Review for TCD

Modeling snow slab avalanches caused by weak layer failure – Part II: Coupled mixed-mode criterion for skier triggered anticracks

by Rosendahl and Weissgraeber

The authors describe a new dry-snow slab avalanche release model. By introducing a Winkler foundation[1] below the slab, the weak layer properties can explicitly be considered (in contrast, for example, to the Heierli model that considers a rigid base below the slab) and the slab displacement field can be described in analytical form.

In part II of their contribution, the authors suggest a coupled mixed-mode criterion of skier triggered anticracks. Whereas classical fracture mechanics assumes a preexisting crack and studies the conditions for crack propagation, the so-called finite fracture mechanics approach, which the authors employ, does not require an initial weakness, but assumes that due to stress overload an initial failure forms provided that sufficient energy is released to form a crack at all. In other words, it is assumed that the stress criterion and the fracture mechanical energy criterion are coupled and need to be satisfied simultaneously.

In general, the authors made an applaudable effort to introduce their model and place it in the context of previous work. I am hesitant in accepting some of their conclusions since they partly reflect some of the assumptions and simplifications inherent to any model. However, if those limitations are properly discussed, I have no principal objections and recommend the paper to be published pending adequate revisions by the authors.

The principles of the model are described in part I. I am not commenting on the first paper – unless reference to it is made in the second part and something is not clear.

1. In the abstract, the authors state that in the limit case of very thick slabs and very steep slopes natural release is obtained. Previous work has shown that natural release cannot be described by a simple stress criterion, even one coupled with a fracture mechanical criterion. Spatial variations in slab and weak layer properties are required to describe natural slab release. Clearly, spatial variations are less decisive for skier-triggering.
2. The concept of finite fracture mechanics assumes that skiers cannot initiate a crack unless sufficient energy is released. Whereas this assumption follows from the model, it is not obvious to me that situations exist where this second conditions is not fulfilled. As far as I understand this means that strength is low, but toughness is high. I am not aware that this scenario is relevant in the case of snow; it seems hypothetical to me.
3. Nevertheless, I agree that in most situations skiers instantaneously induce a macroscopic crack that in most cases is large enough for self-propagation and that no initial weakness is required (as e.g. suggested by Schweizer and Camponovo, 2001) – in contrast to natural release. Natural release is often observed for a load lower than the average stress, which implies that failure starts at locations of below average stability.
4. The authors state on page 7 that the stability of the initial crack is governed by the energy criterion only. Does this mean that the initial finite sized crack Δa automatically fulfills the Griffith criterion? If so, I do not understand how Eqs. 8, 19 and the statement on page 11, line 7 relate. Can you please explain why G_c is part of the energy criterion.

[1] By the way, the Winkler foundation was already introduced by McClung and Borstad (2012) to study avalanche formation.

5. Page 5, line 29: I suggest you introduce a proper reference to part I. In general, I think it is best to make the two contributions self-contained – or merge them.
6. Page 7, line 17: To my understanding, given the continuum mechanical approach, it is best to talk about weak layer failure. Collapse, which I understand as a consequence of failure in a structure that is strong and stiff in compression, but weak in shear, refers to the porous microstructure – not considered in the model.
7. I am not convinced that Eq. 9 represents a suitable strength criterion for the case of snow, a highly anisotropic material.
8. Figure 3: Please more clearly state what kind of experimental data are shown. What means "taken by"? Page 10, lines 1-6: I suggest you consider the strong anisotropy of snow when discussing the failure criteria. I suppose this would change the relative contributions of G_{I} and G_{II} to G.
9. Page 10, line 8-10: both requirements were considered by Gaume and Reuter (2017).
10. Page 12, Table 1: I strongly recommend providing references for the property values presented. For example, the relation between shear, tensile and compressive strength is rather unusual.
11. Page 12, line 7: I suggest using slope angle or incline rather than inclination.
12. Page 12, lines 9-11: It is not clear to me why the critical load in case of the shallow slab on the slope is higher than for the thicker slab. Please explain.
13. Figure 6: As far as I understand the maximum stress at 40 cm depth is always smaller than at 20 cm depth as the additional skier stress decreases with increasing depth. Therefore, I cannot follow the statement that thicker slabs cause larger point loads. Maybe I misunderstand your term critical additional skier load. This should relate to the depth of the weak layer and not to the surface load, since the surface load is a given value, as it is due to a skier. Moreover, I suggest rewording the statement that thicker slabs transfer loads more uniformely. The transfer is always the same.
14. Page 14, line 2 and 6: The stress distribution due to a skier does not depend on the modulus as long as the slab is uniform. Therefore, I cannot follow your argument here. Please explain.
15. Page 18, line 10: To my knowledge, Reuter et al. (2019) did not study the dependence of snow strength properties on strain rate. Some of the studies that explicitly do so include Narita (1983), Schweizer (1998) and Reiweger and Schweizer (2010).
16. Page 18, lines 1-4: I do not understand why the authors state in this context that their approach does not require the assumption of initial flaws in the weak layer; the approach by Gaume and Reuter (2017) does not require this particular assumption either.
17. Page 18, line 10: As mentioned earlier, in my understanding the term collapse refers to the microstructure and is the result of failure; the latter can occur in shear, compression or combined shear and compression. I doubt that one can simply imply from the fact that there is normal deformation, that the failure is compressive.
18. Page 18, lines 13-20: I suggest rewording or partly revisiting this paragraph. For example, the formulation that the critical skier load vanishes in very steep terrain can be misunderstood. As we know by experience triggering is more likely on steeper slopes. In that context, it seems rather counterintuitive that longer cracks are required on steeper slopes.
19. Page 18, line 21: Suggest rewording.
20. Page 18, line 32. Whereas the slab modulus affects the displacement field, the stress due to the skier remains unchanged. Therefore, you have to be careful with using the term bridging that refers to initiation due to skier stress.
21. Page 19, lines 5-15: Whereas I understand that the findings on e.g. crack lengths on slopes results from the model, I doubt whether this specific finding is particular realistic. I suggest revisiting some of the assumptions and discussing them in the light of these results.

22. Page 19, lines 31-24: These section needs to be revisited. If a crack in the flat propagates it is not unusual that a fracture through the slab occurs somewhere. This has frequently been observed. The term shooting crack is used for the situation when cracks propagate. Shooting cracks are best related with avalanche release (Schweizer, 2010).
23. Page 20-21, Conclusions: I suggest you refer also to the limitations of the model and provide an outlook on possible improvements.

References

Gaume, J. and Reuter, B., 2017. Assessing snow instability in skier-triggered snow slab avalanches by combining failure initiation and crack propagation. Cold Reg. Sci. Technol., 144: 6-15.

McClung, D.M. and Borstad, C.P., 2012. Deformation and energy of dry snow slabs prior to fracture propagation. J. Glaciol., 58(209): 553-564.

Narita, H., 1983. An experimental study on tensile fracture of snow. Contrib. Inst. Low Temp. Sci., A32: 1-37.

Reiweger, I. and Schweizer, J., 2010. Failure of a layer of buried surface hoar. Geophys. Res. Lett., 37: L24501.

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Schweizer, J., 2010. Predicting the avalanche danger level from field observations, Proceedings ISSW 2010. International Snow Science Workshop, Lake Tahoe CA, U.S.A., 17-22 October 2010, pp. 162-165.

Schweizer, J. and Camponovo, C., 2001. The skier's zone of influence in triggering slab avalanches. Ann. Glaciol., 32: 314–320.

3 July 2019/Sz