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 Sin-itiro 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.

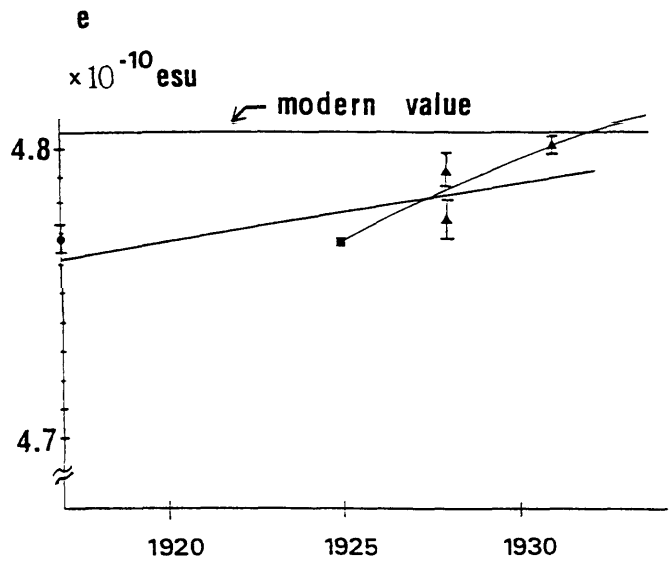

Figure 1. Charge of the electron measurements plotted against the date of measurement. The points marked with triangles represent values measured by the X-ray diffraction method, the square represents the value measured with the shot-effect method, and the circle represents Millikan's 1917 value measured 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.

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

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

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

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:

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

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

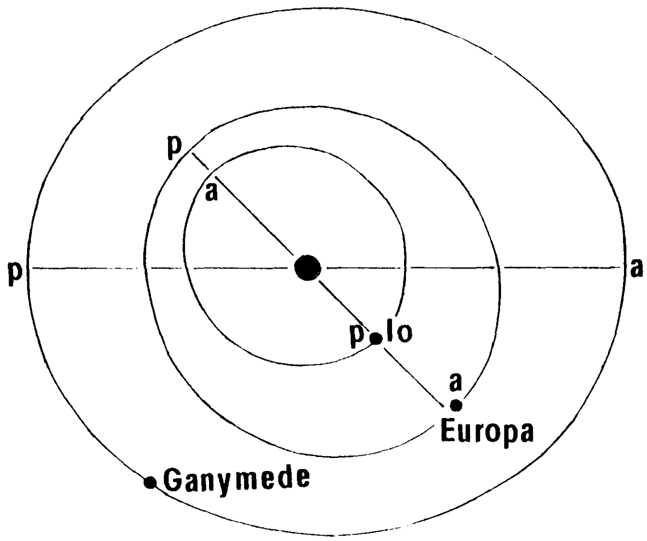

Figure 2. A representation of the elliptical orbits of Jupiter's moons Io, Europa and Ganymede. The eccentricity is greatly exaggerated. Due to the resonance lock between the three moons, Io is at its perijove (p) when Europa is near apojove (a), as shown. Conjunctions of Ganymede and Europa occur when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

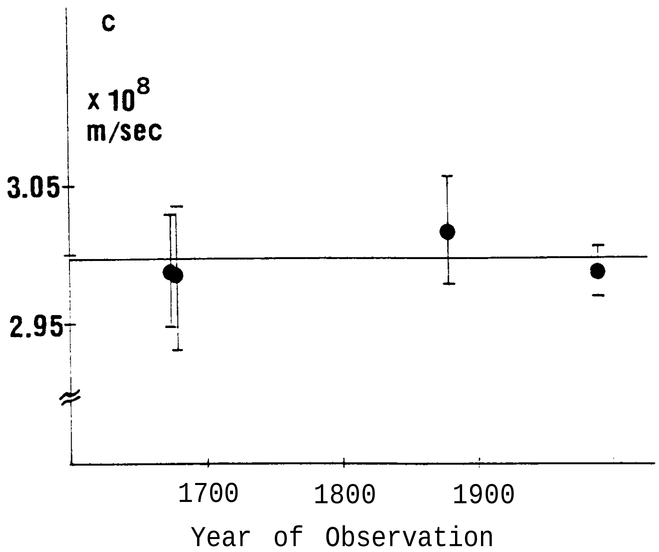

Figure 3. Results of the present calculations of the speed of light for the author's 1988-1991 data, the Harvard data of 1878-1880, and the Roemer method data for 1671-1673 and 1676-1678. The error bars represent the standard deviation found from the residual s for the actual versus calculated eclipse 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

