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CORRESPONDENCE

The Editor,

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SIR, *The role of stress concentration in slab avalanche release: comments on Dr R. A. Sommerfeld's paper*

Dr Sommerfeld has given us some interesting and useful ideas to carry into the field—especially in the area of interpreting fracture patterns at the starting zone of avalanches. In developing his own theory of slab avalanche release, Sommerfeld rejects our theory of basal collapse (Bradley, 1966; Bradley and Bowles, 1967). He concludes that collapse is an unlikely or at best an unimportant mechanism for avalanche release. We take this rejection as an invitation to respond.

We will not debate Sommerfeld's theory. It looks sound enough as a model for wind-slab release and similar avalanches. However, it seems not to explain what has been going on now in the Bridger Range of Montana for the last month and off and on for 11 years of observation. Since late December this season we have had over a dozen large, full-depth avalanches of the classic climax pattern. In a number of different ways these avalanches have been closely associated with snow-pack collapse. While the association does not prove causal connections it at least suggests that the collapsing condition might have *some* importance in the scheme of things. The following summary of this year's field notes explains a little of the nature of the association.

There have been five personal encounters this season with massive snow-pack collapse in which avalanches did *not* follow. In these the snow pack ranged in thickness from 1 to 2.5 m. The fracture system cut across the pack for distances of 50 to 100 m. The fractures showed vertical displacement ranging from a few millimeters to 1.5 cm and were accompanied by a substantial sonic shock. There was no perceptible lateral movement in any of the five. Slope angles ranged from about 30° to nearly horizontal. Two pit studies showed the slab fracture to terminate in the weak zone at the base. The angle of the fracture indicated tensile fracture of the slab without regard to slope angle. (One pit was on the flat, one was on the 30° slope.) Strength-to-load ratios were taken at the weakest zone just above the base of the pack immediately prior to three of the events. These ratios were 1.5, 1.8, 1.7. We view the snow-pack system in these events to be like a broad platform supported by columns. If the pillars are too weak or the platform overloaded in a certain area the pillars beneath fail under compression and the platform fails by tension.

Both in general seasonal timing and in slope orientation (east to north-east facing) the collapses correlated closely with the avalanche events. Two of the five collapse events occurred very close to starting zones. (We agree with Sommerfeld that a collapse at the foot of the slope or out on the flat probably does not cause many avalanches.)

There is still closer association of snow-pack collapse with the avalanches. The starting zones of three of the avalanches were visited shortly after release. In all three there were fractures above the break-away scarps which indicated that this upper portion of the snow pack had dropped vertically about the same time as the avalanche ran but it had not moved laterally. Resistograms taken above the fracture of one of these showed strength/load ratio 1.6 at the weak zone.

This kind of evidence for snow-pack collapse closely associated with some of our biggest and most destructive avalanches we find at least very interesting. We therefore tentatively conclude that whether or not snow pack collapse is *important* probably depends on what you are studying and where you are standing when it lets go.

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SIR, *The role of stress concentration in slab avalanche release: reply to the comments of Dr C. C. Bradley and Dr D. Bowles*

I certainly cannot argue with the careful observations reported by Dr Bradley and Dr Bowles. Our basic disagreement concerns the time sequence of events which lead to snow failure. Any failure criterion would predict the initiating event, so the time sequence is of prime importance. As I understand it Bradley and Bowles model would have a lower layer, which is weak in compression, fail under a compressive load. Then the total load would be thrown onto the upper layers bending them so that they fail in tension. Another possibility would follow this sequence. First a lower layer, in a pack under significant elastic tension would fail in compression in a small spot. The increased gravitational load and the release of elastic energy would cause the basal failure to propagate from the point of initial failure until the release of elastic energy was sufficient to cause tensile failure along a line at some distance from the initial disturbance. A third possibility, consistent with the model which I proposed, is as follows. A snow pack slowly subsides because of mass loss occurring at a layer forming depth hoar. The subsidence causes increased tensile stresses in the upper layers. The snow fails, in tension, at some point on the surface. The failure propagates downward until it reaches the weak layer where it propagates laterally, aided by the increased gravitational load and by the release of elastic energy.

Other models might be postulated which lead to the same result. If some way could be found to determine the relative importance of the different models, we would have a significant advance in our understanding of avalanche release.

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