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Commentaires

REPLY TO COMMENTS BY J.T. ANDREWS ON "CONFIGURATION AND DYNAMICS OF THE LAURENTIDE ICE SHEET DURING THE LATE WISCONSIN MAXIMUM"

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Dr. Andrews is quite right in drawing attention to the important contribution of ANDREWS and FALCONER (1969) and its relevancy to the discussion of the geometry of the Laurentide Ice Sheet in the Hudson Bay area. Our failure to refer to it was simply an oversight.

In his first point Dr. Andrews suggests two possibilities to explain the measured ice flow directions on the Ottawa Islands and for the sake of complete discussion we can add a third. These are: (1) "The major (late Wisconsin maximum?) ice flow across the islands was *from* the southwest and suggests some sort of dispersal center southwest of the Ottawa Islands". (2) The northeastward flow "might reflect a deflected flow from ice flowing off the Ungava Peninsula and being forced northward by Keewatin ice (see SHILTS, 1980)". (3) The flow pattern could simply reflect the passage of Keewatin ice across the islands in the northeasterly direction rather than deflected Labrador (Ungava) ice.

The first possibility is the interpretation we naturally prefer since the presence of a dispersal centre to the southwest of the Ottawa Islands would argue for the existence of a Hudson Dome as proposed in our model. This interpretation also has the advantage of simplicity. Three points concerning the last two possibilities are nevertheless worth making:

(1) The reference by Dr. Andrews to SHILTS (1980) should not be taken, and was not intended, as indicating that Shilts invoked deflection of Labrador ice by Keewatin ice to explain the Ottawa Islands ice flow data. In fact Shilts does not attempt to explain the Ottawa Islands data and his conclusions run contrary to those expressed above by Dr. Andrews. Instead of a northeastward flow at the maximum, Shilts showed Labrador ice flowing well to the west of the Ottawa Islands and reasoned as follows: The fact "that erratic transport occurred along specific flow lines for such long periods of time seems to be incompatible with the concept that there could have been any prolonged period of glacial flow other than that depicted on Fig. 3" (SHILTS, 1980, p. 5); that is, that there could have been no prolonged period when ice flowed northeastward, or in any direction other than westward, across the Ottawa Islands. The "long period of time" referred to is "of the order of several tens of thousands of years" (SHILTS, 1980, p. 5).

(2) If Keewatin ice either crossed the Ottawa Islands in a northeasterly direction or approached the islands closely enough to deflect the flow of Labrador ice to the northeast, then a dichotomy is created; we then have Keewatin ice flowing eastward across nearly the entire width of the northern half of Hudson Bay (to explain the Ottawa Islands data without invoking a Hudson Dome), while Labrador ice, which failed to penetrate far into the northern half of the Bay, flows across the entire width of the southern half of Hudson Bay and onward to Lake Winnipeg (to explain the southwestward dispersal from the Hudson Bay Lowlands without invoking a Hudson Dome). If both flows persisted during the last glacial maximum, especially if both persisted for "several tens of thousands of years", then the major vertical zone of shear bisecting Hudson Bay east to west, begs a physical explanation.

(3) If Keewatin ice crossed the Ottawa Islands or approached closely enough to deflect Labrador ice along a northeasterly path, the zone of coalescence of Labrador and Keewatin ice must have been some 300 km east of where SHILTS, (1980, Fig. 3) places it (see also DYKE *et al.*, 1982, Fig. 3). However, this eastwardly displaced zone of coalescence would not account for the contrasting erratic suites on Coasts and Mansel islands (red erratics from Keewatin present on Coats Island but not on Mansel Island) as well as does the existing models of either SHILTS (1980) or DYKE *et al.* (1982).

Dr. Andrews' second point, that the Tyrrell Sea penetrated Hudson Bay along a corridor located immediately west of the Ottawa Islands (ANDREWS and FALCONER, 1969) can be accomodated in our model by shifting the appropriate symbol westward by one-half its width.

Dr. Andrews has suggested that discussion on the form of the Laurentide ice sheet should not become too heated. We fully agree. Our paper is an interpretation based on our own field data and a re-evaluation of previously published information. The map we presented was based on multiple lines of evidence: the position of ice divides, ice flow patterns, drift composition, major dispersal trains, major deglaciation patterns, postglacial isostatic recovery, and free air gravity anomalies. It also avoids the problem of gross asymmetry of ice masses. We recognize that centres of outflow shifted over time, but contend that there are limiting constraints on the amount of shifting that occurred during a single glacial maximum. We welcome further comments, new proposals, and revisions to this model, particularly from the western prairies, where new research may require modification of the concepts, and invite all interested participants to help construct a viable map showing the configuration of the Laurentide Ice Sheet which we feel better accomodates existing data.

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