Reply to Referee #2

We thank the reviewer for the constructive review and valuable comments that will be very helpful for preparing an improved revised manuscript. In the following we reply to the comments in detail and describe the changes we intend to make in the revised manuscript.

This paper presents a unique dataset of temporal changes in crack propagation propensity over the course of a season, and how that propagation propensity related to temporal changes in the slab and weak layer. The authors utilized the latest tools for their work, including analyzing high speed video with PTV, making measurements with the SMP, and modelling the evolution of the snow cover with SNOWPACK. The paper is a valuable addition to the literature, and I believe it should be published after it is revised.

My first suggestion is that the authors consider a different title. Since the paper really focuses on crack propagation propensity, the title should better reflect that. Perhaps something along the lines of "Temporal evolution of crack propagation propensity in view of slab and weak layer properties" or similar? Or, even more specifically, "Temporal evolution of critical cut lengths. . ."?

Thanks for the suggestions. We intend changing the title to "*Temporal evolution of crack propagation propensity in snow in relation to slab and weak layer properties*"

Also, it would be nice if the authors could briefly describe more of the methods used. I know that they will not want to repeat long sections of previous work, but if it would be useful for the reader if they could provide even a few more details about some of the SMP and SNOWPACK derived parameters. More background information will help the reader better assess those parameters and how they performed.

We will describe the methods in more detail so that the paper becomes more self-contained.

Major comments:
One primary concern about the paper has to do with Figure 1a and the evolution of the cut length of the PSTs. In this graph it appears that the authors are mixing results that go to END with result that are SFs. Can the authors discuss and defend why they feel this is an appropriate treatment of these data? In my experience I have seen situations where SFs have longer cut lengths, but then as the PSTs transition to END results the cut lengths decrease. At this point, I am not convinced that you can treat the two sets of results (END and SFs) as the same and show a temporal trend with them. I would suggest that they defend this, or they only consider the tests that went to END.

Thanks for rising this point. The critical cut length we report in Figure 1 is independent of the subsequent dynamic phase of crack propagation. Whether or not a crack will arrest, possibly resulting in a slab fracture, or run to the end of the column, will depend on slab as well as weak layer properties – just as the conditions for the onset of the running crack depend on slab as well as weak layer properties. More specifically, recent research indicates that the tensile strength of the slab may decide on how far cracks propagate (Gaume et al., 2015; Schweizer et al., 2014). However, the onset entirely depends on the balance between the energy available for fracture, i.e. the mechanical energy released due an incremental advance of the crack, and the fracture energy, i.e. the energy required for crack growth, or in other words the resistance to crack propagation.
We checked our extensive data base of propagation saw tests and contrast in the figure below the critical lengths for tests with fracture arrests (ARR, SF) and those with full propaga-

tion (END) results. There is for our dataset (N = 427) no significant difference between the crack lengths (p = 0.46).

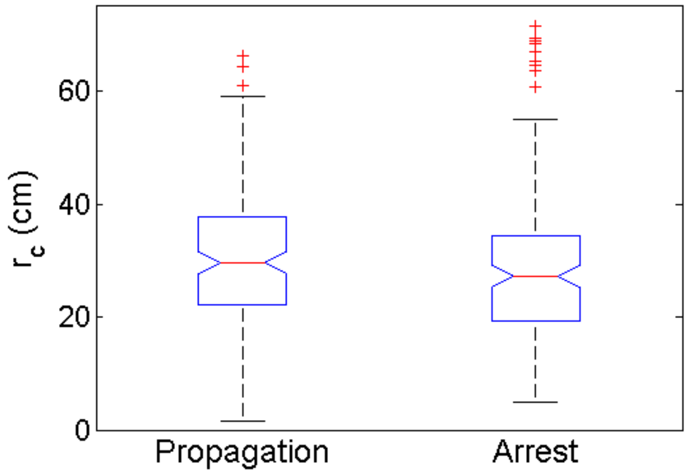

Figure: Critical cut length as observed in PST's as a function of the propagation result. Propagation (on the left) includes all tests where the crack propagated to the very end (END). Arrest (on the right) includes all tests where the crack arrested (ARR) – with or without visible fracture across the slab (SF). Total number of PST results: N = 427, unpublished data.