I thank B. A. Starnes for reading the manuscript and for advice on extrapolations and statistics involved.

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.

References

Anderson, Courtney. 1956. To the golden shore: the life of Adoniram Judson. Little and Brown. Boston.

Arlot, J. E., B. Morando, and W. Thuillot. 1984. An analysis of recently discovered eclipse observations of the first satellite of Jupiter made between 1775 and 1802, and collected by Delambre. *Astronomy and Astrophysics* 136:142-152.

Baecklin, E. 1928. Absolute wellenlangenbest immungen der Roentgenstrahlen. Unpublished Uppsala dissertation.

Bearden, J. A. 1931. Absolute wavelengths of the copper and chromium K-series. *Physical Review* 37:1210-1229.

Bretagnon, P. 1980. Second order theory of the inner planets. *Astronomy and Astrophysics* 84:329-341.

Chaffin, E. F. 1990a. The difficulty in obtaining realistic conclusions about variable "constants." *Creation Research Society Quarterly* 27:6-9.

__________. 1990b. A study of Roemer's method for determining the velocity of light. Proceedings of the Second International Conference on Creationism. Volume II pp. 47-52.

Explanatory Supplementary to the Astronomical Ephemeris and Nautical Almanac. 1961. Her Majesty's Stationery Office. London.

Goldstein, Jr., S. J., J. D. Trasco and T. J. Ogburn III. 1973. On the velocity of light three centuries ago, *Astronomical Journal* 78: 122-125.

__________ 1975. On the secular change in the period of Io, 1668-1926. *Astronomical Journal* 80:532-539.

__________ and K. C. Jacobs. 1986. The contraction of Io's orbit. *Astronomical Journal* 92:199-202.

Greenberg, R., S. J. Goldstein and K. C. Jacobs. 1986. Orbital acceleration and the energy budget in the Galilean satellite system. *Nature* 323:789-790.

Herrick, S. 1971. Astrodynamics: orbit correction, perturbation theory, integration, Volume 2. Van Nostrand Reinhold. New York.

Hull, A. W. and N. H. Williams. 1925. Determination of the elementary charge e from measurements of the shot-effect. *Physical Review* 25:147-173.

Hutchings, Edward. editor. 1985. Surely you're joking Mr. Feynman! W. W. Norton. New York.

Lieske, J. H. 1978. Galilean satellites: analysis of photometric eclipses. *Astronomy and Astrophysics* 65:83-92.

__________ 1980. Improved ephemerides of the Galilean satellites. *Astronomy and Astrophysics* 82:340-348.

__________ 1986a. A collection of Galilean satellite eclipse observations, 1652-1983: Part I. *Astronomy and Astrophysics* 154:61-76.

__________. 1986b. A collection of Galilean satellite eclipse observations, 1652-1983: Part II. *Astronomy and Astrophysics Supplement Series* 63:143-202.

__________ 1987. Galilean satellite evolution: observational evidence for changes in mean motions. *Astronomy and Astrophysics* 176:147-158.

Millikan, R. A. 1917. A new determination of e, N, and related physical constants. *Philosophical Magazine* 34(6):1-30.

Mirsky, Jeannette. 1970. The westward crossings: Balboa, Mackenzie, Lewis and Clark. University of Chicago Press. Chicago.

Null, G. W. 1976. Gravity field of Jupiter and its satellites from Pioneer 10 and Pioneer 11 tracking data. *Astronomical Journal* 81:1153-1161.

