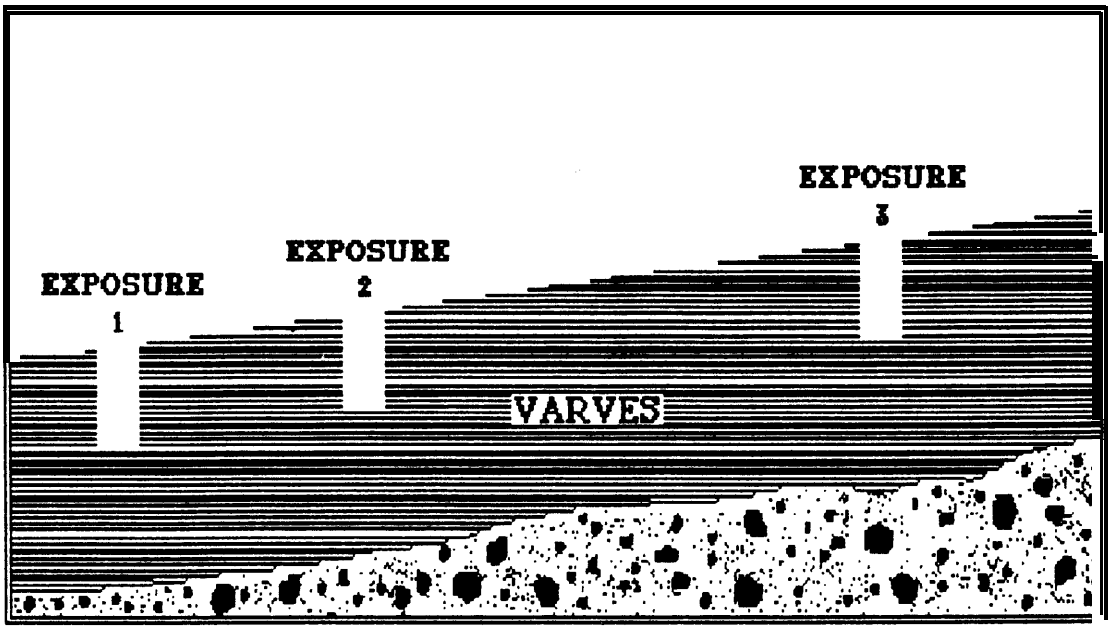

Figure 1. Schematic diagram illustrating "varve" correlations from three exposures. By measuring the couplet thickness pattern, the top portion of exposure 1 should match the bottom and middle portion of exposure 2. Redrawn from Nelson, 1948 by David Oard.

Part I), difficulties have surfaced (Lundqvist, 1975; Ringberg, 1979; Stromberg, 1983). This is why the Swedish Time Scale has been under revision since the 1940's (Stromberg, 1983, p. 104). The revision is now close to completion.

Many of the problems in correlating varve sections are illustrated by the Swedish rhythmites. The rhythmites vary in sharpness, depending upon how fresh or brackish the water was in the Baltic Sea area. Apparently, the water was fully fresh at times, and at other times it was sea water. As a result, investigators have trouble even delimiting a couplet (Stromberg, 1983, p. 100). This is especially the case in the Stockholm-Uppsala area, De Geer's first study location (Stromberg, 1985a, p. 103). In some areas, rhythmites are missing, leaving the investigator the option of either interpolating or correlating sections around the area. Frequent sliding and slumping of the beds also have apparently occurred, complicating correlation. Slides and slumps are difficult to detect in narrow cores, which researchers mostly rely on today.

An accurate correlation of varve sections is difficult because few varves can be traced any significant distance before they change (Ringberg, 1979, p. 213). Continuous horizontal exposures in which to analyze these changes are rarely available in the field. Stromberg (1983, p. 104) states that correlations in the past have been poor, despite the enthusiasm of many investigators. De Geer's chronology was not as continuous as thought, and in fact he connected two large areas by using a varve series *outside* Europe (Schove and Fairbridge, 1983)! Although long distance correlation in Sweden is sometimes claimed (to the astonishment of some investigators), even very short distance correlations can be reckless. This is why varve sections are now taken much closer together, and even these measurements sometimes are difficult to correlate (Stromberg, 1983, p. 97). Stromberg (1983, p. 98) shows a picture of "varves" in a two-foot-wide-pit. The sublayers occasionally thicken and thin and two pinch out horizontally, just in a small pit. How could the varves in this pit be correlated any distance?

Several localities in Sweden display unique problems. For instance in the vicinity of the Fennoscandian moraines of central Sweden, it appears that ice sheet oscillations left a highly confused tangle of sediments that have been difficult to interpret with varve correlations (Stromberg, 1985b). This indicates another questionable aspect of varve analysis. Glaciers usually retreat and advance in the short term while receding over the long term. Therefore, rhythmites should show disturbances in many areas besides the Fennoscandian moraines. When questioned about the lack of evidence for glacial plowing in most areas of Sweden, investigators simply replied that during long-term retreat the advances were very small and did not disturb the varve sequences (Olsson, 1970, p. 222). This explanation seems suspect for a 4,000 year ice sheet retreat. Instead, it suggests that the rhythmites are not annual layers deposited near an ice sheet. The lack of sediment disturbance by an ice sheet may also indicate either rapid deglaciation or a floating ice sheet over the lowlands of Sweden.

As it happens, "In reality only a few varve sequences contain the 'correct' number of varves . . . It is important to consider that correlations agree with other geological criteria in the area investigated" (Stromberg, 1983, pp. 100, 101). Thus, "varve" correlations, like most if not all geochronological methods, are subject to circular reasoning and the reinforcement syndrome (Oard, 1985, pp. 178, 179). One of these geological criteria is the direction of glacial striae. Application of the criterion assumes that all the sediment forming the varves was derived from the melting glacier. As we shall see, this is certainly not true for ancient Lake Hitchcock and may not be true for Swedish pro-glacial lakes either.

Another geological criterion is very likely the preconceived general model of slow deglaciation (Fromm, 1970, p. 166; Lundqvist, 1975, pp. 52-54). Although the varve correlations in Sweden significantly shortened previous estimates of the time since the ice began to melt (Antevs, 1925b, p. 283), the results still indicate slow melting and 9,000 years of post-glacial time. Referring to varve correlation in the Connecticut Valley, Antevs (1922, p. 95) states: "The geochronological studies [varve correlations] confirm the little which was known about the rate of the ice retreat . . ." Thus, varves "confirmed" the belief of slow deglaciation in New England.

The varve correlations are rather crude and in my opinion subjective. In Part I of this paper a recent varve correlation from southeast Sweden was shown. This correlation is one of two alternatives, which differ by 85 years (Ringberg and Rudmark, 1985, p. 109). Some features correlate well, but in my opinion the matching is imprecise. Varve correlations from other areas appear better, for instance those of Antevs (1922) for ancient Lake Hitchcock. However, Antevs' correlations very likely are not correct, as will be discussed below. Referring to the varve sections near the central Swedish coast north of Stockholm, Lundqvist (1975, p. 48) states:

> The general experience of the present author from varve connections in the Ljungan region is that even much better connections than the ones from Forsa can be obtained between limited parts of varve series in a way that is indisputably wrong. For example, a part of one varve series may be 'indisputably' connected with two or even more different parts of another series. In other instances, connections which give a completely impossible picture of the deglaciation can be made in this way.

Lundqvist (1975, p. 52) also relates that two varve chronologies from the same region could not be brought into agreement, although some of the "varve" sections making up the two chronologies must have been taken from nearly the *same* spot.

Stromberg (1985a) summarizes differences between the old correlations and the results of the revised Swedish Time Scale. North of Stockholm the revised scale added about 10 percent more time. To the south of Stockholm, many hundreds of years have been tacked onto the old chronology. These changes from De Geer's old "absolute" and "exact" chronology show how inaccurate varve correlations in Sweden were, and likely still are.

Lake Hitchcock rhythmites in New England also underscore many of the problems in varve analysis encountered in Sweden. Antevs (1922) correlated "varve" sections northward along the length of the lake. From his chronology he claimed that the Laurentide ice sheet took 4,400 years to melt this distance. Although the slow melting rate fits quite well into uniformitarian deglaciation ideas, many difficulties are inherent in his correlational procedure. Several of these difficulties have not been mentioned yet and illustrate that varve correlation has severe theoretical difficulties.

The varve layers in Lake Hitchcock sediments vary considerably (Ashley, 1972; 1975). For instance, the rhythmic couplets range widely in thickness, from 1 cm to 75 cm (Flint, 1975, p. 125). The coarse sublayer sometimes is not laminated, and when laminated it may include as many as 40 laminae. The layers vary significantly between localities and thin or disappear over basement irregularities, a sign that deposition was primarily by underflows or turbidity currents. And as previously stated, the number and thickness of each varve section must be derived from many measurements.

Although admitting the "varves" have not been proven annual, Ashley (1972; 1975) believes the Lake Hitchcock rhythmites are nevertheless annual, based partially on their similarity to the Swedish "varves." But since at least the silt layers were deposited by underflows and turbidity currents, the layers may not be annual. As discussed in Part I, underflows and turbidity currents over a one year period should deposit many layers, especially in a narrow lake with sediment entering from the sides.

Antevs (1922) theoretically misconstrued how lake rhythmites formed, and this misconception influenced his correlations. He believed each couplet was formed by the settling from overflows of both silt in summer and clay in winter. Thus, each couplet would extend a great distance southward down the lake and change thickness slowly (Gustavson, 1975, p. 249). This theory of varve formation, which is illustrated in Figure 1, is now known to be only partially true at best. The coarse-grained layer is formed mainly by underflows that thin much more rapidly with distance from their source than Antevs believed (Smith, 1978; Smith, Venol, and Kennedy, 1982; Smith and Ashley, 1985, p. 180). So correlating varve sections that are separated too far is theoretically questionable. Antevs' (1922) correlations for ancient Lake Hitchcock averaged about 3 miles apart, but many were separated by more than 10 miles. These distances are too far for a reasonable coherence in the couplet pattern.

Modern research also shows that lake rhythmites vary across the width of a lake, adding more variance to the rhythmites. Underflow and turbidity currents are often linear or lobe shaped, being thickest along the axis of flow and thinner along the flanks (Smith and Ashley, 1985, p. 184). The Coriolis Force, caused by the earth's rotation, turns interflows and overflows to the right in the Northern Hemisphere. As a result, laminae formed by these flows, including the clay layers, are thicker on the right side of the flow direction in Northern Hemisphere lakes (Sturm and Matter, 1978, p. 148; Smith, Venol, and Kennedy, 1982; Smith and Ashley, 1985, pp. 178-180). The Coriolis Force is effective even in relatively small lakes. These forces would be acting not only in ancient Lake Hitchcock, but also in the old Swedish lakes.

If all the above problems were not enough, one further problem concerning the Lake Hitchcock rhythmites throws varve correlation theory into disarray. It has been discovered that very few of the couplets derive from the melting ice sheet. All the couplets, except for probably the bottom rhythmites, are nonglacial, collecting sediment from the deglaciated basins to the west and east of the lake (Ashley, 1975, p. 306). This is illustrated in Figure 2. Large deltas and cross-bedded silt layers in glacial Lake Hitchcock sediments

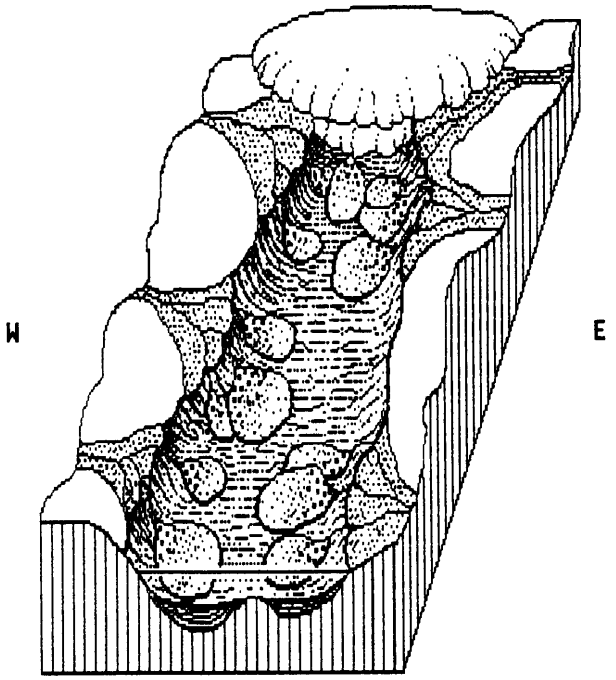

Figure 2. Schematic diagram illustrating a portion of ancient Lake Hitchcock as the ice retreats northward up the valley. Note the sediment mostly enters the lake from the east (E) and west (W), forming delta deposits and rhythmites. Redrawn from Ashley, 1975 by Dale Niemeyer.

show an easterly or westerly current direction (Gustavson, Ashley, and Boothroyd, 1975). The rhythmites connect to these deltas, and the coarse layer thins with distance from the delta.

Although a new article, based on rhythmites from one location in the Connecticut Valley, has defended Antevs' Lake Hitchcock correlations (Ridge and Larsen, 1990), north-south correlation is unwarranted. If the rhythmites were mostly deposited from the sides of the lake, how can varves be correlated northward 400 km up the ancient lake? In view of this information and the common erroneous correlations at both short and long distances, Antevs' (1922) correlations must be incorrect. In discussing Antevs' northward correlations, Ashley (1972, p. 83) agrees: "In my opinion, the method of visually matching curves drawn from varve tapes, which was so successful in Sweden, is unreliable for the Connecticut Valley." It is very likely unreliable in Sweden as well.

Post-Glacial Varves

I have analyzed the correlation procedure for presumed deglaciation rhythmites in Sweden and the Connecticut River Valley. In view of all that has been written so far, I shall briefly examine the post-glacial rhythmites from central Sweden. Apparently, the Angermanalven River Valley in central Sweden is the only area that can potentially lead to a post-glacial chronology (Cato, 1985, p. 117). From an analysis of rhythmites in the Angermanalven River Valley, post-glacial time was calculated to be 9,000 years (Cato, 1985; 1987). Is this deduction any more accurate than the assumptions of annual couplets or exact correlations in the deglacial portion of the Swedish Time Scale?

Before answering this question, it must be understood how these rhythmites formed and how geologists correlated them. As the ice sheet in the area melted, glacial rhythmites were deposited all through the Angermanalven River Valley. The land was approximately 250 meters lower than it is at present, based on the highest Baltic shoreline. As the land rose isostatically, the river delta prograded seaward. Sediments were transported down the river to the delta and then deposited in the brackish estuary. Because the estuary was probably deep, rhythmites formed near the estuary mouth. These river rhythmites were deposited on top of the glacial rhythmites, and supposedly can be differentiated from them. By correlating these post-glacial rhythmites downvalley from the point the ice last melted (the zero point), Liden developed a chronology to the present. Figure 3 is a schematic illustrating the varves in the Angermanalven River Valley. A vertical core through these sediments would reveal, starting from the top down, river delta deposits, followed by the river varves of interest, then possibly a thin layer of fjord clay, and finally the deglaciation rhythmites.

All the problems encountered in correlating lake rhythmites also occur with these river rhythmites as well. However, further problems are inherent in analyzing the river rhythmites. First, the river rhythmites are very thin (Antevs, 1925b, p. 281), and thin couplets are notoriously difficult to correlate. Second, the two sublayers in each couplet showed only slight differences in grain size and color (Antevs, 1925a, p. 5; 1925b, p. 281). How can the annual layer sequence possibly be determined from such non-distinct laminations? Liden even believed, at least in 1911, that the clay layer was deposited in the spring floods, the opposite of deglaciation rhythmites. Moreover, rivers should provide multiple pulses of sediment; there is at least a diurnal discharge variation and longer-term fluctuations caused by weather regimes. At first, Liden established the post-glacial period at 6560 years, but later he stretched the period to 8800 years (Antevs, 1925b, p. 282). This illustrates the subjectivity of the correlations.

It would be nice to examine Liden's work. However, he never published any varve diagrams or correlations. He only published a brief summary of his conclusions in 1938 (Schove and Fairbridge, 1983; Cato, 1985; 1987). According to Cato (1987, p. 5), Liden's detailed work was ready for publication in 1915. Cato has analyzed Liden's data and the manuscript has been *in press* since at least 1985 (Cato, 1985; 1987, p. 5). According to Mats Molen, a Swedish geologist and creationist, the manuscript will not be published until about 1994 (personal communication). Eighty years is a long time to withhold the publication of crucial geochronological data upon which so many studies and other chronologies are based! I am surprised investigators have used the Swedish Time Scale without first examining the basis for the crucial link between the present and the deglaciation rhythmites. Liden's study should be interesting when (or if?) it is published.

Besides the problems of determining the annual couplet and correlating these couplets downstream, two additional problems were encountered in developing the post-glacial time scale. First, the beginning

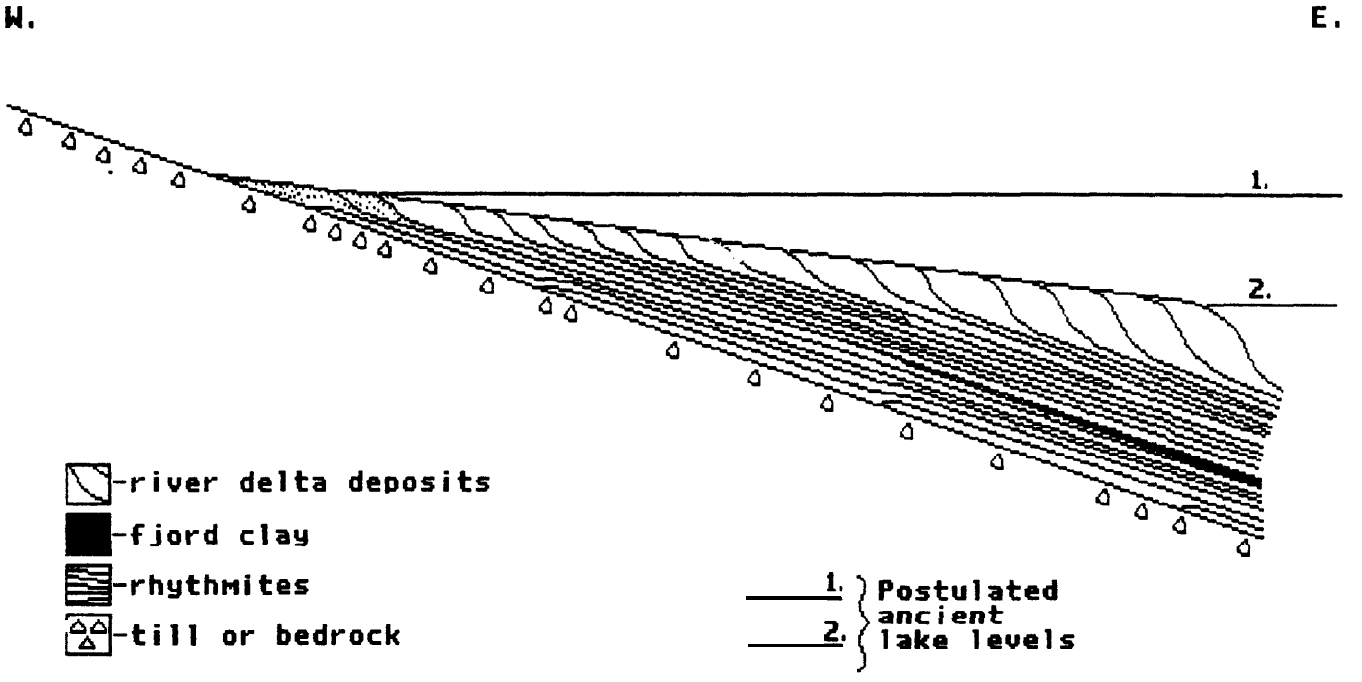

Figure 3. Typical section through the valley sediments along the Angermanalven River Valley, central Sweden. The fjord clay (black) supposedly separates the deglaciation "varves" below from the post-glacial river "varves" above. East (E), West (W). Redrawn from Cato, 1987 by Dale Niemeyer.

of the sequence needed to be tied to the end of the deglaciation sequence developed by De Geer and colleagues. This is referred to as the "zero year" matching. Second, Liden could not connect the youngest rhythmite sequence to the present, since the present rhythmites were underwater. The technology for taking underwater cores in water about 100 meters deep had not been developed at that time. His youngest core was taken about 12 meters above sea level. So to connect that core to the present, he assumed a shoreline uplift rate of 1.25 cm/yr, and hence extrapolated 980 years to connect it to the present.

The connection of the 4,000 year deglaciation chronology to the 9,000 year post-glacial chronology was difficult. De Geer failed more than once to make this connection, but he finally accomplished this by matching "drainage varves" from the Angermanalven River to a river farther south. Drainage varves are very large "varves," up to several meters thick, and assumed to result from the breaching of an ice-dammed lake upstream forming a "jokulhlaup." Several authors are rather suspicious of the mechanism for these "drainage" varves (Lundqvist, 1975, p. 49; Cato, 1987, p. 7). Drainage varves appear now and then in other "varve" sequences. They are not unique and miscorrelation is possible. They could easily be large turbidity currents. Complications developed in making the deglaciation/post-glacial connection when two estimates of the "zero year" in the post-glacial chronology were 80 varve years apart (Tauber, 1970, p. 175).

The connection of Liden's post-glacial chronology to the present has been the subject of intense research over the years. Investigators considered that Liden's postulated isostatic uplift rate of 1.25 cm/yr from his youngest varve sequence to the present was too high. The uplift rate currently is about 0.85 cm/yr, but was higher in the past, since isostatic uplift rate presumably decreases at a logarithmic rate. After recent coring of the river sediments a little upstream from the current delta, as well as in the deep, slightly brackish estuary, and after many difficulties, investigators have added another 365 years to Liden's extrapolation to the present. Hence, the revised Swedish Time Scale has expanded even more. The varve correlations I have seen that establish this connection (Cato, 1987) look about as rough as other varve correlations.

Summary and Discussion

There are many problems in correlating varve sections. Each varve section is actually an average from one locality—a highly subjective procedure. De Geer's "exact" chronology was found to contain innumerable errors. Swedish geologists have been revising his chronology since the 1940s. The postulated mechanism of "varve" formation, formulated by De Geer and Antevs, is not theoretically sound.

The post-glacial time scale from the Angermanalven River contains many of the problems previously discussed in regard to the deglaciation time scale. In addition, these "varve" couplets are very thin and the supposed seasonal layers are little different from each other, making diagnosis difficult. To make matters worse, these "varve" sections have never been published.

In a creationist post-Flood model of the Ice Age, "varves" would be laid down rapidly during catastrophic melting of the ice sheets (Oard, 1990, pp. 109-119). The rhythmites now forming in Muir Inlet in Alaska may be a more suitable analog of the process than observations from modern lakes. As the area became rapidly deglaciated, sediment influx would have waned. Some pro-glacial lakes, especially in North

America, undoubtedly lingered into the very beginning of the post-glacial period. These lakes would be dammed for awhile by moraine or other debris at their southern end. With time, some would be breached and catastrophically drain. Other lakes still linger, like the Finger Lakes in central New York. Rhythmites and/or varves are still being formed in several of the Finger Lakes at present (Mullins and Hinchey, 1989). Hence, some of the rhythmites near the top of the rhythmite sequence of ancient pro-glacial lakes may be annual or close to it.

