The Cryosphere Discuss.,
doi:10.5194/tc-2016-84-RC1, 2016

The Cryosphere
Discussions
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Interactive comment on "Temporal evolution of weak layer and slab properties in view of snow instability" *by* J. Schweizer et al.

Anonymous Referee #1

Received and published: 31 May 2016

SUMMARY:

The authors monitored the temporal evolution of a {weak layer, slab} system during winter 2014-2015 in a field site located next to Davos. Typically, each week between 6 January 2015 and 3 March 2015 (8 days of measurements), they performed on the same site located next to an automatic weather station:

- three propagation saw test (PST) on which they measured the critical crack length, the full or partial crack propagation and the slab displacement field (PIV measurements),

- around five SMP profiles,

- a classical manual snow profile with a density profile

- CT/ECT tests.

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The authors try to explain the observed temporal evolution of the PST critical crack length (general increase with a minimum the 28 January) by investigating the evolution of individual mechanical parameters of the weak layer and slab, namely the load on the weak layer, the weak layer fracture energy and the so-called bulk elastic modulus; and their interaction through the anti-crack model. They used previously developed methods to access these parameters from the measured data. They also used the SNOWPACK model to compute the critical length from the simulated snow profile with meteorological forcings from the automatic weather station. The authors show that monitoring the evolution of individual parameters cannot explain the observed critical crack length trend but that it is necessary to account for the complex interaction between these mechanical variables. The SMP metric is not able to reproduce the observed critical crack length. The SNOWPACK metric shows also an increase of the critical crack length.

GENERAL COMMENTS

The dataset collected by the authors is very interesting combining quantitative stability analysis (PST critical crack length) and highly resolved vertical hardness profile (SMP). Some of the results are of clear interest to the snow and avalanche community: the authors showed that both slab properties and weak layer cannot be individually monitored to understand the crack propagation propensity evolution; they also show that the previously developed SMP stability metric is not capable of capturing the evolution of the critical crack length. However, the methods are not well presented and appear as a black boxes where explanations on the basic assumptions are missing and the methods are mixed without an apparent logic. In particular, the SMP stability metric presentation is not clear in this form. Evaluating the stability metric of SNOWPACK from a modeled snow profile without showing that the modeled snowpack profile has something in common with the observations is not informative. The sensitivity analysis on a three parameters analytic function is based on four single cases. The trend analysis gives too much importance to a single day case that might be not statistically

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representative.

Therefore, I recommend major revisions before publication.

MAJOR COMMENTS:

1) The dataset collected by the authors is very valuable. Indeed, the authors present it as the first comprehensive time series of a {weak layer, slab} system. It uses state-of-the-art measuring techniques (SMP, PST) combined with "traditional" measurements (manual stratigraphy and density, CT/ECT). Since one of the objective and strength of the paper is this dataset, it appears logical to provide this dataset as supplementary files (Caaml file for stratigraphy, stability tests, text file for SMP and avi file for PST videos).

2) The writing style on the mechanical background is often unscientific and requires precision and consistency. I have listed some of these problems:

- about the elastic modulus. You used the following terms without proper definition: "elastic modulus", "bulk modulus", "modulus", "effective modulus", "bulk effective modulus", "micro-mechanical modulus", "slab modulus", "stiffness", "elastic modulus with non-elastic parts of deformation". This vocabulary is misleading and is not suited for a scientific paper, where the mechanical concepts behind the used model should be precisely presented, which can be done in a simple way accessible to the snow community.

- you use the terms "propagation propensity", "propagation criterion r_c_SMP" ,"critical crack length", "propagation propensity metric", "crack propagation propensity" to refer to the same parameter r_c, or maybe not but this is not clear. Why don't you use consistently the well-defined "critical crack length" and explain only in the introduction that the critical crack length is an indicator of the more general concept of crack propagation propensity?

- "initiation probability", "initiation propensity", "initiation criterion", "initiation indices", "skier stability index" ...

- delete vague and unspecific claims "reliable", "reliable in general", "distinct pattern", "relevant mechanical properties", "other mechanical properties"

3) It is hard to follow the history of the {weak layer, slab} system. It is necessary to add a one-page figure with eight sub-figures (one for each day of measurements) showing the manual stratigraphy (at least snow type and density), a SMP profile and the position of the weak layer.

