# Glacial Surges and Flood Legends

Emiliani et al. (1) propose that a peak in the oxygen isotope profile for a sediment core from the Gulf of Mexico represents a sudden influx of glacial meltwater from the Mississippi River 11,600 years before the present (B.P.), and that the attendant rise in sea level initiated legends about floods around the world, including the deluge described by Plato as having occurred 9000 years before the age of Solon.

Emiliani et al. apparently take their cue from a study by Kennett and Shackleton (2), which attributes a peak in oxygen isotope profiles from the western Gulf of Mexico to freshwater influx from the wasting Laurentide ice sheet in the Great Lakes area about 17,000 to 11,500 years B.P. In an apparent attempt to make the explanation more specific, Emiliani et al. note that Bloom's curve showing the rate of retreat of the Laurentide ice sheet (3) has a conspicuous knick for the Valders readvance of 11,800 years B.P., representing a 2 percent increase in ice area. They further note Bloom's suggestion that the Valders ice advance into the Lake Michigan, Lake Superior, and Lake Winnipeg lowlands may have been a glacial surge, which is a rapid distension of a glacier beyond its normal terminus, perhaps resulting from the buildup of water beneath the ice. They assume that the wastage of the distended ice lobe should be equally rapid, accounting for a sea-level rise of "decimeters per year."

This series of arguments from glacial surge to the dialogues of Plato involves several important weaknesses and unjustified speculations:

1) A normal ice advance results in a lowering of sea level. The curve of latest-Quaternary sea-level change does not show the temporary 2-m fall and then rapid rise expected from the Valders 2 percent increase in ice area and presumably in ice volume. A surge might explain the discrepancy, because it involves no change in the mass budget of the glacier and thus no change in climate. However, there is so much scatter in the radiocarbon dates on which the sea-level curve is based that it is not possible to determine whether a 2-m fluctuation in sea level occurred at this time.

2) The Valders ice advance entered only the Lake Michigan, the Green Bay, and possibly the Huron basins. Late-Wisconsin advances into the western Lake Superior Basin and the Lake Winnipeg Basin were not necessarily contemporaneous with the Valders ice advance

of 11,800 years B.P., and in any case they were not very substantial (4). The change in ice sheet area should therefore be less than the 2 percent calculated by Bloom.

3) A surge of the Lake Michigan lobe as far south as Milwaukee, along with a possibly contemporaneous surge into the upper part of the Lake Huron Basin, should decant the water of proglacial lakes into the Mississippi River. Rough calculations of the water volume involved show that sea level would rise a small fraction of a centimeter. As the ice melted, some of the water would be stored once again in proglacial lakes, thereby reducing the rate of subsequent rise in sea level.

4) Recent study of the glacial stratigraphy and geomorphology around Lake Michigan indicates that the so-called Valders ice advance of 11,800 years B.P. extended only to Two Creeks, Wisconsin, and that the 350-km advance to Milwaukee was older (5). This revision reduces the "Valders" ice volume changes and potential marine effects substantially.

5) It is true that a surge carries an ice lobe beyond its normal, climatically determined equilibrium limit, and that the distended portion stagnates and is subject to melting. This melting is not necessarily instantaneous, however, for stagnant ice produced by surging can persist for thousands of years after its emplacement. Such is the case, for example, with glacial surges in the Yukon, because of the development of a protective cover of rock debris melted out of the ice (6). Even if the surged ice lobe were clean, calculations based on mid-latitude solar radiation as well as those based on ablation rates for modern mid-latitude glaciers show that it would take a great many decades to melt the several hundred meters of ice involved. The rate of sea-level rise in this case would be less than a centimeter per year—hardly catastrophic.

6) The "peak" on the oxygen isotope curve [figure 5 in (1)], even if it were statistically significant, is based on two samples apparently representing 20 cm of thickness in the core. The sedimentation rate for this interval, according to the radiocarbon dates, indicates that about 1350 years are involved in the two samples. Furthermore, each of the two dated carbon samples represents 20 cm of sediment and a similar span of time. The chronological control is therefore not good. The selection of a precise date within a few decades of 11,600 years B.P.,

as required for a catastrophe, is unjustified.