Furthermore, we certainly agree with the observation by the reviewer that cut lengths may decrease while the result changes from SF to END. In our experience this is usually related to a change in slab properties, e.g. due to additional load, which then will affect the onset as well as the dynamic crack propagation phase. This is actually what we observe towards the end of January 2015 and is likely the reason for the transition from SF to END fractures in our dataset.

Another primary concern about this paper is that I feel a much more robust discussion of the results is warranted. The authors have presented many interesting results, both in terms of the temporal evolution of various parameters and in the comparison of different methods of tracking those temporal changes (between the field tests, PTV, the SMP, and SNOWPACK). However, in my opinion the authors do not fully discuss many of these findings. Some examples:

- *The temporal changes of effective elastic modulus of the slab derived from PTV and derived from the SMP do not match (Figure 2). However, this discrepancy is not discussed. Which one of these two techniques do the authors believe is closer to capturing the "true" change in the elastic modulus? It seems to me that the PTV results more closely align with changes I've observed in the field. If this is the case for these data, can the author suggest ways the SMP techniques can be improved?*

We agree that there are many open questions, and discrepancies, with regard to the various methods we apply. Most methods have been validated independently, and so far not been contrasted. So far, only Reuter et al. (2013) made an initial attempt to compare various measurements methods; these authors are about to prepare a more in-depth manuscript for a peer-reviewed journal.

We will discuss the discrepancies in the revised manuscript, but it would be beyond the scope to provide a full comparison. What can be said for sure, is that both methods do not provide the true elastic modulus (Young's modulus).

- *The SMP's derived critical cut length did not match the observed changes in the PST. Why do the authors think this is the case? Is this some shortcoming in the SMP technique, or are the data presented in this paper somehow different from the data used to develop the SMP derived critical cut length? Does this finding shed additional uncertainty on the SMP derived cut length?*

We agree with the reviewer's observation of a certain discrepancy and will discuss how our measurements relate to the validation data presented in Reuter et al. (2015, Figure 8b).
The difference in part stems from the fact that the SMP-derived modulus is not really well related to other independent measurements of the modulus (Reuter et al., 2013). We will consider whether it would not be more appropriate to simply derive density from the SMP (Proksch et al., 2015) and then determine the modulus based on the parameterization provided by Scapozza (2004).

- *On page 12, line 10 the authors state that this metric is experimental so it is premature to rate it. I disagree with this statement. If the metric is seen as useful enough to be included in the paper, then I feel that it is appropriate to fully evaluate it and rate its usefulness.*

We will discuss the issue of the usefulness of the new metric more thoroughly in the revised manuscript. We would like to point out that most validation studies consider many single measurements, compare those to the modelled values and find a correlation, of course including some scatter (e.g., Reuter et al., 2015). Predicting the proper temporal trend seems more challenging, in particular if the trend is rather weak, and changes of mechanical properties from week to week may be small.

- *Another point that is not fully discussed is the difference between the elastic modulus values calculated using PTV and those calculated with the SMP. It would be nice to have a paragraph discussing these differences, why they occur, and whether there are ways to get better measurements out of some of the techniques. This could be placed after the paragraph ending on Page 11, Line 19. Looking at Figure 2, the numbers for the SMP seem strange (staying the same or even going down over the season), while the numbers for PTV seem more realistic. What do the authors think about this and how might they explain it?*

We agree and will add some more discussion. We have recently shown (van Herwijnen et al., 2016) that the PTV-derived modulus fits relatively well with results from laboratory experiments in the same range of strain rates. For the SMP, the strain rates are presumably higher, yet the SMP-derived modulus is similar – not higher as would be expected due to the higher strain rate. This is likely due to the size of the cone, which is comparable to the grain size in snow, and therefore local effect (force chains, jamming) influence the results.

Minor comments:

- *Most figures feature a dashed line that is a "running median smoother". It would be helpful to know how the authors calculate this smoother. Also, are the cut lengths in Figure 1a treated the same whether the test went to END or was SF? It appears they were, but the authors may wish to state that in the text.*

We will specify how running median smoother was calculated; it is a running median with window size 3.
The cut lengths are treated the same whether the crack did run to the end or arrested. See also reply above. We will specify this in the revised manuscript.

