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CORRESPONDENCE

The Editor,

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SIR, *The coupling between a glacier and its bed*

A topic of current interest in glaciology is the coupling, both mechanical and hydrological, between temperate glacier ice and deformable glacier beds. Engelhardt and others (1978) and Boulton (1979) have reported observations of subglacial rock debris being actively deformed during glacier sliding. Boulton and Jones (1979) proposed a model in which the glacier surface profile is related to the strength and permeability of a subglacial till layer. Jones (1979) has presented a model in which the subglacial sediment is treated as a viscous slurry. He concluded that this layer of sediment would be destabilized by minute amounts of "free" water within it.

There are several conceptual problems with Jones's model. First, the distinction drawn between clay-bearing till and clay-free till is confusing. According to Jones, "If the bed consists of coarse gravel, the melt water percolates freely through the bed, and the bed only becomes unstable if large amounts of water are trapped in it. If, however, the bed consists of fine till containing clay particles, the clay adsorbs the water and tends to immobilize it in the bed, lowering the permeability". This statement needs clarification. If the clay-bearing till were dry, a measurement of permeability using a non-wetting phase as the pore fluid would yield a permeability value k_0. Were the bed saturated with water, there would be swelling of clays due to adsorption of a small fraction of that water, hence reducing and modifying the geometry of the pore space. A measurement now of the water permeability would indeed yield a value $k_1 < k_0$. However, once the bed becomes saturated—the probable situation beneath a temperate glacier—there will be no *additional* adsorption of water onto the clay grains. In the saturated condition, the variables controlling the permeability, whether the till contains clays or not, are the overburden pressure and the pore pressure. The saturated, clay-bearing till will contain paths for through flow of water as surely as will the saturated, coarse gravel bed. Jones's references to "trapped" or "immobilized" water in the bed are ambiguous; after all, there will always be, in the saturated state, a finite amount of pore water. Is the "trapped" water supposed to be somehow distinct from the pore water?

Jones's notion of water becoming "dammed" at the glacier bed is troubling. Jones apparently conceives of a situation in which a steady state is perturbed by an increase in the production rate of melt water. He states, "If melt water is now produced faster than it can percolate through the bed, the surplus 'free' water is dammed at the glacier bed producing a slurry . . .". This statement appears to imply that there is an easily definable "maximum" rate at which fluid may move through the till. This is incorrect.

Pore pressure in a porous, permeable medium is generally described by a simple diffusion equation (see e.g. Brace and others, 1968):

$$\nabla^2 p = \frac{1}{c}\frac{\partial p}{\partial t}$$

where p is the pore pressure and c is the hydraulic diffusivity. Standard references in rock and soil physics (e.g. Brace and others, 1968) show that

$$c = \frac{k}{\mu[(\beta - \beta_s) + \phi(\beta_f - \beta_s)]}$$

where k is permeability, μ is viscosity, ϕ is porosity; β, β_s, and β_f are compressibilities of rock (or soil), mineral grains, and fluid, respectively.

Should fluid pressure at the glacier sole increase due to an increase in the melt-water supply, there will be a diffusive relaxation of that pressure increment. This relaxation will be speeded by the fact that till permeability increases as the pore pressure increases (Boulton and others, 1974). The flux of water **Q** is given by the well-known Darcy's Law (see e.g. Brace and others, 1968):

$$\mathbf{Q} = -\frac{k}{\mu} \nabla p.$$

Q will similarly adjust to the altered boundary conditions at the bed. Damming at the till–ice interface, if it occurs at all, will be a transient phenomenon, disappearing as the fluid pressure and flow rate reach a new steady state.

Other factors may also affect the pore pressure in a subglacial till layer. Certainly, the permeability of the underlying bedrock is important, especially if fracturing is ubiquitous. Furthermore, subglacial channels might tend to drain much of the melt water that would otherwise permeate through the bed, thereby lowering pore pressure in the till. Results of drilling to glacier beds (e.g. Engelhardt, 1978; Hodge, 1979) show that subglacial water pressure is not easily predicted; hence, models which assume a particular configuration for subglacial water ought to be viewed cautiously.