7) Using more conspicuous oxygen isotope anomalies in three cores from the western Gulf of Mexico, Kennett and Shackleton (2) made a more reasonable case for the freshening of seawater by glacial meltwaters produced during the entire wastage of the ice sheet in the Great Lakes area. Kennett and Shackleton correlated the three cores by matching the midpoints of a major biostratigraphic zone boundary dated at about 11,000 years B.P. in other cores from the Gulf of Mexico and Caribbean Sea. The peaks are dated on the assumption of constant sedimentation rate at each site before 11,000 years B.P. In the core with the highest sedimentation rates, the sharpest and most reliable peak (with eight sample points) is dated as covering the entire interval between about 17,000 and 11,500 years B.P.; in the other two cores, the peak occurs at about 20,000 to 15,000 years B.P. and 12,000 to 10,000 years B.P. The differences among the dates for the four cores involved (three cores of Kennett and Shackleton and one core of Emiliani et al.) indicates that there is little basis for placing the event precisely at 11,600 years B.P.

8) In view of the uncertainty associated with the effects of a "Valders" ice advance on ocean isotopic composition and sea level, and the uncertainties in dating and correlation, it is hardly justified to attribute flood legends around the world to a single such catastrophic geologic event for which there is no evidence. The accuracy of the figure of 9000 years, attributed by Plato to a catastrophic deluge, is accepted by few archeologists and historians. Plato wrote in the 4th century B.C., quoting conversations between Solon (who lived 200 years before) and Egyptian priests (who were encouraging Solon to tell stories of his ancestors). The priests told Solon a story of the submergence of Atlantis 9000 years before by great earthquakes and deluges, and Plato elaborated on the account. Many explanations for the story and the dating have been offered, including the eruption of the volcanic island Santorini (Thera) in the Aegean Sea (7). This event occurred about 1450 B.C., according to both archeological dating and radiocarbon analysis, and it is believed by some to have been responsible for the destruction of the Minoan culture on Crete. The date is closer to 900 than 9000 years before Solon, and the discrepancy is commonly attributed to an error in transcription or translation of early documents (8).

Few anthropologists or archeologists take seriously any attempt to attribute a flood myth to a single worldwide event so many thousands of years before. After all, Greece was still inhabited by hunters and gatherers 11,600 years ago—a people far different from those in the world of Solon and Plato (9). Myths are easily generated by people who live in a natural setting and transmit their culture orally, and the tales are readily embellished by the storyteller, are substantially modified, and often are even merged with other myths (10).

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10. One such story is in Dorothy Vitaliano's readable book about myths and geology (7, p. 151): "A highly illuminating example of how a legend can be transferred from one culture to another literally overnight was related by Alice Lee Marriott in a New Yorker article some years ago. When she was collecting the folklore of a South Dakota tribe, she was challenged one day by the old Indian who was her informant to tell him one of the tales of her people. She thereupon related the story of 'the Brave Warrior and the Water Monsters'—Beowulf. Few changes were necessary; it was 'all within the patterns of legendary behavior, which the old man could understand, and I reflected that there might be more to this universal-distribution-of-folklore than I had realized.' A little later she heard him relate the story to an audience of his people, 'and I must admit the old man made a better story of it than I did. A born, creative storyteller, he added bits here and there to round the tale out and make it richer. So must the story of Beowulf have gone, many centuries ago, from hearer to hearer, improved and embellished until at last it was written down.' The punch line of her article told how a few years later in an ethnological journal she came across a paper entitled 'Occurrence of a Beowulf-like myth among North American Indians,' published by a graduate student who, in violation of an unwritten law among ethnologists, had been using the same informant."

It is reassuring to read in Emiliani et al. (1) that isotopic curves from the Gulf of Mexico are being viewed in terms of "the dynamics of the Laurentide ice sheet." However, this view was anticipated by Kennett and Shackleton (2) who discussed these phenomena in relation to their isotopic analysis of cores from the western Gulf of Mexico. Unfortunately, their cores do not have absolute age calibration, but the conclusions

of Kennett and Shackleton and the interpreted chronology seem very realistic. Certainly at times of maximum extension of the Laurentide ice a broad sector of its front—some 2800 km (straight-line distance) from Montana to western Pennsylvania—was draining into the Gulf of Mexico via the Mississippi River and its tributaries. Such drainage continued during the waning phases of a glaciation until the ice front retreated sufficiently for alternate drainage routes to be opened, mainly to the St. Lawrence River, as recognized by Kennett and Shackleton (2). The abrupt shift of glacial meltwaters from the Mississippi to the St. Lawrence route has been documented by Broecker et al. (3) on the basis of an abrupt change in sediment type off the Mississippi delta.

