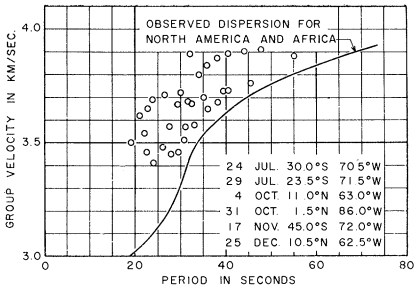

Fig. 3. Antarctic Rayleigh wave dispersion data compared with normal continental curve.

tinental and oceanic crustal thickness, the data indicate that for the profiles represented by heavy lines in Fig. 1, about one-fourth of the path is oceanic and three-fourths is continental. Had we assumed crustal thickness corresponding to shallow ocean, then the oceanic portion would have been much larger.

These results support the view that below-sea-level depths observed in measurements of ice thickness are primary features and not the result of crustal sagging under an ice load. They further show that more extensive areas of the antarctic land mass lie below sea level than have been reported.

Limited data available for the profiles shown by dashed lines in Fig. 1 suggest that the region is almost entirely continental. It may be possible to specify in greater detail the continental and oceanic areas of Antarctica when more surface wave data become available.

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19 January 1959

Theory of Ice Ages

In two recent papers, Ewing and Donn (*1, 2*) have presented a theory to account for repeated continental glaciation during the Pleistocene. This theory states that ice ages began when the North Pole migrated into the Arctic Ocean, the semi-isolated position of which causes climatic oscillations with a period and amplitude of the proper magnitude to account for all the observed environmental changes of the Pleistocene.

The oscillation is presumed to start with an ice-free Arctic Ocean warmed by water exchange with the Atlantic. Such an ocean would provide a rich source of precipitation for the circumarctic lands, inducing glacier growth there and increasing the earth's albedo enough to lower its mean temperature appreciably and cause further growth of glacier ice in northern regions and elsewhere. Ultimately so much water would be locked up in ice on the surface of the land that sea level would be lowered, the exchange of water over the Arctic-Atlantic sill would be materially decreased, the Atlantic would warm, and the Arctic would freeze, cutting off the polar precipitation source. The glaciers would then melt, interglacial conditions would prevail, the sea would rise, the Arctic ice pack would melt, and the whole cycle would start over again.

To me it is not clear why such a system should oscillate at all, let alone with the proper period and amplitude to cause glacial and interglacial ages. Rather, it seems probable that the system postulated would be a continuously self-regulatory one, that is, an ice-free Arctic Ocean would cause glaciers to form, which would immediately remove water from the sea, reducing the flow of warm Atlantic water into the Arctic basin and causing the formation of pack ice which would reduce snowfall on the adjacent land and shrink the glaciers. Apparently the authors of the theory (*1*, p. 1063) feel that the Arctic-Atlantic exchange of water would change abruptly, while the continental ice sheets would change gradually. As the decrease in interoceanic water exchange must be proportional to the reduction in sea level, and this in turn proportional to the volume of continental glacier ice, there does not seem to be any reason to expect such a lag of ice sheet behind ocean. The current temperature change is in phase throughout the world (*3*). Exact evaluation of the factors involved would be very difficult, but even crude mathematical models would be preferable to a subjective statement of the theory, and ought to show whether or not the postulated oscillations are likely.

It is not possible to examine in detail all the evidence on which the theory is based, for seven of the references, some of them very necessary to the argument, are to personal communications, articles in press, or unpublished observations. An examination of the published sources, however, reveals a very uncritical assessment of the relevant evidence. The theory demands that continental glaciers grow in the region around the Arctic Ocean and that the ice be very thick there during the height of a glaciation. This is in direct opposition to the prevailing opinion among glacial geologists that the principal nourishment and greatest thickness of ice were over the southern parts of the glaciers (*4*, pp. 313 ff.).

In attempting to satisfy this demand of their theory, Ewing and Donn refer to a map by J. Tuzo Wilson which is said to show that the glacial ice divide was much farther north than has previously been believed. The map has been published, in a form which shows the complete ice divide, only by Flint (*4*), as part of an exposition of what Ewing and Donn would have us believe is an "earlier" discredited view of the thickness distribution of the Laurentide ice, so one cannot scrutinize the evidence on which it, in turn, is based. It is, perhaps, significant that Wilson omitted much of the ice divide from the map when he published it (*5*). The inferred position of the ice divide appears to depend largely on aerial photographs of geomorphic features and, as Flint points out quite clearly, such features are most likely to represent conditions at the end of the ice age when the ice had retreated to the general region of the divide. Inferred ice divides based on reconnaissance studies of glacial geology are not very reliable, but this one, for what it is worth, is quite in accord with general geological opinion, and does not support the unorthodox views of Ewing and Donn.

