The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-87-AC2, 2019

The Cryosphere
Discussions
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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

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Received and published: 8 August 2019

The reviewer has pointed out several points in the paper where given explanations were insufficient, especially regarding the concept of finite fracture mechanics and the resulting parameter dependences.

We have addressed all points below and improved the manuscript to make the raised points more clear. Since the failure criterion is nonlocal, the results are given in terms of critical (outer) loading on the snow pack. We further detail this and the difference to

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a critical local weak layer stress. Further, we now better explain the concept of finite crack lengths and are more specific in the theoretical background of finite cracks and their distinction from critical crack lengths as obtained in, e.g., PSTs.

We thank Dr. Schweizer for his meticulous review. We have addressed all points in detail and changed the manuscript accordingly.

Reviewer comments

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.

When mentioning "natural release" in the abstract, it is our intention to characterize the situation where, according to our model, no additional external (skier) load can be applied under the given conditions. This should not imply that our model provides a prediction of natural release. On the contrary, it is supposed to indicate the limits of the present model. Because this requires some clarification, we removed the statement

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from the abstract.

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.

Energy release available for crack initiation and crack growth is a structural property. This is evident in the the Griffith criterion $\mathcal{G}_c = \mathcal{G} = -\mathrm{d}\Pi/\mathrm{d}A$, where Π is the total potential energy of the system. That is, the criterion evaluates a global energy balance with respect to crack extension. Hence, toughness on the structural level, i.e., the structural resistance against crack nucleation and crack growth, does not only originate from the material's fracture toughness but also from the amount of energy stored in the structure.

This gives rise to size effects. Small structures store only a small amount of energy so that the energy condition becomes important even when structures without initial flaws are considered. An example is given in Figure 1 in our manuscript. To some extent, the situation resembles a slab that bends owing to skier-loading. The presence of a size effect is evident in the considered experimental data. Finite fracture mechanics explains this size effect without the necessity of assuming large toughness to strength ratios (Weissgraeber et al. [1]).

The comprehensive experimental work of Sigrist [2] reports size effects for both fracture toughness and strength measurements in three-point bending tests of snow beams. This is compelling evidence that both stress and energy are important in the fracture process of snow. The energy balance (first law of thermodynamics) is a fundamental principle. All processes (in our case the transition from the uncracked to the cracked state) must obey this principle.

3. Nevertheless, I agree that in most situations skiers instantaneously induce a macro-

scopic 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.

We agree and do not intend to imply that natural release is covered by our model (see response to point 1). The present model specifically addresses the situation of skier-triggered weak layer failure. We have clarified this in the manuscript.

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 a 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 Gc is part of the energy criterion.

Crack nucleation (no crack $\rightarrow$ finite crack) requires $\overline{\mathcal{G}} \geq \mathcal{G}_c$ (as a necessary but not sufficient condition) (Leguillon [3]). Crack propagation, however, requires $\mathcal{G} = \mathcal{G}_c$ (Broberg [4]). Since the relation between incremental and differential energy release rate is

$$\mathcal{G} = \overline{\mathcal{G}} + a\frac{\partial \overline{\mathcal{G}}}{\partial a}$$

and the present situation is a positive geometry (Weissgraeber et al. [5]), i.e., $\partial\overline{\mathcal{G}}/\partial a > 0$ holds, the differential release rate of initiated cracks will always exceed the fracture toughness. So yes, the initial crack is generally unstable.

However, this is only true in the vicinity of the introduced outer load by the skier. If the crack grows out of the region influenced by skier-loading, the energy release rate decreases (it is now governed by the slab weight only) and the crack can stop (governed by Griffith's criterion). The assessment of the stability of the initiated crack and its ability to propagate in the region that is not affected by the external skier load has to be studied in further research. To do so we could extend our modeling approach with a touchdown condition that eventually leads to an upper bound of the energy release rate.

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.

We have introduced a proper reference to allow for a clear link between the two papers. The two parts are closely interconnected but apart from the definitions of the basic fracture mechanics quantities, they are self-contained.

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.

The denomination "collapse" can be understood differently. Let us develop two thoughts on this topic:

i) Continuum mechanics typically considers boundary-value problems. That is, a problem with a given fixed boundary that cannot change without changing the problem statement. Within the considered boundaries, continuum mechanical constitutive equations describe the material response and do not account for material separation. Hence, they would allow for arbitrarily high loads and do not consider crack nucleation or crack propagation as this creates new boundaries. Yet, we do treat fracture problems using continuum mechanics without the need to consider the microscale at which atomic bonds break (Anderson [6]). This is a very useful approach that can be readily transferred to compressive failure. That is, collapse on the macroscale may be treated just like tensile failure without the consideration of the microstructure.

ii) If we were to consider the microstructure. Collapse can very well be the consequence of pure unidirectional normal loading. Engineering structure such as rods, beams, plates and shells can buckle. They lose structural stability and exhibit a sudden sideways deflection at a critical (pure) compressive load (Gross et al. [7]). For a brittle and low-strength material like snow (ice), buckling can be expected to be accompanied by structural failure. Of course, superimposed shear loading is likely to facilitate structural failure. However, collapse does not require shear.

