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Interactive comment on “Snow fracture in relation to slab avalanche release: critical state for the onset of crack propagation” *by* J. Gaume et al.

Anonymous Referee #1

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Overview This manuscript addresses a topic that is relevant to snow and avalanche mechanics. It addresses a topic of current interest and debate. Specifically, a model for a layered snow cover consisting of a “weak layer” with a flaw or crack that is overlain with a homogeneous elastic “slab” layer is presented. It addresses conditions for the the critical crack length within the weak layer that will result in its failure and propagation. The primary interest in the topic is the consequence of failure with respect to avalanche initiation. The paper builds on previous work and suitably acknowledges those contributions. Several, but not all, of the earlier contributions utilized a linear elastic fracture mechanics approach. The approach presented in this manuscript uses a discrete element method (DEM), conceptually based on a mechanics of materials or elasticity approach, incorporating the concept of stress concentrations leading to propagation. From this an analytic expression is developed. Perhaps the major contribution of the

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Discussion paper

paper is to add to the discussion the influence of slope angle on weak layer failure. This is in contradiction to a widely accepted notion that the fracture is slope independent. It also attempts to add a more robust inclusion of the independent material properties of the layers. The results are intuitively reasonable. That said I have a number of questions and comments that I feel need to be addressed. A number of these comments are suggested in order more precisely clarify details to relate the physical description to what is calculated in the model. This is important, especially since it provides results that are counter to earlier work. There are some more technical issues that need to be explained or justified. I feel that this is an interesting and relevant paper that I would like to see published if the comments can be adequately addressed. I am presuming that the the issues can readily be resolved and clarified. Assuming that this is the case, the revisions should be a relatively minor effort by the authors. However, I am rating this version as major revision since I feel that the paper needs to be reviewed again prior to acceptance. Detailed comments Line 27 I don't think you mean to say that cracks form below an overload, but that the additional overload in association with a crack may lead to failure. Line 63 The specific fracture energy would have to be between the slab and the rigid weak layer, since the rigid material cannot support elastic potential energy. I think that this term is an ill defined in the original paper. Line 51 suggest . . .which {allowed to solve} the problem. . . to . . .which {allowed solution} to the problem. . . Line 80 – suggest . . .anticrack model, {these} strength of material. . . The strength of materials approach certainly can account for bending. I suggest that you make the change to emphasize that the methods implemented for snow that you reference do not have bending. Line 98 Does soft contact imply that the contacts (bonds) are elastic. Are the grains taken to be rigid in your simulations? Figure 2 You should present labels for ac, λ and lo in caption. Caption Fig 2 τg = gD sin(Ψ) This would be the shear stress at the interface of slab and WL. Thickness of WL is not included. Does this then imply that the stress concentration is at the interface between the slab and WL or is shear stress in WL assumed uniform throughout the thickness? Caption Fig 2 How is the residual stress explicitly defined? Is this residual stress used anywhere in this development?