This analysis of supposed varves can be further applied to other claimed varves in the geological record—for instance, pre-Pleistocene "glacial" varves, post-Ice Age lake rhythmites, the Green River "varves" of the Colorado Plateau, and the claimed varves in the bottom sediments of the Black Sea. Since tree ring chronologies have been constructed in a similar fashion as "varve" correlations, I wonder if similar difficulties were also encountered with the former.

The Swedish Time Scale was the first "absolute" time scale. Since then, other "absolute" time scales, such as the Carbon-14, K-Ar, U-Pb, and Rb-Sr methods, have proliferated. These other radiometric time scales likely have just as many problems as the varve chronology. Creationists have indeed found many difficulties with them, but much more needs to be done. The recent work of Austin (1992), in which a Rb-Sr isochron date for a "Pleistocene" basalt flow was older than a "Precambrian" basalt in the lower Grand Canyon, is another significant step.

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QUOTE

A warning is necessary, however, since the paleontologist uses evolutionary models to work out phylogeny, he may devour his own intellectual flesh. Thus, a theory of linear evolution yields linear phylogenies which support a linear theory of evolution. Some major works on evolutionary theory should bear the sign cave canem.

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QUOTE ON VARVES

Before radiocarbon was used, other methods, particularly the study of varved clays, were used. This method is much like the study of tree rings, as the varves are assumed to be yearly deposits in glacial lakes. The dark part of each varve is deposited during the summer, and the light part in the winter. . . . Thus the thickness of varves records the climatic conditions, and the sequence of relative thicknesses is correlated from lake to lake until the life span of the glaciers is covered; then the total number of varves is counted. Apparently due to error in correlation, this method gave too high an age estimate.

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MARK TWAIN ON SCIENCE (ESPECIALLY GEOLOGY)

In the space of one hundred and seventy-six years the lower Mississippi has shortened itself two hundred and forty-two miles. This is an average of a trifle over one mile and a third per year. Therefore, any calm person, who is not blind or idiotic, can see that in the Old Oolitic Silurian Period, just a million years ago next November, the Lower Mississippi River was upwards of one million three hundred thousand miles long, and stuck out over the Gulf of Mexico like a fishing rod. And by the same token any person can see that seven hundred and forty-two years from now the Lower Mississippi will be only a mile and three quarters long, and Cairo and New Orleans will have joined their streets together, and be plodding comfortably along under a single mayor and a mutual board of aldermen. There is something fascinating about science. One gets such wholesale returns of conjecture out of such a trifling investment of fact.

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IN MEMORIAM

FRANK L. MARSH

October 18, 1899- July 14, 1992

Dr. Marsh was on the original Team of Ten that corresponded and worked together to form the Creation Research Society. Many of his articles have appeared in the Quarterly and the Society distributes his book, *Variation and Fixity in Nature.* He was elected as a Fellow of the Society in 1976.

Frank Marsh was born on a farm in northwest Illinois and as soon as he was able, he helped care for the large yard, vegetable and flower gardens then at 12 years of age worked in the fields. During childhood his most pleasurable diversions were botany, bird study and the collection of butterflies and moths. He graduated as valedictorian in 1921 from Fox River Academy and two years later received a premedical diploma from Emmanuel Missionary College. He was accepted in the Seventh-day Adventist medical school in Loma Linda. Unable to meet the entrance fees for medical school, Dr. Marsh entered nursing school at Hinsdale Sanitarium and Hospital where he worked. He graduated in 1925 as vice-president of his class and was founding editor of the yearbook, *The Flouroscope.*

In the next few years he continued his education, receiving a B.A. in science and English at Emmanuel Missionary College (EMC). During this period he married Alice Garrett and taught at EMC Academy while Mrs. Marsh completed her college work. Also Dr. Marsh took more courses at EMC and in 1929 received a B.S. in science with a Bible minor. He received a M.S. degree in zoology from Northwestern in 1935. His research involved tracing five levels of parasites on Cecropia moths and about 50 years later his work was published in *CRSQ!* He taught at Union College in Lincoln, Nebraska for 15 years. During this time he worked on a doctorate at the University of Nebraska where he received a Ph.D. in 1940 with a major in plant ecology.

In 1950 Dr. Marsh became head of the biology department at EMC. In 1958 he accepted an invitation to work at the new SDA Geoscience Research Institute and labored there for seven years until he was 65 years old. He then taught biology at Andrews University for six years until he retired in 1971. Throughout his professional career as well as in his retirement years, Dr. Marsh maintained a very active writing schedule. His articles and books in the defense of the creation model of science have proven helpful to many people. He died of congestive heart failure at the age of 92. He is survived by his wife of 65 years, two children and four grandchildren. The Society has lost a good friend and creationism a faithful worker.

Emmett L. Williams

ERROR AND WORSE IN THE SCIENTIFIC LITERATURE

RICHARD D. LUMSDEN*

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Abstract

When is anything clearly and conclusively demonstrated in science? Most scientists, whether evolutionists or creationists, appreciate that ideas are always being tested, findings evaluated, and that few theories are ever proven, at least to the extent that they become scientific laws. Of late, however, there have been issues of veracity that have gone beyond the traditional academic uncertainties. Some of the current stumbling blocks to science as a search for truth are reviewed. Not all are unique to the purely secular scientific establishment.

Introduction

Uncritical deferral to the putative "facts" of natural science over against biblical literality suggests naivete about the "real world" of the practice, interpretation, and publication of secular scientific investigation. However, a hypercritical view of science may engender recourse to unsound metaphysical principles.

While a critical evaluation of the scientific literature (secular and creationist) is always in order, when attempting to sift fact from fiction, creationists *must* be as rigorous in guarding themselves against committing biased selectivity, where data would be fitted to their models, as the evolutionists *should* be. The hypothesis should not become a self-fulfilling prophesy. Promulgating error through teaching (which includes publication), irrespective of its source or the teacher's (author's) purpose, is biblically condemned.

Dr. George Matzko's (1991) note addressing the veracity of the science literature we read—therefore to what extent one should take it at face value—is timely and provocative. While the cited account of the stresses and strains besetting "big time research" (Lepkowski, 1991) suffers from some hyperbole, the pressures generated in the secular academic scientific establishment and their effect on research quality are real enough. Critiques such as Matzko's (1991) and Lepkowski's (1991) are always in order. For that matter, so are those of Van Till et al. (1988, 1990) and Ross (1989), which would take strict creation science to task. Debate is the way of science. However, one must be careful not to build straw men in the process.

The Ivory Tower

Prior to my present academic affiliation, I served as a grant-funded, grant-dependent and therefore grant-seeking professor and administrator at a major secular university for more than 20 years. At such institutions, research productivity, measured chiefly by the number of papers published per unit time, and, to a degree, the dollar amounts of extramural funds acquired to support it, has become the major criterion, *de facto* if not *"de jure,"* for tenure and promotion in the science faculty. This is not to say that the universities eschew quality for quantity. The two are not mutually exclusive. Reviewed publication and grant support are generally taken as evidence of peer recognition of quality scholarship, and justifiably so in most instances.

However, since doing research today takes the fiscal resources of Solomon (if not his wisdom), the institution's administrators encourage principal investigators to pursue grant support aggressively. They take what they can get, accept what it takes to get it, and grantsmanship becomes a valued commodity in its own right. Then, with success, administrative "encouragement" devolves to expectation. Graduate programs, in particular, have come to rely heavily on extramural funding. Moreover, grants, besides supporting the research effort often include coverage of a percentage of the principal investigator's academic salary and an overhead allowance (the rate for which can be 50% or more of direct costs) to the institution. Academic departments may find themselves justified (or otherwise) on the basis of their grant support.

This emphasis on research stems, first of all, from the traditional role of the *university,* compared to a school or college, as a generator of knowledge as well as a disseminator of it. The term "research university" is a redundancy. Moreover, in recent times, universities have come to a realization of the commercial value of their "intellectual properties," especially in the technology sector. Developing and marketing these properties have become a major academic enterprise, given the ever rising cost basis for higher education as a whole. Patents may count as much as publications.

Professor Page Smith (quoted in Lepowski, 1991, pp. 40-42, and Matzko, 1991, p. 111) finds it unconscionable that ". . . professors are *forced* to do research to make a living, in order to avoid being humiliated—and terminated" (my emphasis). This statement misconstrues the issues. For a *salaried* university professor, tangible scholarship is neither an avocation nor an elective activity. Research has always been part of the job description (at the *university)* and the qualifications the professor submitted for appointment consideration in the first place. After all, the Ph.D. is a research degree, not simply a teaching certificate. The expectation that the employee do his job is hardly outrageous. On the other hand, it is the university's obligation to provide the facilities and wherewithal to do the research, as well as the teaching, it expects its faculty to conduct. The "distortion" to which Page subsequently alludes is the *de facto* requirement that the professors capitalize, as well as conduct, the university's research mission.

Where an individual's research is concerned, intramural budgets are seldom adequate to support more than "pilot work" or to get a beginning investigator off the ground. Without extramural funds, the fledgling Assistant Professor does not stay airborne very long. In today's academic marketplace, would-be scientist scholars must first be entrepreneurs of a sort, and since the most fundable areas of research are the

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most competitive, there is an element of "natural selection," if not survival of the fittest. This situation is not altogether conducive to the pursuit of scientific knowledge for its own sake or inevitably encouraging of its best purposes. Preoccupied with the means, the ends may be less emphasized, or worse, the means may become ends in themselves.

Scientific competition, especially when there is a financial factor involved, can be less than ennobling, as witnessed by some recent instances of alleged plagiarism, formal litigation (Crewdson, 1992; Marshall, 1992), and even criminal prosecution (Holden, 1992).

However, it is my observation that instances of outright fraud—e.g., the deplorable Baltimore, Imanishi-Kari et al. affair (alleging data fabrication) that Matzko (1991) cites (detailed by Hall, 1991)–are remarkable for their rarity. Of course, this celebrated case, precipitated by the article of Weaver et al. (1986), challenged by Margot O'Toole (Zurer, 1991) and others, including four of the paper's co-authors (!) (Hall, 1991), is by no means the first in modern times. One of similar magnitude developed at Yale during the late 1970's (Broad, 1980), and there have been others less publicized. Such peccadillo need not stem invariably from seeking a vested position, an interest in or requirement for large sums of research funding, or other material concerns. More often, perhaps, the basic motivation behind fraud, per se, is vanity, especially when vanity is offended by challenge, or fanatic commitment to a waning theory. We are reminded of some of the personalities who have "starred" in the history of evolution theory (Taylor, 1987), in particular.

Sources of Error

What is more frequent are unpremeditated errors, such as those engendered by quirky instrumentation, faulty reagents, contaminants, undiagnosed artifacts, flawed experimental design and/or data processing, including over-reliance on computer enhancement of raw data. The pressure to publish, generated by competition for date priority, an upcoming tenure decision or grant application deadline, can force hasty work that amplifies the potential for erroneous results. Or, interpretations, even experimental design, may be forced, in order to concur with a prevailing theory or the author's working hypothesis. A frequent example of the latter is encountered in radiometric dating, where it has been blatantly admitted that:

> In conventional interpretation of K-Ar (potassium-argon) age data, it is *common* to discard ages which are substantially too high or too low compared with the rest of the group or with other available data such as the *geological time scale.* The discrepancies between the rejected and the accepted are *arbitrarily* attributed to excess or loss of argon (Hayatsu, 1979, p. 974) (my emphasis).

Too often, studies that produce data contrary to the expected findings are considered failures and remain unpublished.

Thus are the hazards implicit in ". . . uncritical acceptance of data published in establishment journals . . ." (Matzko, 1991, p. 110); his critique of Van Till et al. (1988) is well-considered. Regarding the confidence that it is a "well-founded conclusion of science that the solar system is 4.6 billion years old" (per Matzko's 1991 citation, p. 111), I cite Chesterson, as quoted by DuPraw (1968, p. 11): "[There are things and there are theories] . . . and compared with [things] evolution and the atom* . . . are merely theories." Here, evolution as fossils would be "things." For Van Till et al., the sun and planets are "things"; their age an educated opinion; how well educated is the question? Is there any scientific reason to doubt that figure of 4.6 billion years**? If so, the age of the solar system is not a conclusion of science, only perhaps a conclusion of some scientists. To a majority of the latter, 4.6 billion years is the favored presumption at this time.

Thus, unqualified generalizations, as found in secondary and tertiary sources of scientific information (i.e., textbooks, encyclopedias, commentaries, magazines, television documentaries) become a potential source of error, especially for teachers, students and lay persons. Simply reiterating a hypothesis does not make it more *factual.*

In essence, only well-defined observations constitute a cumulative heritage of reliability. The question is, when is anything *conclusively* demonstrated in science? Little of it is chipped in stone, and even stones change with time. The phrase "Science says . . .", often used by the lay press, educational television, and authors of introductory textbooks to connote monolithic authority for what follows, is vacuous.

During a debate with Duane Gish, evolutionist Steven Shore, an astronomer on the Hubble Space Telescope program, is alleged to have stated (Morris, 1992, p. 4) that ". . . one of the beauties of science is that it is often wrong."

Bronowski (1956, p. 82) notes, "There was never a great scientist who did not make bold guesses, and there was never a bold man whose guesses were not sometimes wild." Yet, ". . . such is the nature of science, (that) their bad guesses may yet be brilliant by the work of our own day."

Van Till and his co-authors' (1988, 1990) higher view of the scientific literature, essentially across the board, compared to Scripture, strikes me as almost humorously naive, at best. A tip-off should have been the rampant inconsistency in the scientific literature; Pauling and Watson/Crick (3 vs. 2 stranded DNA structure) could not have both been right. How much of the current literature dealing with the controversial evidence for/against the Big Bang, or Cold Fusion would it take to make the same point? "Science held hostage" indeed, but by whom? Van Till et al. might note that these conflicting bodies of scientific *data* are being generated in laboratories, not seminaries. Seminal *opinions* are another matter altogether.

By contrast, where are there any inconsistencies of this magnitude in Scripture? For that matter, where are there any real inconsistencies in Scripture (vs. various exegeses) ? Does the Bible really address "scientific" questions in a scientifically productive way

*Reference to atoms as theoretical entities may be a bit conservative. Chesterson, who published this view in 1925, even considered the solar *system* as theoretical, given what was known of its organization and organizational principles at the time. But his point is well taken.

**The most direct evidence for billion-plus year ages of the Earth, moon, and meteorites derives from geological radiometry (Badash, 1989). For a recent evaluation of these isotope dating methods and the data, see Austin (1988, 1992).

(Van Till et al., 1988, pp. 42-43)? See Morris (1984) and Gish (1991) for a number of specific examples. DeYoung (1991) provides an insightful review of Van Till et al. (1990), where their view of science and Scripture are concerned. DeYoung (1991, p. 73) asks ". . . how can incomplete, imperfect science theories be the final interpreter of Scripture?" Meanwhile, the "hard science establishment" is not greeting the compromises of the sort that Hugh Ross, Van Till et al., or the American Scientific Affiliation (Sheler and Schrof, 1991) would offer with much respect (Stone, 1992).

Secular science has its own problems with which to deal. Even the most carefully executed work, by brilliant and unhurried minds, is subject to a degree of uncertainty. Consider Albert Einstein's testimony (as quoted in DuPraw, 1968, p. 1): ". . . I believe in perfect laws in a world of existing things, in so far as they are real, which I try to understand with wild speculation." Meanwhile, Einstein's most sophisticated ideas, including his theory of general relativity, continue to be tested (Ruthen, 1992). I suggest that Einstein himself would have heartily applauded these experiments, irrespective of how they impact his theories. Note also Amberson's commentary on the "cutting edge" of scientific research (as quoted in DuPraw, 1968, p. 7):

> . . . in the world of science, fashion is a prevailing mode of thought or action determined by recent innovation . . . resulting in a wave of attention and emphasis . . . yet in [its] wake . . . critical questions are neglected, which often escape attention through several succeeding waves.

Just because an idea is new does not make it right.

Outright retractions of erroneous or misleading findings are uncommon. More often, one encounters a followup article that reconsiders the earlier report in light of new information. Exceptionally, one or more co-authors may withdraw from a paper, subsequent to its publication (Hall, 1991), to disavow and thereby call attention to an egregious error (or worse) committed by a recalcitrant cohort. But, at that point, one cannot go back and edit the data tables in the original paper, where they remain as a pitfall for the unwary who have not gotten the latest word from the author(s) on the subject, and unwittingly take a "high view" of the unqualified first version, hence the value of periodic reviews. Even in the best of cases, the progress of one's research and what one finally publishes is not co-variant (Figure 1). No matter how diligently one follows the scientific method, the truth of the matter may prove elusive. The hazard is in what may get published along the way.

There have been instances where inaccurate results nonetheless produced valid conclusions. An example is the classic work of Gorter and Grendel (1925) on the lipid bilayer structure of cell membranes. Their extraction method undervalued the total lipid content and their calculation of cell surface area was flawed; however, the errors in these "hard data" essentially canceled, and fortuitously, these authors derived what has proven by other means to be a correct model for this aspect of membrane structure.

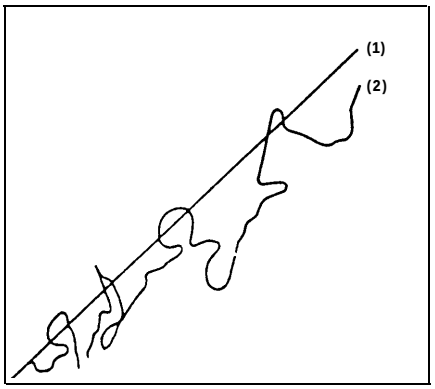

Figure 1. The sequence of research, as an author would have his readers perceive it in the publication (line 1), and of the same research as actually carried out in the laboratory (line 2). Note, interim publications may arise at any point along line 2. Derived from Szent-Gyorgyi (1900), courtesy Academic Press.

Editorial Screening

The security for the reader of a published research report rests largely on the quality of pre-publication peer review. By that standard, the term "reputable" journal is derived. Peer reviewers are for the most part highly competent, hands-on experienced in the subject area, and conscientious. But some papers are more equal than others. It is not exceptional that a reader (or a reviewer per se), faced with a paper that promotes an interpretation contrary to his theory, will most diligently critique the means by which the data were acquired. There is room for conjecture that the converse obtains, as well. When one of the authors of a submitted paper happens to be a Nobel Laureate, "peer" review may be even less rigorous. On the other hand, even a Nobel Laureate can find that challenging the establishment's position on a particular subject is tough going. From physicist Hannes Alfven (1988, p. 251): "If scientific issues always were decided by Gallup polls and not by scientific arguments science will very soon be petrified forever." And "With the referee system which rules U.S. science today . . . my papers are rarely accepted by the leading U.S. journals."

We have, nonetheless, the ideal to which even these editorial boards would subscribe, if not always practice. From Bronowski (1956, pp. 87-88):

> The society of scientists has a directing purpose: to explore the truth . . . It must encourage the single scientist to be independent, and the body of scientists to be tolerant [of dissenting views].

Tolerance ends, however, at dishonesty (see Bronowski 1956, pp. 73-76) .

Scrutiny

Matzko (1991, p. 111) suggests that ". . . most published data is (sic) never replicated, some of it (sic) never even read . . . research grants are given to produce new findings, not rehash old ones." The latter is, to a degree, true enough (given the granting agencies' budget-imposed priorities for funding, and how, in the "rehash," new findings might actually emerge). And, in the process of reviewing a grant application, tenure or promotion candidacy, etc., papers may be counted before (if ever) read (hence, perhaps, so many

ambiguous titles !). However, if the work is in an active area of major interest—prerequisite to grant fundability in the first place—the findings *will* be read, certainly by the competition, and to a degree repeated—not necessarily to check their veracity, but as aspects of the protocol become incorporated into the experiments of other investigators, results compared. One of the reasons why scientific fraud is considered so heinous is that one begins his own work with the usually well-founded assumption that according to the methods and materials used, the published data were in fact obtained as published. However, the methods and their accuracy may be questioned, and accordingly, the conclusions drawn from the results challenged. Where the reliability of the results is questionable, the work is usually stigmatized as less than careful or thorough, not fraudulent. Given the pressure to publish or perish, one can risk publishing *and* perishing as a result of sloppy or ill-considered work, or otherwise by publishing prematurely (see Figure 1). For a recent example of premature publication, at best, and its consequences, see the latest developments in the "Cold Fusion" controversy (Taubes, 1991).

Because of coincidental scrutiny, at least, fraud is as remarkable for its foolishness as its abuse of trust. Consideration of the risk:reward ratio alone should be a deterrent, as cynical as that may sound. Frankly, while fraud may elude an editorial board, it is hard to hide in the long run, and its disclosure is devastating for the perpetrator. First reactions to the Imanishi-Kari et al. debacle included the question "how could they have been so stupid?", or arrogant (see Hall, 1991). Baltimore's alleged lack of oversight, at best, drew sardonic expressions not only about his administrative abilities, but because it implied he did not know the details of what he was publishing from his own laboratory! The situation of a Nobel Laureate, who founded and subsequently served as Director of the Whitehead Institute for Biomedical Research and then became President of Rockefeller University,* who cannot evaluate "his" own work (as co-authorship implies) sounds oxymoronic, but should serve as a warning flag to those of the Van Till et al. (1988, 1990) persuasion insofar as their unbridled confidence in the pronouncements of science.

In stark contrast to the Baltimore, Imanishi-Kari et al. imbroglio is the circumstance of Andrew Lyne of Jodrell Bank's University of Manchester Astronomy Laboratories. Like Baltimore, Lyne is a paragon in his field, at an eminent institution engaged in collaborative research. Last year, Lyne and his colleagues published evidence for the existence of a planet outside this solar system (putatively one orbiting a pulsar in the constellation Sagittarius) —a spectacular, first-of-its-kind discovery, enthusiastically received by the editors of *Nature,* and heralded by the *London* and *New York Times,* et al. On January 15, 1992, Lyne rose at a meeting of the American Astronomical Society and announced apologetically, with no self-serving qualifications or innuendoes directed at others of his team, that they (collectively) had been wrong (Flan, 1992)—"in a moment of awful comprehension one night last week I realized [that we had committed an inadvertent procedural error in analyzing the radio wave data] . . ." and, when the flawed analysis was corrected ". . . the planet just evaporated" (Ritter, 1992). The response? A justly deserved standing ovation from his peers—not for having made a mistake, of course, but out of respect for an honest mistake honestly corrected and in timely fashion, since it has now alerted others pursuing this kind of research to a potential error. In the week prior to Lyne's retraction, Alexander Wolszczan, of the Arecibo Observatory, announced the detection of two, possibly three, putative planets around a different pulsar (Wolszczan and Frail, 1992). Most importantly, the Jodrell Bank's mistake was *self*-detected and immediately self-corrected, by a scientist who patently values integrity above the fame and fortune that attends a major, though in this case spurious, discovery, and who clearly wasted no time pondering the embarrassment or other possible material consequences of its rectification. Meanwhile, Wolszczan, while acknowledging the Lyne phenomenon, has stated that ". . . it does not change my thinking about what I have found" (Ritter, 1992). "I am 99.9% certain [that what I have detected] . . . are planets" (Sawyer, 1992).

