believe, get an up-stream ridge. The true situation should show aspects of both an up-stream ridge and a down-stream valley. Have you considered this for the hydrologic potential? It would seem that a ridge would force water to diverge around the island.

I. M. WHILLANS: You are correct about the importance of the direction of integration for the first iteration. The model is intended only to provide a rough description of ice and water movement. Implicit in my approach are the assumptions that Kelleys Island does not affect the general ice flow in a major way and the ice flow does not "see" Kelleys Island before it comes to it. The effects of longitudinal stress gradients are comparatively small and I think that the assumptions are fair. Certainly the resulting ice-sheet form is in qualitative agreement with what we observe on glaciers today. An up-stream ridge due to the longitudinal stress gradients would cause water to be diverted somewhat, but this water could subsequently be collected in the lee of the obstruction as I have shown.

S. R. MORAN: The solution mechanism proposed appears to require very long travel of water over sediment and rock containing $CaCO_3$. Then in a few kilometres across Kelleys Island it dissolves abundant carbonate. Have you considered the physical (chemical) probability of the efficacy of the proposed solution mechanism?

WHILLANS: In view of the wide range in Ca^{++} concentration reported in the literature, such a calculation would not affect the argument. Even with solution rates of 1/10 or 1/100 of what I used, subglacial water could have dissolved that much limestone.

G. S. BOULTON: You suggest a water velocity in the grooves of 6 m s^{-1}. How does water travelling so rapidly fail to transport boulders, which are so abundant in the area, and fail to produce forms typical of boulder transport in fast-moving glacier streams?

Why do you rule out glacial abrasion, when most of the features you describe are so typical of glacial abrasion by "streamed" basal debris? Why are signs of limestone solution so singularly lacking?

The water-flow theory which you have used to assess the water discharge around Kelleys Island is appropriate to a glacier resting on a stable impermeable bed. Are not water-flow patterns in the Lake Erie area likely to have been largely controlled by the changing hydrogeology of the substratum.

WHILLANS: I find that erosion by solution is sufficient. Other mechanisms can also operate, and I invoke glacial abrasion to produce the striae. Perhaps this abrasion smoothed the solution pits.

The hydro-geology of the substrate would affect water flow. My concern here is to calculate the water potential field which drives the flow.

THE ICE-ROCK INTERFACE FOR THE CLIMAP 18000 YEARS B.P. AND 120000 YEARS B.P. EXPERIMENTS

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ABSTRACT. Most numerical models of present ice-sheet dynamics predict basal thermal conditions for an assumed geothermal heat flux and measured ice thickness, surface temperature, and snow precipitation. These models are not ideally suited for reconstructing former ice

sheets because what is known for present ice sheets is unknown for former ones, and vice versa. In particular, geothermal heat fluxes are immeasurable at an ice-sheet bed but can be measured after the ice sheet is gone, and the thermal conditions predicted at an ice-sheet bed can be inferred from the glacial-geological–topographic record after the ice sheet is gone. The Maine CLIMAP ice-sheet reconstruction model uses these inferred basal thermal conditions to compute ice thicknesses from basal shear stresses.

Basal shear stress is assumed to reflect the degree of ice–bed coupling which, in turn, is assumed to reflect the amount and distribution of basal water under the ice sheet. Under the ice-sheet interior, basal water exists in a thin film of constant thickness covering the low places on the bed. This film expands for a melting bed and contracts for a freezing bed. Along the ice-sheet margin, basal water exists in narrow channels of varying thickness corresponding to troughs on the bed. These water channels become deeper for a melting bed and shallower for a freezing bed. In areas covered by the Laurentide and Scandinavian ice sheets, myriads of interconnected lakes in regions of greatest postglacial rebound are interpreted as evidence suggesting the interior basal water distribution, whereas eskers pointed toward terminal moraines and troughs across continental shelves are interpreted as evidence suggesting the basal water distribution toward the margins. Continental-shelf troughs were assumed to correspond to former ice streams, by analogy with observations in Greenland and Antarctica.