The strong anisotropy of weak layers concerning normal and shear strength is of course a direct consequence of the porous microstructure . This can be readily accounted for in continuum mechanical models.

7. I am not convinced that Eq. 9 represents a suitable strength criterion for the case of snow, a highly anisotropic material.

This is a reasonable remark. We are not aware of a conclusive study about mixed-mode strength criteria for weak layers and it is not the scope of the present work to investigate the topic. It is just our intention do demonstrate that FFM works with different mixed-mode strength hypotheses.

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 GI and GII to G.

We have expanded the description of the experimental data shown in Figure 3 and payed tribute to the strong anisotropy of snow in the discussion of the failure criteria.

9. Page 10, line 8-10: both requirements were considered by Gaume and Reuter (2017).

Their work makes use of an empirical equation obtained from a fit to numerical analyses to round rigid particles with interaction criterion. This length is considered to be the critical length of a crack required for unstable propagation. The second length is the size of the overloaded area below a concentrated skier load. The strength condition and the stability of the crack are considered in an uncoupled fashion.

Since they do so, we must ask the question: How does their model explain size effects? And what happens if a crack is short enough so that I should not propagate according to the energy criterion but according to the strength criterion it will?

These are questions that FFM provides a definitive physical answer to. It provides a unique solution based on experimentally measurable material properties. It is not possible to solve one equation for the length scale and another one for the critical load because both unknowns appear in both equations which, hence, are implicitly coupled.

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.

We have provided references for the exemplary values chosen. In the Figures 6, 7 and 9 we show and discuss the general parameter dependences.

11. Page 12, line 7: I suggest using slope angle or incline rather than inclination.

We have changed the wording as suggested.

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.

Considering remark 13 below, it seems there is a misunderstanding of our denomination "critical additional skier load". As depicted in the pictograph (sketch in the top right corner of, e.g., Figure 5) and indicated by the unit (kilo-newtons), the y-axis label "Critical additional skier load F [kN]" in our diagrams refers to the surface point load (concentrated force load) that, when applied, triggers weak layer failure.

A weak layer below a slab is loaded by the slab's weight which causes stresses in the weak layer. An additional concentrated force load applied to the slab's surface (e.g., by a skier) increases the weak layer stresses. The "critical additional skier load F" denotes the critical value of this additional surface load at which our model predicts anticrack nucleation within the weak layer. The term "point load" does not refer to a "maximum stress" at a certain point within the weak layer, as you assume in remark 13. It denotes a concentrated force.

In other words, if a concentrated force F that is smaller than the value indicated in our diagrams, is applied on the surface the snowpack, the entire snowpack would stay intact. With reference to remark 13, the force load F a skier needs to apply to the snowpack to trigger failure is not a given value but computed as the result of the present model.

In order to avoid this misunderstanding for the readers, we changed the denomination to "critical skier force F" in the manuscript added the following paragraph to the beginning of the results section:

> *In each study slabs loaded by their own weight and an additional concentrated force are considered. The force represents the outer load that a skier imposes on the snowpack. The given failure criterion predicts the critical magnitude of this additional concentrated force that leads to anticrack nucleation in the weak layer. We call this failure initiating force the critical skier force.*

Concerning remark 12, the critical skier force F, i.e., the load bearing capacity of the snowpack is higher in flat terrain because the weak layer normal strength and mode I fracture toughness are higher than the corresponding material properties in shear. We discussed this on page 18, lines 13-20 in out manuscript:

> The lower strength of the weak layer in shear governs the decrease of the critical loading with slope angle.

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.

See our response to remark 12 for a clarification of the term "critical additional skier load".

We combine a local stress criterion with a global energy balance of fracture mechanics. The given coupled stress and energy criterion in the framework of finite fracture mechanics is all in all a global criterion that cannot be evaluated locally. So the resulting quantity must be a global quantity as well. On the y-axis, we show the critical outer (surface) load onto the snowpack that leads to failure of the weak layer. The transfer of this load from the point where it introduced by the skier through the snowpack to the weak layer changes with the bending stiffness of the slab. This is an effect of the layering. Because the stiff slab rests on a compliant weak layer, its bending stiffness governs the overall snowpack deformation and, hence, weak layer stresses.

We have further clarified this in the manuscript and changed the wording.

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.

The restriction "as long as the slab is uniform" is important, must hold in thickness direction and actually extends through the weak layer. If we consider a homogeneous elastic half space, stresses within this homogeneous body that originate from force loads, are indeed independent of the Young's modulus. However, a stiff slab on a soft weak layer is by no means a homogeneous body. Further, we are not interested in stresses within the homogeneous slab but in stresses within the soft weak layer.

When the Young's modulus of the weak layer is smaller than the slab's Young's modulus, the load transfer depends on the Young's modulus of the slab. By load transfer we refer to the resulting weak layer stresses owing to a surface load.