*The Rockefeller University Board of Trustees accepted Dr. Baltimore's resignation as President on December 3, 1991.

Quality Assessment

When debating a creationist, an evolutionist will not uncommonly resort to *ad hominem* remarks about his opponent's scientific credentials or "his" journals. Creation scientists, as a group, have been pejoratively characterized by the establishment variety as talking (or writing) about science while doing very little of it themselves, though when pressed, the establishment will admit to some striking exceptions to this generalization. In any case, it is not uncommon to find creationist papers structured entirely of data elsewhere acquired. At that point, there may be some substance to the view that one who has no direct experience himself with the techniques, the study object/system, raw data acquisition and subsequent analysis, is not in the best position to evaluate these data or draw conclusions from them. However, this admonishment should not be directed solely at creation "armchair" (or library vs. laboratory) scientists. We have, for example, the copious treatises on biology and its evolution from *astronomer* Carl Sagan, who, in my estimation, seldom lets details, data or otherwise, stand in his way. He is a classic case of the perilous transfer of expertise to which Ph.D.'s are so prone.* Defenders of this principle might, on the other hand, identify some who practice it as Renaissance men. In any event, in defense of armchair science, sound theoretical reviews and re-assessments can be more valuable contributions than a handful of original "nature notes."

I am a relatively recent convert to the creationist (vs. evolutionist) persuasion, and as the Editor already knows, have a lot of creationist literature yet to read. I find that much of what I have read, in or ancillary to my scientific discipline (biochemistry, biophysics, cell biology), is altogether solid. Especially exemplary are the publications of Frair, Gish, Marsh, Ouweneel, Thaxton, Wilder-Smith, and Emmett Williams, among

*Among Dr. Sagan's latest exploits are Gifford Lecturer in Natural Theology at the University of Glasgow, and co-chairman of the Joint Appeal by Science and Religion for the Environment.

others. I have encountered exceptions to the contrary. As is the case for the establishment literature, there would seem to be peaks, valleys, and in-between plateaus of scientific quality in the creationist literature as a whole. One should be instinctively wary of conclusions supported only by references to secondary or tertiary sources (e.g., textbooks, encyclopedias, the "science news") irrespective of their putative authority. In particular, I concur in our *CRSQ* editor's view that the credibility of creation science is not well served by the plethora of self-published works, vs. peer reviewed monographs and journal articles (see, e.g., DeYoung's comments, 1991, p. 70). Painful as it can be, a rigorous pre-publication review may bless the author as much as the reader, certainly the former's scholarly reputation, once the manuscript is in print.

On Faith and Metaphysics

Evolutionists belabor the putative omnipotence of time and random chance, to the chagrin of probability mathematicians and information theorists. What has been particularly distressing for me in reading a discourse about purely natural phenomena, when violation of one or more physical laws is requisite to support a creationist's paradigm, is to find the creationist author invoking some nebulous biblically unrecorded act of supernatural intervention in the process. In some cases, this paraphrases the foppery of "theistic" evolution and Gaia-ism. Otherwise, constructions such as "the Creator *must* have . . ." startle me by their imperative presumption. By no means do I have a philosophical or theological argument with the reality of Divine miracles, but . . .

Walters (1991, p. 129) addresses this issue, noting:

> . . . any creationist models that require violation of physical laws . . . should be viewed critically . . . It is true that the Creator can override the 'laws' of nature as we know them, but it is also true that He rarely chooses to do so.

Who, after all, created these laws, set them into action, and for what reason, in the first place?

Harrison (1933, pp. 319-320) would warn us creationists, as he was warning his fellow embryologists in context, to beware the ". . . anthropomorphisms and relics of our demonology*. . . which may lend a false sense of security to our explanations but may also suggest foolish questions that can never be answered."

Ideally, a scientist does not "believe in" a theory, but either accepts or rejects it on the weight of the evidence. Yet, one always begins his work with a biased view–the hypothesis is just that. Ham (1987) states the matter more strongly (p. 8) —"Scientists are not objective truth-seekers; they are not *neutral."* There is a sense of comprehensive finality about that statement with which I am uncomfortable. I would suggest, in agreement with Ham, that as a human being, a scientist (like anyone else) may hold a philosophy that varnishes his concept of ultimate truth. But, in the actual practice of his science, on a specific problem (if not world-view!), what we call "scientific discipline" will, in most cases, prevail. This is much of what the "training" of a Ph.D. is all about. The investigator who lacks that quality will, in all likelihood, suffer a short career as a practicing scientist, irrespective of the grant or two he may acquire or the handful of papers he may get published in the interim. However, he may ascend to great heights in the teaching profession, or as an author of commentaries. And, given the tenet of "academic freedom," he may remain there indefinitely.

*Harrison uses the term *demonology* here in the sense of great energy, urgency, or skill, not in the spiritual sense.

Separating the Wheat from the Chaff

There is, then, "good" science and "bad" science (in the sense of its execution and veracity, not morality), irrespective of its source. Where that source is the evolutionary camp, a creationist scientist should never "throw out the baby with the bathwater." The virtue of a partial truth is that it contains some truth. Even mistakes can be revealing. As I have been telling my students for years, there is no such thing as "wrong data" (as long as they are not fraudulent!), though data may have been improperly acquired, or misinterpreted, or be contradictory to the expected result. When the latter obtains, one should reconsider the basis for the expectation. Thus, no one in my laboratory ever threw away—with my concurrence—a printout from the scintillation counter, the X-ray spectrometer, or notes on an experiment that "did not work." That was not necessarily being super-ethical or even paranoid.

In any event, when using data, theirs or others, creationists *must* be as rigorous in guarding against their commission of "investigator interference" (i.e., following a protocol that forces data consistent with a preconceived conclusion) and biased screening of data, as the evolutionist *should* be.

The Lord blesses "good" science as the discoveries reveal His creation, His Being, and so glorify Him (Romans 1: 19-20). On the other hand, authors and teachers [authors when published become teachers, if they are not already] should also heed keenly Matthew 18:6,

> But whoso shall offend [by errant teaching, etc.] one of these little ones [the naive student or reader] . . . it were better for him that a millstone were hanged about his neck and that he were drowned in the depth of the sea.

How, ultimately, does one discern the truth? See Proverbs 3:5. If in some minds I, like George Matzko (1991, p. 111) exhibit, in the process, a "hopeless naivete when approaching the Scriptures," so be it.

Conclusions

What is the basis for the present shortcomings in science? Is it, at least where the secular establishment is concerned, the pain of riches (or deficit of same) and reputation . . . or technical limitations? In part, yes. But consider also Lammerts' sage view (quoted by Meyer, 1991, p. 85) that

> . . . all research should be undertaken *prayerfully* with the objective of helping one's fellow man to better understand, enjoy, and thankfully appreciate . . . the evidence of the intricacy of His creations (my emphasis).

When asked for bread, does He give a stone (Matthew 7: 9)? As a reviewer of grant proposals for the National Institutes of Health, the National Science Foundation, etc., I never read a prospective research protocol that began that way—i.e., that the principle investigator might seek and then be guided by a Wisdom beyond his own or his peers. Might it be that the "wise," otherwise, would be caught in their own craftiness (1 Corinthians 3: 19)?

Acknowledgements

I thank Drs. Richard Bliss, Edward Boudreaux and Duane Gish for a critical reading of the manuscript and their helpful comments, and Gaynell Lumsden for her valuable assistance with the literature search and manuscript preparation.

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Addendum

As reported in the July 16, 1992 issue of *Nature* (p. 177), the Imanishi-Kari / Baltimore saga continues. The technical reliability of an earlier forensic analysis of Imamshi-Kari's data books by the U.S. Secret Service has been challenged in a reanalysis commissioned by Imanishi-Kari's defense attorneys, and Baltimore has claimed ". . . extensive confirmation" (unspecified) of the *Cell* paper. Identifying the controversy as one of a very complex scientific nature that a grand jury "were clearly incapable of understanding" the U.S. Attorney is no longer seeking an indictment in the case. Baltimore has retracted his retraction, which some observers feel was pressured in the first place by other problems he was having at Rockefeller University. Imanishi-Kari, interpreting these events as exonerating, says she will be requesting NIH to release funds from a 1989 grant frozen during the course of the Congressional, NIH and Justice Department proceedings. Has a matter of principle devolved to one of principal? Meanwhile, Congressman John Dingle, who chairs the Oversight and Investigation Subcommittee of the House Energy and Commerce Committee, has observed that ". . . the decision not to prosecute does not change the fact that the *Cell* paper was retracted (by the other four original co-authors) because of serious, and extensive, irregularities."

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QUOTE

It has long been assumed that preserved sedimentary rocks record primarily normal or average conditions for past epochs but this uniformitarian assumption must be challenged.

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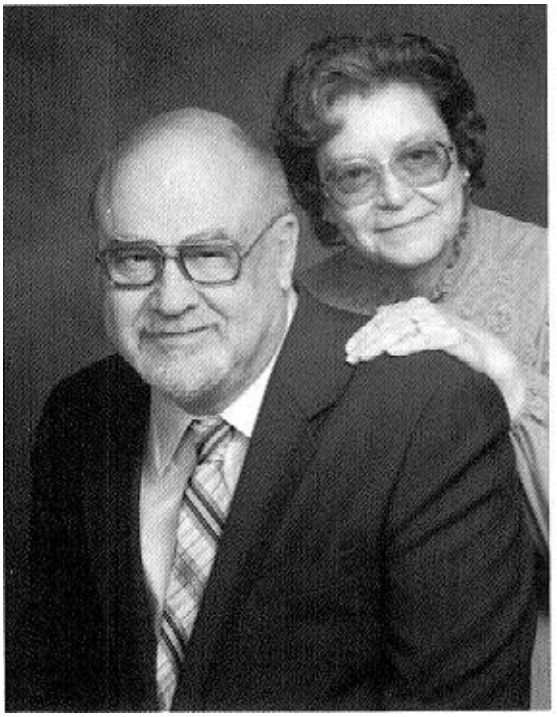
A SALUTE TO TWO MEMBERSHIP SECRETARIES AND THEIR WIVES

There were talented people in the Bible who performed down-to-earth tasks in producing God's temple. Fabrication of its palm trees, cherubim, and other important decorative features depended on a few key individuals like Huram-Abi and other craftsmen (2 Chronicles 2:13).



Dr. Wilbert H. Rusch, Sr., and his wife Marge. Dr. Rusch was the first Membership Secretary of the Society.

In the New Testament the Apostle Paul also captured this principle; that a small number of gifted and motivated people can be immeasurably important. He wrote in 1 Corinthians 12: 12-31 that Christ's Church resembles a human body. Like organs, every person in the "Body of Christ" plays a part and the less visible or less prominent members are sometimes the most important to its function.

The Creation Research Society is no exception to this Corinthian concept. It has been blessed with dedicated Board Officers (and their wives) who have contributed immensely to its success. They have worked on routine chores that have enabled the Society to exist. We desire to recognize and honor these individuals. We wish to pay tribute to their patient, voluntary service, and to describe the types of tasks they undertake on behalf of all who read and use our material. In future articles we will honor others who labor in key CRS Board positions.

In 1963 when the Society was founded, Dr. Wilbert H. Rusch, Sr., became its first Membership Secretary and its first Treasurer. During the early years, "Bill" kept the identity of CRS members confidential—a policy that still remains in effect. This allows members to decide for themselves if they wish to disclose their membership in the Society. Even board members do not have access to the membership list -- only the Membership Secretary. The list is never traded/or sold, although requests routinely come.

Dr. Rusch also established a systematic approach to updating the membership lists and shipping quarterlies. In the early years he managed this with a typewriter and with the able assistance of his wife (Marge Rusch), his daughters, Jean and Joanne, and Ken and Marilyn Behnke who still help mail the journals.

Bill eventually made the switch to a computerized system. In so doing, he drew upon the computer expertise of various members of the Rusch family, thereby saving the Society considerable amounts of money. The schedule of demands varied from week to week but usually Bill spent 30 hours each week on CRS work, all without pay. He is still closely involved, receiving the quarterlies from the printer and getting them mailed.

This same tradition of untiring service has been maintained to the present by Dr. Glen W. Wolfrom who was elected Membership Secretary in 1985. In addition to working a regular full-time job in industry, Glen Wolfrom labors *gratis* on CRS activities, working enough hours to equal a second full-time job. He is assisted by his wife, Becky Wolfrom. Responsible for all membership records, they process renewals and address changes, and produce the journal shipping labels to send to Dr. Rusch. They do the foreign mailings and late U.S. mailing (beyond the initial bulk mailing of each issue), as well as recording and processing receipts for all payments, book orders, and contributions to the Society.

CRS Quarterlies have a timeless value as shown by Dr. Emmett Williams in his published reviews of back issues. They are a lasting creation asset. The *CRSQ* back issues are available and are selling briskly, thanks

Dr. Glen Wolfrom and his wife Becky. Dr. Wolfrom became the second Membership Secretary in 1985.

Mrs. Marilyn Behnke has worked effectively for CRS in bookkeeping and mailings. Her husband Mr. Ken Behnke has also worked with the quarterlies.

to the Wolfroms who currently store them and fill the purchase orders.

Most of the forms and letters sent to CRS members are drafted at the desk of Dr. Glen Wolfrom: membership forms, Christmas book appeals, ballots, annual meeting announcements, and many others. All members are reminded that they can help the Wolfroms by renewing promptly each year and not waiting to receive the first or subsequent renewal notices.

Glen supervises the printing of the CRS Book Catalogue and he prepares the key-word indices for the Quarterly each year. As Bill Rusch did years ago, Glen handles voluminous correspondence that comes addressed to the Membership Secretary on a wide array of issues. We all are very grateful that Glen's main hobby (perhaps his only hobby?) is serving the Lord in the Society. Hats off to Glen, Becky, Bill, Marge, Ken, Marilyn, and the others who assist them. In all sincerity, we say: "Thank God for Membership Secretaries!"

George Howe*

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BOOK ANNOUNCEMENT

Biology in Balance by Dennis L. Englin and George F. Howe. 1991. D'Litefully Yours Publications. Newhall, California.

Biology in Balance is a life science textbook that refers significantly to the Creator and the Bible. The principles of modern biology are lucidly described and the subject of origins is analyzed from several vantages, including young-earth creationism which the writers hold. The authors are veteran classroom professors who have designed this work to communicate the data and ideologies of biology in a framework that includes philosophy and theology. In the "Guide to the Literature" section, the reader is introduced to creationist sources. This is a *preliminary* edition produced by photocopy with hand drawings and bound in plastic material to control the cost. For information contact: G. F. Howe
24635 Apple St.
Santa Clarita, CA 91321

PANORAMA NOTES

A New Series of Scientific Creation Booklets

There is a great need for short monographs on origins, booklets that are both technically accurate and also readable. Many people unfamiliar with the creation view indicate a willingness to read creationist literature, but are not interested in working through long books. This is one example of how this type of literature would be useful. At the last meeting of the CRS Board of Directors a monograph series of this type was authorized as soon as funds become available.

The Board of CRS invites authors to submit short manuscripts for possible use in this project. Authors should write for the level of the general reader with high school and junior high students, and educated readers especially in mind. Authors should supply their own photographs and sketches so that each unit will be profusely and tastefully illustrated. Each booklet should cover some key scientific theme with various origins explanations: the origin of life, the origin of man, the origin of the universe, fossils and the Flood, mimicry, plate tectonics, glaciers, and polyploidy would be examples of possible subjects. The booklets should also explain the scientific factors that support a belief in special creation rather than in the competing evolutionary models described.

The manuscripts, exclusive of photographs and reference lists, should be no longer than 40 typewritten double-space pages. They should be documented carefully according to the procedures followed in Quarterly articles and references should be listed and handled in Quarterly style format. Referencing and documentation should cover the literature in CRSQ on a particular subject and also important leads from the origins literature at large.

Selection and editing of the manuscripts will be directed by the Chairman of the Publications Committee. He will involve various committee members and others as referees and possible editors of booklets. Individual papers in the series may undergo substantial editorial changes to improve their appeal and general readability. We would like to hear from potential referees who are willing to work with submitted manuscripts. The author of a booklet that is selected will share a royalty on booklet sales.

We also invite contributors who wish to send possible names for this whole series of CRS readers. Please send your finished manuscript in triplicate, your choice for a project name, or your willingness to referee and edit various manuscripts to:

Dr. George F. Howe
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Reprinted CRSQ Volume 10

Introduction

The *Creation Research Society Quarterly* has been published since 1964 (28 complete volumes). In an effort to make these volumes available many of the missing issues have been reprinted. Brief synopses have been written on volumes 1-9 and have appeared in the previous nine quarterlies. In each synopsis, major articles are reviewed to give a person interested in scientific creationism a general idea of the contents of that volume. Many of the articles are of continuing interest and value.

Biology

Genetics:

The claims of "genetic engineering" advocates were explored by two creationists. Duane Gish (1973, pp. 10-17) carefully examined cloning, eugenics, *in vitro* fertilization, repair of defective genes, genetic surgery and synthesis of genes. He noted (p. 10) that:

> The idea that man may be able to alter specific human characteristics and thus "control his own evolution" is seen as science fiction rather than as serious science.

Harold Armstrong (1973, pp. 17-18) asked: "How much like engineering is 'genetic engineering'?" He briefly discussed the topic within the framework of what created material is available with which to "engineer." No genetic engineering is ever likely to produce changes from one kind to another.

Garrido (1973, pp. 166-169) employed molecular biology to demonstrate that the formation of new organs or development of new functions is impossible. Any transformist scheme is not scientific and is dictated by atheistic prejudices. Reviewing the findings in the science of genetics in the twentieth century, Tinkle (1973, pp. 44-47) claimed that they militate against the concept of macroevolution.

Kinds:

The late Frank Marsh (1973, pp. 31-37) continued his excellent work on the Genesis kinds. Using DNA studies he showed that the kind limitation could not have been violated. Much of his work is summarized in *Variation and Fixity in Nature,* available from Creation Research Society Books. In an interesting article, Jones (1973, pp. 102-108) stressing the Genesis kind concept, offered answers to the question of how many animals were in the ark. Discussing the division of clean and unclean animals, Noah bringing pairs to the ark, vertebrate families, caring for the animals (food) and hibernation possibilities, the author concluded that the size of the ark was sufficient for Noah to care for the possible 2000 animals on board. The ark was considerably bigger than its animal and food load to reduce any chance of air fouling. Many creationists will find this treatise very appealing.

In reporting on a research project on the bacterial species *Proteus mirabilis,* Moore (1974, pp. 187-190) found the organisms stable to various stresses. The species showed no proneness to evolve or change as would be expected from an evolutionary viewpoint, but it demonstrated the principle of "like yielding like" as predicted by a creationist model of science.

Botany:

In a classical field study involving plant succession, Lammerts and Howe (1974, pp. 208-228), using five species of California wildflowers, detected no evidence for evolutionary natural selection. In the plots studied, the wildflowers did not develop any evolutionary tendencies. The variation that did occur was restricted in years of moisture stress as "natural selection" caused the plants to revert to a typical or normal form. Thus natural selection is a conservation or preservation process, not a mechanism to cause improvement or macroevolutionary changes.

Population Control:

The excellent biologist, Norbert Smith, often wrote on population control without predation or disease. In a laboratory study, Smith (1973, pp. 3-10) observed asexual reproduction of the planaria (flatworm), *Dugesia dorotocephala* under conditions of crowding. Reproduction rates were reduced indicating a "built-in" (designed) regulation preventing overpopulation. Smith suggested some possible pre-Fall applications as well as discussing population control on the present earth. For a recent note on population control without predation, see Williams, Howe and Meyer (1992, pp. 157-158); also a prior article by Smith (1970, pp. 91-96) is helpful in studying the topic.

Miscellaneous:

Telfair (1973, pp. 53-61) examined evidence from morphology, classification, natural selection, mutation, biogeography and anthropology and stated that the evolutionary hypothesis is inadequate to explain the findings from these sciences. He preferred the creation model of science as a framework of explanation. In a biological laboratory study Cagle (1973, pp. 135-142) examined the nitrogen fixing bacterium, *Azotobacter.* Employing electron microscopy in a well-illustrated paper, the author claimed that the resting cells of these bacteria have a *created* advantage which is necessary for the perpetuation of the nitrogen cycle. One can visualize design in the intricate workings (processes) of nature! As a change of pace readers may enjoy reading the brief selection (Anon., 1973, p. 123) on the South African brown egg-eating snake. The illustrations of the creature having a meal are fascinating. The questions asked lead to a conclusion that the egg-eater was designed since his peculiar dietary habit could not have evolved.

Geology

Most creationists seem to prefer a theory of mono-glaciation after the Flood during the so-called ice age. William Springstead (1973, pp. 47-53) reviewed the

writings of both catastrophic and uniformitarian authors and concluded that the evidence best fits a concept of one glacial period only. Burdick, (1973, pp. 109-110) discussed his field work on the discovery of human skeletons in Cretaceous strata. Walter Lammerts added some interesting comments on the find in that it presents problems to both creationists and evolutionists.

Walter Peters (1973, pp. 89-96) presented field evidence for rapid sedimentation which would be expected during the Genesis Flood. He explored the formation of bentonite, Permian conglomerates, mid-continent Pennsylvanian sediments, mid-continent Mississippian limestones, Devonian limestones, Silurian polystrate fossils, catastrophic Ordovician sedimentation and Cambrian fossil deposition.

Steve Austin [Nevins (1974a, pp. 191-204)] performed an excellent creationist field study on the post-Flood strata of the John Day Country. The strata studied indicated formation by volcanic catastrophism. The 7000-feet thick strata consist primarily of terrestrial lava flows, gigantic ash-flow tuff beds, boulder breccia, tuff-breccia beds and volcanic siltstone and sandstone. Austin suggested that the formations were deposited rapidly. Creationists should carefully study this investigation as evidence for continuing catastrophism after the Flood as the earth was experiencing considerable volcanism before the ice age. This writer refers to such catastrophism as aftereffects of the Flood. Areas in west Texas, particularly at Big Bend National Park and west along the Rio Grande River show similar formations as described by Austin in northeast Oregon. The article following Austin's work (Northrup, 1974, pp. 205-207, 228) offered continuing commentary on the John Day Country.

In an earlier issue of the Quarterly [Nevins (1972, pp. 231-248)], Austin detailed evidence that demonstrated that the Capitan Limestone was not a fossil reef. Wonderly (1974, pp. 237-241) argued that it was a fossil reef. Austin [Nevins (1974b, pp. 241-244)] satisfactorily answered Wonderly's criticism. This is an interesting exchange between an old-earth creationist and a young-earth creationist.

Radiocarbon Dating

"The Bible, Radiocarbon Dating and Ancient Egypt" is the title of an article by Ronald Long (1973, pp. 19-30). The author discussed problems of Egyptologists in trying to reconcile differences between historical and astronomical data and C-14 determinations. Dendrochronology was also elucidated as Long concluded that corrected data closely match Egyptian-Biblical chronology. Clementson (1974, pp. 229-236) examined radiocarbon and dendrochronological data and suggested that the radiocarbon activity level in the biosphere has been decreasing over the past few millennia. He correlates this information to yield a date for the Flood.

