

Goodchild did not "fall back on imagination alone". His interpretation of sand and gravel as a product of water washing was founded on experience. His method was both experiential and inferential, involving the interpretation of well-documented observation. I find it odd that this method could be dismissed as leading to a dead end.

Boulton employs the technique of interpreting my meaning to make it appear absurd. Such cleverness may win debating points, but it does nothing to promote understanding. Evidence from the direct observation of process is desirable not because, as Boulton implies I suggest, it is an optional extra, but because it is a luxury not always available to geologists. As examples, the theory of continental drift, the flood hypothesis for the Channeled Scabland, and much of our knowledge of the behaviour of deep-sea turbidity currents all originated without the luxury of direct-process observations.

I am under no illusion that reconstructions of the past are real; we are concerned here with validity not truth. This humbling thought applies to any explanation of ancient sediment and land forms, whether we appeal to known or deduced processes. But, since Boulton sees fit to question my sense of reality, I feel free to examine his. He claims that, whereas his concept of subglacial melt-out till (Boulton, 1970) is based on observation, others base theirs on inference alone. Despite this assertion, I find no direct observations in Boulton (1970) on the process he defined as subglacial melt out. On the other hand, he described 2.4 m of till he believed to have been deposited subglacially by the melting of stagnant ice beneath Nordenskiöldbreen. This till is clearly of melt-out origin according to the definition used by those whose thinking he finds flawed:

Melt-out till — till formed by the melting of debris-rich ice that is neither sliding nor deforming internally in the zone of formation (Shaw, 1982, p. 1549).

Boulton (1971) caused confusion by referring to this subglacial till as a type of lodgement till. It appears to me more logical to classify it as subglacial melt-out till, with the consequence that Boulton's argument against the

under-melt principle is also an argument against his own observations. I believe he is wrong on both counts. I find no fault with the conclusions he drew using Goodchild's tradition of inferring process from sedimentary characteristics. The thermal and dynamic regime of Nordenskiöldbreen over the 200 years or so of till deposition cannot possibly be *described* from a synoptic view of the ice/bed material interface near the present glacier margin. Thus, Boulton's claim to objective reality is spurious; like Goodchild's, his conclusions depend on inference.

I find J.G. Goodchild's influence to be of lasting significance and his under-melt hypothesis remains unchallenged. No *evidence* has been presented to the contrary. Despite his claim to a broad view of sedimentology, I believe that Geoffrey Boulton's observations are so restricted in time, geographical extent, and environmental context it is hardly surprising that he recognizes rather limited sedimentological associations for melt-out till.

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- Shaw, J. 1982. Melt-out till in the Edmonton area, Alberta, Canada. *Can. J. Earth Sci.*, 19(8), 1548-1569.

ERRATUM

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1. 51 *should read* "D.A. Hodgson, Jean-Serge Vincent, Lou King, Gordon"
1. 58 *should read* "Vladimir Kotlyakov and Mikhail Grosswald of the Institute of"