The second line of evidence involves isobases. Ewing and Donn quote Charlesworth (*6*, p. 1321) to substantiate their idea of a northern ice divide. But if we turn to the page cited, we find a map from a paper by Daly showing the maximum uplift to be centered, not near the Arctic Ocean, but southeast of James Bay. Furthermore, the map depicts isobases of uplift since the postglacial marine transgression, and so tends to underestimate the crustal warping at the southern edge of the ice under full-glacial conditions. This map, even more than Wilson's, is subject to revision when more evidence is available about postglacial rebound in Canada, but it, too, supports the generally accepted view, and is, in fact, one of the classic statements of it.

Ewing and Donn's suggestion (*2*, p. 1160) that the crustal deformation data for the Great Lakes region be extrapolated through Hudson Bay is based on the assumption of a far-northern center of accumulation, and so cannot lead to any independent confirmation of it.

In no part of the theory is any cognizance taken of the fact that large parts of the land around the Arctic Ocean, far from being centers of glacier accumulation, were never glaciated at all. For example, the Glacial Map of North

America (*7*) shows unglaciated areas in Peary Land and in northern and central Alaska. In Siberia even larger areas were ice-free throughout the Pleistocene (*4*, plate 3; *6*, p. 721). The field evidence is of variable quality, but there is little doubt about the general phenomenon. Some of these areas, such as unglaciated Alaska, have been studied extensively and repeatedly for many years (*8*). Furthermore, pollen studies have shown that vegetational changes in the circumpolar area have been parallel (*9*) and synchronous (*10*) with those in temperate latitudes, which they would certainly not have been if polar climatic changes had been out of step with those in the rest of the world.

Finally, the suggestion that the North Pole shifted into the Arctic Ocean at the beginning of the Pleistocene is difficult to accept in the face of fossil evidence (*11*) indicating the stability of the present circumpolar zonation, at least during the Cenozoic. Evidence from paleomagnetic studies such as Hospers' (*12*), cited by Ewing and Donn, is not pertinent unless it can be shown that the magnetic and mechanical poles shift together. At present this is more of an interesting idea than an established fact, but the conclusion based on it, contrary to what Ewing and Donn imply, is, in Hospers' words (*12*, p. 59): "If polar wandering has taken place at all, it has not exceeded 5°–10° since Eocene times" (*13*).

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13. This paper was aided by support from the National Science Foundation and the American Academy of Arts and Sciences.

24 June 1958

It is difficult to write a discussion of Livingstone's paper appropriate for the general reader. He has raised several valid questions, but they are largely obscured by many critical remarks which seem to be based on subjective factors.

His debate of the question of whether our proposed system would maintain oscillations, and his demand for a mathematical model instead of a "subjective statement of the theory," seems to overlook the nature of the criteria which distinguish between nonoscillatory and oscillatory systems, particularly such a system as the one involved here. Our explanation of the glacial-interglacial oscillations is qualitative and subjective, as are those accepted for the oscillations of geysers, singing flames, bowed strings, whistles, fluttering flags, and so forth. If Livingstone appreciates the great difficulty of a mathematical statement of the criteria of oscillation for even such simple systems as these, he should hardly expect such a statement for glacial-interglacial oscillations.

In Livingstone's question about the abruptness of the alternation in Atlantic-Arctic interchange of water, which would result from a gradual change in sea level, there is no mention of the influence of the Arctic Ocean ice sheet on the rate of interchange. Apparently he is considering only the restricting effect of the passage between Greenland and Scandinavia. Nevertheless, it was pointed out in part I of our theory that the presence of an Arctic ice cover would severely restrict the wind-driven circulation. Conversely, it was noted that the opening of the Arctic would produce a strong wind-driven circulation in a counterclockwise direction. The conclusion that this wind-driven circulation would increase the Atlantic-Arctic interchange is of fundamental importance to the question of abruptness, and, in fact, to our whole argument. This critical point was carefully discussed with many competent authorities, before and after publication, and no objection to it has been raised.