A mathematical analysis was conducted on the life spans of the Genesis patriarchs and some carbon-14 dating data were discussed (Strickling, 1973, pp. 149-154) in reference to the analysis. Some tentative comments were offered on comparative dates obtained from the Septuagint and Masoretic texts.

Astronomy and Thermodynamics

Ten years of creationist astronomy writings were reviewed by Mulfinger (1973a, pp. 170-175). A report on the first Velikovsky symposium was given by MacIver (1973, pp. 142-148). The author presented some implications for creationism. Ten years of creationist thermodynamic writings were reviewed by Williams (1973a, pp. 38-44). Primarily creationist interpretations of the first and second laws were elucidated. Hubert (1973, pp. 169-170) used cybernetic and thermodynamic arguments to show flaws in evolutionary thinking. An interesting exchange of ideas was presented as to whether the degenerating effects of the second law of thermodynamics operated before the Fall (Kofahl, 1973, pp. 154-156; Williams, 1973b, pp. 156-157; Morris, 1973, p. 157). This debate continued for several issues thereafter as other creationists offered their opinions.

Chemistry

A treatise on trace elements was authored by C. E. A. Turner (1973, pp. 83-88). The elements previously thought to have no biological significance were shown to have profound effects on life. The author then related his discussion to design by a wise Creator. Kent (1973, pp. 97-102) presented some nuclear quadrupole resonance studies on chromium (III) chloride and discussed them within a creation approach.

Education

A unique science fair exhibit was the subject of an article by George Mulfinger (1973b, pp. 62-68). This exhibit, prepared by the Bob Jones University Art Department under the guidance of Mulfinger, showed evidence that refuted evolutionary postulates. Panels noted fossil pollen finds in the Grand Canyon, the Napa image, objects found in coal, evidence from Paluxy River, fossils from the La Brea Tar Pits, polystrate fossils, missing links (lack of intermediate species) and rapid formation of speleothems. Readers can glean the well-illustrated discussion for ideas on science fair exhibits.

John Moore (1973, pp. 110-117) discussed a retrieval system to review articles in the journal *Evolution.* His key word categories would be helpful to any student or researcher in biological science. Holroyd (1973, pp. 158-162) developed a thesis that natural theology is a scientific subject. He particularly mentioned the work of Paley.

Conclusion

This volume of the Quarterly also contains many technical notes, letters to the editor and book reviews. The quality of the articles is impressive. After 10 years of publication, the Quarterly is a "gold mine" of creationist scientific writings.

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SPACE MEDIUM THEORY OF LASER GYRO AND LASER SPEEDOMETER

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Abstract

The basic equations for the laser gyro and a proposed laser speedometer are derived and the physical principles explained. The laser gyro and laser speedometer are "closed box" instruments for measuring angular velocity and linear velocity respectively. Two of the "invisible" reference frames from which these motions can be measured are identified in space medium theory. This opens new avenues for progress in science and presents a challenge to Einstein's special theory of relativity.

Introduction

The laser gyro is the most advanced navigational instrument for sensing angular velocity. The Sagnac experiment (Sagnac, 1913) is said to be a forerunner of the laser gyro. The instruments are similar in this respect: 1) Light is propagated around the instrument clockwise in one beam and counterclockwise in the other. 2) Light speed is c in the light propagating medium through which the instrument rotates. 3) Light speed within the instrument is slower than c going around in the same direction as the rotation and faster than c going around in the opposite direction. The anisotrophy of light speed in the reference frame moving with respect to the earth's surface is predictable from space medium theory (see Barnes, 1986).

This paper focuses on physical processes in the laser gyro that are not present in the Sagnac experiment, nor any of the other historical optical experiments for detecting angular velocity. What is most important is that the laser gyro yields *frequency difference,* whereas the Sagnac and other instruments yield only *phase shift.* The frequency difference is between *two resonant oscillator frequencies* in the resonant chamber of the laser gyro as it rotates with respect to the light-propagating space medium.

However, the first part of the laser gyro derivation involves the time difference for light to transit the rotating circuit in the two different directions. That portion of the derivation is the same as that in the Sagnac experiment. So the Sagnac equation will be derived and used as a first step in the derivation of the laser gyro equation.

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Derivation of the Sagnac Equation

This Sagnac equation derivation is similar to one by Georg Joos (1964). Figure 1 is Sagnac's rotating instrument with its light source and four mirrors. Light transits the rectangular path in two beams, one clockwise and the other counterclockwise. Light is propagated in the space medium that does not rotate with the instrument. This derivation uses the light speeds with respect to the rotating instrument's frame of reference. The first step is to get the time of transit in each beam, then to get the equation for the time difference as a function of the angular velocity. From the instrument's frame of reference this involves light speeds less than and greater than light speed c.

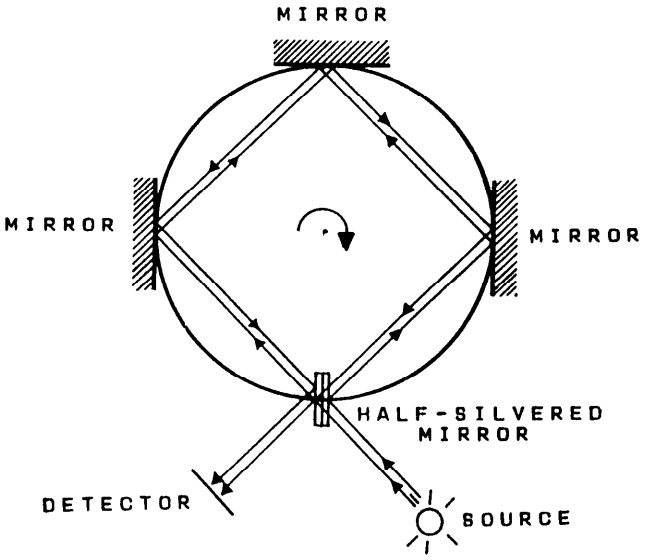

Figure 1. The Sagnac experiment.

Taking the axis of rotation as the origin, light travels along a curve whose polar equation is given by $r = r(\phi)$. If at any given instant an element of the curve makes an angle $\theta = \cos^{-1}(rd\phi/ds)$ with the direction of its *translational motion,* arising from the turning of the whole apparatus, then the relative speed when going around in the same direction as the rotation is

$$v_- = c - \omega r \cos\theta = c - r^2\omega\frac{d\phi}{ds} \tag{1}$$

and in the opposite direction

$$v_+ = c + r^2\omega\frac{d\phi}{ds}. \tag{2}$$

The difference in the two corresponding time intervals is

$$T_- - T_+ = \oint\frac{ds}{c - r^2\omega\dfrac{d\phi}{ds}} - \oint\frac{ds}{c + r^2\omega\dfrac{d\phi}{ds}}. \tag{3}$$

Since the second term in the denominator is small, this becomes

$$\Delta T = \oint\frac{2r^2\omega d\phi}{c^2} = \frac{2\omega}{c^2}\oint r^2 d\phi = \frac{4\omega S}{c^2} \tag{4}$$

where S is the area enclosed.

The phase shift in length is the time difference multiplied by speed c,

$$\Delta L = \frac{4\omega S}{c} \tag{5}$$

where ω is the angular velocity of rotation, S is the area enclosed by the light path. Sagnac measured displacement ΔZ of fringes in fractions of a wavelength, namely

$$\Delta Z = \frac{4\omega S}{c\lambda}. \tag{6}$$

Derivation of the Laser Gyro Equation

The three-mirror triangular light path in the laser gyro is shown in Figure 2. One laser light beam transits the path clockwise and the other counterclockwise. A small portion of the light from each beam passes through the apex mirror and, by aid of the corner prism, both enter the readout detector as shown. When the instrument is in rotation the difference in frequency in those two beams is converted to angular velocity by the readout detector.

The derivation of the laser gyro equation is the same as that for the Sagnac equation up through Equation (4), the time difference equation. But from there the derivation must take into account the fact that the laser gyro is a *resonant optical system.* When in rotational motion it has two resonant frequencies. The difference in these two frequencies is proportional to the angular velocity. As noted by Joseph Killpatrick (1967); "The wavelength must be an exact integer fraction of the path around the cavity. This last condition determines the oscillation frequency of the laser." In that moving frame of reference the wavelength is constant and the speed of light is different from c.

Denoting the length of the circuit in the cavity frame of reference as L, and N as the number of wavelengths, the wavelength

$$\lambda = \frac{L}{N}.$$

In the space medium, there is a change in the path length and a change in the wavelength,

$$\Delta\lambda = \frac{\Delta L}{N}.$$

The frequency change is proportional to the ratio of changed length ΔL (extra length in the light propagating medium) to fixed circuit length L (inside the chamber),

$$\Delta f = \frac{(\Delta L)f}{L}. \tag{7}$$

Since $\Delta L = c\Delta t$, and in view of Equation (4), the frequency difference is

$$\Delta f = \frac{4\omega S f}{cL} \tag{8}$$

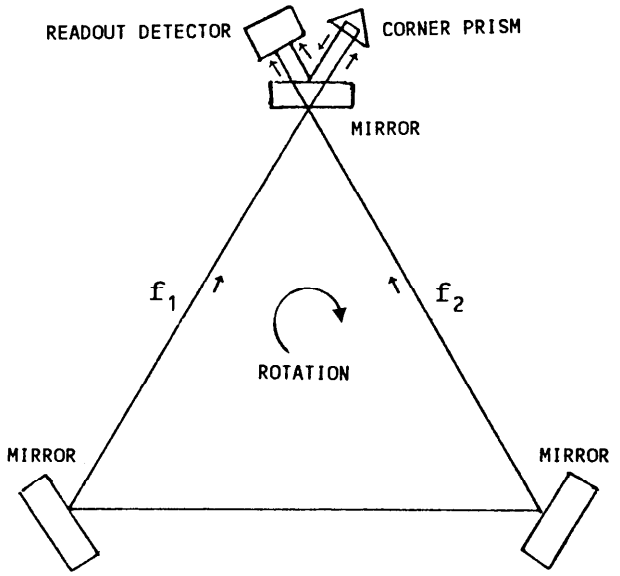

Figure 2. Laser gyro beam paths.

and when expressed in terms of wavelength is

$$\Delta f = \frac{4\omega S}{\lambda L}. \quad (9)$$

This is the basic equation for the laser gyro.

The frequency difference is measured in the readout detector. That is accomplished by the beam-combining optics and two photo cells in the detector. The digitized output and associated electronics provide angular velocity and angular orientation about the axis. There are three such laser gyro elements in the strapped unit in an aircraft, supplying rotational data from all three of its orthogonal axes of rotation.

Note that it was the space medium that enabled the author to derive the laser gyro equation in ordinary time and space, with no Einsteinian relativity involved.

Constituents of the Space Medium

The space medium is different from the old absolute, single, all pervasive ether. It is a massless medium in space, but not fixed to space. The constituents of space medium are the independent vector electric and magnetic fields of all the electrons and protons in the universe. These are vector components in the medium. The net electric or magnetic vector may be zero, but these component vectors are there. *The components of a vector never vanish.*

At the earth the dominant constituency of the space medium is earth-entrained. The reason for this is that the electrons and protons in the earth are so much closer than the other electrons and protons in the universe.

In the Sagnac experiment the instrument was rotating with respect to the dominant constituents of the medium, that are fixed with respect to the earth. The other constituents of the medium, vector components from all the other electrons and protons in the universe, do not rotate with the earth. The earth rotates in that constituency of the medium. It is strong enough for a laser gyro at rest on the earth to sense the rotation of the earth. In fact that feature of the three laser gyro combination senses true north for the navigation instrument.

Laser Speedometer

In a previous paper (Barnes, 1991) I proposed this laser speedometer to detect translational velocity through the space medium. In this analysis of the laser gyro, there was a clue that translation alters the resonant frequencies. However, at the output of the laser gyro it is not detectable because the increase (or decrease) in frequency is equal in both beams. There is no difference in frequency. To detect the translation a difference is needed and that would require two separate outputs, a forward and a rearward laser output. That being the case there is no need for a three mirror laser, so a straight two-mirror laser with outputs at both ends was proposed.

The necessary requirement for resonance in the two-mirror speedometer is the same as that in the laser gyro, namely a constant wavelength that is an exact integer fraction of the total path length. The total path length is up and back, twice the distance between the mirrors. The resonance is standing wave resonance, with electric nodes at each mirror and at every half wavelength between the mirrors. When at rest there is only one frequency. When in translational motion there are two resonant frequencies, one in the forward moving light waves and the other in the rearward moving light waves.

Figure 3 is a diagram of the laser speedometer moving with speed v as shown. When the laser is at rest, with respect to the light propagating medium, its resonant frequency is f_0. There are two resonant frequencies, one above and one below f_0, when the laser is moving with velocity v. Inside the laser, light propagates with speed c-v in the forward direction of motion, and with speed c+v in the rearward direction.

The physics of this phenomenon is quite different from that in old optics experiments in which light waves make only one pass around the circuit and there is no resonance. In the laser gyro and in this laser speedometer the resonance is formed by traveling waves that transit the circuit many times placing wave on top of wave. The lasing enhancement of the light waves takes place in the direction of the traveling wave propagation and provides the gain that is necessary to sustain the oscillations.

The frequency of the forward beam is f_1, and of the rearward beam is f_2. When the resonator is moving forward with speed v, the speed of light inside the laser is c-v in the forward direction and c+v in the rearward direction. The wavelength is constant, so

$$f_1 = \frac{c-v}{\lambda} \quad (10)$$

and

$$f_2 = \frac{c+v}{\lambda} \quad (11)$$

The frequency difference

$$f_2 - f_1 = \frac{2vf_0}{c}. \quad (12)$$

This is the basic equation for the laser speedometer. The difference in frequency at the laser's two outputs is proportional to the speed v. These two output beams are routed by means of mirrors to the readout detector as shown in Figure 3. It converts frequency difference into a speed reading.

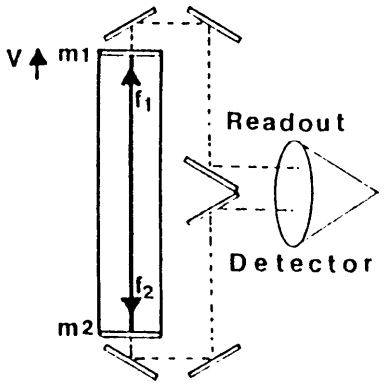

Figure 3. Laser speedometer.

It should be noted that these frequency changes with speed are not doppler shifts. There are no doppler shifts in the laser speedometer. Even in the outside routing of the two laser beams from the laser to the output detector there is no doppler shift. That is because the distance between the laser output and the output detector is fixed. There is no doppler shift in a moving system when the distance between the source and receiver is fixed.

Summary

It has been shown from ordinary time and space physics that the laser gyro generates oscillatory frequencies, sustained by the gain supplied by the lasing process. Not one, but two oscillator frequencies develop when the device is in rotational motion with respect to the space medium. The frequency difference is proportional to the angular velocity. Frequency difference is converted to angular information, useful in navigation.

Of most importance in this paper is the theory of the two-mirror two-output laser speedometer. The same physics, as that in the laser gyro, yields two oscillatory frequencies in its laser cavity as it moves with constant velocity in the space medium. The resonance condition is standing waves with electric nodes at the mirrors between which are equal half-wavelengths. In both the laser gyro and laser speedometer the laser's frequency bandwidth is sufficient to provide the gain at both frequencies, and narrow enough to prevent extraneous resonance frequencies.

The present technology limits this speedometer to low speed. The problem is the limited frequency range of the readout detector. That is a matter of improving technology, not a limitation in the physics of the theory. If this speedometer's detection of constant velocity is verified by experiment, even at low speed, it will falsify Einstein's special theory of relativity.*

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*Dr. Barnes has stated that he would welcome criticism on the concept presented in this paper.

RADIATIVE EQUILIBRIUM IN AN ATMOSPHERE WITH LARGE WATER VAPOR CONCENTRATIONS*

DAVID E. RUSH AND LARRY VARDIMAN**

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Abstract

Equilibrium temperatures are found for several hypothetical atmospheres with large water vapor concentrations (vapor pressure from 50 mb to 1013 mb) at stratospheric levels. Radiative equilibrium is computed using the Air Force radiation algorithm LOWTRAN 7 with no clouds or aerosols. The initial starting condition of an isothermal atmosphere at about -100°C warms to over +100°C at the base of the water vapor layer and becomes isothermal to the surface within 1 to 2 years. Temperatures are sufficiently warm to maintain large quantities of water in vapor form but are too hot for the surface to be habitable. The temperature and pressure in the highest levels are such that cirrus clouds will form. These clouds would reflect a large portion of incoming solar radiation, thereby likely altering atmospheric stability and surface temperature.

Introduction

Climate modelers have varied the concentration of carbon dioxide in order to explain the evidence for different climate regimes in the earth's past. However, few have seriously considered large water vapor concentrations at stratospheric levels. The idea that the atmosphere of the ancient earth may have been overlain by water in one phase or another was apparently first suggested, at least in modern times, by Vail (1965). For Vail, many water canopies existed throughout earth's long history. They resulted from out-gassing of the earth's interior, and their collapse over geologic time formed the oceans. The idea of a vapor canopy appealed to creationists, where it took root and began to be incorporated by them in models of earth history (Whitcomb and Morris, 1961). A water vapor canopy played an important part in their model. It continues to have a major role in many creationist models of the ancient earth (e.g. Dillow, 1982).

There is no direct support for the existence of a water vapor canopy surrounding the earth in the past. However, a survey of the solar system reveals that five of the nine planets, including the one closest to us in distance and size, Venus, have thick cloud canopies. An important effect of canopies in the solar system today is to moderate temperatures beneath them. Planets that do not have canopies show a much wider variance in temperature-diurnally, yearly, and latitudinally. Earth is characterized by a fairly large and permanent temperature gradient between its equator and poles. This temperature gradient produces a pressure gradient, which becomes the driving force behind

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weather systems of the planet. Nearly all climate modelers agree that at some time in the past the earth enjoyed a warmer, more uniform climate from pole to pole.

Possible Causes of a Warm Climate

In modeling the ancient earth, climate modelers usually assume that the total amount of water vapor in the atmosphere has not changed significantly in the past. That is, even though the mixing ratio today varies widely with location and time, the assumption is that the total amount of water vapor in the atmosphere has been essentially unchanging over very long periods of time. The amount of ozone, which has a fairly minor effect overall compared to others, has also been assumed constant. So, large amounts of carbon dioxide are normally introduced into their models (e.g. Hunt, 1984; Budyko et al., 1988, p. 3; Berner et al., 1983). Carbon dioxide is uniformly mixed throughout the atmosphere (unlike water vapor and ozone), and is assumed to have had a much higher concentration in the past. The excess carbon dioxide is now thought to be present in rocks of the crust.

This paper addresses another possible cause for a worldwide equable climate—a canopy of pure water vapor resting on top of today's atmosphere. The layer of water vapor is assumed to compress the atmosphere beneath and mix downward only by molecular diffusion. To prevent the water vapor from condensing, its radiative equilibrium temperature must be hot enough to produce a saturation vapor pressure greater than the total pressure due to the water vapor above. The expectation is that the temperature at the base of such a canopy is quite warm, producing extremely stable atmospheric conditions beneath. Concentrations of water vapor up to one atmosphere will be considered in the canopy.

A successful vapor canopy will meet two criteria:

- The canopy must be stable.
- The surface temperature must be hospitable to humans.

Development of the Model

A number of simplifying assumptions have been made:

- The problem is addressed in one dimension only. Two and three dimensional analyses, which involve meridional heat and mass transfer, in the atmosphere and preferably the oceans also, are much too complex for the first phase of this study. Such routines require expensive main-frame time, and in any case are extensions of a one-dimensional analysis.
- Radiation only will be considered. Other processes active in today's atmosphere are convection, diffusion, conduction, and latent heat release/gain. Of these, only convection and latent heat processes (besides radiation) noticeably modify the temperature profile in the stratosphere and below. However, the radiation calculation must be done first, and its shape will determine whether or not convection can take place. If convection is active, knowledge of water vapor content as a function of altitude may then be used to figure latent heat effects, which will further modify the temperature profile. The addition of these two effects is relatively simple, and could be added to this work if necessary.
- Calculations will be done for a clear sky, with no clouds and no aerosols.

The goal of this initial study is to obtain, purely radiative temperature profiles for various water vapor canopies covering today's atmosphere. The key element in this whole process is determining radiances. For nearly 20 years scientists at the U.S. Air Force Geophysical Laboratories (AFGL) have developed a public domain atmospheric radiance (and transmission) program called LOWTRAN. The present version, LOWTRAN 7, is dated 1989, and contains 18,000 source lines of Fortran code (Kneizys et al., 1988). The spectral data used is from the Laboratories and is generally considered the finest available anywhere. The program is capable of calculating atmospheric absorption and radiance for a wide range of absorber concentrations, pressures, and temperatures (Kneizys et al., 1983 and 1988). Its primary purpose is not for climate modeling as such, but since it gives radiances it may be used to calculate fluxes, and hence temperature profiles. A number of programs, totaling some 2200 Fortran lines, were written for this research to manage LOWTRAN 7 for the task.

The atmosphere is divided into 20 or more "atmospheric levels" of specified altitude, pressure, temperature, and absorber concentration. Pressure and absorber concentration at each level are constant, altitudes and temperatures vary with time. All radiance calculations are taken at constant pressure "flux levels," chosen so that each atmospheric level is exactly halfway between two flux levels. The region between two flux levels is called a layer. The cooling rate of a given layer is then:

$$\frac{dT}{dt} = \frac{g\Delta F}{C_P \Delta P} \tag{1}$$

where dT/dt is the rate of change in temperature of the layer, g is the acceleration due to gravity, C_p is heat capacity at constant pressure, and ΔP is the pressure change across the layer. The heating (cooling) rate is then converted to a new atmospheric level temperature by the equation,

$$T_{n+1} = T_n + \frac{dT}{dt}\Delta t \tag{2}$$

where T_n is the temperature at the nth iteration, and Δt is the time interval. The process is then repeated as often as needed until pre-set criteria for equilibrium are met. For more detail on development of the canopy model see Rush (1990).

Initial Canopy Conditions and Testing

Model runs for today's atmosphere and four different canopies were carried to completion. Water vapor amounts in the canopies were 10, 50, 125, and 1013 millibars. Unless otherwise noted, characteristics of the atmosphere and other assumptions were similar to Manabe and Möller (1961) and Manabe and Strickler (1964), hereinafter called MS. The atmosphere beneath the canopy differed only slightly from the U.S. Standard Atmosphere. The solar zenith angle was set

at 60°, the day fraction at 0.5, and the latitude at 35°N. This approximates average conditions on the earth. A surface albedo of 0.13 was used (Barron et al., 1981), a value midway between today's values of 0.08-0.20 in humid regions (Laval and Picon, 1986). It happens that the ocean albedo at a solar zenith angle of 60° is also 0.13 (Ramanathan et al., 1989). This is somewhat less than today's average surface albedo for the earth (including polar regions) of 0.14-0.18 (Ramanathan and Coakley, 1978).