Peale, S. J., P. Cassen and R. T. Reynolds. 1979. Melting of Io by tidal dissipation, *Science* 203:892-894.

Peters, C. F. 1975. Eclipses of natural planetary satellites. *Celestial Mechanics* 12:99-110.

Setterfield, B. 1981. The velocity of light and the age of the universe, Part I. *Ex Nihilo* 4(1):38-48 and Part II *Ex Nihilo* 4(3):56-81.

__________ and T. Norman. 1987. The atomic constants, light, and time. Available from Lambert T. Dolphin, 1103 Pomeroy Ave., Santa Clara, CA 95051.

Shiba, Kamekichi. 1932. The most probable values of e, e/m and h, *Scientific Papers of the Institute of Physical and Chemical Research (Tokyo)* 19(385):97-121.

Simon, J. L. and P. Bretagnon. 1975a. First order perturbations of the four large planets. *Astronomy and Astrophysics* 42:259-263.

__________ and P. Bretagnon. 1975b. Results of first order perturbations of four large planets. *Astronomy and Astrophysics, Supplement Series* 22:107-160.
__________ and P. Bretagnon. 1978a. Second order perturbations of the four large planets. *Astronomy and Astrophysics* 69:369-372.
__________ and P. Bretagnon. 1978b. Results of second order perturbations of the four large planets. *Astronomy and Astrophysics, Supplement Series* 34:183-194.
Tattersfield, D. 1981. Orbits for amateurs with a microcomputer. John Wiley and Sons. New York.
Tittemore, William C. 1990. Chaotic motion of Europa and Ganymede and the Ganymede-Callisto dichotomy. *Science* 250:263-267.
Wadlund, A. P. R. 1928. Absolute X-ray wavelength measurements. *Physical Review* 32:841-849.
Yoder, C. F. 1979. How tidal heating in Io drives the Galilean orbital resonance locks. *Nature* 279:767-770.

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

MICHAEL J. OARD*

Received 10 January 1992; Revised 1 March 1992

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 upglacial 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

*Michael J. Oard, M.S., 3600 7th Avenue South, Great Falls, MT 59405.