Jones's application of earlier results on the rheology of slurries is also questionable. Jones referenced Roscoe (1952), who reviewed experimental work on the apparent Newtonian viscosity of suspensions of spheres and of dextrose solutions; Roscoe showed that the data could be fitted by an equation of the form

$$\eta = \eta_w (1 - \alpha C)^{-2.5},$$

where η is the apparent viscosity of the suspension or solution, η_w is the viscosity of water, and C is the volume concentration of suspended or dissolved materials. The parameter α is unity for extreme dilution, as shown by Einstein (1906); this is the value adopted by Jones.

It is crucial to point out that Roscoe's curve-fitting is valid only for concentrations C less than about 0.4. At higher concentrations experimental work on suspensions (Frisch and Simha, [[c]1956]) shows non-Newtonian behavior. In addition, Johnson ([[c]1970]) has demonstrated that both clay slurries and natural debris flows are strongly non-Newtonian, exhibiting yield-strength phenomena. A subglacial till will certainly have a concentation of solids C far exceeding 0.4. These observations strongly suggest that not only will subglacial till not behave as a Newtonian fluid, but also that to describe a saturated till as a suspension, as done by Jones, is incorrect. It seems more logical to consider the manner in which adsorbed water and pore pressure affect the physical properties of a dry, compacted till, than to speculate on how the solids affect the viscosity of the interstitial water, which is after all only a minor constituent of the saturated till.

Lastly, it is necessary to examine Jones's statement that "significant deformation of the till occurs when the proportion f of free water at the [till–glacier] interface is about 10^{-6}." (In the present notation, $f = 1 - C$.) This is certainly perplexing, because the porosity of any till certainly exceeds 10^{-6}. Is this "free" water supposed to be somehow independent of the water contained in the pores? If so, what exactly is the character of this "free" water? I can conceive of two fundamentally different configurations for water at the bed: either within the pores of the till layer, or somehow segregated at the till–ice interface. If Jones means the "free" water to be the latter, then what exactly is the meaning of "concentration" of the "free" water? After all, in such a layer, the *only* constituent would be water. On the other hand, if the "free" water is supposed to mean *pore* water, then the "proportion of free water" is simply the porosity ϕ. The saturated till *cannot* contain "surplus" or "free" water somehow distinct from pore water; increasing the pore fluid content of the till simply means that the pore pressure will increase. I suggest that the notion of "free" water used by Jones, and hence any numerical results of his analysis, are without any firm meaning. Any apparent agreement between Jones's predictions of velocity profiles in deforming till and Boulton's (1979, fig. 7) field data is fortuitous.

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SIR, *Short-term irregularities of discharge of glacial melt-water streams*

Sudden falls in the discharge of a glacial melt-water stream, sometimes to less than half the previous discharge, followed by an equally rapid rise, have been recorded from a number of glaciers, including Austre Okstindbreen, Norway (personal communication from W. H. Theakstone) and Gornergletscher, Switzerland, (personal communication from P. L. Comer). They appear on the discharge record as events lasting at most half an hour, and sudden fall and rise is sometimes followed by a small peak before the discharge returns to its natural level. Because these events have not been observed directly, they have most commonly been attributed to malfunctioning of the water-level recording equipment.

Ballantyne and McCann (1980) observed this pattern from an ice marginal stream on Ellesmere Island and, because of the frequency of the events, ascribed them to collapse of the ice margin, which caused temporary blocking of the stream.

The distinctive pattern of such events was seen on the discharge records of the outflow stream of the Pasterzengletscher, Austria, during the summer of 1980 (Fig. 1). The hydrograph shows that the events occurred in clusters, being concentrated in early August and at times of high discharge. One was observed as it occurred by Dr P. Ramspacher on 27 August. At about 16.00 h. the discharge was seen to fall to a very low level and, as the water level subsequently started to rise, the stream became heavily laden with sediment and carried many small ice blocks. No collapse of the ice margin into the stream had occurred. Temporary damming of the stream, therefore, must have been caused by collapse of the subglacial tunnel up-stream of the portal. However, it is unlikely that the collapse occurred very far up-glacier, as the sharp features of the discharge hydrograph were not smoothed. It is probable that the thin ice close to the snout is most commonly subject to collapse, and high discharges appear to precipitate these events.