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SIR,

Estimating glacier melt from bulk-exchange coefficients

There are two ways to study ablation–climate relations. The first is to measure energy exchanges at the glacier surface and the second is to find simple correlations between ablation and selected climate elements. Supporters of the first approach claim that theirs is more physical, while supporters of the second are sure that their approach is more useful. Kuhn (1979) made an admirable step towards reconciling these approaches with his heat-transfer coefficient α, which is physically based and is useful for calculating sensible-heat flux from air temperature. The paper by Hay and Fitzharris (1988) can be similarly welcomed as describing physical models to estimate turbulent-heat fluxes from simple meteorological data, but Ambach (1986) has already done this in greater detail.

The calculation of sensible- and latent-heat fluxes by Ambach (1986) is based upon energy-balance data from the ablation area of the Greenland ice sheet (Ambach, 1963) and from the accumulation area (Ambach, 1977). Although the paper's title refers to nomographs, all assumptions are clearly presented so that the reader can choose between graphical and numerical methods. The sensible-heat flux Q_S for a Prandtl-type boundary layer over a melting glacier surface is given by:

$$Q_S = K_S b v_2 T_2 \quad (1)$$

where b is the average atmospheric pressure at the site in question, v is wind speed, and T is air temperature. The subscript "2" denotes a measuring height of 2 m above the glacier surface. The coefficient K_S depends upon surface-roughness parameters z_{0W} and z_{0T}, referring respectively to wind and temperature profiles. Ambach (1986) gave different K_S values for snow and ice surfaces because of their differing surface roughness.

For a similar assumption of a melting glacier surface, Hay and Fitzharris (1988) expressed their sensible-heat flux Q_H by:

$$Q_H = \rho_a c_p K v_2 T_2 \quad (2)$$

where ρ_a is the density of air, c_p is the specific heat of air at constant pressure (1010 J kg^{-1} deg^{-1}), and K is a dimensionless bulk-exchange coefficient for the surface layer. Equation (2) can be criticized as the site elevation is only implicit in the density ρ_a. This can be overcome by setting $\rho_a = \rho_0(b/b_0)$ where ρ_0 is the density of air (1.29 kg m^{-3}) at standard temperature and pressure, b_0 is standard air pressure (101 300 Pa), and b is the actual air pressure at the site which depends on elevation. Substituting into Equation (2) gives:

$$Q_H = \gamma K b v_2 T_2 \quad (3)$$

where $\gamma = (\rho_0 c_p)/b_0$. The resemblance between Equations (1) and (3) is clear with:

$$K_S = \gamma K. \quad (4)$$

For sensible-heat flux in J m^{-2} s^{-1} units, $K_S = 2.46 \times 10^{-5}$ for the assumptions of Ambach (1986), and $\gamma = 1.29 \times 10^{-2}$. Substitution of these into Equation (4) gives $K = 1.91 \times 10^{-3}$, which is lower than 3.9×10^{-3} found by Hay and Fitzharris for Ivory Glacier, New Zealand. However, in the latter case, the same bulk-exchange coefficients have been assumed for wind, temperature, and humidity profiles, which implies that the same roughness parameters are also assumed for the three quantities. The surface roughness on Ivory Glacier is 0.014 m, which is much greater than the 0.002 m assumed by Ambach (1986) for the wind profile over an ice surface. Substituting this greater roughness for both wind and temperature profiles into the Ambach model gives $K = 6.83 \times 10^{-3}$, which is higher than the value found by Hay and Fitzharris.

The above discussion indicates a factor-of-two agreement between the approaches of Hay and Fitzharris (1988) and Ambach (1986), which is very encouraging and supports the basic concept of a simple relation between sensible-heat flux and the product of temperature and wind speed as expressed by Equations (1) and (3). However, the aerodynamic assumptions by Ambach (1986) seem less restrictive than those of Hay and Fitzharris, and may therefore be more correct. More important, Ambach (1986) took account of the aerodynamic differences of snow and ice surfaces, e.g. the sensible-heat flux to a snow surface is only 70% of the flux to an ice surface with the same temperature and wind conditions. This is definitely needed for any serious attempt to explain the past behaviour of glaciers with high accumulation rates like Ivory Glacier.

My colleague and I have previously stressed the simple relation between ablation and temperature in analysis of our

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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.

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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.

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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,