The day of the year is 109, a day in mid-April, a time of average earth-sun distance. This gives an average value for the solar constant. Spectral intervals used unless otherwise noted: Solar direct — 3500-40000 cm^{-1} (2.86-0.25µm^{-1}) $d\nu$ = 20 cm^{-1}; Scattering — 8000-40000 cm^{-1} (1.25-0.25µm-1) $d\nu$ = 1000 cm^{-1}; Longwave — 20-3500 cm^{-1} (500-2.86µm^{-1}) $d\nu$ = 20 cm^{-1}. Longwave fluxes were calculated by the formula F = *pI*. Shortwave scattering fluxes were calculated by numerical integration of radiances over the hemisphere. Shortwave directly transmitted flux is obtained straight from LOWTRAN 7. It was not necessary to calculate each of the solar fluxes at every iteration, as they are only slightly temperature dependent. New solar fluxes were obtained only once every 30 iterations, or less often as equilibrium was approached, saving much computer time.

The model run for today's atmosphere showed very close agreement with MS, which is widely used for calibration yet today (Liou, 1980). Our profile was typically colder than MS below 10 km and warmer above. Our surface temperature was 320 K, in good agreement with modern values.

Discussion of 50 mb Canopy results

The 50 mb canopy model run will be described in some detail and the other results summarized. 50 mb of water vapor is equivalent to 20 inches of precipitable water. Figure 1 shows radiative equilibrium approached from a cold isothermal beginning point of 170 K. The top of each line represents the "top" of the canopy. Thus at Day 0, the atmosphere canopy is cold and low. By Day 10, the surface is already heated to 296 K, and lower layers of the atmosphere are also rapidly heating. This is due to the intense solar radiation that has been absorbed by the ground and reemitted as infrared, where it is readily absorbed by the relatively high water content of the lower layers. The middle layers are relatively low in water vapor and ozone (carbon dioxide has the same mixing ratio everywhere) and so they tend to be transparent to both shortwave and longwave. The upper layers constitute the stratosphere in today's atmosphere and therefore are low in water vapor and high in ozone. They are heated by absorption of solar ultraviolet.

The discontinuity at about 18 km in Day 10 represents the top of the atmosphere and base of the canopy. Note that the great majority of the mass of the atmosphere-canopy is below 18 km. The lower portion of the canopy is heated primarily by absorption of longwave from the ground, but also somewhat by solar absorption. From 18 km (pressure = 50 mb) to about 52 km (pressure = 1 mb) the canopy cools as longwave radiation is emitted to space. The top cools so well that it is several months before it again attains its starting point of 170 K. However, additional months of heating do not have much effect, and it ends at

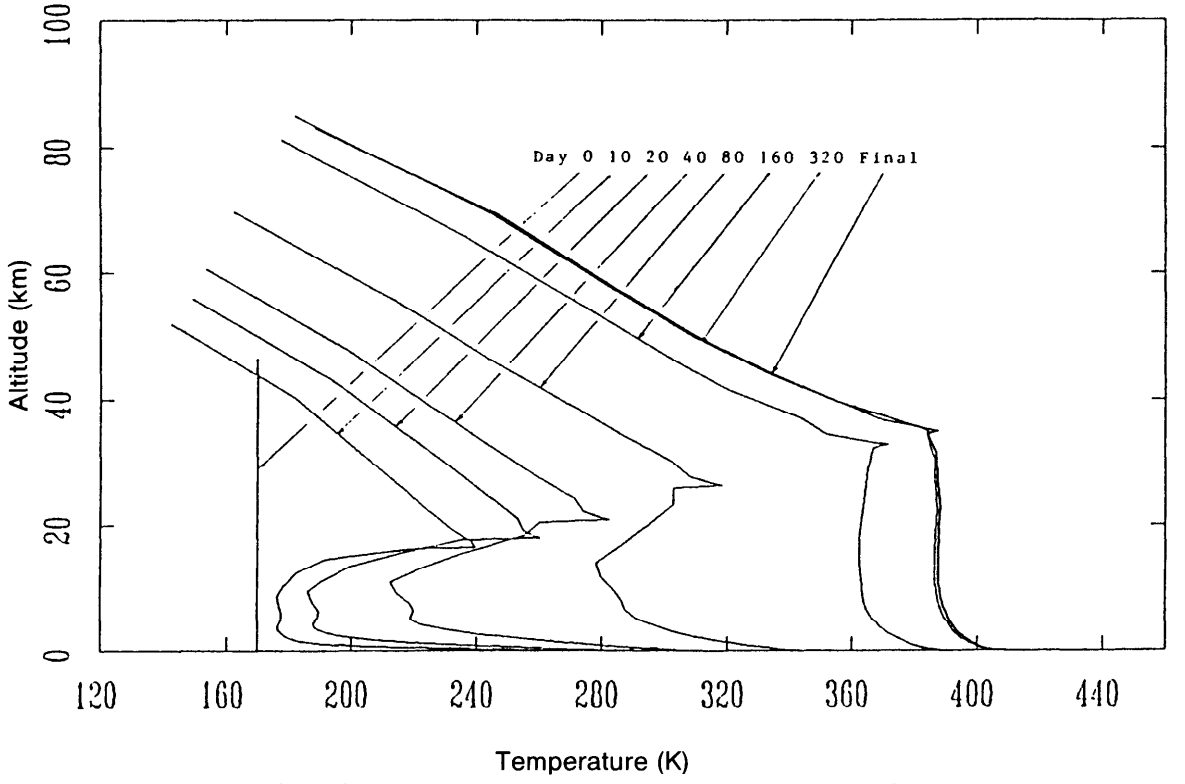

Figure 1. Vertical temperature profiles of a 50 mb canopy above today's atmosphere starting from an isothermal 170 K condition approaching equilibrium.

only 189 K, or -84°C. Since the water vapor pressure at the top of the canopy is much higher than the saturation vapor pressure at 189 K (2×10^{-5} mb), the vapor will turn to ice. At the next level, this is also true. The vapor pressure is 4.615 mb, the saturation pressure (at 246 K) only 0.27 mb, and the vapor will also tend to become ice. At the next level down however, the vapor pressure is 11.597 mb, the saturation pressure (at 284 K) is 13 mb, and the vapor will remain in the vapor form. At all lower levels of the canopy the vapor pressure is lower than the saturation pressure and the water will be in the vapor phase. A temperature of 387 K at the base of the canopy (50 mb) guarantees the vapor phase. In the final profile, the canopy base has risen to 35 km and the top (1.279 mb) to 83 km.

The critical lapse rate for water vapor, if we assume no phase change, is the adiabatic lapse rate, which is 5.3 deg/km. If the observed lapse rate exceeds this, convection will occur as hotter, less dense gas tends to rise and colder, more dense gas tends to sink. If the observed lapse rate is less than the critical one, there is no tendency to overturn and the canopy is stable. The lapse rate in the lowest canopy layer between 48 and 50 mb is 9.6 deg/km. Therefore, convection will begin. The next layer also shows a tendency to convect. Beyond this, all higher layers have a lapse rate less than 5.3 deg/km, and will be stable.

The situation is similar to that of the atmosphere today, where unstable lower layers often send mass and heat up into higher, stable layers. The model does not contain any convective adjustment, so the very thin layer of instability near the bottom of the canopy will dissipate quickly in a real atmosphere where layers are allowed to release the instability. The thick stable layer above will cap any minor adjustments at the bottom of the canopy.

The topmost layers of the canopy involve a phase change, so their critical lapse rate will be different from the adiabatic one. The atmosphere itself is nearly isothermal down to the lower layers, which show slightly higher temperatures. Only in the lowest two (thin) layers is the critical (adiabatic) lapse rate of 10 deg/km for "air" exceeded. There will be a slight convective transport of heat from the surface into the first several hundred meters. This will lower the surface temperature by a degree or two, and raise the temperature just above by a corresponding amount. Overall, the atmosphere will be quite stable. At the surface, the initial rapid heating has slowed so much that the final six months see a rise of only one degree, to 409 K, or 35 degrees above the boiling point of water at 1063 mb.

Figure 2 shows the heat balance of the earth at equilibrium and transmission values for the infrared, all for the 50 mb canopy. Of 100 units of incoming solar, 19 units are absorbed by water vapor in the canopy, (all absorption values include a small amount due to absorption of reflected [outgoing] solar) 8 additional units (of the 100) are absorbed by the atmosphere, and 5 units reflected by the atmosphere-canopy to space. That leaves 68 units that reach the surface, most transmitted directly but some scattered. 8 units are reflected at the surface, contributing to the total planetary albedo of 13 units, and leaving 60 units absorbed by the earth. These 60 units are then re-emitted as longwave radiation. Actually, because of the very high surface temperature 478 units total are emitted by the surface, but 418 of these have been received as longwave from the canopy and atmo-

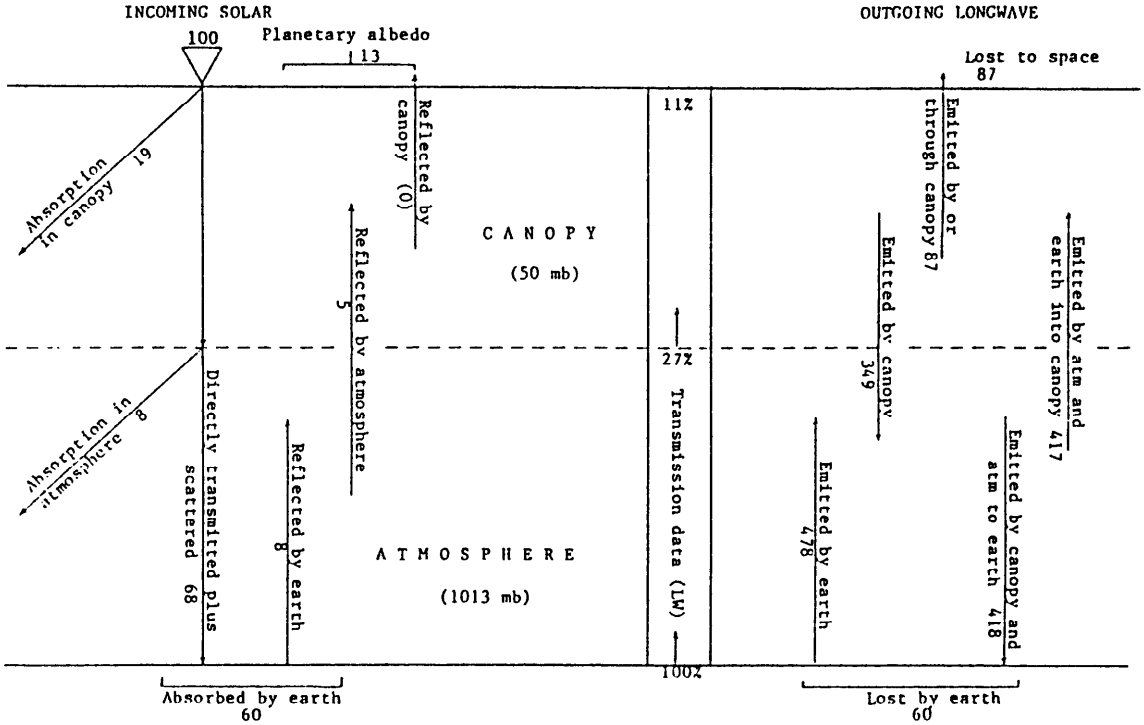

Figure 2. The heat balance of the earth-atmosphere-canopy system for a 50 mb canopy over today's atmosphere. The left portion of the diagram shows the flux of short-wave radiation, the right portion the flux of long-wave radiation, and the middle portion the transmission of long-wave radiation upward to space. The canopy is above the dashed line and the atmosphere is below the dashed line.

sphere, for a net infrared loss of 60. This balances the net solar gain of 60.

An infrared energy balance of the atmosphere shows 478 units of terrestrial radiation, 349 units of infrared from the canopy entering, 418 units leaving to the ground, and 417 leaving for the canopy, for a net infrared cooling of 8 units. This balances the solar absorption of 8 units. A balance on the canopy shows it receiving 417 units from below, losing 349 units downward, and 87 upward to space. This is a net infrared cooling of 19 units, which balances the solar absorption of 19 units.

Overall, the 87 longwave units emitted to space plus the 13 shortwave units reflected to space account for the original 100 units received from the sun. It is readily apparent that the canopy is very effective at trapping the earth's radiation. Without the windows to space that exist today, temperatures build until the canopy's emission to space finally equals the net incoming solar. To be more precise, the windows are not totally closed with the 50 mb canopy. Also shown in Figure 2 are transmission data. These percentages show the amount of surface longwave radiation that arrives unimpeded at the canopy base (27%) and at the canopy top (11%). Without the canopy, 27% of the terrestrial radiation would escape straight to space, but only 11% does. This difference may at first seem small, but it means that the entire earth-atmosphere-canopy system must heat to the point where it radiates enough extra energy to space to make the difference.

In conclusion, only 50 mb of water vapor added above the present atmosphere would raise the surface temperature as determined by a radiation balance from 320 K to 409 K. A better comparison is to include convection effects. Convection lowered the MS ground temperature in today's atmosphere (less clouds) from a pure radiational 332 K to 300 K, much closer to the observed 288 K. As mentioned in discussion earlier, convection in the atmosphere under the 50 mb canopy would probably lower the surface temperature only a degree or two. So it seems that addition of only 50 mb of water vapor above the present atmosphere would raise the surface temperature more than 100 degrees.

Kasting and Ackerman (1986) added 10 *bars* of CO_2 and obtained a surface temperature of only 400 K, including convection effects, at present solar luminosity. Truly, the water molecule has an amazing ability to absorb radiation. The contrast with CO_2 is all the more marked when it is seen that a large part of the Kasting and Ackerman "CO_2 caused" temperature increase is actually caused by water vapor from increased oceanic and lake evaporation. In the 50 mb canopy, there certainly would be increased tropospheric water content from evaporation, but it has not been considered.

Discussion of Other Canopy Results

Vertical temperature profiles for canopies with 10, 125, and 1013 mb of water vapor show similar distributions as the 50 mb canopy but hotter for thicker canopies and cooler for the thinner canopy. Figure 3 shows the surface temperature as a function of the mass of the canopy. As the mass of the canopy is slowly increased from zero, the surface temperature

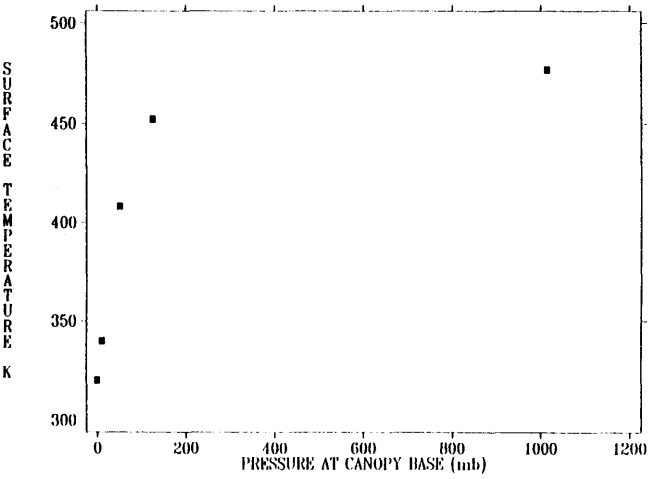

Figure 3. Radiative equilibrium surface temperature as a function of mass of the overlying canopy.

rapidly increases. At a canopy mass of 125 mb, the longwave windows to space are nearly closed, and additional water vapor has little marginal effect. At 1013 mb (1 atm), the windows are totally closed. No longwave terrestrial radiation escapes straight to space.

Surface temperatures are directly related to the mass of the canopy and produce too warm a surface temperature to be hospitable for life under pure radiative equilibrium for all canopies studied. However, for the 10 mb case inclusion of convection would noticeably decrease the surface temperature, perhaps into the suitable 300-310 K range. In each case the temperature at the top of the canopy is below freezing. The cold temperature causes the saturation vapor pressure to fall below the ambient pressure, producing a cirrus cloud layer. Near the surface of the earth and at the base of the canopy, thin layers are convectively unstable, based on the temperature lapse rate.

Conclusions

It was stated earlier in this paper that two criteria for the vapor canopy would need to be met: 1) Stability, and 2) A surface temperature suitable for habitation. The first criterion was met. For any size canopy considered, at least from radiation analyses of pure water vapor canopies, it was shown that the temperature is always sufficienty high throughout most of the canopy, particularly at the base, to easily ensure the vapor phase. The second criterion is not as straightforward to evaluate. Radiation considerations strongly suggest surface temperatures are not suitable for the 50, 125, and 1013 mb canopies. The greenhouse effect of the canopy is simply so effective that the surface temperature becomes inhospitable. This could also be true for the 10 mb canopy, though convection considerations may alter this conclusion. Inclusion of convection in the denser canopies would not change this verdict.

It does seem reasonable to suppose that somewhere between 0 and 50 mb there exists a value that would lead to a successful canopy. Remarkably, this is the same conclusion reached by Kofahl (1977) with his "sliderule estimates." He suggested a total water vapor content in the atmosphere-canopy of six inches, or five inches (12 mb) more than the atmosphere alone.

Morton (1979) was apparently the first to conclude that the canopy would have made the earth's surface too hot for human habitation (Kofahl did not calculate surface temperatures). Morton made a number of assumptions that greatly simplified the problem, and his surface temperatures are much higher than ours, but the general conclusion is the same: Life as we know it would not have been possible under a canopy of 1013 mb (1 atm), nor even with a canopy of only 50 mb.

When other features such as clouds are added to the model, this conclusion could be modified greatly, however. Preliminary explorations with cloud layers at the top of the 50 mb canopy have shown significant radiation effects which lower the surface temperature drastically. Unfortunately, while the surface temperature decreases when clouds are added, so does the temperature of the canopy, reducing its stability.

Recommendations*

Numerous improvements in this model need to be made including convection, latitudinal transport of heat, and inclusion of aerosols and other minor gases. However, the first effort should be to determine the effect of cirrus cloud layers near the top of the water vapor layer.

*Editor's Note: Readers may be interested in a recent Quarterly paper on the vapor canopy. Walters, T. W. 1991. Thermodynamic analysis of a condensing vapor canopy. *CRSQ* 28:122-131.

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My colleague at *National Review*, Joseph Sobran, however, has assayed the task, and in conclusion I would like to draw upon his words in one of his recent essays.

The liberal, he writes, possesses an "integral world view" that

> sees man as an animal; an animal whose destiny is a life of pleasure and comfort. Those who view things in this light tend to believe that this destiny can be achieved by means of enlightened governmental direction in removing (and discrediting) old taboos, and in establishing a new economic order wherein wealth will be distributed more evenly. It is interesting to note that they describe such a redistribution as being "more equitable," because that suggests [note: the environmental thesis again] that they ascribe inequalities of wealth to differences in circumstances rather than ambition, intelligence, fortitude, or any of the myriad other moral virtues that may lead to fortune. . . .
>
> It is interesting to note, too. . . . that they never deride or censure human behavior as "bestial" or "animal," because they see man himself as an animal in essence, and cannot be indignant about behavior proper to an animal. They are indigant about suffering, which is to say animal suffering—pain, hunger, physical discomfort—and the frustration of animal appetites in general. . . .
>
> This is a morally passive view of man. . . . The middle-class virtues are assumed to blossom spontaneously under the right material conditions; progress comes inevitably, so long as there are not reactionaries "impeding" it. . . . Although [this view] asserts the obligation of those who are well off to share their abundance with the "less fortunate," they can never make demands of the less fortunate themselves. . . . It is characteristic of them to invoke the poor early in any public discussion. . . . As James Burnham has penetratingly put it, the liberal feels himself morally disarmed before anyone he regards as less well off than himself. . . . If pleasure is man's destiny, it is his right. Nobody should have to endure hardship, even if he brings it on himself. Parenthood, when it comes unlooked for, is cruel and unusual punishment, and people who fornicate no more deserve to be assigned its duties than a man who kills somebody deserves to be hanged.

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SOME BIOLOGICAL PROBLEMS OF NATURAL SELECTION THEORY

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Abstract

The many difficulties with the natural selection *hypothesis are reviewed, including the problem of extrapolating generalizations from limited artificial selection research to megaevolution. Using evolutionary criteria, the hierarchy found is the reverse of that expected by evolution theory; animals lower on the evolutionary scale were found to reproduce in greater numbers, and were as a whole more resistant to variations in the environment. Individual survival after birth tends to be mostly the result of chance; in most cases natural selection eliminates only the sick and the deformed. Environmental variations which cause evolution—temperature, the population of other animals, and the surrounding plant life, all of which have been fairly stable for eons—can result in only very limited degree and types of changes. The natural selection hypothesis also involves circular reasoning; an extant species survived because it was fit, and must be fit because it obviously has survived. The commonality of* overdesign, *or the existence of complex mechanisms that do not effect survival, but may add much to the quality of life, also creates a severe problem for the natural selection theory.*

Introduction

One acquainted with the biological world is keenly aware of its incredible complexities and natural wonders. As to the meaning of these observations, Macbeth (1971, p. 68) notes, "Bitter controversies rage over what the demonstrated facts signify, how they have come about, and why they are as they are." This paper focuses on the major problems of positing natural selection as the primary explanation for the complexity and diversity universally displayed in the living world. The importance attributed to natural selection as a cause of evolution varies widely. Some students of nature conclude that it is the *only* essential causative factor, others that it is of almost no importance. One reason why this divergence of views about the importance of natural selection in megaevolution exists, Macbeth (1971, p. 42) notes, is because "We are dealing with something invisible. The operations of natural selection, real or imagined, are not accessible to the human eye." By natural selection is usually meant, ". . . the belief that random variation can, when subjected to selective pressure for long periods of time, culminate in new forms, and that it therefore provides an explanation for the origins of morphological diversity, adaptation, and when extended as far as Darwin proposed, speciation" (Brady 1982, p. 79). Darwin's definition of natural selection was the preservation of favorable individual differences and variations, and the destruction of those which are injurious, and the survival of the fittest (Johnson, 1976, p. vii).

The theory that natural selection is the major driving force of evolution is based on the fact that not all conceptions result in births, and only a certain percentage of animals that are born alive survive to adulthood, and even less are able to successfully reproduce. It is also assumed that those that survive to reproduce are more likely to be better adapted to the environment, and are generally biologically superior. As a result, each generation is assumed to produce animals that are slightly better adapted to local conditions than the previous one. Slight genetic mistakes or imperfection called mutations may result in some new traits. Although most mutations are neutral or maladaptive, it is believed that a very few may aid a given population's adaption, and these may eventually change the composition of the gene pool, slowly producing more and more variety. This process of natural selection is the means of selecting the best of this variety, causing evolution. As Gould (1977, p. 22) explains, its force comes from the following logic:

> (1) Organisms vary, and these variations are likely inherited by their offspring. (2) Organisms produce more offspring than can possibly survive (many do at least). (3) On the average, offspring that vary strongly in directions favored by the environment will survive and propagate. Favorable variations will therefore accumulate in populations by natural selection.