4) In Heierli's model, the total mechanical energy of a PST crack of length r is composed of two terms: V(r) = w_f * r + Vm(r) where w_f * r is the weak layer fracture energy and Vm(r) accounts for elastic deformation energy and changes in gravity potential energy of the slab. In case of a uniform slab, Vm(r) can be computed analytically knowing the density, thickness and elastic modulus of the slab. In case of a FE model of a multilayer slab (density, thickness and elastic modulus per layer known), Vm(r) can be calculated numerically. This is done for the SMP analysis. In case of a measured displacement/deformation field of the PST tests, Vm(r) can also be calculated. This is done in the PST analysis. In both cases (SMP, PST method), the calculated Vm(r) is used to fit the analytic mono-layer solution. The fitted analytic solution is then differentiated to obtain the critical crack length knowing the weak layer fracture energy (SMP method) or the weak layer fracture energy knowing the critical crack length (PST method). I don't understand why the dVm(r)/dr is not computed directly from the calculated Vm(r)(or with smoothing of Vm(r)). This is not explained in the proposed references (Reuter et al, 2015 or van Herwijnen and Heierli, 2010). The bulk elastic modulus is a fitting parameter and it is unclear how physically-relevant it is. There is no clear reason why Vm(r) on layered material should fit directly the mono-layer analytic solution. Provide a proper explanation and discussion on that. Moreover, recall the main hypothesis (elastic linear, only the slab contributes to deformation energy) of Heierli's model.

5) Section 2.4 describing the SMP signal processing is vague and unscientific. Many critical details are missing. It does not allow the reader to reproduce the presented method and appears as a black box. It requires a deep rewriting. It mixes method using different concepts that measures the same things differently e.g. Johnson and Schneebeli (1999) and shot-noise model used by Proksch, 2015. The window size for analysis, the SMP version, the adjustment parameters of (Proksch et al, 2015, calculated on a few alpine snow samples), the finite element layer mesh, etc. are missing. There is additional linear scaling with no convincing explanation. The calculation of layer Young's modulus from SMP elementary failure element is known to be poor and is inconsistent with the one based on density (Scapozza, 2004) used by the snow cover modeling (p5 l30). The failure initiation criterion S is not detailed and it is hard to notice that it does not incorporate snow load in comparison to SK38 which does, ... The reference to other papers is far from being sufficient and clear explanations won't take more than 30 lines.

6) The authors used the snow cover model forced by a nearby automatic weather station as an input of a new critical crack length estimator (Gaume et al. 2014a, 2016). Without any clue on how close the snowpack simulation to the observed snowpack, it is impossible to exploit the results of this analysis. It is well-known that one point evaluation of a snow cover model on stability criterion is difficult. Note that the only variables missing in Eq. (1) is the weak layer strength that could be fitted to get r_c_snp = r_c_obs, similarly to what is done for the PST.

Additionally, it is not clear to me how the avalanche activity index (concerning the area all around Davos ?) can help to analyze the measurement done in this particular site.

7) The pattern of the PST critical crack length is a general increase with a local minimum for one measurement day (28 January). As discussed (p6 l20-23, p10 l3-6), the spatial variability can significantly affect the stability even a few meters away. Given the poor representativity of one day of measurement to define a trend, and potential spatial variability, it would be reasonable when speaking of trend to not focus on the

minimum observed the 28 January but on the general trend (continuous increase of r_c). Note that this does not challenge the fact that the SMP should reproduce the same trend (since measured a few cm away from the PST); but the comparison with SNOWPACK is challenged. The explanations “we deem it unlikely that the observed pattern is entirely the result of spatial variability and does not reflect the temporal evolution”, “Previous studies performed in level study plots have shown that measurements in general are reliable and that the effect of spatial variations is relatively small” are not convincing, at least in this form.

8) The sensitivity analysis is poor and based on four different cases. To my opinion, this cannot be called a sensitivity analysis. Differentiating Eq. (2) with respect to E, sigma and wf provides a way to perform this sensitivity analysis properly.

Note that the general comments are general and require re-wording of several parts of the paper and additional explanations, and not only taking into account specific minor points listed below.