Think of a stiff steel plate and a rather soft plastic board (slabs of different Young's moduli) resting on a mattress (weak layer), say both plates 1×1 m and 2 cm thick and the mattress 2×2 m and 10 cm thick. When an 80 kg person steps onto the center of the steel plate, its deformation will be hardly visible. Stresses within the mattress are low because the weight of 80 kg is transferred rather homogeneously over the entire 1 m^2. However, if the same person steps onto the plastic board, it will deform (bend) considerably causing localized stress below the person. This effect is reflected in Figure 7.

We have elaborated this in the manuscript.

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).

In the work by Reuter et al. [8] strain rate effects have been studied to some extent (cf. Figure 7 of their work). However, we agree that the works of Narita [9], and Reiweger and Schweizer [10] should be referenced at that point, since they provide insight into the strain rate dependence of the failure mechanisms.

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.

We state this in this context because it is an important property of robust models. However, we agree that it sounds like we imply that Gaume and Reuter [11] did make assumptions about initial flaws. We have rephrased the paragraph accordingly. However, concerning the model by Gaume and Reuter [11], it shall be pointed at the concerns raised in response to remark 9.

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.

In a mechanical perspective, buckling denotes a primary macroscopic stability failure mechanism owing to pure normal loading. It is directly and inseparably linked to an abrupt (compressive) deformation of the structure. Hence, when pure normal deformation is present, pure compressive failure is possible and likely.

However, there is no need to distinguish different microscopic failure modes on the continuum level. We agree that microstructural failure may occur in shear, compression or combined shear and compression. Yet, the aim of continuum mechanical models is to smear microstructural effects over the macroscopic scale. This is done by considering different compressive and shear strengths as well as different mode I and mode II fracture toughnesses. Hence, on the continuum level, we denote macroscopic failure owing to macroscopic normal deformation 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.

This remark relates to the previously discussed misunderstanding of our denomination "critical skier load F". As we pointed out in our response to remark 12, the term "critical additional skier load" refers to the outer force load that a snowpack can carry on top of its own weight before an anticrack nucleates in the weak layer. Hence, a vanishing critical skier load in steep terrain corresponds to the described observation: Triggering is more likely on steeper slopes because the "critical additional skier load" required to initiate weak layer cracks is lower (and vanishes on very steep slopes). In other words, on steep slopes a very small load can trigger weak layer failure.

We agree that longer cracks in steeper slopes are somewhat counterintuitive. However, it is to note that the length of nucleated finite cracks provides no information on how likely or unlikely anticracks can be triggered. Only the critical skier load, which is also a result of the model, does. Both load and crack length are results of the complex interaction between stress and energy within the finite fracture mechanics failure model. In the present case, the crack length becomes long because the mode II energy release rate (shear) is much smaller than the mode I energy release rate (collapse). There is a number of factors that may cause this behavior: i) We did not consider weak layer anisotropy, ii) we considered normal and shear deformation uncoupled and iii) we estimated the mode II fracture toughness $\mathcal{G}_{\mathrm{IIc}}$. Once all these points are addressed in a refined future model (which we are already working on), we may obtain a different picture.

We have improved the paragraph clarifying the denomination "critical skier load" and discussing the crack lengths.

19. Page 18, line 21: Suggest rewording.

Please refer to our answer to remark 14. Deformations of a homogeneous slab on a soft weak layer depend on the slab's bending stiffness EI. This stiffness has a linear dependence on the slab's Young's modulus E and a cubic dependence on its height $I = bh^3/12$ (assuming a rectangular cross-section). The thought experiment suggested in response to remark 14 can be done with two plates of different thickness, as well. A 0.1 mm thick steel plate on a soft mattress will deform when an 80 kg person steps onto it, a 10 cm steel plate will not. We have, therefore, revised the paragraph to better explain our understanding of load transfer.

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

As discussed in response to point 14, the Young's modulus of the (stiff) slab does affect the stresses in the (soft) weak layer. The (bending) deformations of the slab change and hence the strains and stresses in the weak layer, too.

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.

The statement that the shear fracture toughness $\mathcal{G}_{\mathrm{IIc}}$ is much smaller than the compressive fracture toughness $\mathcal{G}_{\mathrm{Ic}}$ agrees with experimental findings on other materials such as glassy foams (Heierli [12]). The specific identified ratio of $\mathcal{G}_{\mathrm{Ic}}/\mathcal{G}_{\mathrm{IIc}} \approx 20\,..\,40$ likely depends on assumptions of the present work, e.g., isotropy of the weak layer and uncoupled normal and shear deformations. Unfortunately, no experimental data is available to unambiguously determine the ratio and to test the validity of our assumptions. Therefore, we just added a discussion of effects of model assumptions to the paragraph, as suggested.

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).

We have removed the confusing statement.

Page 20-21, Conclusions: I suggest you refer also to the limitations of the model and provide an outlook on possible improvements.

We have added the following limitations to our conclusions:

- Homogeneous slab
- Uncoupled normal and shear displacements

- Isotropic weak layer

As an outlook the following improvements are vital:

- Layered slab
- Coupled normal and shear displacements
- Measurements of elastic snow properties, in particular weak layer tensile and shear moduli
- Measurements of failure and fracture envelopes

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Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-87, 2019.