Three modes of glacial erosion are considered to be responsible for the lakes, eskers, troughs, and associated topography. Quarrying is by a freeze–thaw mechanism which occurs where the melting-point isotherm intersects bedrock, so it is important only for freezing or melting beds because high places on the bed are frozen, low places are melted, and minor basal temperature fluctuations shift the isotherm separating them. Crushing results when rocks at the ice–bed interface are ground against each other and the bed by glacial sliding, so it occurs where the bed is melted and is most important when the entire bed is melted. Abrasion of bedrock occurs when rock cutting tools imbedded in the ice at the ice–rock interface are moved across the interface by glacial sliding, so it is also most important when the entire bed is melted. If basal melting continued after the entire bed is melted, abrasion-rates drop because the basal water layer thickens and drowns bedrock projections otherwise subjected to abrasion. Basal freezing reduces both crushing and abrasion-rates by coating quarried rocks with a sheath of relatively soft ice and transporting them upward from the ice–rock interface.

An initially flat subglacial topography will develop depressions where glacial erosion is greatest and deposition is least, and ridges where the opposite conditions prevail. We interpret the central depressions represented today by Hudson Bay and the Gulf of Bothnia as caused by erosion on a melting bed under the Laurentide and Scandinavian ice sheets, respectively. The arc of lakes, gulfs, and shallow seas surrounding these depressions are interpreted as resulting from a freezing bed under the former ice sheets. The present watershed separating the depressions from the arcs marks the approximate former basal equilibrium line where the bed was melted. The Canadian and Baltic continental shields beyond these arcs are blanketed by material eroded from within the arcs, and represent areas having a frozen bed where evidence for abrasion is missing and a second zone having a melting bed where evidence for abrasion is present. This basic pattern was assumed to be imprinted on the bed during the steady-state period of maximum ice-sheet extent, and maintained in varying degrees during growth and shrinkage of these ice sheets.

DISCUSSION

G. S. Boulton: Even if one accepts your ideas of what the erosion process beneath glaciers might be, your analysis involves an assumption which is quite hair-raising in its implications. You assume that the principal geomorphic features of the lands glaciated during the Wisconsin were all produced during the Wisconsin maximum: no effective erosion in pre-Wisconsin glacial periods and none during the build-up and decay phases, as different erosional zones moved over the landscape. There may also have been a great divergence from a steady state during rapid build-up and decay. Does not the uncertainty of these assumptions make the basal thermal boundary conditions you infer very insecure?

If one compares the frequency of lakes over the Laurentide Shield with the geology of the shield, there is a very good match. Does not this suggest that bedrock variables are more important in controlling erosion than glacier variables?

T. J. Hughes: The erosion–deposition zones beneath late Wisconsin ice sheets are assumed to have also existed during all previous maxima of Pleistocene ice sheets. Oxygen-isotope data from ocean cores suggest that ice-age conditions existed during 90% of the Pleistocene, in about 100 000 year cycles with 10 000 year interglacials. Pleistocene ice sheets needed only about 15 000 years to grow and 10 000 years to decay. This leaves 65 000 years of nearly steady-state conditions, when the dominant erosional-depositional imprint on the landscape would be made. This steady-state condition allows perturbations of a few hundred kilometres along the ice-sheet margins. The perturbations would involve ice streams and ice lobes.

For the Laurentide ice sheet, I expect lakes beneath Hudson Bay (this is the inner part of my central melting zone) and lakes just beyond the Precambrian Canadian Shield (this is the outer part of my freezing zone). I only predict the existence of lakes in these places. Their actual number, size, and distribution would depend on such things as geothermal heat flux, rock types, pre-existing subglacial topography, etc. These things are not specifically part of my model but could easily be incorporated into it.

D. J. Drewry: In your model you show a condition of basal freezing beneath the central dome of the ice sheet. This is surely where we would expect melting, as confirmed for the current Antarctic ice sheet by sub-ice lakes. In addition you apparently ignore the effects of spatial variations in geothermal heat which must alter the size and distribution of melting/refreezing zones.

Hughes: We have a condition of basal melting, not freezing, beneath the central domes of our ice sheets. However, the area of the melting zone consists of frozen and melted patches, with the melted patches expanding in number and size from the dome, until the whole bed is melted at the basal equilibrium line separating the inner melting zone from the freezing zone beyond it. The initial melted patches formed nearest the central dome may well have been shallow lakes, because I think glacial erosion was concentrated in those melted patches. We ignore spatial variations in geothermal heat because we deduce the distribution of melting, melted, freezing, and frozen basal zones from the glacial geology and topography created by Pleistocene ice sheets. However, David Sugden has used the geothermal heat distribution to reconstruct the maximum Laurentide Pleistocene ice sheet, and he obtains melting, melted, freezing, and frozen basal zone that are very similar to ours.