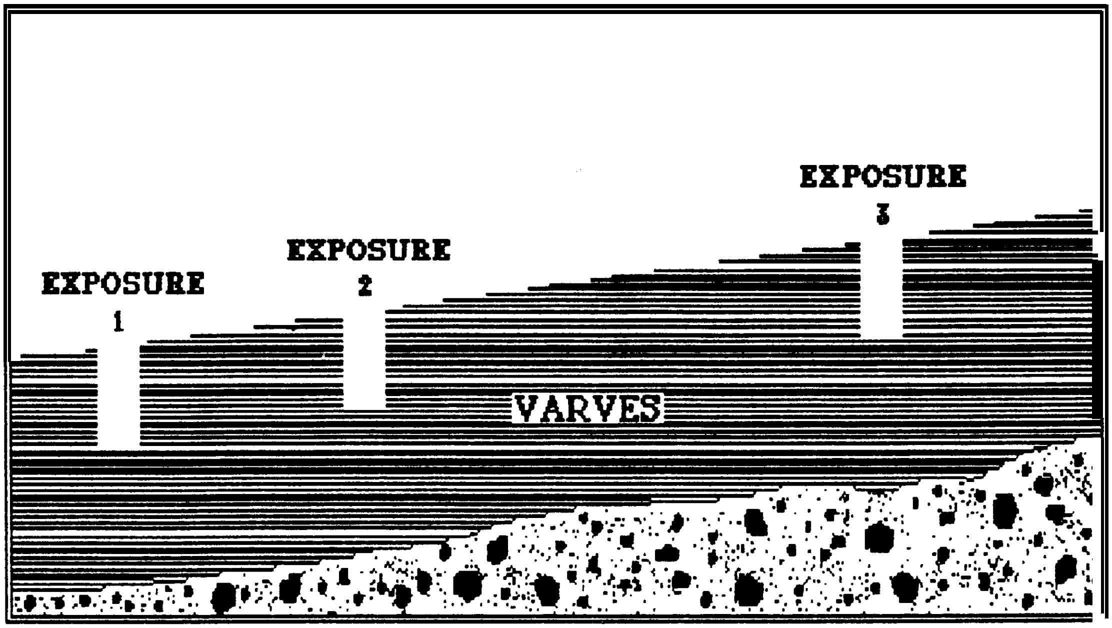

Figure 1. Schematic diagram illustrating "varve" correlations from three exposures. By measuring the couplet thickness pattern, the top portion of exposure 1 should match the bottom and middle portion of exposure 2. Redrawn from Nelson, 1948 by David Oard.

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

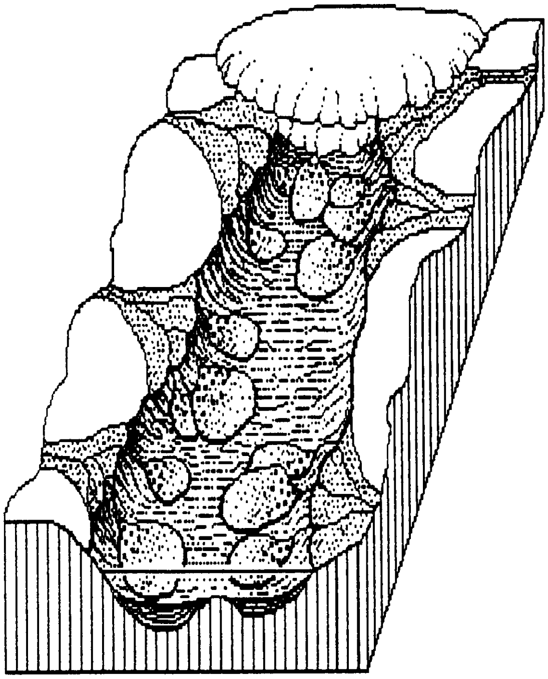

Figure 2. Schematic diagram illustrating a portion of ancient Lake Hitchcock as the ice retreats northward up the valley. Note the sediment mostly enters the lake from the east (E) and west (W), forming delta deposits and rhythmites. Redrawn from Ashley, 1975 by Dale Niemeyer.