Yet Gould admits that, although Darwin convinced much of the world that evolution has occurred, the natural selection concept never achieved much popularity during Darwin's lifetime, and did not prevail as the putative major cause of evolution until the 1940's. It now typically forms the *core* of modern evolutionary theory (Ayala, 1974). As Johnson (1976, p. vii) notes, natural selection is no minor theory, but is considered ". . . so fundamental and outwardly simple that few introductory texts assess the actual evidence and fewer still describe the methods and assumptions required of its study." The reason that some of the early evolutionists had difficulty accepting this concept varied. Ruse (1982, p. 49) notes that Huxley, the most vocal supporter of evolution in Darwin's day, "always had doubts about the overall effectiveness of natural selection." As Ruse (1982, p. 51) notes, the reasons for the resistance to natural selection include:

> It is one thing to accept selection per se, and it is quite another to agree that selection can be everything that Darwin claimed for it. There is much drawing back from selection as an all-powerful evolutionary mechanism, even by those who were turned into evolutionists by the Origin. The general feeling was that evolution had to be powered primarily by something else. Many readers felt that selection working on blind, small variations simply could not be the causes of the wonderful adaptations like the hand or the eye. Therefore, not a few of Darwin's contemporaries, primarily

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> for religious reasons, supposed that the main cause of evolutionary change are instantaneous, God-designed 'jumps' from one form to another—as from the fox to the dog. That is, they believed in an evolution powered by 'saltations.'

Ironically, the essence of Darwin's contribution lies in his contention that natural selection is the major creative force or source of evolution, not just the executioner of the unfit (Gould, 1977).

Although many researchers conclude that natural selection is the major cause of evolution, most ascribe varying degrees of importance to other factors. Some of these include chance recombination of existing genes which produce positive, negative or neutral characteristics (neutral meaning of equal survival value compared to the parent gene structure), population fluctuations due to chance factors, geographical factors such as oceans or mountains which cause breeding isolation, gene flow, and changes in the length of reproduction and fertility periods. Each of these, separately and in combination though, are totally insufficient to account for evolution (Williams, 1966).

The best-known major rivals of the gradual evolution via natural selection model are *vitalism, Lamarckism, mutationism,* the *neutralists theory* (the theory of evolution by random walk) and *Goldschmidt's hopeful monster* theory, all of which have now been largely rejected, although occasionally books surface that defend one of these theories, especially *vitalism* and *Lamarckism* (Ayala, 1974). The *hopeful monster* idea, in a revised form with a modern cover called punctuated equilibrium, has recently gained rapid acceptance in the biological world. Many feel that its acceptance is due less to the evidence supporting the view, but more because the competing theories contradict the empirical evidence.

The major concern of megaevolutionists is to explain the incredible diversity in the living world. Pandas, elephants and mice are all biologically basically similar, yet manifest many differences. Even more different are the reptile, mammal, bird and insect and fish divisions. A viable theory of origins must explain this often unexplainable diversity, and the fact that literally millions of different species of animals and plants exist. The explanation that each living type was separately created by God in the creative week described by Genesis was historically accepted by most westerners, and probably most scientists as well, until the middle 1800's (Gould, 1981). Darwin believed that he had an answer which was beguilingly simple, and this simplicity partly explained its rapid and often uncritical acceptance. His answer was that scientists had for decades misinterpreted what they found in the fossil record: they actually were examples of animals that were not survivors and from which today's more perfectly adapted life forms arose. Darwin taught that those forms that still exist today were better able to survive climatic changes and the competition for mates, food, air, and space resources, and that the predecessors of modern forms were generally weaker, smaller, and less well adapted than other contemporary animals. In short, the extinct forms were wiped out by what Darwin called *natural selection.*

Darwin (1958, p. 120) concluded after he read Malthus' work on population that, ". . . it at once struck me that under these circumstances favorable variations would tend to be preserved and unfavorable ones destroyed. The result of this would be the formation of new species." The forces of drought, wind, animal predators, cold, heat and disease all tend to kill the weaker animals, leaving the stronger to reproduce. Since most creatures produce far more offspring than can possibly survive, "natural" selection can select the best or most fit, and these then will be likelier to reproduce. Darwin stressed that only the most fit, the strongest, and the most able survived the vicious competition for life: only the fastest runners, those with hardier hearts, better eyes and other sensory organs, stronger or longer legs (enabling them to run faster), and those with the most effective means of defense—quills on a porcupine and stink on a skunk—win in the constant struggle of life. Darwin then went far beyond this truism, expounding that all life, everywhere today and in the past, was created by evolution and is still evolving by a process that results from a never ending struggle for survival.

This, in short, is Darwin's theory of evolution, an idea that was by no means new to Darwin. Gould (1977, p. 23) claims that,

> Contrary to popular belief, evolution was a very common heresy during the first half of the nineteenth century. It was widely and openly discussed, opposed, to be sure, by a large majority, but admitted or at least considered by most of the great naturalists.

Darwin simply went farther than most and, importantly, was able to widely popularize the theory. According to Gould, Darwin's work consisted of uncompromising philosophical materialism in contrast to other evolution theories, most of which utilized vitalism or elements of a theistic evolution. Darwin's claim that, except possibly for the first few life forms, primarily random variation and natural selection were needed to account for the estimated over 2,000,000 species of animals and plants that now exist.

Much interest existed in Darwin's time in animal breeding and, in spite of the claim that Darwin obtained his theory primarily from his observation of the Galapagos Island finches and Malthus' work, the germ of his idea quite possibly stemmed in part from the logical deduction that, if we can breed a meatier cow, a faster horse, a fatter chicken, then we could also produce an *even more* meaty cow, a *still faster* horse, or *yet fatter* chicken. He then argued, if humans can bring about such changes in animals, could not nature itself also be constantly selecting the best by killing the less fit? Is not the bull that earns the right to breed the most powerful one, the most attractive peacock the one that has the most right to mate? The major difficulty that Darwin saw was that the changes obtained by animal husbandry were small: farmers could improve sheep's wool or make a redder rose, but obvious limits seemed to exist: Humans could not breed horses from dogs (some felt they could someday) or wings on dogs (this seemed harder to comprehend, but not impossible).

Although most biologists of the time concluded that clear limits to change existed, Darwin believed on faith that no limit existed. As he stated in his *Origin of Species*, "I can see no difference in a race that bears being rendered, by natural selection, more and more aquatic in the habits . . . [and larger and larger] until a creature was produced as monstrous as a whale." And (1962, p. 63),

> Slow though the process of selection may be, if feeble man can do so much by . . . artificial selection, I can see no limit to the amount of change, to the beauty and infinite complexity of the co-adaptations between all organic beings, one with another, and with their physical conditions of life, which may be affected in the long course of time by nature's power of selection.

Darwin reasoned that since many mammals—horses, cows, sheep, pigs, dogs, cats and goats—were all basically similar (each had a backbone, a brain and skull, four legs, hearts, kidneys, and similar reproductive systems) if we could breed faster horses, why could we not breed *any* mammal (or at least most mammals) from some common ancestor? After all, as much difference *appears* to exist between a poodle and a German shepherd as between a Pekingese and a cat. Darwin (1962, p. 82, 92) thus developed the opinion that all animals and plants could vary in any directions *to an almost unlimited degree.*

That all animals differ slightly, even from their own brothers and sisters, is obvious; in a litter of cats, some are slightly larger than others, some are solid white, others darker in color. Darwinists believed that these slight variations gradually, almost imperceptibly, could have changed a species into a new one. If in each generation the slightly faster runners, better jumpers, or stinkier stinkers were likelier to survive, the future generations of these animals would run faster and faster, or jump higher and higher. Hitching (1982, p. 12) concluded,

> The idea seem so blindingly obvious, and so satisfying complete that, in England at least, it quickly replaced the biblical account of creation, and became a new way of looking at the living world. With a few hiccups, it has held its place [throughout the scientific world] ever since.

Support for natural selection depends heavily upon the validity of its analogy with artificial selection (Tinkle, 1976). Darwin might have been justified in utilizing the animal breeding analogy to illustrate a limited process, but the use of natural selection as the *major support pillar* for macroevolution is problematic. In the first chapter of *The Origin*, Darwin discusses extensively artificial selection and extrapolates far beyond what his data warrants (Gale, 1982). The two major problems with this analogy between artificial and natural selection include:

> 1) Almost all the traits that breeders breed for have nothing to do with survival, and thus nature would not select for them; we breed dogs for certain appearance traits, horses for speed traits, cows for milk traits, and chickens for egg traits.
> 2) Animal breeders have found that select traits are often *lost* if random breeding again occurs, or if breeding for other traits is done. Few if any permanent changes in the animal usually occur, only the probability of certain traits appearing is altered.

The problem, both then and now, was going from the known to the unknown. Humans have produced many new strains of animals through breeding which have made our life easier and more pleasant. Although these strains were different in certain major ways from their predecessors, they usually soon reverted back to the previous types if allowed to interbreed with them again. Totally new major traits were never developed, but existing ones were re-arranged and favorable ones retained so that certain traits were more pronounced. This type of evolution (if it could be called such) is often termed *microevolution*, as opposed to *macroevolution*. Breeding solid black horses is microevolution, breeding winged horses is macroevolution. This dichotomy is artificial, and a *clear* distinction cannot always be made—and what is now macro may be classified as micro, meaning possible. Microevolution is what we have achieved, thus have experimentally verified, and this is probably a more realistic definition. Macro is what we hypothesize *could* be achieved, or which, according to fossil evidence and conjecture, *might* have occurred in the past, given a set of assumptions about the fossil evidence.

Now that researchers have a tremendous amount of experience in breeding animals, it is clear that it can be carried only to a very limited level, and many traits tend to revert to where we started—fruit fly traits, after eight to ten generations, tend to revert back to normal (Tinkle, 1976). The fact is, extensive breeding by millions of researchers and breeders has not produced a single undisputed new species in 400 years of experimenting (Johnson, 1991). As Eiseley (1958, p. 223) noted:

> . . . careful domestic breeding, whatever it may do to improve the quality of race horses or cabbages, is not actually in itself the road to the endless biological deviation which is evolution. There is a great irony in this situation, for more than any other single factor, domestic breeding has been used as an argument for the reality of evolution.

Deevey (1967, p. 636) concludes, "Remarkable things have been done by cross-breeding . . . but wheat is still wheat, and not, for instance, grapefruit. We can no more grow wings on pigs than hens can make cylindrical eggs." A more contemporary example is the average increase in male height that has occurred the past century. Through better health care (and perhaps also some sexual selection, as some women prefer taller men as mates) males have reached a record adult height during the last century, but the increase is rapidly disappearing, indicating that we have reached our limit.

Darwin's error was in stretching this comparison too far, sooner or later we reach limits, and no one has yet observed helpful macroevolutionary changes taking place. Since we do not have several billions of years of direct observation, we have not been able to directly test this assumption. Nevertheless, some ani-

mals such as fruit flies live a very short period of time, enabling us to observe multi-thousands of their life generations. Even with a drastically higher artificial increase in the number of mutations, which are supposedly the source of variation which gives rise to the "stuff" from which natural selection can select, no evidence exists that large changes have, or can, occur (Lester and Bohlin 1984).

Even Gould (1977, p. 39) admits, " . . . although I wear the Darwinian label with some pride, [I] am not among the most ardent defenders of natural selection." More blunt is Bethell (1976) who concludes, "Darwin's theory [of natural selection] I believe is on the verge of collapse. . . . Natural selection was quietly abandoned, even by his most ardent supporters, some years ago." Gould, in an article defending natural selection (1977, p. 40-41) admits that, "Bethell argues quite correctly that [Darwin] relied upon analogy to establish it [his definition of survival of the fittest] a dangerous and slippery strategy." Yet, many scientists are still struggling not only to define it, but also to demonstrate that it has a role in megaevolution (Maddox, 1991, p. 653).

The assumption that all life and all of its traits owe their existence primarily to natural selection, thus these traits must be adaptive, is still supported primarily by thought demonstrations. Natural selection explanations are often similar to dream interpretations: the explanation may be logical and fully understandable, yet there is no way to empirically document it. The logic that any particular character was or might be adaptive was regarded by many as sufficient proof that it owes its origin to natural selection, but this evolutionary speculation has few connections with the concrete facts of cytology and heredity or with actual experimentation.

The Fossils and Natural Selection

The fossil record does not support the case for natural selection. One excellent summary (Gliedman, 1982, p. 90-91) reflects the current opinion well:

> No fossil or other physical evidence directly connects, man to ape. . . . The problem for gradualists [those who support gradual evolution or orthodox Darwinian evolution] is that . . . these ancestral species remain essentially unchanged throughout their million-year life spans, yet each of them differs substantially from its immediate predecessor. . . . Sudden-change theorists find plenty of support for their point of view in the glaring list of critical evolutionary events that no gradualist, including Darwin, has ever explained satisfactorily. In addition to the lack of a missing link to explain the relatively sudden appearance of modern man, gradualists cannot easily explain the mysterious 'Cambrian explosion' 600 million years ago. This was an evolutionary leap that transformed the earth . . . from a mess of simple microscopic bacteria and blue-green algae to a planet bursting at the seams with primitive representatives of every type of multicellular plant and invertebrate animal-from the lowly protozoans to such complex creatures as the trilobites, . . . the best that gradualists can do is point to the ground beneath their feet; the fossils buried in the earth somewhere, they say, and may someday be discovered.

The lack of transitional forms is a serious problem that can no longer be attributed to hypothesized undiscovered fossils (Johnson, 1990; Gould, 1989). All of the multi-millions of fossils so far discovered fit quite well into existing groups and rarely is it even argued that a fossil type fits between two orders or even families. Animals have come and gone, but very few of them meet even the minimal requirements necessary to claim that their fossil type is one of the many billions of different transitional forms that must have existed if the gradualist view is correct. To explain this difficulty, believers in the punctuated equilibrist's view of Gould postulate that relatively few links exist, and very few fossils can be found because the rate of evolution during the gaps was geologically rapid. The theory also argues that the transitional forms were highly unstable, thus rapidly died off, leaving behind very few fossils. But once an animal was in a stable slot in the environment, though, it existed for long periods of time consequently leaving behind many more fossils during this stage.

The major problems with the punctuated equilibrium view is that it is based on almost a total lack of transitional forms; consequently one might ask, "How do we know that these creatures existed and were unstable if we have no evidence of them?" The reason that this is concluded is if they were stable and survived for long periods of time, we would have abundant evidence of them. Since we do not have this evidence, given evolution is true, they must have existed, but only for a short while and this is why no evidence of them now exists. This argument from lack of evidence is, at best, misleading and, at worst, involves the circular reasoning fallacy. In the punctuated equilibrium view, multi-millions or more transitional forms must also exist, just fewer than in the old view. Arguing primarily from lack of evidence is also true of the gradualist model: none of these links have been discovered for certain. Hitching (1982, p. 40) concludes that:

> Today most museums and textbooks accept gradualism as readily as they accept natural selection. Logically, then, the fossil record ought to show this stately progression. If we find fossils, and if Darwin's theory was right, we can predict what the rock should contain; finely graduated fossils leading from one group of creatures to another group of creatures at a higher level of complexity. The 'minor improvements' in successive generations should be as readily preserved as the species themselves. But this is hardly ever the case. In fact, the opposite holds true, as Darwin himself complained; "innumerable transitional forms must have existed, but why do we not find them embedded in countless numbers in the crust of the earth?" Darwin felt though that the "extreme imperfection" of the fossil record was simply a matter of digging up more fossils. But as more and more fossils were dug up, it was found that almost all of them, without exception, were very close to current living animals. Size and shape may have varied, such as the woolly mammoth compared to elephants today, but the variations were small.

Fossil intermediates are consistently missing in virtually all of the most important places, and some paleontologists argue that no true, major transitional forms have been shown to exist, and that all claimed transitional forms are, at best debatable. Macroevolutionists generally concede that, although the evidence for intermediates is at present limited, they have faith that they will be found in the future if we just keep digging. The limited evidence, such as the few hypothesized transitional form claims as *Archaeopteryx*, often do not stand under examination. *Archaeopteryx* is probably the best-known and oldest example of a supposed intermediate, and the creature's traits, as well as where it fits in the fossil record, are still being hotly debated. Benton (1983, p. 99) concluded that "no consensus on *Archaeopteryx"* exists, and that scientists are still debating even such basic questions as, "can the bird fly, is it ancestral to birds, did it originate from dinosaurs or from some earlier stock and, indeed, is it even a bird?" He (1983, p. 99) quotes a detailed study on the brain case of *Archaeopteryx* that concludes that the "details of the brain case and associated bones at the back of the skull seem to suggest that *Archaeopteryx* is not the ancestral bird, but an offshoot from the early avian stem." The relationship of *Archaeopteryx* in the origins of bird controversy is so controversial that Thulborn and Hamley in an extensive review identified seven hypotheses concerning the affinities of *Archaeopteryx* (Benton, 1983, p. 100).

This notorious lack of transitional forms is not due to any shortage of fossils. Billions have now been unearthed, so many that quality specimens are often sold to collectors for as little as a quarter. Petroleum, oil, natural gas, chalk, cement and many other petrochemicals and minerals are claimed to be products of fossils, thus are called fossil fuels or minerals. Over 250,000 different species of fossil plants and animals are known to exist, and almost all of them are extremely similar to the 1.5 million species now known to be living on earth (and about one million of these are insects) while the rest fit into known extinct types (Day, 1989). When a fossil is unearthed, it most always is known type. Discovery of a new species, whether extant or extinct, is a once in a lifetime event for many zoologists that is often rewarded by naming the species after the discoverer.

Rensch (1959) admitted that few, if any, examples of micro changes (which he calls *transpecific* evolution) exist in the fossil record. He added hopefully that finding intermediates in the future should not yet be regarded as impossible. Most research areas along this line have turned out to be dead-end roads which have diverted biologists from other far more promising areas of research. Darwin's explanation for the lack of transitional forms, the alleged extreme imperfection of the geological record due to our poor search efforts, can no longer be used to explain away the evidence. We now have enough fossils to be assured that we have a fairly good idea of the variety of past animal life, especially those types with hard parts. We can even make some reasonable conclusions about the extinct forms and variety of animals, such as jelly fish and bacteria, which are not preserved either as well or as often as animals with hard parts. Our good knowledge of many ancient insects is partly due to the many types that are preserved in amber or other substances which prevent the decay of the soft, fragile parts (Reid, 1985). These were described eloquently by Zahl (1978, p. 237):

> Recently, in a laboratory at Harvard's Museum of Comparative Zoology, I focused a magnifying glass on a clear marble-sized sphere in which a tiny fossil fly hung suspended. . . . This elegant piece of tea-hued amber, along with its elfin inclusion, was only one of several thousand stored in drawers stacked from floor to ceiling in the Museum's Department of Fossil Insects. . . . In each was a fly, ant, grasshopper, beetle, or spider, all perfectly lifelike as though some magic wand had cast the spell of frozen sleep upon them. . . . embalmed you might say, fifty million years earlier; yet its tenants looked singularly like the fly, ant, grasshopper, beetle, or spider in my own garden. Had evolution overlooked such genera during the intervening fifty thousand millennia?

Trilobites, although long extinct, have been studied extensively and we now know a great deal about the morphology, growth, molting, appendages and internal anatomy of the 60 known species. We even have good insight into how their holochroalic eyes work. Enough is known about the past living world to produce a fairly good picture of it. And, this picture precludes macroevolution.

Natural selection, although it "explains" minute changes, is far less viable in explaining the events called for by the theory of punctuated equilibria. Many of the challenges to Darwinian evolution are specifically challenges to natural selection. And these are such that the theory at the very least requires severe modification (Leigh, 1971). As Hitching (1982) stated, "Darwin's explanation of evolution is being challenged [today] as never before, not just by creationists, but by his fellow scientists." The fact is that: ". . . for all its acceptance as the great unifying principle of biology, Darwinism, after a century and a quarter, is in a surprising amount of trouble." The reason is because Darwinism or its modern version, neoDarwinism, ". . . has not, contrary to general belief, and despite very great efforts, been proved." Given the above, why then is natural selection accepted? Macbeth (1971, p. 77) attempts an answer:

> [Does] the evidence mean that Darwinism is correct? No. Sir Julian Huxley said, once the hypothesis of special creation is ruled out, adaptation can only be ascribed to natural selection, but this is utterly unjustified. He should say only that Darwinism is better than the other. But when the others are no good, this is faint praise. Is there any glory in outrunning a cripple in a foot race? Being best-in-field means nothing if the field is made up of fumblers."

That changes have occurred in nature and in animals, no informed person doubts. Nor does anyone deny that species have arisen and disappeared—the dinosaurs and trilobites are the most prominent of thousands of good examples. Many creatures that once roamed the earth no longer exist today, and some species around today evidently did not exist a

long time ago. The concern is that *microevolution* is labeled *evolution,* then based on the evidence for microevolution the claim is made that *evolution* has occurred. Microevolution has been well documented and creationists have no difficulty with this fact; they stress that we should go only as far as the empirical data carries us (Johnson, 1991). The fact is, the documented changes are minor and fully explainable by innate variation laws. Most creatures that are around now are close to identical to their ancestors who lived far back in time—some even from almost the very beginning of the fossil record, such as many types of bacteria, insects, jelly fish, reptiles and fish.

Natural Selection and the Origin of Cells

A long standing major difficulty with the selection hypothesis relates to the hypothetical earliest levels of evolutionary development. For selection to occur, a living organism must exist that is capable of successfully reproducing, and also of ingesting, assimilating, and processing food. Secondly, a stable supply of food must be available which it can use to manufacture the various complex elements and also produce the chemical reactions necessary to obtain the energy needed to insure the organism's survival. Although many have tried, explanations of the origin of single cells by selection theory are still wanting. For selection to take place, even at the cellular level—a structure consisting of dozens of complex interrelated, functioning organelles must *first exist.* Many complex subcellular structures must somehow spontaneously form in conditions much different from today, and then resist the push toward entropy. Most all would disintegrate, but evolutionists must assume that some did not. These few must also have had a means to prevent destruction by too rapid atrophy, and also, among other things, must be able to ingest, to respirate, and also to effectively reproduce. Only when all of this has occurred could selection select the animal which survived best and produced more offspring.

Reproduction Rates and Bacterial Evolution

The organism which had a highly effective reproductive system and a *longer* reproductive life span to produce *more* of its offspring would be favored. A major result of the survival of the fittest force would be the *length* of the reproduction period, an effect that Darwin called *differential mortality* and today is often called *differential reproduction.* No selection advantage exists in living *after* one can no longer reproduce:

> We must keep always in mind that by the 'fittest' Darwin meant the one with the largest surviving progeny. This can be and often is a comparatively weak individual. In this sense rabbits are 'fitter' than lions, since they have been able to reproduce and occupy a larger area, in spite of man, than lions, which are fighting a losing battle against man. (Solbrig, 1966, p. 9)

Differential reproduction is so important that, in Simpson's (1967, p. 138) words:

> Suppose all the individuals in the population lived for precisely the same length of time, with no elimination of the unfit . . . hence no Darwinian selection. . . . Suppose further that [one species had] a hereditary fondness for apples [and] had twice as many offspring as those without this characteristic. Then there would be very strong, clearly non-Darwinian selection.

Given the fact that small mammals tend to have different survival rates, it would seem the one that consistently had the largest litter would eventually dominate the others. Put another way, given two identical animals except that animal A has an average of 10 litters of four animals each during its reproductive lifetime, and the other an average of six litters with three animals each, this trend would eventually result in animal A predominating and the demise of animal B. Evolution would therefore seem to consistently select for longer reproductive lengths—first years, then centuries, etc.—presumably without limit, although increases could well be smaller and smaller as time progressed, similar to the half-life phenomena. Obviously, this has not happened.