The reply to Livingstone's comment that some of our important references (actually 7 out of 60) are to personal communications and to articles in press is that we are working with new information. His statement that we contradict the prevailing opinion of geologists regarding the source of nourishment and regions of maximum thickness is correct. We consider that their opinion is based on older data and on the assumption that nourishment came from the south, and that it is being modified on the basis of recently published and "unpublished" references.

Our interpretation of the ice divide well to the north of that previously inferred has been justified by the data shown on the Glacial Map of Canada, 1958 (*1*). The ice divide shown there, based on glacial movement indicators, is supported by the evidence for extensive postglacial uplift. Objections to the use of ice movement indicators to explain any but the "last gasp" of glacial movement can be justified on the basis of topography in small or isolated regions. But the striking uniformity of such movement shown on J. T. Wilson's map in Flint (*2*) and on the Glacial Map of Canada is on a continent-wide basis and cannot be unrelated to the position of the ice divide of the glacial stage. When these trends are so consistent it seems impossible to conclude that the late movements were significantly different from earlier ones, despite the evidence of position of erratics relative to source areas.

In his criticism of our interpretation of postglacial rebound, Livingstone gives only a portion of our argument. We used Daly's map in Charlesworth (*3*) as a reference for the well-documented data on postglacial rebound in the Great Lakes area. We used more recent and more reliable evidence quoted on the same page in Charlesworth (and since published on the Glacial Map of Canada) as the reference for uplift around Hudson Bay. It is the combination of these data, rather than the assumption of a northern divide, that justifies extrapolation of uplift from the Great Lakes through Hudson Bay and the Arctic islands.

From the elevated beaches shown in northern Canada (*1*) and the Arctic islands, and from other sources, it appears that the areas considered to have been unglaciated are diminishing.

Furthermore, the presence of unglaciated areas marginal to the Arctic Ocean does not in itself constitute an objection to the theory. As we pointed out, the meteorological model explaining precipitation in the North is based on the existence of a second polar frontal zone comparable to that in the middle latitudes at present. Precipitation is produced by storms that follow well-defined mean paths along this present middle-latitude belt. The paths are controlled by factors of topography, the upper-air jet stream, and others, less well understood. Thus, precipitation would not be expected in a uniform belt surrounding the Arctic Ocean, but rather in areas affected by the principal storm tracks of the Arctic polar front. Regions that were not strongly affected by precipitation could maintain an unglaciated condition from the absorption of insolation by the ground surface, in the same manner that ice-free areas are maintained in Antarctica and Greenland at present.

Livingstone's pollen studies which indicate that the temperature changes in polar latitudes are parallel and synchronous with those in temperate latitudes appear confined to postglacial time and are hence irrelevant to our thesis.

Livingstone's statement that evidence of pole shift from paleomagnetic studies "is not pertinent unless it can be shown that the magnetic and mechanical poles shift together" ignores the basis of all modern theories of the origin of the earth's magnetic field (*4*).

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3. J. K. Charlesworth, *The Quaternary Era* (Arnold, London, 1957), vol. 2, p. 1321.
4. This report is Lamont Geological Observatory Contribution No. 331. *Erratum*: In part II of our the [*Science* 127, 1159 (1958)], line 13 of the section "Pluvial Stages" (p. 1161) should have read: ". . . and the desert(s) of Central Asia and Australia, the (African) Kalahari, . . ."

3 November 1958

Uranyl-Ion Exchange Resin Reaction and Demineralization

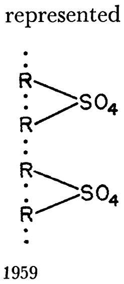
Abstract. Complex ion formation on an ion exchange resin with one ion of an electrolyte results in the release of exchange sites which are then available for the sorption of the remaining ion. The result is a demineralization with but one ion exchange resin. The exchange capacity of the resin is limited by the nature of the complex formed.

Complex ion formation directly at the exchange sites of an ion-exchange resin in a specific form is common (*1*). Thus, an anionic ion-exchange resin in the sulfate form will add on uranyl ions in the formation of a uranyl sulfate complex, $[UO_2(SO_4)_n]^{2-2n}$, a representative type being $[UO_2(SO_4)_2]^{--}$(*2*). If R represents an exchange site of an anionic ion-exchange resin—for example, IRA-400 or Nalcite SAR—this reaction may be formulated as

$$R(SO_4) + UO_2^{2+} \rightarrow R[UO_2(SO_4)_2]^{--}$$

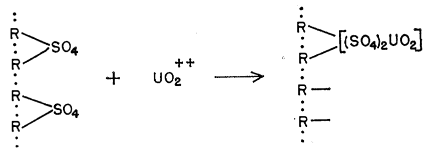
The uranyl sulfate complex, like the SO_4^{--}, is doubly negatively charged and so remains sorbed by the anionic resin.