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

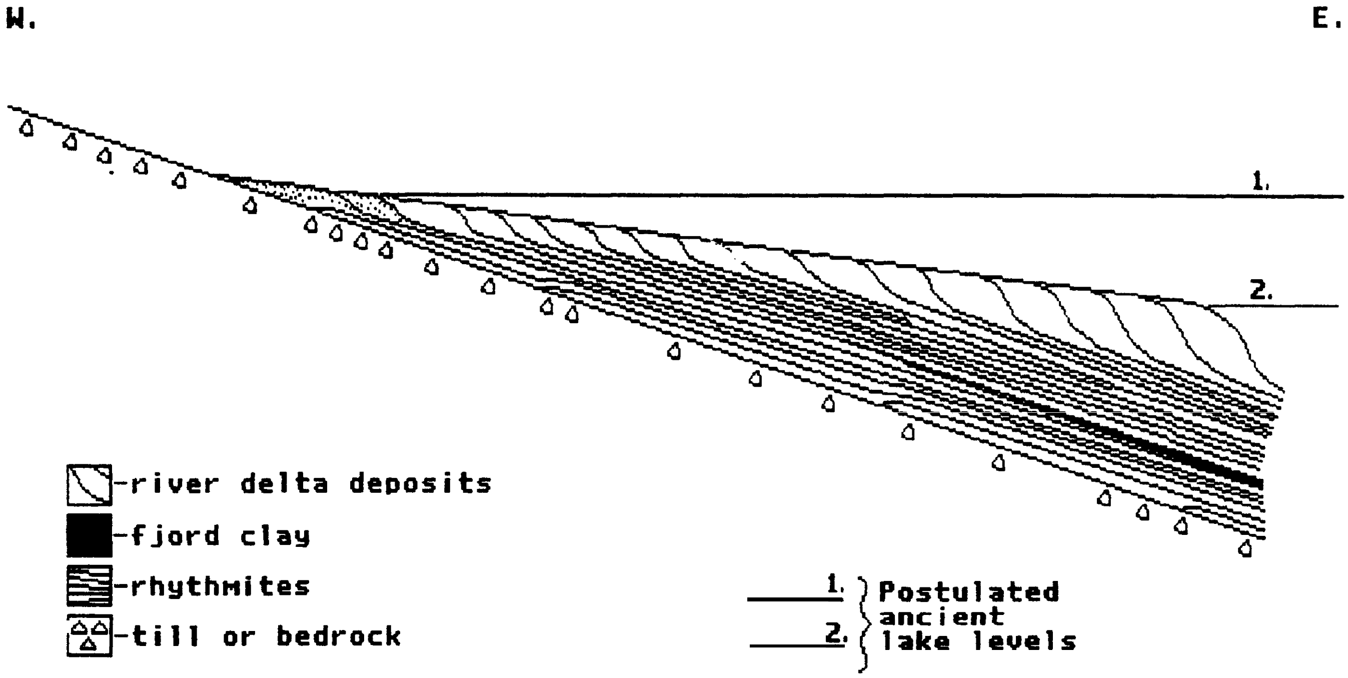

Figure 3. Typical section through the valley sediments along the Angermanalven River Valley, central Sweden. The fjord clay (black) supposedly separates the deglaciation "varves" below from the post-glacial river "varves" above. East (E), West (W). Redrawn from Cato, 1987 by Dale Niemeyer.

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.

References

Antevs, E. 1922. The recession of the last ice sheet in New England. American Geographical Society Research Series No. 11. New York.

________ 1925a. Retreat of the last ice-sheet in eastern Canada. Geological Survey of Canada, Memoir 146. Ottawa.

________ 1925b. Swedish late-Quaternary geochronologies. *Geographical Review* 115:280-284.

Ashley, G. M. 1972. Rhythmic sedimentation in glacial Lake Hitchcock, Massachusetts-Connecticut. Contribution No. 10, Department of Geology, University of Massachusetts. Amherst, MA.

________ 1975. Rhythmic sedimentation in glacial Lake Hitchcock, Massachusetts-Connecticut. In Jopling, A. V. and B. C. McDonald, editors. Glaciofluvial and glaciolacustrine sedimentation. Society of Economic Paleontologists and Mineralogists Special Publication No. 23. Tulsa, OK. pp. 304-320.

Austin, S. A. 1992. Excessively old "ages" for Grand Canyon lava flows. *Institute for Creation Research Impact Series No. 224,* pp. i-iv.

Cato, I. 1985. The definitive connection of the Swedish geochronological time scale with the present, and the new date of the zero year in Doviken, northern Sweden. *Boreas* 14:117-122.

_. 1987. On the definitive connection of the Swedish time scale with the present. *Sveriges Geologiska Undersokning* Ser. Ca (68):1-55.

Flint, R. F. 1975. Features other than diamicts as evidence of ancient glaciation. In Wright, A. E. and F. Moseley, editors. Ice ages: ancient and modern. Seel House Press. Liverpool. pp. 121-136.

Fromm, E. 1970. Errors in the Swedish varve chronology. In I. U. Olsson, editor. Radiocarbon variations and absolute chronology. John Wiley and Sons. New York. pp. 163-172.

Gould, S. J. 1978. Morton's ranking of races by cranial capacity. *Science* 200:503-509.

Gustavson, T. C. 1975. Sedimentation and physical limnology in preglacial Malaspina Lake, southeastern Alaska. In Jopling, A. V. and B. C. McDonald, editors. Glaciofluvial and glaciolacustrine sedimentation. Society of Economic Paleontologists and Mineralogists Special Publication No. 23. Tulsa, OK. pp. 249-263.

