The Cryosphere Discuss., 8, C3186–C3187, 2015
www.the-cryosphere-discuss.net/8/C3186/2015/

The Cryosphere
Discussions
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Interactive comment on "A process-based approach to estimate point snow instability" *by* B. Reuter et al.

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Received and published: 6 March 2015

Please find our response to the reviewer's comments in the supplement.

Interactive comment on The Cryosphere Discuss., 8, 5825, 2014.

Reply to Referee #2 (Bruce Jamieson) RC C3160 / SC C2799

General comments
An excellent contribution. Well argued and very well referenced.

Specific comments
The relationship between critical crack length and crack propagation propensity could be clearer. Shorter cut lengths in PST tests (assumed similar to critical crack lengths) are not simply e.g. inversely, related to crack propagation propensity.
In various papers, Gauthier related validated propagation propensity to cut lengths less than 50% of column length AND crack propagation to the end of the column in a PST test. Statements such as in lines 3-4 of page 5829 oversimplify criticality. One way to clarify this is to define criticality not simply in terms of the start of propagation but propagation over a distance on the scale of 1 m.

We agree that Gauthier and Jamieson (2008) combined a threshold of critical crack length with the experimental PST fracture result to define crack propagation propensity and hence we will change the wording on page 5829 in lines 3-4.
Here we used a dataset of fully propagating PSTs with beams longer than 120 cm and critical crack lengths of up to 60 cm (corresponding well to previously published suggestions) to compare our model results with. Further we seek to derive a critical value for modeled crack lengths by comparing our model results with presence of signs of instability. We report a value of the critical crack length of 40 cm which is below Gauthier's suggestion (of below half the column length) given a column length of 120 cm, which is the shortest column length in our data.

Page 5833 lines 10-15. Some clarification of the failure mode in the stability criterion is needed. The shear stress term in the denominator is traditionally slope parallel e.g. Habermann et al. (2008), but this is not the case for strength derived from the SMP (numerator in Eq 5.), which is "an indentation test". A statement that "slope-parallel shear strength over shear stress is not being used because . . ." would be helpful.

We will add the following sentence:
"A slope-parallel shear strength over shear stress formulation is not being used because the SMP is an indentation test measuring an effective strength resulting from the mixed-mode breaking of bonds at the tip."

Page 5835 line 5 "the FE model reproduced the maximum shear stress very well . . .
R2 = 0.94." How do the intercepts from the two methods compare? A statement about the intercepts or a graph would help.

We will provide information about the regression slope for a linear regression of the shear stress derived with the finite element and the analytical solution (regression slope m = 1.20) instead of providing the intercepts of the regression, as the solution does not converge for finite depths.

Page 5837 line 5 – 14: If the deflecting beam is never supported by closing the gap between the slab and the bed surface, say so, and note that this may be different from real slab bending over collapsing weak layers.

We will add the following sentence:
"In our model, the deflecting beam is never supported by closing the gap between the slab and the bed surface which, however, may be the case in field experiments in particular with soft slabs."

Technical comments on clarity & presentation Page 5831 line 18: hand hardness index for each manually identified layer.

We will change as suggested: "… for each manually identified layer..."

Page 5833 line 23-24: presumably this because snow is much more sensitive to dynamic stress than quasi static stress. This is worth mentioning.

Fig. 1.