POST-WISCONSIN CHRONOLOGY

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Accurate computation of the length of time that has elapsed since the final disappearance of the Wisconsin ice-sheet from the Great Lakes region presents a problem that has aroused the active interest of a number of scientists through the years. Recent time may eventually prove to have been but another interglacial stage. Certainly, its span to date is considerably shorter than that of any of the other post-glacial stages that preceded it, and the existence of large continental ice-caps at the present time furnish reason not only for assuming that the glacial period has not yet been concluded, but that the general date of the disappearance of ice from the northern states was more recent than may previously have been believed.

It is the purpose of this paper to point out what the author believes to be inconclusive or inconsistent generalizations in several of the hypotheses that have received prominent attention in past years, with reference to the computation of local post-glacial chronology, and to suggest for consideration a new method of computation that seems to offer more positive data and an exact dating procedure.

Obviously, the length of time since the disappearance of the last ice-sheet is a matter of local variation; and therefore the recent stage for Champaign County, Illinois, for instance, is of somewhat different length than that for the Michigan Peninsula area. Nevertheless, a reasonably exact minimum figure, based on positive evidence rather than on assumption, should be worked out and announced.

Figures regarding the rate of recession of Niagara Falls, no matter how carefully computed, involve too much in the way of personal opinion and interpolation by assumption to fix even a general date for the retreat of the ice from the Lake Ontario basin. Separate investigators have reached separate conclusions, involving a time-spread of from 5,000 to 50,000 years. Considering the undeter-

minable factors involved in the problem this wide margin is not only understandable, but represents the limit of accuracy that is probably attainable. The carving of the gorge has obliterated a preglacial valley that is now occupied by the Niagara River, from the falls to beyond the whirlpool—half the distance to Lake Ontario. Some of the erosion must be credited, therefore, to preglacial stream action. Consider, too, that the limestone cap thins out rapidly to the north, until, at the edge of the escarpment at Lewiston, the exposed section consists almost entirely of shale.

This geologic evidence seems to indicate that the so-called falls must have been originally no more than a series of rapids, cutting quickly through a shale section. Thus, to determine the rate of recession for the past 100 years, based on actual human observation, and then to interpolate that rate on a linear scale corresponding to the total distance from the present falls to the bluff at Lewiston, is to neglect consideration of important factors that were certainly involved to a great extent.

In passing, it should also be pointed out that the falls are known to have varied greatly in width from time to time, and that such variations necessarily affect the rate of recession. The comparative accomplishments of falls recession and side erosion within the gorge, by a swiftly flowing stream, cannot be established, in my opinion. Therefore, any figure on the rate of recession, projected back through thousands of years, can be of little value.

Let us now consider critically the study and interpretation of varves as dating media. The work of De Geer, Mrs. De Geer, Lidèn, Antevs, Sauramo and others have added greatly to our knowledge of sedimentary processes and have supplied valuable data for interpreting the records of the highly complicated history of Pleistocene continental glaciation.

Their work, though, has been largely inspired by the assumption implied in the definition of varves: namely, that they are "annual layers of silt, as deposited in lakes or other bodies of still water." How it can be proved that these layers invariably represent annual accumulations that can be counted and correlated to establish exact chronologies, has not been satisfactorily demonstrated. They are admittedly regarded as "annual by agreement," in the face of ample evidence to the contrary, even in the case of ideal occurrences. In many cases it has been shown that so-called "varves" are but the sedimentary products of individual storms and intervening spells of fair weather.

In this connection, some valuable information might be obtained on the chronological significance of still-water silt layers by taking a core of the silts at the bottom of Lake Decatur, or some other artificial lake of moderate size and known age, receiving sediments from a typical glaciated watershed.

Soil-profile studies, as developed and applied by Leighton, MacClintock, and others, have been used as a yardstick for measuring postglacial intervals, but their value for the reckoning of time is only general and comparative, and dating conclusions based on them cannot be expressed in terms of less than thousands of years.

What, then, can be considered as authoritative, accurate, and incontrovertible evidence of the passage of time, in terms of a definite number of years, during the recent period before modern man appeared and became curious about this subject? It is herein suggested and proposed that the science of dendrochronology can supply the most satisfactory answer to this question. Such workers as Douglas, Stallings, Keen, and others have proved to the satisfaction of botanists that the tree-ring calendar has undisputed validity, and that cross-dating, when done by experts, can and does establish a positive local chronology to the limit of the availability of suitable specimen material.

It is true that tree-ring chronology studies so far carried on have had as their primary objectives either the exact dating of human cultures and occupancies, or the tabulation of climatological or botanical data for past years. The notable success of these studies, in extending the calendar backwards more than 3,000 years in western and southwestern states, leads the writer to believe that similar studies offer the best promise for establishing the time span of the post-Wisconsin stage.

Such studies, it is suggested, would begin with specimens taken from modern trees growing in a locality where a single

species, such as the larch or tamarack, has very evidently been involved in the formation of woody peat beds, such as those of southeastern Wisconsin, and whose descendants flourish today in the same locality. Using the proved methods of study and analysis established by Douglas the investigation could proceed, using cross-dating techniques, with older and older specimens, which can reasonably be expected to be available in any one or many of the woody peat bogs of the Great Lakes region.

Mrs. De Geer attempted, a few years ago, to correlate the varve chronology of Sweden with the tree-ring chronology of the modern California sequoias. The results were inconclusive and led to the conclusion that the tree-ring calendars were unreliable. This conclusion seems unwarranted, in view of the fact that the annual nature of tree rings has been established, and that they do correlate with local modern weather records. In this connection it is worth noting that Douglas, through his tree-ring studies, has shown that the reason for, and time of, the abandonment of the cliff dwellings of the southwest were quite different from the previously accepted theories. They were abandoned by their occupants because of a 21-year drought that began in the year 1276—not because of extermination by enimies, and certainly not in ancient times.

It is by no means held that the proposed peat-log tree-ring studies would yield a complete postglacial chronology for any particular township location, nor is it possible to expect to find critical specimens that record the exact date of the retreat of the ice from a particular spot. The latter possibly cannot be ruled out, as we find numerous specimens of locally-derived wood in the drift itself, and there is no reason for doubting that trees grew on the surface of the ice itself, near the ice margins. They do on the Malaspina glacier in Alaska; and the Pleistocene ice-sheets, according to Thwaites, were drift-mantled near their margins. Furthermore, plant life flourished immediately adjacent to the ice fronts, as Thwaites and others have pointed out.

As for specimen material in the past, however, we can assume that the oldest logs present probably do not date back to the time of the disappearance of the ice. It is held by the writer, though, that the time-hiatus was short and that the originally barren drift was quickly mantled by vegetation, as is the case with strip mine debris—a fair comparison.

The point to be stressed is that the proposed study offers promise for establishing a positive method of fixing the *minimum* length of time, in terms of actual years, since the final retreat of the Wisconsin ice in a typical and restricted locality. It may conceivably solve some related problems dealing with the still-obscure events that took place in the critical areas near the ice front. If logically planned and studiously executed, and if successful, it would yield an exact chronology that would at least check the results of studies of varves, the recession rate of Niagara Falls, and comparative soil profile development. It is further believed by the writer that this check would supply more exact figures than any of the other methods cited, and might well supplant them in favor by substituting fact for supposition. It is even suggested that promising results secured from preliminary investigations of post-Wisconsin tree-ring evidence would lead naturally to the application of this dating technique to the larger problem of Pleistocene chronology in its entirety. At least, this suggestion is hereby placed in the record.