________ G. M. Ashley and J. C. Boothroyd. 1975. Depositional sequences in glaciolacustrine deltas. In Jopling, A. V. and B. C. McDonald, editors. Glaciofluvial and glaciolacustrine sedimentation. Society of Economic Paleontologists and Mineralogists Special Publication No. 23. Tulsa, OK. pp. 264-280.

Lundqvist, J. 1975. Ice recession in central Sweden, and the Swedish Time Scale. *Boreas* 4:47-54.

Mullins, H. T. and E. J. Hinchey. 1989. Erosion and infill of New York Finger Lakes: implications for Laurentide ice sheet deglaciation. *Geology* 17:622-625.

Nelson, B. C. 1948. Before Abraham. Augsburg Publishing House. Minneapolis.

Oard, M. J. 1985. Ice ages: the mystery solved? Part III: paleomagnetic stratigraphy and data manipulation. *Creation Research Society Quarterly* 21:170-181.

________ 1990. An ice age caused by the Genesis Flood. Institute for Creation Research. El Cajon, CA.

________ 1992. Varves—The first "absolute" chronology. Part I—historical development and the question of annual deposition. *Creation Research Society Quarterly* 29:72-80

Olsson, I. U. editor. 1970. Radiocarbon variations and absolute chronology. John Wiley and Sons. New York. pp. 163-172.

Ridge, J. C. and F. D. Larsen. 1990. Re-evaluation of Antevs' New England varve chronology and new radiocarbon dates of sediments from glacial Lake Hitchcock. *Geological Society of America Bulletin* 102:889-899.

Ringberg, B. 1979. Varve chronology of the glacial sediments in Blekinge and northeastern Skane, southeastern Sweden. *Boreas* 8:209-215.

________ and L. Rudmark. 1985. Varve chronology based upon glacial sediments in the area between Karlskrona and Kalmar, southeastern Sweden. *Boreas* 14:107-110.

Schove, D. J. and R. W. Fairbridge. 1983. Swedish chronology revisited. *Nature* 304:583.

Smith, N. D. 1978. Sedimentation processes and patterns in a glacier-fed lake with low sediment input. *Canadian Journal of Earth Sciences* 15:741-756

________ M. A. Venol and S. K. Kennedy. 1982. Comparison of sedimentation regimes in four glacier-fed lakes of western Alberta. In Davidson-Arnott, R., W. Nickling, and B. O. Fahey, editors. Research in glacial, glaciofluvial, and glaciolacustrine systems. Geobooks. Norwich. pp. 203-238.

________ and G. Ashley. 1985. Preglacial lacustrine environment. In Ashley, G., J. Shaw, and N. D. Smith, editors. Glacial sedimentary environments. Society of Economic Paleontologists and Mineralogists. Tulsa, OK. pp. 176-200.

Stromberg, B. 1983. The Swedish varve chronology. In Ehlers, J., editor. Glacial deposits in north-west Europe. A. A. Balkema. Rotterdam. pp. 97-105.

________ 1985a. Revision of the lateglacial Swedish varve chronology. *Boreas* 14:101-105.

________ 1985b. New varve measurements in Vastergotland, Sweden. *Boreas* 14:111-115.

Sturm, M. and A. Matter. 1978. Turbidites and varves in Lake Brienz (Switzerland): deposition of elastic detritus by density. In Matter, A. and M. E. Tucker, editors. Modern and ancient lake sediments. Blackwell Scientific Publications. London. pp. 147-168.

Tauber, H. 1970. The Scandinavian varve chronology and C14 dating. In Olsson, I. U., editor. Radiocarbon variations and absolute chronology. John Wiley and Sons. New York. pp. 173-196.

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.

Beerbower, James R. 1968. *Search for the Past: An lntroduction to Paleontology.* Prentice-Hall. New York. p. 163.

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.

Foster, R. J. 1973. General geology. second edition. Charles E. Merrill. Columbus, OH. p. 239.

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.

Twain, M. 1965. Life on the Mississippi. Airmont. New York. pp. 101-102.

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