Reviewer report

This is a public review report, stating that we have no conflict of interest.

Full title: Seasonal increase of the mixed-mode fracture toughness of faceted-crystal weak layers

We are glad to review this manuscript. It is a very interesting work that has carried out a large number of Propagation Saw Test (PST) experiments and studied the fracture toughness under the mixed mode. This paper also obtained some results. Under both compression and shear fracture modes, test the fracture toughness with a natural weak layer composed of faceted crystals. This is an excellent job and the content is also suitable for The Cryosphere. However, we believe that the manuscript still needs improvement. We have provided specific comments as follows:

Specific comments:

1. You mentioned the word "seasonal" in the title and content, but it is not a quantitative term, so it is not precise to describe the fracture of the weak layer. Your experimental period is three weeks, but the word "seasonal" is misleading, making people think it is a multi-year seasonal cycle experiment.

2. In the Abstract section (5-6 lines), you describe "We introduce a novel method..." However, we believe that this method already exists or there are related measurement techniques available. For instance, DIC measurement technology has been used to track the variation of the displacement field (Bergfeld et al., 2021), the tilting device has also emerged (Adam et al., 2024) and van Herwijnen et al. (2016) has also proposed a method for measuring the effective elastic modulus.

3. In Lines 13-14, you said that “the increases in both mode I and mode II fracture toughness are associated with grain size growth and growth of the bonds between the grains.” but in the main text (lines 301-302), You said again that “However, with only four sampling days and no statistical analysis, these observations should be regarded as indicative rather than conclusive.” This is contradictory. What should be obtained in the abstract is a conclusive and definite result.

4. In the Introduction section, we feel it is insufficient. The relevant results of some recent super shear cracks of very long PST (Trottet et al., 2022, Bobillier et al., 2024) and the super shear phenomena discovered in field experiments (Simenhois et al., 2023) have not been described at all. The author should, when presenting the current situation, evaluate the super shear model, including its advantages and disadvantages. Since other authors have indicated that PST does not represent the fracture process of real avalanche formation (contact us for discussion), large-scale or extensive experiments may be closer to real avalanches.

5. Figure 1. the content of the picture (figure 1b) does not match the description. The picture shows what is like a schematic diagram of PST, but describes it as "field experiments including extracting, tilting and filming the sample". You should use

on-site experiment pictures or pictures of the experiment in progress instead to make it clearer. Of course, I can understand what you mean.

6. Section 2.2 Mixed-mode fracture test. The model is the standard PST model (1m*0.3m), which is the common PST model we see. However, recent articles have shown that standard PST cannot fully demonstrate the dynamic propagation of cracks, with a PST length of at least 5 meters or more. For super-shear cracks, the supercritical length even exceeds 5 meters. What's your opinion, or what's your evaluation of these results?

7. The Result section, you chose the faceted-crystal weak layer, but most of the previous weak layers were surface hoar or depth hoar. Section 3.1 The critical cut length increases with the increase of the tilt angle and, of course, is related to the direction of the cut weak layer. Both show an increasing trend, but others have reached the same or similar conclusion (Adam et al., 2024). In addition, have you made any new discoveries or found out the dependence between the critical length and the angle? The critical length is also a quantity that I pay close attention to. Because the early anticrack model (Heierli et al., 2008) held that the critical length was independent of the slope angle, later Gaume et al. (2017) obtained through DEM that the critical crack length decreased with the increase of the angle. Subsequently, there were a large number of conclusions, including the super shear model, also supports this conclusion. However, Adam et al. (2024) concluded from the classical PST experiment that the critical crack length increases with the increase of the angle. This is a series of completely opposite conclusions. What are the reasons for these differences?

8. We observed that the highest elastic modulus of the snow slab measured in your experiment reached 66 MPa, and the average was also 40 MPa. This is already a rather high elastic modulus of the snow slab. The snow layer might be dense and compacted. The elastic modulus of loose snow or fresh snow is lower than this. van Herwijnen et al. (2016) experiments measured the elastic modulus ranging from 0.08 to 34 MPa, with the elastic modulus mostly concentrated around 5 MPa and generally lower than 10 MPa (van Herwijnen et al., 2016; Fig.8). The elastic modulus in the numerical model is also basically at 5 MPa (Trottet et al., 2022; Bobillier et al., 2024; Bergfeld et al., 2022). What is the reason for this difference? Is it related to the loading device you placed on the upper surface of the snow slab?

9. Section 3.3 Mixed-mode interaction law for fracture toughness this relationship has been proposed by Adam et al. (2024), and the relationship between the critical energy release rates of mode I and mode II are also quite similar, which has appeared in the published article. Therefore, the results of these studies are merely increments. What are the differences between you two?

10. In the Discussion section, we hope to conduct a comprehensive analysis of the changes in different critical lengths under the angle, such as the different conclusions drawn in the early stage as the angle decreases and in the current stage as the angle increases. This is quite important, and we hope to present a comprehensive analysis. In addition, the reasons for the high elastic modulus of the snow slab layer and

whether it is related to loading should be discussed in detail. In terms of section 4.4, limitation analysis, we would like you to analyze the differences between the typical model and the super shear model. Does the claim they made that the short PST cannot fully exhibit crack propagation exist?

Other minor issues:

1. The citation of references is not standardized and does not need to be included when applying (e.g.). There are many places in the introduction section that need to be corrected.

2. The units are not uniform. For instance, in Picture 3, the unit of length is millimeters, while in the main text, it is meters (89 lines). It is suggested that the author standardize the units.

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Reference

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[4] Gaume J, van Herwijnen A, Chambon G, et al. 2017. Snow fracture in relation to slab avalanche release: critical state for the onset of crack propagation. Cryosphere, 11(1): 217-228
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