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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 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

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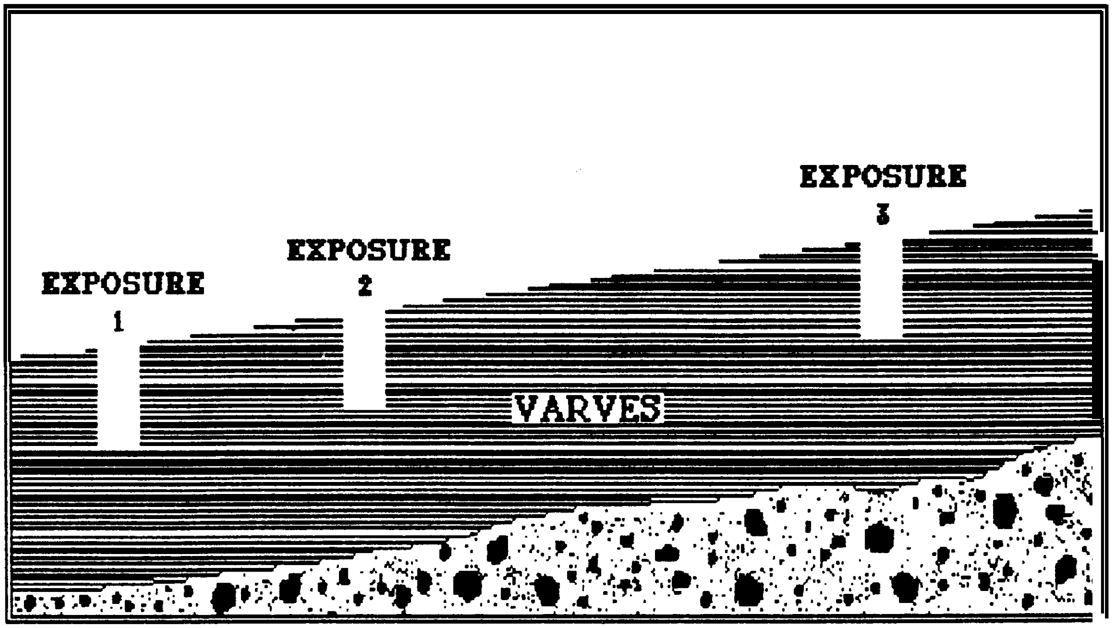

Figure 1. Schematic diagram illustrating "varve" correlations from three exposures. By measuring the couplet thickness pattern, the top portion of exposure 1 should match the bottom and middle portion of exposure 2. Redrawn from Nelson, 1948 by David Oard.

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

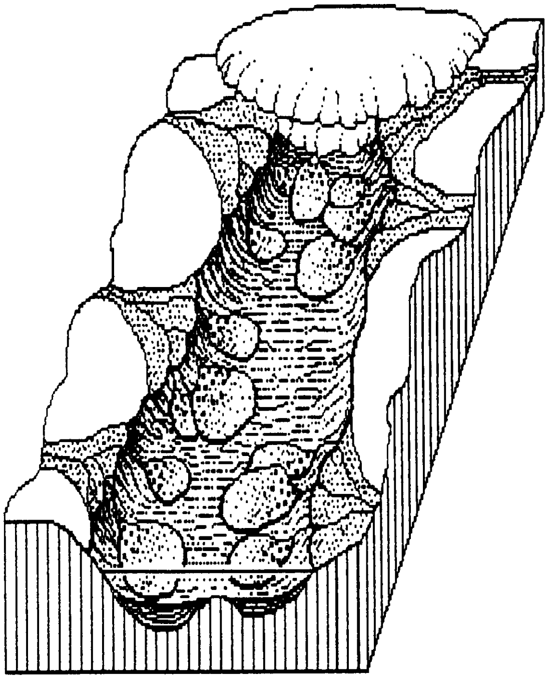

Figure 2. Schematic diagram illustrating a portion of ancient Lake Hitchcock as the ice retreats northward up the valley. Note the sediment mostly enters the lake from the east (E) and west (W), forming delta deposits and rhythmites. Redrawn from Ashley, 1975 by Dale Niemeyer.

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

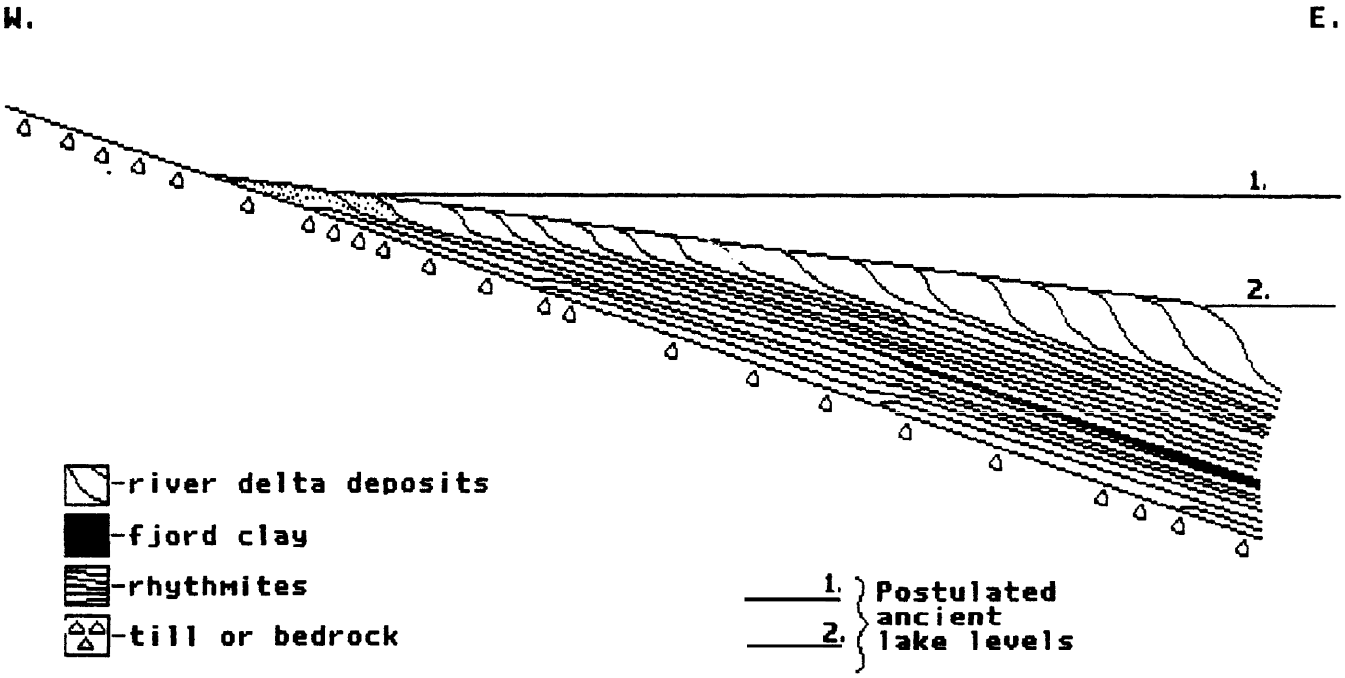

Figure 3. Typical section through the valley sediments along the Angermanalven River Valley, central Sweden. The fjord clay (black) supposedly separates the deglaciation "varves" below from the post-glacial river "varves" above. East (E), West (W). Redrawn from Cato, 1987 by Dale Niemeyer.

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.

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