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Interactive comment on "Modeling snow slab avalanches caused by weak layer failure – Part II: Coupled mixed-mode criterion for skier-triggered anticracks" *by* Philipp L. Rosendahl and Philipp Weißgraeber

Michael Zaiser (Referee)

michael.zaiser@ww.uni-erlangen.de

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The paper deals with the formulation of a failure criterion for collapse-like failure of snow and application to skier triggering of avalanches. It combines a fracture mechanical and a strength-of-materials approach to forumlate a criterion for weak layer failure that does not rely on assuming a pre-existing flaw. Fracture mechanical and strength-of-materials ('snow stability index') approaches to snow failure have both been used in the snow literature, and sometimes in a manner that confounds their respective domains of application. Thus a unification of sorts is an inherently desirable undertaking.

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The authors first discuss contradictions that may arise from an uncritical application of either strength-of-materials or fracture mechanical criteria to situations where those criteria do not apply. A nice example is given by Eqs. (3) and (4) which are particularly instructive as I have encountered very similarly flawed reasoning in a recent manuscript under review for The Cryosphere: Basically, one cannot simply apply an energy argument to an uncracked specimen and deduce a failure stress from it, nor can one invert the argument and convert a failure stress into a fracture toughness by evaluating the overall energy stored within the sample. By contrast, I find the argument surrounding Figure 1 less convincing. Reference is made to Bazant 1984 but, if we adopt Bazant's reasoning I see no reason why the strength of an uncracked specimen as shown in Figure 1 should be size dependent (in fact, Bazant's relation predicts strength to be size independent at small sample sizes whatever the crack length).

The FFM criterion attempts to resolve the well-known conundrum that exists in theories of fracture, namely that the transition from stress-induced damage accumulation (no crack) to the propagation of a critical crack is not well understood. It does so by combining a strength of materials criterion to obtain an upper estimate of the crack length (a crack can at maximum form over a length where the stress is above sigma_c) with a lower estimate (the ensuing crack must be able to propagate). The crack forms if the lower bound falls below the upper one.

Taking the example provided by the authors makes me wonder whether I understand the criterion correctly. Consider the tensile beam of Eqs. (3),(4) of the manuscript. Load this beam at the stress sigma_c such that the stress based criterion for mode I failure is fulfilled over the entire cross section. Finite fracture mechanics seems to indicate that all energy stored in the beam is released if a cross-section spanning crack is formed. If that is true, however, then Eq. (3) entails a critical beam length l $\sim$ (2EG_c/sigma_c^2) below which the beam cannot fail because that energy is insufficient. Now I insert typical values of steel, say KIc = 40 MPa m^1/2, sigma_c = 500 MPa, to obtain with EG_c $\sim$ KIc^2 a critical length in the range of 1 cm. Ehem. Do the

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Discussion paper

authors want to imply that my 1cm tensile sample is too small to break??????? Clearly I must be misunderstanding something.

I have a second objection. Consider again a thin tensile beam with thickness a and length l » a loaded at a stress slightly above sigma_c. Let us now assume the energy criterion is fulfilled: sigma_cˆ2 l > 2 E G_c. So the beam breaks. Now consider the same beam but embedded as surface fiber into a bending beam as in Figure 1. Let the bending moment be such that a region of thickness a from the surface is above the critical stress. In that case, the energy release will be less than for the free standing beam, and it may well be that the crack cannot form since sigma_cˆ2 a < 2 E G_c. However, the stress state in the considered volume is identical in both cases. The only thing which the volume elements (and the microstructure, grains, dislocations, atoms......) in the beam know about the outside world is the local stress acting on them. How do they understand that, in the first case, they should form a crack instantaneously, and in the second case, not?

I kindly request the authors to clarify the above two points.

Once we accept the basic approach, the development of stress and energy based failure criteria looks sensible. Concerning the skier loading, I have a question since the loading model is not very clearly described. I presume the authors assume plane strain conditions in which case F would need to be a load per unit length. On line 11, page 11 there is a mysterious b which seems not defined. As to material parameter choices, I commented on that point in relation to the companion paper.

A minor point: It is noted as an inherent flaw of models that assume subcritical damage accumulation that 'such models would predict avalanche release if only enough skiers ski the same slope in close temporal succession'. While such may not be generic behavior, I cannot see why this should be impossible to happen. I know of several passages in the avalanche literature reporting that several skiers may ski a slope before number X triggers an avalanche, and I have seen it myself happening once.

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