Unfortunately Emiliani et al. have underestimated the effects of the melting ice sheet on the isotopic composition of Gulf of Mexico waters in that they consider only meltwater flow in the southward direction during the "Valders" readvance, which they date at 11,600 years before the present (B.P.). The report of Emiliani et al. (1) presents some misconceptions about the fluctuations of the Laurentide ice front near the end of the last glaciation that need some clarification. Our investigations over the last 5 years or so (4, 5) have shown that (i) the "Valders" readvance did not form an exaggerated ice lobe in the Lake Michigan Basin as previously believed and (ii) the type Valders Till as traced from Valders, Wisconsin, underlies rather than overlies the Two Creeks forest bed. Therefore, the till overlying the Two Creeks forest bed has been renamed "Two Rivers Till" (5) and is evidence of the "Two Rivers" readvance (6). There is no longer any reason to believe that the Two Rivers (formerly "Valders") readvance was an ice surge because the revised ice front position now indicates that the amplitude of that readvance was about 60 percent of earlier estimates. Thus, the ice front at that time did not exceed that of the preceding (Port Huron) readvance in the Lake Michigan Basin, as previously believed. In fact, the Port Huron readvance was of much greater amplitude than the Two Rivers readvance, and it has not been considered a surge. Furthermore, it has been suggested (7) that the Two Rivers ("Valders") readvance was essentially confined to the Lake Michigan Basin with only very limited counterparts, if any at all, in other ice lobes along the southern front of the Laurentide ice sheet. In short, the readvance formerly called

"Valders" was a relatively minor readvance and should not be considered a surge even in the Lake Michigan Basin.

The isotopic curve for the Gulf of Mexico [figure 5 in (1)] is very striking in that the most dramatic change in isotopic values occurs prior to 11,600 years B.P., in fact, prior to 12,220 years B.P. Between about 14,500 and 12,220 years B.P. the $\delta^{18}O$ value shifted from $-0.3$ to $-2.20$ per mil. This shift of 1.9 per mil is 75 percent of the entire range of $\delta^{18}O$ values in core GS7102-9. The curve of $\delta^{18}O$ values actually flattens out after 12,200 years B.P. and begins to decrease toward present-day values after 10,865 years B.P.

The large shift in isotopic values between 14,500 and 12,220 years B.P. corresponds exactly to the time of major retreat of the Laurentide ice front. Kennett and Shackleton (2) have placed the greatest deviation in their isotopic values at about 13,500 years B.P. At 14,500 years B.P. the ice was at or near its maximum extent throughout much of the United States sector, but by about 12,000 years B.P. both the James River ice lobe and the Des Moines ice lobe had retreated some 1200 km, the Lake Huron lobe some 400 km, and the Erie lobe some 400 to 500 km. In contrast, the documented retreat of the Lake Michigan lobe where the subsequent Two Rivers advance occurred was only about 300 km (8). The meltwater of all of these lobes drained into the Mississippi system, introducing vast amounts of $^{18}O$-poor water over a 2000- to 2500-year interval. We contend, along with Kennett and Shackleton (2), that this important retreat of the Laurentide ice was responsible for the dramatic shift in isotopic values in Gulf of Mexico waters.

On the other hand, the disintegration of the Two Rivers ("Valders") ice—surge or not—occurred at a time when the configuration of the Laurentide ice front was such that (i) all the meltwater east of the state of Michigan drained eastward via the St. Lawrence River into the Atlantic Ocean and (ii) much of the meltwater in the Canadian Plains sector was temporarily stored in Glacial Lake Agassiz before draining into the Missouri-Mississippi system. Lake Agassiz had an areal extent of about 31,000 km² at any given time and an average depth of about 100 m (9), and thus contained some 3100 km³ of glacial meltwater. Therefore, the meltwater in the Canadian Plains sector did not run off immediately to the Gulf of Mexico. Moreover, after 9500 years B.P., if not earlier, much or all of the Lake Agassiz outflow passed eastward via Lake Superior to the St.

Lawrence (9). The importance of these drainage changes can be seen clearly in the curve for core GS7102-9 [figure 5 in (1)] which shows little isotopic change between 12,220 and 10,865 years B.P., a period encompassing much of the Twocreekan retreat and *all* of the Two Rivers ("Valders") advance and retreat. Thus, we contend further, much as Kennett and Shackleton did (2), that the isotopic values ceased their rapid decline after about 12,200 years B.P. and shifted toward present-day values at the time of diversion of the major part of meltwater drainage away from the Gulf of Mexico to the Atlantic Ocean.