Natural selection would, in short, favor primarily those animals that 1) produce more offspring, 2) have longer fertility periods, 3) and live longer, thus having more time and opportunity to reproduce their kind. Those that on the average live longer but have *shorter* fertility periods are, according to evolution, at a disadvantage in the long run. These three factors all facilitate the events which fit the standard definition of "survival of the fittest." The data as a whole also reveal that natural selection is not functioning to any significant degree anywhere so as to change these features. According to the current evidence, the number of offspring, longevity, and length of the fertility period of most animals have been remarkably stable for the past several thousand years (Prince, 1980; Tributsch, 1984; Johnson, 1991). Since natural selection has evidently not changed even these three simple characteristics very much, all of which would seem to be highly influenced by it, the mechanism would not be expected to select in the direction of developing extremely complex mechanisms for animals, such as those found in the bombardier beetle or the archer fish. Conversely, it would select structures which *directly or indirectly facilitate* that which is defined as evolutionary success, namely the number of offspring living at any given time. As Miller and Van Loon note:

> it gradually became apparent . . . that the influence [of natural selection] was much more subtle, and that it was more a question of differential reproduction rather than differential survival and that what counted was not so much the life or death of certain individuals, but the extent to which any particular type could outbreed its competitors. (1982, p. 169)

Measured by this standard, because some insects give birth daily to thousands of offspring, they are for this reason *far more* successful evolutionarily speaking than mammals, most who give birth to only a few offspring annually.

Actually, a major problem with the survival of the fittest theory is that reproductive rates often are the opposite of what evolutionary theory predicts. Animals that have supposedly evolved to the *highest* rungs on

the evolutionary ladder in terms of the number of changes from the original hypothesized *unicellular ancestor* often have the *lowest* reproduction rates (Ortner, 1983). Most mammals give birth to one or two litters for only a few mating seasons. Many female mammals, if impregnated, have only one or two offspring per mating season. The creatures on the bottom of the so-called "evolutionary scale," such as bacterium and viruses, have by far the *highest* reproduction rates.

If reproduction fecundity is a main criterion of evolutionary "success," bacteria and viruses are without question among the most successful living organisms. Cholera bacteria reproduce at such a rate that a single pair can produce an estimated 700,000,000,000,-000,000,000 (700 quintillion) offspring in a mere 24 hours, fully 3,000 tons worth. Further, an offspring reproduction rate such as this would seem to provide an almost inexhaustible gene pool for mutations. Thus, if so many mutations occur per 1,000,000 organisms, the higher the number of organisms, all other factors being equal, the greater the total amount of mutations (and the more mutations, the greater the probability of a orable ones). The far higher reproduction rate of bacteria coupled with their short life span would result in more offspring and more total generations per year. Given this, they would produce far more mutations than the majority of animal types and, therefore, have the greatest chances for the occurrence of a favorable one, thus evolution.

It would also seem, given evolutionary assumptions, that a state of equilibrium would never occur among bacteria. A few bacteria out of the multitrillions living at any one time are bound to be blessed with a difference that produces a slight selection advantage which could in turn gradually alter the entire gene pool. The result would seem to be a greater likelihood of improving their adaption state, and thus should have caused it to evolve to a "higher" evolutionary level. The weaker bacteria forms would eventually become extinct, and only if new bacteria were somehow "spontaneously generated," or life at an even "lower" level was occasionally formed and able to evolve to the higher bacteria level, could this type continue to exist. If so, the bacteria existing today would have to be a recent result of this progress. Research on natural selection and mutations as a variation source has recently found that frequencies of genes that control certain traits of some microorganisms can be influenced. We have by this means, though, *not yet* produced a single major beneficial change in the physical structure of any organism, only weeded out undesirable one (Lester and Bohlin, 1984).

Although bacteria should have evolved at a much faster rate then the "higher" animals, no evidence exists that they have undergone evolutionary change in recent (or even ancient) history. The so-called *Archae bacteria* are not a pre- or primitive bacteria such as their name implies, but only ". . . a distinct and separate group of prokaryotes," and even this claim is a matter of definition and debate. The earliest bacteria thus far discovered, estimated to be two billion years old, ". . . closely resemble the microcolonies of certain modern soil bacteria" (Schopf, 1965, pp. 1365-1366). Borchgrave (1988, p. 62) noted that an Oberlin College team of biologists concluded that the evidences which they found:

> . . . indicate that the single-celled organism without nuclear membranes has changed little since it originated 2 billion years ago. The organism . . . has several of the same characteristics as today's myxobacterium, found in abundance in soil. The size of the slime's cells, its spores and the cysts that house the spores appear to be similar . . . [to] the myxobacterium of 2 billion years ago, like its present-day counterpart, was not photosynthetic but instead derived its energy from organic compounds of decomposed materials in the stromatolites.

The enormous reproduction level of bacteria noted above does not occur due to a rapid depletion of available food and moisture, and also an accumulation of toxic metabolic waste products in the animal's environment. Yet, the sheer number of bacteria produced should eventually result in mutations that will enable them to overcome even these problems. Evolution predicts that the organisms will eventually evolve so that their own waste products were *not* toxic. They might be expected to evolve selective membranes, toxic neutralizers or another means to protect themselves against the poisons. If bacteria have existed on earth for two-billion or more years—longer than most every other living thing—plenty of time should have been available for the necessary mutations to have occurred. As zoologist Grasse (1977, p. 87) notes, the question of *why* they did not evolve these innovations poses a major problem for evolution:

> Bacteria, . . . are the organisms which, because of their huge numbers, produce the most mutants. This is why they give rise to an infinite variety of species, called strains, which can be revealed by breeding or tests. Like *Erophila verna,* bacteria . . . exhibit a great fidelity to their species. The bacillus *Escherichia coli,* whose mutants have been studied very carefully, is the best example. The reader will agree that it is surprising, to say the least, to want to prove evolution and to discover its mechanisms and then to choose as a material for this study a being which practically stabilized a billion years ago! What is the use of their unceasing mutations, if they do not [produce evolutionary] change? In sum, the mutations of bacteria and viruses are merely hereditary fluctuations around a median position; a swing to the right, a swing to the left, but no final evolutionary effect. Cockroaches, which are one of the most venerable living insect groups, have remained more or less unchanged since the Permian, yet they have undergone as many mutations as *Drosophila,* a Tertiary insect.

Another of the thousands of examples that illustrate why mutations have been unable to bring about major changes is illustrated by yeast cells. The process needed to manufacture many alcoholic beverages involves using yeast to produce carbon dioxide, alcohol (both yeast cell waste products) and the energy necessary for its own growth, all from the fruit on which it parasitically lives. But when the alcohol content reaches about 14%, the yeast's own waste product

begins to kill the yeast. The many millions of years that evolutionists believe is available has *not* been able to overcome this simple problem. Similar examples of this inability of evolution by mutations to overcome limitations abound in the worlds of viruses, mycoplasms, rickettsia, fungi, nematodes and even class insecta.

Mutations and Evolution

Mutations are usually viewed as the major source of the variation that natural selection selects to cause evolution. It is universally recognized that the vast majority are clearly neutral or harmful, most always resulting in no change or a weakened or even deformed creature (Williams, 1977). Goldschmidt (1942) who postulated an early *punctuated equilibrium* theory, observed mutations in fruit flies for many years. The changes, he lamented, were almost all small so that if a thousand mutations were combined in a single fruit fly, a new species would not result but, at most, only a weird fruit fly which probably would not survive birth (Goldschmidt, 1952, p. 94). Extremely few examples exist for which any case can be made for a major favorable result from a mutation, and even these few examples are debatable. Even the assumption that weakened or deformed creatures are far more apt to be eliminated by natural selection is not valid; weaker creatures are often eliminated only if they are so *severely* deformed that they cannot live. Many spontaneous abortions and early infant deaths are due to this factor. Inferior creatures, especially among the higher mammals such as the primates are often protected by the group and, consequently, not uncommonly survive. Medicine has improved tremendously the infant mortality rate, and consequently many of the "weak" humans who would normally not survive are now living as long or beyond the normal life expectancy. A defect in humans, to be of selection value often must be so great that it causes the individual with it to be highly *unlikely* to survive the child bearing years, let alone compete in the natural selection game.

The discovery of the mechanism of heredity by Gregor Mendel in 1866, and the extant research on mutations, gives clear evidence mostly for *deevolution.* If the creature survives a mutation which is not harmful enough to impede early survival, it will likely be passed on to one's offspring. In this way, all species slowly accumulate mutations with each passing generation. Some evidence exists that the number of natural mutations has been increasing in humans, causing more diseases such as hemophilia (bleeding disease). Over 4,000 diseases are now known that are caused by past mutations, most of which were not in the human family several thousand years ago. Their victims have survived long enough to reproduce and pass on what was likely a mutation to their offspring. This is evidence, though, for deevolution, the opposite of evolution. As Mayer (1964, p. 296) admits, it

> . . . is a considerable strain on one's credulity to assume that finely balanced systems such as certain sense organs (the eye of vertebrates, or the bird's feathers) could be improved by random mutation. This is even more true for some of the ecological chain relationships (the famous yucca moth case, and so forth). However, the objections of random mutations have so far been unable to advance any alternative explanation that was supported by substantial evidence.

The inadequacy of arguing from reasoning that an idea is valid because it is a "better explanation" compared to competing ones, Macbeth (1971, p. 78) explains as follows:

> If such a theorist makes a suggestion that is better than other suggestions, or better than nothing, he feels that he has accomplished something even if his suggestion will obviously not hold water. He does not believe that he must meet any objective standards of logic, reason or probability. This is a curious state of affairs, but if the reader. . . can view it as a possibility he will feel less surprised in the frequent cases where he finds the theorists propounding ideas of striking frailty.

Attempts to Rank Animals by an Evolutionary Scale

The "higher" or "lower" (or more or less evolved) classification used by evolutionists is a distortion of reality and for this reason is avoided by informed biologists. Animals clearly appear to be designed for a certain type of life, and each one "fits" quite well into its own habitat. The severe difficulties in "ranking" animals in an evolutionary hierarchy, given the limitations of molecules and the built in flexibility found in all living structures, from cells to organs (plus the fact that all of them are perfectly designed and every organ perfect if not diseased) has resulted in a new taxonomy system called **cladistics** (Duncan and Stuessy, 1984). All organisms face the same needs, and all are normally capable of doing what is necessary to meet these needs. A luxury airplane is not necessarily more fit or better able to fly than a small Lear jet; both are well designed for their respective purposes.

Comparisons made between humans and animals show that many of the so-called "lower" animals are *more* "highly" developed in some areas than humans. A 170 pound man expending energy at the rate equivalent to that of a ruby throated hummingbird would burn about 300 thousand calories daily, requiring consuming 285 pounds of hamburger, about double his weight, daily. He would also need to evaporate about 100 pounds of sweat each hour just to keep his skin temperature slightly below the boiling point of water. Hummingbirds, famous for their speed, can fly about 60 miles an hour. They also can effectively "hover" like a helicopter, a feat which requires a wing movement of over 200 beats per second (a speed which cameras could not freeze until the advent of high speed film). For this reason, their wings appeared blurred in most older photos. The extremely fast wing movement also enables them to fly backwards, sideways or in any direction that they wish (Gause, 1969).

Although it is well-known that the "simplest" living things are actually extremely complex, the supposedly oldest living things are also as fully "developed" as their modern counterparts. These microfossils of cell chains that resemble a string of beads were discovered in rocks collected from a desolate corner of Western Australia. Paleobiologist Schopf (1965, p. 1365) noted

that these bacteria-like organisms that lived at the bottom of the shallow sea were ". . . surprisingly complex." In his words, ". . . these microfossils tell us that life was a whole lot more complex at that time [three and a half billion years ago, only about a billion years after the earth was supposedly formed] than any of us had really guessed." At least five fossil varieties of this type of bacteria were identified by the team of scientists which gathered at UCLA to argue about their origins of life views. They further concluded that this discovery also indicates that life existed a ". . . billion years older than expected." This leaves much less time available for life to have arisen after the earth began. Over 80 percent of the four and a half billion years of the earth's assumed existence (at least three and a half billion years) contained life, an assumption which creates serious problems for evolution.

The Case of Insects

Insects are also near the bottom of the evolutionary scale, but are likewise extremely successful according to evolutionary criteria. Almost a million species are now known, and hundreds of new ones are being discovered each year—this compares to less than 36,000 types of mammals, fish, and birds. Farb (1962, p. 11) estimates that the total number of insect species may reach

> upward of one million, and one authority believes the number may be as high as ten million. But even now the total already known is about three times the number of all other animal species on the Earth combined. There are reasons for the high level of success of insects.

Some of the many reasons for their obvious success, including their incredible ability to live in a wide variety of inhospitable environments include:

> There is scarcely a place on the planet Earth that is not home to least one kind of insect. Some 40 kinds of insects live in the bleak Antarctica. . . . Wherever they live, insects . . . endure with a unique kind of indestructibility. Some of them have been frozen solid at temperatures more than 30 degrees below zero F and still lived; other kinds inhabit hot springs where temperatures reach 120 degrees F. Still others survive in as great a vacuum as man has the power to create. . . . many insects can endure long periods without water; they possess fuel reserves and can get the water they need by burning these reserves. This is so-called metabolic water; it is produced by the burning of carbohydrates in the body, where they are broken down into water and carbon dioxide. (Farb, 1962, p. 11)

The flea is an excellent example which illustrates how many insects, in contrast to most vertebrates, can tolerate drastic environmental changes. Fleas can typically survive for as long as 17 months without blood, their main diet, until they find a host. One flea type which preys on humans can exist as long as 500 days without nourishment. Of the 1,200 species of fleas that exist (only about 200 make their home in North America) some types can exist in a frozen state for months and, after thawing out, are usually as healthy as before. After being buried under thick layers of snow in the frigid Antarctic for as long as nine months or more, as soon as they are freed they look for a "host" on which to survive parasitically.

After the infant fleas hatch, they spin cocoons in which to grow and mature. Then, after becoming fully developed, they lie dormant within their cocoons patiently waiting for a person or animal to pass by. When one is discerned, usually by smell, the flea bursts rapidly from its cocoon and "jumps" onto the host if it is within 36 inches (proportionate to a human jumping about 550 feet). The flea, known for its great strength, can pull 400 times its own weight. Farb (1962, p. 22) notes that fleas are remarkable, but no living creature matches the beetle, the most successful order of animals on earth:

> . . . the total number of beetle species may be as high as 250,000. . . . By comparison, all the species of vertebrate animal—fish, amphibians, reptiles, birds and mammals—total fewer than 36,000. At least three characteristics contribute to this unparalleled success. . . . Complete metamorphose . . . an effective shield, protecting the soft body underneath and the beetles have kept their primitive mouth parts, designed for chewing abundant soft foods.

These examples vividly illustrate the difficulty in developing an evolutionary hierarchy as required by the theory. Rather than a classical tree relationship as among animals, we find a variety that defies any overall ranking system.

Intelligence and Survival

The brain becomes more and more complicated as we go up the animal kingdom scale. Vertebrates, animals with spines, have more nerves than spineless creatures, and their brain is larger and more complex. This enables vertebrates to effectively carry more messages from its more complex body to its brain, which in turn must be more complex to deal with this quantity and quality of information. The brain and most of the body structures in "higher" animals are far more complex, requiring many more neurons and their supportive neuroglia cells. This complexity does not necessarily increase the animals survival advantage, but it often actually makes survival more precarious because more structures exist to break down. Animals with more complex brains are also often less able than lower forms to withstand some of the major environmental pressures that supposedly originally caused their evolution, especially temperature and food supply pressures (Colinvaux, 1978).

Contrary to what is often assumed, intelligence does not necessarily facilitate survival, at least in the animals below humankind (Colinvaux, 1978). The term intelligence is used here in the classical sense, and does not refer to inherited instincts, without which almost no animal could survive. Many animals which have almost no intelligence survive quite well, including bacteria, insects, coelenterata, platyhelminthes (flatworms), aschelminthes (roundworms), mollusca, and crustaceans. Conversely, any animals which possess a comparatively much higher level of intelligence, such as whales, dolphins and many primates, are now threatened with extinction. With the exception

of humans, a reverse *correlation* often exists between the ability to survive and intelligence.

One of the most successful animals from an evolutionary standpoint, the turkey, is considered one of the more inept members of the animal kingdom. When frightened by thunder or other loud noises, they sometimes pile up on top of other turkeys along their coop fence and smother. They will even drown in their own water trough or stare up at the sky during the rain storm with their mouths agape until they suffocate! Although mankind is partly responsible for this condition because turkey breeders are concerned primarily with size, and certainly not with intelligence, those in the wild also exhibit most of these traits. Turkeys may have survived until today partly because the stiff horny spurs on the back of their legs are a fairly effective defense against predators, and their mating process is very efficient. In addition, they display unique behaviors such as creating small "dust storms" which kill lice, mites, and other parasites that are prone to live in their feathers. The dust blocks the breathing organs of the parasites, killing them. Regardless of the reason, in spite of being dangerously stupid, turkeys actually live longer and are hardier than many other birds (Masckenzie, 1977).

Claims that temperature and similar environmental factors caused the evolution of human intelligence, which in turn has aided in our survival, are common in the literature, but are often nothing more than speculation. Pendell (1977, p. 76) for example, proposed that: "The population of *Homo erectus* in Europe must have been thinned to almost zero by the Mindel Glaciation. Only the few who boasted an almost *Homo sapiens* intelligence could have survived." This conclusion is largely guesswork which lacks empirical evidence and is also poorly conceived intellectually. Those specimens that survived this ice age did not need intelligence nearly as much as a good supply of food, firewood and animal skins, plus access to warm caves and the insight to huddle together under thick blankets by the fire. If a few of them possessed reasonable intelligence, they could likely have directed the process of locating food and the other necessary things for the whole tribe, or better yet had known to move south as the climate changed. Humankind has most always lived in groups and, with some notorious exceptions, has always taken care of the weaker among them. The children must be cared for by adults and, except in extreme cases, rarely perish because of their lack of intelligence. If lack of intelligence impedes survival, it is often the group's or the tribe's leaders' lack that causes most of the problems. The high level of group and social support systems that are typical of primates makes it unlikely that much winnowing out of the less intelligent generally occurs.

We would also expect that the lower forms of life would display a low level of tolerance for variations in such factors as temperature or lack of regular food and water, and would need only certain kinds of food to survive. Darwinian natural selection would also cause us to expect that those organisms at the *higher* end would possess better, often more complex organs which would help them to survive by blessing them with a greater ability to:

1. live for longer periods of time without food.
2. live on a wider variety and types of food (animals that metabolize most anything are ideal).
3. live on food types that are abundant (as cellulose).
4. live in large temperature variances (such as from 0 to 300 degrees Celsius, or close to these extremes.)
5. resist or develop tolerance to many poisons, ions, acids and bases from a pH of 2 to 12 or wider, etc.
6. effectively escape or defend themselves against predators of all types and sizes.

According to these criteria, the so-called simpler forms of life tend to be *more evolved.* As the law of parsimony (Occam's razor) predicts, if two structures equally achieve the same results, the simpler structure (or simpler explanation) is preferable. A simpler structure has fewer parts to wear out or malfunction, and thus cause a breakdown. A clear technology advance is the development of a machine which does the same job with fewer parts, especially fewer moving parts, or with a less complicated design.

An example is the so-called simpler eyes of insects or ears of certain animals which are more effective than the same structure in humans. This fact questions *the purpose* from an evolutionary standpoint of *more complex structures.* If a motorcycle will transport one to the next town as effectively and quickly as a Cadillac, natural selection will not evolve a Cadillac, even though this mode of transport may be much more comfortable and luxurious. The functions of life, growth, survival, and reproduction *are all carried out as effectively* if not more *so in bacteria, insects and worms as in humans.* The major difference is that humans travel through life with more luxuries. This concept, called "over-design," supports for the creation world view (Bergman and Howe, 1990).

Greater and greater ability to survive in spite of food deprivation would seem to be a major thrust of Darwinian natural selection. Presumably, the only limits are the ability to survive total deprivation, and to stop and start one's total biological system at will. That it is possible is proven in that some animals can evidently survive for centuries in a state of extreme hibernation without food or water. DeGarmo (1982, p. 19) reported that bacteria brought by ship from earth, *Streptococcus mitis,* apparently survived on the moon surface between April 1967 and November 1969. The organisms were discovered in a piece of insulating foam in a television camera retrieved from Surveyor III by the *Apollo* astronauts. The ability to withstand greater and greater temperature and other environmental changes, which would be determined by the general ranges which exist in an area, also would evolve. Thus, no need exists to evolve the tremendously complex organisms with the endless variety of sense organs, communication and systems of locomotion that are found everywhere in the real world, both today and far back in the past. Selection would seem to *eventually* cause the evolution of the most possibly fit animal (likely a single-celled organism) which would eventually *literally cover the earth,* impeded only by space and the availability of food-both which would affect only its ability to reproduce. Even here, though, selection would *increase* its food

flexibility requirements to the extent that the cell could exist on only oxygen, carbon, nitrogen, and trace amounts of a few other elements. The fact that this logical outcome of evolution is not found argues against megaevolution by natural selection.

The Limits of Variation

Empirical research has verified that animals and plants can be bred only to a certain point. Important economic reasons exist behind attempts to breed prettier flowers and meatier cows, but nowhere are small improvements as critical as the breeding of faster race horses. At stake is many millions of dollars which can be gained even if the breeding produces only a very small advantage. Hill (1988) in a study of horse breeding concluded that, in spite of enormous efforts by the leading geneticists, race horses today do not run much faster than their great-great-grandsires did, and many of the improvements "cannot be attributed to genetic change, but to better training, health, tracks, and wider screening of the population." He notes that "despite the efforts of breeders," the winning times of thoroughbreds in the English classic horse races "have not fallen substantially over the past fifty years" (1988, p. 678). This is not due to lack of effort, but "the lack of improvement is disturbing because the horse-breeding industry is a large and competitive business, with much attention being paid to performance and to pedigree . . . we need to explain the apparent selection limit . . ." (1988, p. 678). While it would be premature to conclude, especially in view of genetic engineering progress, that the industry has exhausted all possibilities of breeding a faster horse, it is clear that there are definite limits which are fairly narrow. Breeding a slightly faster horse does not argue against this, only that the limits may be slightly wider than we currently assume (Gaffney and Cunningham, 1988, p. 722). The Gaffney and Cunningham study found that the best horses were not getting faster, but the pack's arithmetic mean was higher. Although the gene pool was improving, the top horses had reached their physical limit.

Drosophila melanogaster research has found that excessive breeding of some traits often produced sterility, thus we could expect that intense natural selection, as repeatedly confirmed in the laboratory, would result in sterility or other problems, not a new and better species however it is defined.

Selection as Counter-Evolution

Numerous other problems exist with the claim that the animals which supposedly had a longer evolutionary history and are more complex are the *higher* forms, such as the mammals, birds, dinosaurs, etc. and that the lower forms—insects to bacteria—are "primitive" and have historically experienced little change. Intensive selection would logically eventually cause the organism's extinction for the reason that it results in a higher and higher level of adaption, thus a narrower and narrower level of specialization, making it increasingly difficult to survive environmental changes. Flexibility and a low level of adaption to a specific niche appears to be far more important for survival than a high level of fitness. Natural selection would then "select" animals into a slowly narrowing ecological niche in which extinction would be inevitable. The data cited above support the conclusion that animals which are "higher" on the evolutionary scale are more likely to become extinct—inferring that Darwinian "selection" tends to evolve animals into a position in which they are more likely to be selected out of existence. In other words, Darwinian selection, as presently understood, almost invariably leads to extinction.