A. Dreimanis: As the Laurentide ice sheet was a dynamic ice body, its zones of glacial outflow changed during its life, even around the 18 000±4 000 B.P. time interval. Therefore the areas of outflow were at times quite close to the marginal zone of the ice sheet, e.g. in the southern half of Lake Huron and in the Lake Ontario basin during the latter part of that time interval. Therefore, the regional picture as proposed in your model was probably more complex, constantly changing with time and areally. Similar shifts of the areas of glacial outflow and the changing activities of various lobes have also been reported from the Scandinavian–Baltic ice sheet.

Hughes: Our reconstructions are based on the assumption that the large-scale topographic features covered for most of the Pleistocene by the Laurentide ice sheet were a result of erosion–deposition processes related to a central melting zone extending to about the Hudson Bay watershed, surrounded by a freezing zone extending to about the edge of the Canadian crystalline shield, surrounded by a melting zone extending to the ice-sheet margin in the south and a frozen zone extending to the ice-sheet margin in the north, but crossed by ice streams. If these zones remained relatively stable throughout most of an ice age, the characteristic subglacial topography for each zone was a result of the approximately steady-state erosion and deposition processes I described. Our ice-sheet reconstructions are not significantly changed if I am wrong in this assumption, because the sequence, number, and widths of freezing and melting zones have very little effect on the ice-sheet elevation profiles we calculate along flow lines.

W. Shilts: You have proposed a model for the Laurentide ice sheet that requires a single large ice "dome" centred on Hudson Bay. Our data on dispersal of distinctive rock types on the west side of the Bay (based on examination of over 7 000 till samples collected between the Manitoba border and Chesterfield Inlet, an area of over 100 000 km^2) indicate that ice flow was sustained from the general region of the Keewatin ice divide southward or westward *into* the Bay for a considerable period of time—i.e. probably throughout the period of Wisconsin ice cover in Keewatin. Furthermore, *no* fragments of the Paleozoic rocks which underlie the Bay to within a few kilometres of its western shore have ever been found on land, indicating that ice *never* flowed out of the Bay at this latitude. This is in contrast to the area south of Churchill where abundant Paleozoic erratics have been found in all till units in more than 50 deep bore holes at least 100 km west or south-west of the nearest Paleozoic outcrops. How do you reconcile your simple, "single dome" model with these data? I visualize the Bay as the locus of a depression or saddle in the ice sheet, receiving flow from two or more centres on land to the east and west draining through Hudson Strait by means of a major ice stream.

Hughes: Our minimum Laurentide ice sheet reconstruction should fit your interpretation if the glacial geological features you describe were formed during the long steady-state stage of the ice sheet. Our maximum Laurentide ice sheet reconstruction does not violate your field observations if the steady-state bed was frozen over Hudson Bay and if all glacial geological features you describe resulted from the disintegration stage. Raised beaches showing that isostatic rebound was greatest over Hudson Bay are the best evidence that a single Laurentide steady-state dome was located there. Your glacial geological evidence showing no transport of material from Hudson Bay toward Keewatin proves that the bed must have been frozen beneath this steady-state dome. Our maximum reconstruction has a melting zone for flowlines from Hudson Bay toward Keewatin, and the bed is completely frozen beneath the dome and completely thawed at the zone boundary. Steady-state Laurentide ice will be both clean and frozen to the bed in the area you studied, using conditions in our maximum reconstruction. Basal sliding-rates from Keewatin into Hudson Bay exceeding 1 km a^{-1} are quite acceptable during the disintegration stage, provided that disintegration was caused by a calving bay that migrated up the surging Hudson Strait ice stream and carved out Hudson Bay in the manner described by Hughes and others (1977). In this case all of the glacial geological features you describe could have occurred in less than 300 years. Both our minimum and maximum Laurentide reconstructions are compatible with your field observations.

REFERENCE

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