One further point concerns the coincidence "within all limits of error" of dates of 11,600 years B.P. both for the "Valders" event as seen by Emiliani *et al.* (1) and for the deluge "9000 years before Solon." In the first place, the age for the "Valders" event (11,600 years B.P.) is an interpolated age without any assigned confidence limits. Second, if the date for Solon's flood is a true calendar date, it may be out of phase with the radiocarbon age for the "Valders" event by as much as 600 years. Dendrochronological calibration of radiocarbon dates (10) has shown the radiocarbon dates to be consistently 600 to 700 years too young in the calendrical range from 3600 to 5350 B.C. (5550 to 7300 years B.P., approximately). The calibration curve does not yet extend beyond 7300 calendar years ago, but it is unlikely that a radiocarbon year equals a calendar year at 11,600 years B.P.

Several conclusions seem in order:

1) The isotopic curve for the Gulf of Mexico does faithfully reflect the major events of the Laurentide ice front.

2) The focus of Emiliani *et al.* (1) on the "Valders" (Two Rivers) advance and retreat does injustice both to the ice sheet record and to the isotopic record because the most dramatic shifts in both of them occurred in the period between about 14,500 and 12,200 years B.P., as demonstrated by Kennett and Shackleton (2).

3) The Two Rivers ("Valders") readvance was almost certainly not a surge, the surge hypothesis being based on a reconstruction of ice lobation now shown to be incorrect.

4) The isotopic curve of core GS7102-9 from the Gulf of Mexico does not record the Two Rivers ("Valders") event because at the time of that event and thereafter most of the meltwater of the Laurentide ice front drained via the St. Lawrence into the Atlantic Ocean, as concluded by Kennett and Shackleton (2).

5) The correspondence in calendar age between Solon's flood and the Two Rivers ("Valders") event is more apparent than real because of variations in radiocarbon activity.

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The recent reports by Kennett and Shackleton (1) and Emiliani *et al.* (2) are important because in them an attempt is made to link the isotopic record of the oceans (more specifically the Gulf of Mexico) with the complex history of the southern margin of the Laurentide ice sheet. In a companion technical comment, Farrand and Evenson (3) state why serious reservations must be expressed against the correlation of Emiliani *et al.* of a shift in the isotopic record in core GS7102-9 with the "Valders" readvance. My purpose in this comment is to point out that the suggested rise in sea level of "decimeters per year" (2) at about 11,600 years before the present is far too rapid.

Let us take the extreme case of a surge along the entire southern margin of the Laurentide ice sheet. Since we are concerned only with order-of-magnitude calculations, I will take the 2800-km straight-line figure of Farrand and Evenson (3) as the length of the perimeter draining southward. Let a 100-km surge occur with an average ice thickness of 1 km. Furthermore, assume that this ice mass (2800 by 100 by 1 km) melted in a *single year*. The mass of water contained in this surge is $2.5 \times 10^{14}$ m³, whereas the area of the world oceans is $3.61 \times 10^{14}$ m². The global ocean equivalent rise contained in this ice mass is thus only about 0.7 m. As a result, there is no compelling reason to correlate the "Valders" readvance with the folklore of the flood, and certainly the suggested *rate* of sea-level rise cannot be ascribed to such an event. There is already much difficulty in accounting for the rapid retreat of the Laurentide ice margin as it is (4) without having to consider the energy requirements necessary for ablating the ice sheet at a rate that would lead to a "decimeters per year" rise in sea level.

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None of the above authors seems to be aware of the fact that the eastern Gulf of Mexico is scavenged by $29 \pm 5 \times 10^6$ m³ sec⁻¹ of Gulf Stream water [the Loop Current (1)]. The measured isotopic effect [2.5 per mil; see (2) and (3)] demonstrates, for the top 200 m, where 50 percent of the transport occurs, a salinity decrease of 8.3 percent for an ice meltwater composition of −30 per mil (2) or a decrease of 16.6 percent for a composition of −15 per mil (4). Corresponding fluxes are $1.2 \times 10^6$ and $2.4 \times 10^6$ m³ sec⁻¹, and corresponding sea-level rises are 10.5 and 21.0 cm year⁻¹. Renewed precipitation from the thawing northern North Atlantic (5) and northeastern Pacific was probably feeding the channeled surges which best explain the magnitude of the flood. Although it is not possible to establish exactly the duration of the flood because reworking by benthic metazoa spreads out the oceanic record, I reject Wright and Stein's indiscriminate usage of age estimates to discount our ¹⁴C ages as well as Farrand and Evenson's preference for estimated ages to documented ¹⁴C ages. Furthermore, I wish to point out to Farrand and Evenson that maximum flooding in a continuously flushed basin does not corres-

pond to maximum change in isotopic composition but to zero change at the peak. I would point out to Andrews that valid conclusions cannot be drawn from calculations which fail to take precipitation into account.

The isotopic evidence demonstrates that there was a massive flood in the Gulf of Mexico, peaking about 11,600 years ago. Since nature is self-consistent, I suggest that those who have commented on our report revisit their tunnel valleys and spillways to look for the matching evidence.

