

ablation data from Greenland (Braithwaite and Olesen, 1985, in press). However, we have recently tested a simple energy-balance model using turbulent-flux equations from Ambach (1986) and long-wave radiation equations from Ohmura (1981). Ablation calculations by the model are surprisingly accurate and will be described in a future paper.

*Grønlands Geologiske Undersøgelse*, ROGER J. BRAITHWAITE  
Øster Voldgade 10,  
DK-1350 København K,  
Denmark

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SIR,

*The influence of J.G. Goodchild*

John Shaw (1988) is correct in assuming that, unlike him, I do not regard J.G. Goodchild as one of the "grandfathers" of glacial geology, but I certainly do not deny importance to the study of land forms and sediments in inferring past processes.

Goodchild's (1875) descriptions of the glacial sediments of the Vale of Eden are excellent for his time, and are even now a useful guide which helps us in interpreting the origin of these sediments. However, I believe that the inferences which he drew from them are largely incorrect, primarily because of his lack of any real knowledge of actual glacial environments and the consequent need to fall back on imagination alone as the explanatory tool. Though Goodchild cannot be blamed for this lack in 1875, there are many who follow his tradition and who have no excuse to ignore the knowledge of actual physical processes, which has built up since then, in constructing their hypotheses. I fear that the confusion between the real and the hypothetical may be illustrated by John Shaw's comment that the quote he gives from Goodchild represents an "accurate description" of the formation of melt-out till. It is *not* a description; it is an imaginative inference, a distinction which is often forgotten by those who do not moderate their inferences from ancient sediments by studies of modern processes. I adhere to the view that, if geological features can be explained equally well by processes which are known to occur and hypothetical processes, the former should be preferred.

I would argue that Goodchild's view of "under-melting", also held by Carruthers (1953) and many modern glacial geologists, including Shaw, as a widespread process giving rise to thick melt-out tills deposited beneath stagnant ice, is seriously flawed. I would argue that it is thermodynamically improbable, that it requires either excessive erosion rates to load the lower parts of a glacier with sufficient debris or an inexplicable late-stage change in regime, and indeed that the observations which have been used to justify the hypothesis can be explained by recourse to known rather than hypothetical processes. When I originally coined the term *melt-out till* (Boulton, 1970), it was observed and inferred to be a much more limited phenomenon and different in its sedimentological associations than Shaw believes it to be.

I take a broader view of sedimentology than John Shaw appears to, when he writes that "direct observation of processes is desirable", as if it is an optional extra. I regard good sedimentology as the integrated study of processes and products on all scales. It is commonly found that the influence of origin from sedimentary product alone is ambiguous, and I would plead with those glacial geologists who do not already do so to take a more holistic view.

*Grant Institute of Geology,*  
*University of Edinburgh,*  
*Edinburgh EH9 3JW,*  
*Scotland*

G.S. BOULTON

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SIR,

*The influence of J.G. Goodchild*

Geoffrey Boulton answers my letter on the contributions of J.G. Goodchild to glacial sedimentology by two thrusts. First, he avers that Goodchild's under-melt interpretation is largely incorrect. Secondly, he alleges that some glacial geologists ignore actual physical processes. I find his first argument to be unsubstantiated and his second unwarrantable.

If Boulton is to demonstrate that Goodchild's hypothesis is wrong, he is obliged to show, by means of evidence, that it is contradicted by either observation or sound theoretical principles. Alternatively, he could show that another hypothesis better explains the original and any subsequent observations on the glacial deposits of the Vale of Eden. He does neither in his reply and we are asked to discard an important hypothesis on the basis of an unsupported belief. In a similar vein, Boulton's dismissal of the under-melt hypothesis, first proposed by Goodchild, is on the basis of opinion not evidence. Let him expound his theoretical reasons for discarding this hypothesis and explain by other means the observations used to support it. There is a world of difference between alleging that something can be done and actually doing it.

I cannot imagine any glacial geologist knowingly ignoring actual physical processes. The actualistic works of Boulton, Lawson, Powell, and others are widely cited in the glacial sedimentology literature. But, when the evidence speaks against known processes, land-form and sediment interpretation requires imaginative inference. Even then,