Such complex ion formation has implications in water demineralization. The sulfate form of an anionic ion exchange resin may be represented as follows:

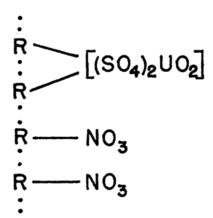


where $\cdot\cdot R \cdot\cdot R \cdot\cdot$ indicates a portion of the ion-exchange resin matrix and the sulfate radicals are shown attached to the active centers throughout the resin.

If this resin is treated with a solution of uranyl nitrate, for example, reaction of the uranyl ions to form the uranyl sulfate complex may proceed thus:



This results in the release of two exchange sites in the resin matrix. These are now available for the sorption of the nitrate ions of the uranyl nitrate, resulting in



Whatever the specific formula of the uranyl sulfate complex may be, two exchange sites will always be left after complexing to sorb the nitrate ion or other anion of the initial uranyl compound. Thus, a demineralization has been accomplished with only a single ion-exchange resin of the anionic type.

Five grams (dry weight) of IRA-400(SO_4), with a total exchange capacity of 15 milliequivalents (meq) were shaken with 30 ml of 1-percent uranyl nitrate solution, 10.0 g of $UO_2(NO_3)_2 \cdot 6\ H_2O$ per liter. This solution is 0.040*N* in uranyl ion; 30 ml of it would contain 1.2 meq or approximately 8 percent of the total ion-exchange resin capacity. After shaking with the resin, the supernatant solution was filtered. Separate portions of the filtrate were tested for uranyl ion by the addition of 0.25*M* potassium ferrocyanide, and for nitrate ion, by the addition of $FeSO_4$ and concentrated H_2SO_4 (brown ring test). Both tests were negative, indicating that the sulfate form of the IRA-400 resin had sorbed both the positive uranyl ion and the negative sulfate ion.

Upon the assumption that the ion $[UO_2(SO_4)_2]^{--}$ is formed, only half the total ion-exchange resin capacity is available for sorption of uranyl ions. The other half is used in sorbing the nitrate ions. If the uranyl ion were to form a complex ion $[UO_2(SO_4)_3]^{4-}$, only one-third the total resin exchange capacity would be available for the sorption of uranyl ions. For the formation of an ion $[UO_2(SO_4)_n]^{2-2n}$, the fraction of the resin exchange capacity available for uranyl ion sorption would be $1/n$.

With a complex ion of the type $[M_a(X)_b]^{az_1-bz_2}$, where the valence of the metal M is z_1 and that of the nonmetal X is z_2, the fraction of the ion-exchange resin capacity available for the sorption of the metallic ion, in terms of equivalents, is az_1/bz_2.

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22 August 1958

Histology of Mammoth Bone

Abstract. Compact bone from a frozen Alaskan mammoth was examined histologically and chemically to determine whether there had been any detectable alterations since the death of the animal. Histological sections closely resembled similar specimens from modern elephants. Total nitrogen and acid-extractable carbonate were at levels to be expected in fresh bone.

For the chemical investigation of archeological bone, a control is usually provided in the form of a fresh animal, or human, bone. A possible substitute worth considering would be bone which is old but which, nevertheless, has not undergone appreciable alteration. For this purpose it is desirable to investigate the characteristics of "glacier-preserved" bone, since in this case there has, presumably, been little if any organic decomposition or interchange of substance between the bone and the surrounding environmental matrix.

The American Museum of Natural History in New York kindly supplied us with a fragment of compact bone from the mammoth skeleton discovered in 1907 at Elephant Point in Eschscholtz Bay, Alaska, by L. S. Quackenbush. Quackenbush made it very clear that the mammoth was embedded not in masses of pure ice, but in frozen silt distributed between ice layers in so-called "ice cliffs" (*1*). The explanation offered by Quackenbush for the fine preservation of hair, wool, tendons, and even some of the soft tissues is that a flood-plain sediment was frozen soon after burial of the mammoth and remained at a mean temperature below 28°F, thus causing the deposit to become progressively and permanently solidified. Quackenbush refers the remains of this particular mammoth to the Pleistocene period. A minimum of several thousand years since the bones were deposited must be conceded.

The fragment, roughly, 4 by 2 by 2