With respect to Plato and the "Atlantis Connection," I refer the reader to (6).

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7. Only 3 weeks were allowed by the editors of *Science* for the preparation of this reply. Since my co-authors of (3) are presently scattered from Florida to Taiwan, it was impossible to contact them. This reply, therefore, is necessarily written by the senior author alone. I am grateful to G. Maul for discussions on the eastern Gulf Loop Current. Financial support was provided by the National Science Foundation (grants GX36155, GA36189X, and DES 74-23459).

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# Criteria for the Discovery of Chemical Elements

The availability of suitable heavy-ion accelerators in a number of laboratories in Europe and the United States should make it possible to synthesize and identify additional heavy transuranium elements. The predicted small yields of such nuclides require identification of atomic number to be made with individual atoms. This places a large burden on the experimenter and can lead (and in fact has led) to differences of opinion as to the extent of experimental proof required to establish definitely that the production of a new element has been observed. There is also the possibility that superheavy elements may be found in natural sources. We attempt here to define criteria for adequate proof that a new element has been synthesized or found in nature, and identified—that is, discovered.

The basic criterion, of course, must be the proof, by some means, that the atomic number ($Z$) of the new element is different from the atomic numbers of all previously known elements. This means, in general, that the atomic number should be established. It should not be necessary to establish the mass number, except insofar as this evidence is directly related to the method used for establishing the atomic number.

Chemical identification constitutes an ideal proof that an element with a new atomic number has been produced. Two important requirements should be met in this kind of experiment. First, the chemical procedure should be of a type that is valid for application to individual atoms; the use, for example, of ion exchange

adsorption-elution or partition between solvents has been shown to meet this criterion in many situations, and such methods also provide safeguards against complicating surface adsorption and entrainment effects. Second, it must be possible to determine the presence or absence of the new element in the appropriate chemical fractions in an unequivocal manner. If the new element is observed through its decay by high-energy alpha-particle emission or spontaneous fission, or both, the chemical identification can be confined to separation from all known elements with atomic number greater than lead ($Z = 82$).

Unfortunately, chemical identification is not always feasible in the initial experiments, as demonstrated by the reported discoveries of the last several synthetic elements; this circumstance has contributed significantly to the competing claims for the discovery of these elements. (No such differences of opinion have arisen over the discoveries, based on chemical identification, of mendelevium and earlier transuranium elements.) Fortunately, there are methods based on the observation and use of nuclear properties that should be adequate to furnish unambiguous identification of atomic number.

Also satisfactory is the identification of characteristic x-rays in connection with the decay of the isotope of the new element. In actual practice this is likely to involve measurement of the half-life and precise, unique energies of the alpha particles of the new element in coincidence with the characteristic x-rays of

the daughter nuclide. However, it might be possible to measure characteristic x-rays of the new element itself (primary product) if these can be associated with the subsequent immediate decay of this nuclide. Thus, such short-lived x-rays, which may be emitted in the course of, or as an aftermath of, the production of the primary product, might be followed very shortly by emission of alpha particles or fission fragments which could be detected by delayed coincidence techniques. The characteristic x-rays must, of course, be distinguished from gamma rays of similar energies—perhaps by identification of the complex structure of the x-rays.

The proof of a genetic decay relationship through an alpha-particle decay chain in which the isotope of the new element is identified by the observation of previously known decay products should be acceptable. This method depends on measurement of the half-life and precise, unique energies of the alpha particles of the new isotope, and measurement and identification of the half-life and decay properties of the daughter, whose identity, including atomic number, has been previously established. Time correlation between parent and daughter should be established. Use of a genetic relationship as evidence for a new element implies that the mass number of the new element isotope is experimentally determined by its relationship to a daughter nuclide of known mass number.

Detection of a spontaneous fission activity and measurement of its half-life cannot per se establish that an element with a new atomic number has been produced. Even when additional information, such as fragment mass and kinetic energy distributions, can be obtained, the atomic number assignment for new elements cannot be made on this basis alone since the systematics and theoretical predictions cannot be extrapolated with the necessary certainty into new regions. Similarly, the use of the predicted half-lives for spontaneous fission decay and alpha decay and of predicted alpha-decay energies cannot yet be considered sufficiently reliable for establishment of the atomic number of a new element.

The present understanding of production yields, excitation functions, angular distributions, and so forth is not sufficient to allow measurements to establish with certainty that a nuclide with a new atomic number has been produced, although such data may be useful as supportive evidence. It is particularly difficult to establish and interpret the