Line 113 How does this compare with Scapozza, equation 5? Both the slab and WL are taken to be isotropic. The triangular structure may, perhaps in a future iteration, allow for anisotropy for forms such as surface hoar and depth hoar. Figure 3 Perhaps call normal stress the "slope normal stress". The normal stress value, as well as shear, is a function of the coordinate system chosen. Caption You should state that compression is taken as positive. Caption Ψ is labeled in the text as the slope angle. Should this be a critical slope angle? Should this be Ψt instead? Line 115 I'm not clear why you can state that these are satisfactory based on fig 3? While the initial slopes of increasing normal stress are similar, the actual values are quite dissimilar. Normal stresses are actually of opposite sign for the initial slope on the figure? Line 125 "specific fracture energy" is used only in the anticrack model. Correct? "...the penetration resistance using the snow micropenetrometer (SMP) in the weak layer according to Reuter et al." This would not be possible if the layer was actually rigid, as is assumed for the anticrack model. Resistance would be infinite. Eq 1 Ψt is not defined. Line 140 So is K = 1 kPa ? From figure Eq 5 – Is this for the slab only? Or both slab and WL? Line 148 – Is this Poisson's ratio for the slab and the weak layer? Is this used for calculation of the elastic mismatch? Is the elastic mismatch used in your development or merely mentioned as an aside? Line 151 I presume that you mean to relate the "tension in the slab" to imply the slope parallel normal stress due to gravity. Slab bending may also cause a slope parallel tension component as well compression and shear in this same coordinate orientation. Line 152 Is the crack tip calculated to be the thickness of the weak layer or at the bonds between the slab and WL? If it is at the interface, as appears to be implied at some points in the presentation, it would seem that this may differ from the max within the WL. Figure 4 Young's Modulus, E, is a function of density, in equation 5. Figure 4a has the critical crack varying with E with a fixed density of 300 kg m3. Similarly, figure 4b has a fixed E with crack length varying with density. Something seems wrong here? Is this somehow related to the calculation of E described in line 112? Line 163 Is this statement implying that you are considering only the influence of shear stress on the stress concentration? Relating to failure envelope? Again, is this assumed to be at the interface between the layers or within the WL itself? Equation 7 You are examining the case in which "WL has no thickness". But at this point λ (line 171) goes to zero so a/λ goes to infinity. The max shear in the WL then goes to infinity in equation 7? There is a singularity at the crack tip. This is also apparent in the bending component in equation 8. Even for any very thin layer of finite thickness this will be huge. This needs to be discussed and explained. Line 177 – suggest {slope} normal stress. Line 180 – If you assume a rigid weak layer, the elastic properties of the weak layer are irrelevant. It is also not clear why the rigid layer is assumed if the shear stress is independent of these properties. Line 180 I think you need to more explicitly describe the beam boundary conditions assumed. "...according to beam theory" is rather vague. I am also not seeing where this reference (Timoshenko and Goodier, 1970), is dealing with cracks, except for a brief mention relative to Griffith fracture. Can you be more specific? Line 182 – suggest replace {allows} with {provides a means} Line 182 Is it using Dundur's elastic mismatch parameters in the numerical solution? Does this imply that you are calculating the crack to be at the interface between layers? How does scaling with λ instead of D come into play? Line 191 I presume that the residual stress, τr, is not used as the result of this condition. And this paper is not concerned with the sliding following failure of the week layer. Line 211 This is an important result, since in the avalanche community the concept of slope independence has been quite widely accepted of late. Line 215 Here again I think that it should be clearly stated that you are referring to the stress due to the gravity along the slope and independently, the "stress state" as the result of bending. Line 215 Parts of this development seem to imply that the failure would be at the bonds between the slab and the weak layer. I think that what is being calculated should be clearly stated. As an aside, in the physical situation for depth hoar the failure may well be at the lower interface or at the upper and lower. The crystals themselves often appear to remain intact. Line 221 – suggest changing "...accounts for slab bending only..." to "accounts for {stresses due to} slab bending only" Line 223 Should clearly state that you are referring to the shear stress at the crack tip induced by the slope parallel loading of the slab. This results in the shear stress at the interface between the slab and WL. Line 224 ac t goes toward zero as WL thickness goes to zero. Following discussion above. Figure 6 caption – suggest changing "represents" to "represent" Line 227 – suggest changing "decreases" to "decrease". Line 252 – I think that you should state that you are using a mechanics of materials or perhaps an elasticity approach, if that is appropriate. Line 254 Suggest changing "...allows to reconcile shear and collapse..." to "...reconciles the shear and collapse..." Line 321 – suggest changing ...that {skier triggered avalanches are} more likely on steep rather than on flat slopes..." to "...that {avalanche initiation} is more likely on steep rather than on flat slopes..."

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That said I have a number of questions and comments that I feel need to be addressed. A number of these comments are suggested in order more precisely clarify details to relate the physical description to what is calculated in the model. This is important, especially since it provides results that are counter to earlier work. There are some more technical issues that need to be explained or justified.

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Detailed comments

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Figure 2 You should present labels for a_c, λ and l_o in caption.

Caption Fig 2 $\tau_g = \rho g D \sin(\Psi)$ This would be the shear stress at the interface of slab and WL. Thickness of WL is not included. Does this then imply that the stress concentration is at

Fig. 1.

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