MINOR COMMENTS:

abstract: the following terms are too vague : “distinct pattern”, “other mechanical properties” “some of the relevant mechanical properties”

p1 l25: “how much stress due to a skier is transferred”. Misleading sentence. All the stress is transferred to the ground. But it is distributed on a larger surface. Reword.

p1 l28: “with respect to the weak layer, a snowpack a weakness is” -> “the weak layer is”

p2 l2: “conceptual model”. Describe this model in a few words.

p2 l7: “though the strengthening may lag behind the loading”. Sound unscientific. Delete.

p2, l27: References to the model Surfex-Crocus (Vionnet, V. et al. Model Development

The detailed snowpack scheme Crocus and its implementation in SURFEX v7 . 2. Geoscientific Model Development 5, 773–791 (2012)) and Mepra (e.g. 1. Giraud, G. MEPRA an expert system for avalanche risk forecasting. in International Snow Science Workshop 97–104 (1992)) are clearly missing.

p3 Section 2.1: Is the snowpack completely dry during measurement period?

p4 l1-2: “The weak layer . . . December 2014”. Explain how you know that.

p4 l2-3: “While no fracture . . . January 2015”. I don’t understand. Reword.

p4 l7: “The manual snow profile served as a reference”. Do you mean that you performed manual stratigraphic matching to adjust the other snow profiles to the manual profile?

p4 l10: “at least three PST”. It appears from Figure 1a) that there two other dates where less PST were performed.

p4 l14: "we cut the layer of faceted crystals at its upper interface". One of the main difficulty of the PST is to follow the weak layer of interest. As explained in Section 2.1, there was another FC layer just above the weak layer of interest. Showing the SMP profiles (see main comments) could help the reader to evaluate the likelihood of deviation of the saw cut in the weak layer.

p4 l18: Give version of SMP.

p4 l25: "the displacement of the markers was used to estimate the mechanical energy V m (r) with increasing crack length". As far as I understand, at this step, you also need the load, i.e. the density of the manual profile. Add explanation if this is correct.

p4 section 2.3: The critical crack length of the modeled PST is inherently equal (or very close) to the observed critical crack length since the observation is used to fit w_f. This might not appear clearly to the reader. Please add this kind of explanation.

p5 l28: "the shear modulus of the weak layer which was estimated". How ?

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p5 l30: I suggest to explicitly indicate the power law relation used here.

p6 Eq2: To my opinion, this equation in this form does not give any information to the reader. Delete or give detail on all terms.

p6 l23-26: "By then, the weak layer of ... resulting in a load of almost 4 kPa." Belong to the load section 3.2?

p7 l29: "0.3 J m -2 to about 1.5 J m -2". Recall that this range results from a linear scaling between w_f_SMP and w_f_PTV.

p8 l3: S = shear_strength / skier stress should be described in Methods. Adding two lines of description is not a big deal and would clarify the message. See main comments.

p8 l10: SK38 = shear_strength / (skier stress + weight_stress) should be described in Methods. See main comments.

p8 l22-24: The CT/ECT tests could be better used to evaluate the initiation criteria (SMP, SK38).

p9 l14-18: I don't understand this paragraph. The rc_obs is used to compute w_f_PTV. That w_f_PTV as input in Heierli's model gives the same trend for r_c does not appear to me as a finding ??? Clarify.

p9 l27-28: "Only when the load had reached 2 kPa, all cracks fully propagated towards the end of the column. This finding suggests that the slab was initially not strong enough to support the propagation". I don't understand the logic link between these two sentences (load/strength ?). Clarify.

p10 l7 "5.9 cm". This is not a range.

p10 l15-19: "The errors associated with the parameters ... the dots in the PTV analysis)." This a new info that belongs to Methods and Results sections.

p10 l19-22: Adding error-bars on the figures 2a, 3a would help to illustrate this discussion. Moreover, you might go further in this discussion. Indeed w_f depends only on one layer whereas E is an integrated value on the slab layers and might thus be less sensitive to the spatial variations of one layer.

p10 l26: "validated" -> "evaluated"

p11 l3: "is in line with the observations in particular when considering the CT and ECT scores.". What are the others ?

p11 l10: "– suggesting that the propagation propensity decreased". Delete

p11 l10-11: "This behavior follows from the fact that two of the essential variables, the bulk modulus and the weak layer shear strength also increase with time." From your sensitivity analysis (figures 6a,b) and the fact that you get the same results for Eq. (1), this is not a sufficient explanation.

p11 l14-15: "However, it seems premature to rate this metric as it has to be considered as being still in an experimental state." I agree this is a very valuable criterion to help to synthesize the data of snowpack models. However, the explanation is evasive. To my opinion, evaluation of this metric on one point stability observations with potential errors in meteorological forcing and SNOWPACK modeling is the main problem. See main comments. Delete or reword.

p11 l19: "The parameter most strongly influencing the critical cut length seems to be the load". Not shown in results. Can be quantified. See main comments.

Figures: what is the running median smoother (kernel size ?)

Figure 1: a) give r_c in m for consistency. b) indicate in the figure what is the black solid line.

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