- *Page 2, Line 1. It is true that Sigrist and Schweizer (2007) were "among the first to emphasize the importance of the slab layers and weak layers", but there were others that emphasized that point either prior to, or at the same time as, 2007. Those include the MS thesis by B.C. Johnson (2001), the paper by Johnson and Jamieson (2004 in CRST), the PhD dissertation by van Herwijnen (2005), the paper by van Herwijnen and Jamieson (2007 in CRST), and the thesis by Gauthier (2007). Since this has been an important point, I'd encourage the authors to add some of those other publications to this citation. Other earlier work also talks about the slab, but in terms of "emphasizing" the slab, it really began to be more clearly stated in the 2000s with the work by Johnson, Jamieson, van Herwijnen, Sigrist and Schweizer.*

We will reword the sentence to emphasize the explicit interaction of slab and weak layer properties for evaluating the critical cut length since this is what we refer to. Of course, the general importance of slab properties for crack propagation has been stated previously; we do not mind to add some more references.

However, the studies mentioned by the reviewer primarily focus on the relevance of the slab properties and not explicitly on the interplay between slab and weak layer properties. Only the analysis by Sigrist (2006) finally made it clear how slab and weak layer properties interact in crack propagation, which had already been shown for failure initiation. van Herwijnen and Jamieson (2007) related snowpack properties to failure initiation and crack propagation propensity and showed conceptually how slab properties, in particular slab depth, affect failure initiation and crack propagation in different ways.

- *Page 2, line 18 and 19. Do the authors believe that the "shear strength of the weak layer is important for failure initiation" in the case of a triggered avalanche from flat terrain?*

Yes, it is certainly important. In flat terrain, a skier not only induces compressive stresses, but also shear stress of similar intensity (Monti et al., 2016; Schweizer, 1997). Hence, the shear stress induced by a skier is very significant even in flat terrain. It is thus more likely that under these mixed-mode conditions the failure begins in shear (or mixed mode) rather than in pure compression because weak layers are weaker in shear than in compression (Reiweger and Schweizer, 2010).

- *Page 7, line 6. When the authors state that "cracks did not always fully propagate", it would be useful if they stated how many tests were done and how many propagated (i.e., something along the lines of "when we did the first PSTs, cracks fully propagated in two of five tests, while slab fractures occurred in the other three tests" or whatever the numbers were).*

We will add this information as suggested, see also Figure 1a.

- *Page 7, line 8. Like above, it would be nicer to know the number of tests instead of just writing "all tests".*

We will add this information as suggested.

- *Page 7, line 18. It seems to me that the data demonstrate that the propagation propensity decreased definitively (rather than slightly) between the first two days because on the second day all of the tests were SF while on the first day there were some that went to END. This could be due to the shallower nature of the snow in that part of the plot, as discussed by the authors, or it might have to do with a change in the slab. I have observed a decrease in propagation propensity (from more END results to more SF results) when a slab loses tensile strength due to near-surface faceting.*

Thanks for pointing this out. We will include this point into the Discussion section.

- *Page 9, line 15 and Figure 5. Did all of the ECTs propagate across the column (ECTPs) or did some not (ECTNs)? That should be made clear here or on Figure 5.*

Not all ECTs propagated across the column. We will add this information as suggested.

Typographical/grammatical errors:
- *Page 1, Line 8, add "(PSTs)" after "propagation saw tests" since "PST" is used later in the abstract.*
- *Page 1, Line 21, delete "considering the slab,"*
- *Page 1, Line 32, replace "but" with "and"*
- *Page 2, line 14 and 15. It seems the information for those two sentences comes from a single reference, but the authors cite both Jamieson and Schweizer, 2000 and Schweizer et al., 1998. Which reference is correct?*
- *Page 3, line 3, delete "exists"*
- *Page 4, line 13, add an "s" to "PST" so it reads "PSTs"*
- *Page 5, line 3, delete the comma that is after "modulus"*
- *Page 7, line 9. You cannot have two semicolons in the same sentence. You will need to re-word to rem*ove one of them.

Thanks for these suggestions which we will consider in the revised manuscript.

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