An example of Darwinian natural selection theory carried to its limits and selecting an animal out of existence, is the saber-tooth tiger. Its demise is attributed to its large teeth which evolutionists also claim were originally produced by selection. Their teeth evolved so large that the cats evidently could not open their jaws wide enough to allow entry of their normal food. This contradiction exists, it is argued, because the environmental changes may produce a structure which is advantageous in one situation but a handicap in another, and new traits are actively selected for or against if the environment changes. No known changes in the environment of the sabre-toothed tiger have occurred to cause this, and both those factors for and those against the trait would be operating at the same time. Selection must explain both the existence of these gigantic teeth and the ultimate demise of the animal (and it is not even clear if they caused the animal's extinction). It cannot explain these factors for the reason that it can cause only a fine-tuning of tooth size, not an extreme and, in this case, non-functional development as Darwinian natural selection teaches.

It is likewise hypothesized that the Irish Elks became extinct about 10,000 years ago, largely because of their enormous antlers—a trait that is claimed was originally formed due to selection. The Irish Elk (not an elk, but the largest deer known today) lived in Ireland and also as far east as Siberia and China, and as far south as Northern Africa. Its antlers were probably the largest of any animal, ever—up to 12 feet long, sometimes longer than the elk's own body length and weighed about 90 pounds (Gould, 1977, p. 79). It is assumed that the antlers developed from selection, and nature continued to select until the animal with them grossly lacked functional body proportions.

Darwin's Concerns

Even Darwin recognized that the natural selection theory had serious problems. For example, Gould (1980, p. 32) noted, "Darwin lived to see his name appropriated for an extreme view that he never held—for Darwinism has often been defined, both in his day and in our own, as the belief that virtually all evolutionary change is the product of natural selection." According to Gould, Darwin openly objected to this "misunderstanding" of his position. In the introduction of the 1872 edition of his *Origins of the Species*, Darwin stated:

> As my conclusions have lately been much misrepresented, and it has been stated that I attribute the modification of species exclusively to natural selection, . . . in the first edition of this work, and subsequently, I placed in a most conspicuous

> at the close of the introduction—the following words: 'I am convinced that natural selection has been the main but not the exclusive means of modification.' This has been of no avail. (Quoted in Gould, 1980, p. 32)

A major reason that Darwin took this position, Gould (1980, p. 32) concludes, was because ". . . organisms display an array of features that are not adaptations and do not promote survival directly." Darwin attempted to explain away, or in some way account for these mechanisms, but largely failed and he knew this. In respect to *Homo sapiens*, Grasse (1977, p. 85-86) pointed out that, although the *source* of selection, namely mutations, differentiate individuals, yet

> . . . the human species, despite the magnitude of its population and the diversity of its habitats, both of which are conditions favorable for the evolution of the human species, exhibits anatomical and physiological stability. In wealthy western societies natural selection is thwarted by medical care, good hygiene, and abundant food, but it was not always so. Today in underdeveloped countries, where birth and death rates are equally high (tropical Africa, Amazon, Pakistan, India, Patagonia, some Polynesian islands), natural selection can exert its pressure freely; yet the human type hardly changes. In the population of the Yucatan, which since the Spanish conquest has been subjected to terrible vicissitudes, one can find Mayan men and women who are the exact replicas of their pre-Colombian ancestors from Palanque of Chicken Itza. For several millennia the Chinese have numbered hundreds of millions. The conditions of their physical and social environment have favored intensive selection. To what result? None. They simply remain Chinese. Within each population, men differ by their genotype, and yet the species *Homo sapiens* has not modified its plan or structure or functions. To the common base are added a variety of diversifying and personifying ornaments, totally lacking evolutionary value.

Some Conclusions

For many, a key impediment to the acceptance of evolution, according to Gould, is that Darwin argued that evolution has *no purpose*, but is merely a process which both happens to result in increased numbers of animal types in the future and improves their survival chances, and nothing more. Numbers were assumed to be the only measure of success. The more successful species would have more of its offspring around; more would be reproduced, and more would survive. From this vantage point, bacteria are far more successful than elephants, thus more evolved. In the selectionist's view, any harmony and order in the world arises solely from an incidental and accidental result of individuals universally selfishly seeking their own advantage—see Wilson (1975). In contrast to this view, it is obvious that purpose is everywhere, and one who asks why in the natural world can usually find empirically supported, logical answers. As Darwin stressed, evolution has no direction, nor does it inevitably lead to higher or more complex life, although most evolutionists have written and argued as if it causes only movement upward, from amoeba to humans. Selection selects only for adaptation to local environments, and in their view this adaptation is achieved only by cold cruel selection—some die, others live. Its "goal" is survival only, and those who are more likely to survive are better adapted, and thus are more likely to pass on their traits to their better offspring (Gould, 1989).

Natural selection would not evolve upward, for example, bacteria into humans, but at best would evolve simple bacteria into better adapted bacteria, or flies into better adapted flies. The fossil record shows no evidence of anything beyond this. No clear example has ever been found of a lower clearly less adapted animal in the fossil record which can be shown to be evolutionarily related to similar, more advanced type of an animal living today. There exist hypothetical cases and examples of differences for which reasons for assumed changes are speculated, but no example exists of an animal that lacks wings, and evolves such step by step because these wings are clearly an advantage for it in escaping predators. Not one wingless fly has ever been uncovered, although millions of modern type flies preserved in amber have been uncovered. The many examples we have, such as flies trapped in amber or animals preserved in other ways, finds that, aside from the introduction of a few mutations producing deevolution, there is virtually no difference between the fossils and modern examples.

The easy-to-grasp and compelling natural selection argument is used to help explain all biological data, but it may actually explain very little. Human life consists of many activities which are mentally pleasurable. Walking in forests, listening to music, creating poems, doing scientific research, aesthetic enjoyment of nature, and myriads of other activities are often not related in the least to survival or adaptation in the Darwinian sense. Some writers have struggled in vain to "explain" by natural selection the existence of creations like music and art, all of which involve extremely complex body structures to accomplish. Music in its many variations is loved the world over, and yet certain music preferences have not been shown to increase reproduction rates or to facilitate survival. Many, if not almost all of our most rewarding activities and "peak experience producers" are not only unexplainable by this theory, but contradict it.

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BOOK REVIEWS

A Living Dinosaur? by Roy P. Mackal. 1987. E. J. Brill. Leiden, Netherlands. 340 pages. $30. The book is available from the author at 9027 S. Oakley Ave., Chicago, IL 60620-6131.

Reviewed by Don DeYoung*

Dinosaur sightings have been reported from Africa for centuries. Although most scientists scoff at the idea, author Roy Mackal takes seriously the topic of cryptozoology, the study of "hidden" creatures. He has had a distinguished academic career at the University of Chicago in both biochemistry and engineering, and is now retired.

Mackal directed official investigations of Loch Ness phenomena in the 1960s and 1970s. Next, his attention turned to the Likouala region of the Republic of the Congo. This frontier contains more than 55,000 square miles of remote jungle, swamps, and lakes—an area the size of Georgia. Sparsely inhabited by pygmies, the region is largely unknown to outsiders. See Linden (1992) for a geographic description.

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This book especially interests me because a creationist missionary friend, Eugene Thomas, has ministered in the Congo for nearly 40 years. He has told me personally of near encounters with large, unknown creatures during his African ministry. Gene accompanied Ray Mackal as guide on expeditions in 1980 and 1983 (Anon., 1981). These searches were partially funded by the National Geographic Society. However, the society later refused to publish the story because it wanted "more than fresh footprints" (private communication from author Mackal). The expertise of Gene Thomas is referred to frequently in the book. This is a special compliment since Roy Mackal is not a creationist.

The Africans describe a large aquatic reptile which they call *Mokele-mbembe* (mo-KAY-lee em-BEM-bee); their drawings resemble a small aquatic brontosaurus. Numerous eyewitness reports extend from the 1940s through at least 1990. The beasts are rare, retiring, and regarded as somewhat mythical by the natives. The main location centers on Lake Tele, 400 miles north of Brazzaville, in the heart of the equa-

torial forest of central Africa. Mackal's expeditions did not result in the capture or undoubted-sighting of the *Mokele-mbembe.* However, his thorough compilation of evidence and consistent native testimonies is impressive. The book is mainly a travel narrative; maps and photographs are included.

Dinosaurs are usually thought to have become extinct 70 million years ago. Thus evolutionists will not appreciate Mackal's objective conclusion that "prehistoric" creatures still inhabit African jungles. Dr. Mackal is now planning a third expedition, perhaps using a dirigible to hover above the jungle. It is clear that the question of contemporary dinosaurs is far from settled.

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Galileo Connection: Resolving Conflicts between Science and the Bible by Charles E. Hummel. 1986. Intervarsity Press. Downers Grove, IL. 293 pages, $8.95.

Reviewed by Clifford L. Lillo*

Although the title suggests that the author sets a clear distinction between true "science" and biblical text, Hummel actually shows that some well-known early scientists believed the Bible and science to be compatible. The author investigates the lives of four prominent scientists: Copernicus, Kepler, Newton, and Galileo Galilei, all of whom accepted Christian theology while continuing careers of science.

In a Prologue, Hummel quotes from a biography of Galileo which gave the viewpoint that Galileo's main conflict was with the religious authority of the church. To which Hummel asks,

> But was the conflict so clear-cut? Whose system did Galileo set out to disrupt, the religious authority of Rome or the scientific authority of Aristotle? (p. 13)

The answer is clear in a chapter titled: "Galileo: Physics and Astronomy." After giving a brief biography of the scientist, Hummel states,

> Unfortunately, Galileo had a knack for antagonizing people. His outspoken criticism of the academic establishment made him many enemies. . . . By his last year at Pisa, his faculty colleagues had suffered so much at Galileo's hands that in revenge they attended his lectures and hissed at comments with which they disagreed (p. 86).

In 1604, Galileo gave public lectures on a recent supernova and stated that Aristotle must be wrong in believing that no change could take place in the heavens. This caused more controversy with university philosophers. "Disgruntled professors at Pisa now allied themselves . . . in a secret . . . resistance movement known as the *Liga"* (p. 92). However, Galileo continued to experiment, make observations with his telescope, and to publish his results. With his book, *Discourse on Floating Bodies,* Galileo again "tweaked the scientific establishment" (p. 93). The leading figure of the *Liga* was the Florentine philosopher Ludovico delle Colombe who sought revenge.

> Disappointed by their failure to break through Galileo's lines on the fronts of physics and astronomy, the *Liga* adopted a new strategy. Carrying the attack into court circles, they would make his scientific discoveries a religious issue (p. 94).

Galileo's former pupil, Benedetto Castelli, had been appointed to the chair of mathematics at Pisa and, in 1613, Galileo wrote a letter to Castelli in which,

> . . . he accepted the challenge of waging the battle on theological grounds and resolving conflicts between scientific and biblical accounts of natural events (p. 105).

According to Hummel, Galileo was summoned before Cardinal Bellarmine in March 1616 and admonished for advocating Copernicanism. After Galileo's good friend, Maffeo Barberini, became Pope (Urban VIII) in August 1623, and Giovanni Ciampoli, a staunch supporter of Galileo, received an appointment as Secretary of the Briefs at the Vatican, Galileo hoped that the decree of 1616 might be rescinded but it was not.

Hummel says that Galileo's trial of 1633:

> . . . was not the simple conflict between science and religion so commonly pictured. It was a complex power struggle of personal and professional pride, envy and ambition, affected by pressures of bureaucratic politics (p. 116).

In other chapters, Hummel offers statements about Copernicus, Kepler, and Newton which show they maintained an ability to perform scientific experiments while remaining faithful to their Savior:

> The great astronomer [Copernicus] saw no conflict between his Christian faith and scientific activity. During his forty years as a canon, Copernicus faithfully served his church . . . studied the world "which has been built for us by the Best and Most Orderly Workman of all" (p. 55).
>
> When someone asked him [Kepler] in a lucid moment where he thought his salvation lay, he answered confidently, "Only and alone on the services of Jesus Christ." In Christ the astronomer found his refuge and solace (p. 79).
>
> For Newton the world of science was by no means the whole of life. He was deeply committed to his faith in the Creator of the world who also revealed himself in history and Scripture (p. 142).

According to the book jacket:

> Charles E. Hummel holds advanced degrees in chemical engineering (M.I.T.) and biblical literature (Wheaton), and has served as president of Barrington College. He is currently director of faculty ministries for Inter-Varsity Christian Fellowship and is the author of *Fire in the Fireplace: Contemporary Charismatic Renewal.*

In this book, Hummel has shown that some of the best-known scientists in history accepted both biblical text and scientific experiments and observations. Present day scientists should do no less.

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Living Fossil—The Story of the Coelacanth by Keith S. Thomson. 1991. W. W. Norton. New York. 252 pages. $19.95.

Reviewed by Don B. DeYoung*

Biologist Thomson holds a Ph.D. from Harvard and presently directs the Academy of Natural Sciences in Philadelphia. He has carefully traced the modern history of the coelacanth, and has also had opportunity to dissect several of them.

The story begins with Marjorie Courtenay-Latimer, curator of a small museum in Cape Province, South Africa. On December 22, 1938, a fishing trawler delivered to her "the most beautiful fish I had ever seen" (p. 23). The unusual paired fins and armored scales convinced her of an unusual find. She turned for advice to ichthyologist James Smith, and the excitement began. He realized that the fish was previously known by experts only in fossil form. The last known relatives had appeared in Cretaceous time, 80 million years earlier. The book has many good illustrations, including Latimer's original coelacanth sketch. Also shown is a reward poster that Smith circulated among fisherman in search of more species.

Louis Agassiz first named the coelacanth from fossils in 1836. The spines of the first dorsal fin are hollow, hence *coel*, Greek for space, and also *acanthus* for spine. The fish, fossil or living, averages 6 feet long and weighs 150 pounds. It is called a living fossil, a term coined by Charles Darwin, although Thomson readily admits this a meaningless oxymoron (p. 71).

Thomson clearly states that the coelacanth is a fish in every sense, "not a fish trying to be an amphibian" (p. 130). That is, living coelacanths do not hobble around the sea bottom on their fins, pretending they have legs!

In the past 50 years 300 living coelacanths have been captured, all in the vicinity of the Comoro Islands north of Madagascar. The living fish were previously known only to the islanders, who used the tough scales to roughen the inner tubes of bicycle tires when they glued on a patch (p. 60)!

The fish has proven more noble than scientists, who have continually fought over specimens and research discoveries. Experts may have also driven the coelacanth to the brink of extinction. In reading about the continuous search for additional coelacanths, one wonders why 300 are not enough of this rare creature. The Natural History Museum in Paris alone has 20 specimens (p. 226). The newly-organized Coelacanth Conservation Council may finally help to protect them (p. 225).

Thomson discusses some embarrassing facts for evolution. First, there is a time problem. The Comoros are recent volcanic islands, existing for "only five million years." How could coelacanths have continuously inhabited this region of turbulent, evolving crust for the 65 million years since the end of the Cretaceous (p. 102)?

Second, why is this "primitive" fish a complex livebearer rather than a "simpler" egg layer? When non-egg embryos were found inside a female fossil coelacanth in 1926, experts concluded that the embryos must have been *eaten* by the mother (p. 91)! After all, they reasoned, primitive fish must lay eggs; we now know better. Third, many other features also show advanced design. The coelacanth jaw and intracranial joint allow complex head movement (p. 168). The terminal tail lobe is a special trimming fin which adjusts for torque forces produced by other fins (p. 164). The rostral organ appears to be a sensitive electroreceptor (p. 175).

The coelacanth has been completely misunderstood by science. It is actually a magnificent testimony to a recent creation. The coelacanth has not evolved or changed at all since it first appeared on the fifth day of creation. Although one must occasionally read between the lines, this book is very helpful in learning the true story of the coelacanth.

*Don DeYoung, Ph.D., is editor of the *CRSQ*.

LETTERS TO THE EDITOR

Scientific Premises?

I confess that it is difficult for me to accept the claims of certain scientifically orientated organizations that they are also Christian in character. Although they appear to require a member to adhere to a specific "statement of beliefs," the scientific methods they often utilize explicitly contradict the Scriptures. Many of these organizations are flourishing within our society. These organizations claim to investigate the relationships between "science" and religious doctrine with their eventual goal a convergence of the two disciplines. Almost all such associations that have attempted to enlist my aid in their cause, contradict the hypothesis selection methodology put forth by Paul throughout 1 Corinthians 2. Indeed, I was a member of one such influential association but dropped my membership when I encountered such contradictions within the editorial policies for publication in their primary journal. The MA-model (Herrmann, 1990) has a great deal to say, indirectly, about such Scriptural contradictions and the behavior of developing natural systems.

The MA-(mathematical) model explicitly established that there are numerously many (indeed, too many to count) different scientifically describable scenarios for how our universe may have come into being, how life began on Earth and how the solar system was formed. This model shows that we can gain no knowledge from observing the radiation (electromagnetic or particle) falling on earth as to which of these scenarios is correct since all such radiation satisfies each scenario. Thus we have many distinct scientific theories that are modeled by such radiation. The MA-model is not obtained through some vague form of philosophical argument. Scientists who reject the model's conclusions must also reject the basic tool used by their discipline.

Certain premises have been selected by the majority of the scientific community—premises that correspond to just one of these theories. The selection of such hypotheses cannot be based upon the canons of the scientific method—they must be based upon "other considerations." Paul's pronouncements in 1 Corinthians 2:13-14 imply that hypothesis selection relative to the theologically related origins question should not be dictated by "worldly wisdom," but such selection should be "spiritually discerned." a *true* spiritually discerned selection should not contradict the Scriptures. As an example, the hypotheses selected for the generation of the Big Bang cosmology with the requirement of the Hubble expansion and the universe age of 15 billion years, do yield many Scriptural contradictions. But this theory can be consistently embedded into a very weak theology such as Aristotle's "Prime Mover."

Many of the scientific-Christian groups refuse to consider discarding such a contradictory selection—a selection that has no scientific truth value—and are now engaged in "re-interpreting" the Scriptures to correspond to such a speculation. It is my personal opinion that such a re-interpretation is destined to produce a "new" Christianity—new cults—that will have no relation to the Spirit of God.

I point out that the Big Bang cosmology is one of the possible MA-model scenarios. But there are many other MA-model possibilities that do not contradict the Scriptures, even when interpreted "fundamentally." The significance of the MA-model with respect to this question lies in the fact that it demonstrates mathematically the existence of many such models for the origins of our universe, the origin of life and the like. One of the major implications of this discovery lies in the area of the philosophy of science.

The MA-model has demonstrated that the theoretical sciences are ultimately based upon considerations that are not related to the scientific method but, rather, these sciences are based upon pure philosophical predilections. Since public money is spent on fostering theoretical science, these predilections and the reasons for hypothesis selection should be exposed to public opinion. The public should be properly informed as to the true basis of these scientific endeavors and, especially, the fact that the scientific method itself cannot determine which origins theory taken from a storehouse of such theories is correct.

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Does the Earth Move?

The topic of geocentricity continues to be of interest to many creationists. Two new items of information are now available. First, a 90 minute debate between Martin Selbrede (geocentric view) and Don DeYoung (heliocentric view) is available on cassette tape. Second, Martin Selbrede also has a video presentation of his ideas. Both items are available from:

Geo/Helio Productions
1541 Old Ranch Road
Camarillo, CA 93012

Don DeYoung, Editor

Typographical Errors

In publishing and writing, the most insidious, feared and often overlooked mistakes are typographical errors in manuscripts and printed pages. They tend to humble authors, editors and proofreaders, particularly when they appear in the final paper.

In this vein, Dr. Richard D. Lumsden has provided the ultimate in typographical errors. I quote from his letter of May 4 to me.

> . . . one of my colleagues, publishing her first paper, failed to notice in reading the galley that her name had been misspelled in the title of the article. When the article appeared in the journal with the by then indelible lapsus, for subsequent bibliographical consistency, she felt compelled to have her name changed legally to match!

Beware of typographical errors.

Emmett L. Williams
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QUOTE

The flowering of modern science in the seventeenth century is usually associated with the rise of inductive as against deductive reasoning. But Alfred North Whitehead, in his *Science and the Modern World,* surely one of the more profound histories of thought, points out that the mere observation of facts will never create true science. There must be method and hypothesis into which the facts will flow. There must likewise be faith that there is an order in nature and in the universe that can be discovered, for "without this belief the incredible labors of scientists would be without hope." In the medieval view God at least was rational, and this belief in its turn affected all that was to come. In explaining the modern world of high technology Whitehead states; "My explanation is that the faith in the possibility of science, generated antecedently to the development of modern scientific theory, is *an unconscious derivative from medieval theology."*

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QUOTE

I am suggesting that the natural sciences may open windows through which you and I may look upon images of beauty and truth. What we call "science" necessarily speaks in symbols and images, abstractions and intellectual constructions. By way of fuller illustration, however, let me quote to you a great-souled woman who died young, Simone Weil. First, a passage from her most influential little book, *The Need for Roots:*

> The true definition of science is this: the study of the beauty of the world. (The motive of the scientist, if it is pure, must be the love of beauty.) The savant's true aim is the union of his own mind with the mysterious wisdom eternally inscribed in the universe. Scientific investigation is simply a form of religious contemplation.

Simone Weil compares the scientists of Hellas with the scientists of her own time—to the disadvantage of the latter—and then suggests (in her posthumous book On *Science, Necessity, and the Love* of *God,* published in 1968) that because modern science falters and seems to lose its way, modern folk think that no truth whatsoever remains:

> In the present crisis there is something compromised which is infinitely more precious than even science: it is the idea of truth, which had been very closely associated with science in the eighteenth century, and especially in the nineteenth. The association was very erroneous, but the habit has persisted with us. The disappearance of scientific truth appears to our eyes as the disappearance of truth, thanks to our habit of mistaking the one for the other. So soon as truth disappears, utility at once takes its place, because man always directs his effort to some good or other. Thus utility becomes something which the intelligence is no longer entitled to define or judge, but only to serve. From being the arbiter, intelligence becomes the servant, and it gets its orders from the desires. And, further, public opinion then replaces conscience as sovereign mistress of thoughts, because man always submits his thoughts to some higher control, which is superior either in value or else in power. That is where we are today. Everything is oriented towards utility, which nobody thinks of defining; public opinion reigns supreme, in the village of scientists as in the great nations. It is as though we had returned to the age of Protagoras and the Sophists, the age when the art of persuasion whose modern equivalent is advertising slogans, publicity, propaganda meetings, the press, the cinema, and radio took the place of thought and controlled the fate of cities and accomplished coupe d'etat. So the ninth book of Plato's *Republic* reads like a description of contemporary events. Only today it is not the fate of Greece but of the entire world that is at stake. And we have no Socrates or Plato or Eudoxus, no Pythagorean tradition, and no teaching of the Mysteries. We have the Christian tradition, but it can do nothing for us unless it comes alive in us again.

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