

Interactive comment on “Temporal evolution of weak layer and slab properties in view of snow instability” by J. Schweizer et al.

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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 snowcover 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

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view of slab and weak layer properties” or similar? Or, even more specifically, “Temporal evolution of critical cut lengths. . .”?

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.

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.

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? - 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? - 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. - 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?

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. - 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

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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. - 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? - 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). - Page 7, line 8. Like above, it would be nicer to know the number of tests instead of just writing “all tests”. - 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. - 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.

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 remove one of them.

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