

Interactive comment on “Validating modeled critical crack length for crack propagation in the snow cover model SNOWPACK” by Bettina Richter et al.

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Reply to Referee 2

We thank the reviewer for the insightful and constructive comments. Below we will answer point by point.

Technical comments:

Page 4, line 6: please briefly explain Neumann boundary conditions and why this was chosen for the snow surface.

To estimate snow surface temperature (TSS), SNOWPACK either directly uses mea-

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sured values for TSS (Dirichlet boundary conditions) or estimates TSS from energy fluxes (Neumann boundary conditions). At the field site WAN7, the TSS sensor malfunctioned and we could not use TSS as input for SNOWPACK. While for WFJ we could have used measured TSS, we wanted to make the simulations consistent. Differences in the surface energy budget resulting from these two boundary conditions do not affect our results (see answer to reviewer 1).

Page 4, line 7: add citation for the chosen geothermal heat flux of 0.06 W m^{-2} .

We will add the following citations: Pollack et al. (1993) and Davies and Davies (2010).

Page 4, line 22: the density of the weak layer (ρ_{wl}) does not yet appear to have been defined before being used inline in the text.

Thank you for noticing this error. We will define the weak layer density (ρ_{wl}) before.

Page 5, figure 1: In this figure, please make clear in the text and caption where the a and b values came from or how they were derived.

We used the shear strength as it is implemented in SNOWPACK by default. The values of a and b for different grain types are those given in Jamieson and Johnston (2001, Table 8). They provided fits for the different grain types based on their extensive sets of shear frame measurements. We will clarify where these values come from and refer explicitly to Table 8 of Jamieson and Johnston (2001).

Page 6, line 1-2: Can you comment or add a citation for how accurate these parameterizations are? Such that if it were possible to measure the weak layer shear strength and/or the elastic modulus of the slab in the field, should this be done? Or are these parameterizations thought to be adequate?

The parameterization of the elastic modulus was derived based on laboratory measurements performed by Scapozza (2004). As suggested in Gaume et al. (2017), slab density was related to Young's modulus by a power law fit of that data. Scapozza (2004) reported a correlation coefficient r^2 of 0.9 for the original parameterization. The

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parameterization of shear strength is based on field measurements and was related to density in Jamieson and Johnston (2001). They reported correlation coefficients r^2 of 0.31 to 0.54, depending on grain type. In the revised manuscript in section 2.3 (SNOWPACK), we will clarify where these parameterizations originate from. As can be seen from the correlation coefficients the parameterizations are adequate, clearly more for the laboratory than for the field measurements. While it is clear that direct field measurements of these quantities would improve the predictions of the model, such measurements are rather difficult to perform and time consuming, in particular there exist no reliable measurement for the elastic modulus of snow. Better estimates of shear strength and elastic modulus in terms of density and especially microstructure would definitely be useful. Ultimately, SNOWPACK would greatly benefit from microstructural based parameterizations of shear strength and elastic modulus. At present, these rather simple parameterizations are the best possible available. We will also add some discussion on these parameterizations in section 4 (Discussion).

Page 6, line 16: why was a range of 5 cm chosen?

We checked whether the weak layers were close to the five lowest values in the simulated vertical profile of critical crack length (r_c). Given the vertical resolution of simulated snow layers, SNOWPACK produces many more layers than observed. We wanted to apply a relatively simple method, which would not require layer matching. Due to the differences in layer thickness between modeled and observed snow profiles, some of the five lowest values in the simulation would likely be very close to each other. Therefore, a weak layer was considered as “detected” if it was located within ± 5 cm of a minimum in the vertical profile of critical crack length. While it is clear that the threshold value of 5 cm above and below the minimum r_c value is somewhat subjective, we are confident that it is not a gross misrepresentation when assessing snow instability. We will give more detail on this approach in section 2.5 (Model performance measures and weak layer detection).

Page 6, line 17: Curious, were there ever weak layers identified in the field that could

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not be tested with a PST test? (e.g. was the weak layer ever too thin or too difficult to follow with a saw blade?) Also, what are your general thoughts on the speed at which the saw blade is moved through the weak layer? Could this affect your results?

We often observed fractures in weak layers while performing a CT and ECT that we could not test in a PST. Typically, these are weak layers that are surrounded by layers of very soft snow (e.g. new snow or very freshly buried surface hoar). In those cases, it is very difficult to visually identify the weak layer and stay in the weak layer with the snow saw during a PST. While in some cases it is possible to get PST results in such layers, generally the results will be very inconsistent. We will add some more discussion on PST experiments on page 15, lines 1-2. The speed of the snow blade does not significantly influence the results of the critical crack length as shown by van Herwijnen et al. (2016).

Page 9, line 13-15: perhaps you could further address this discrepancy in the weak layer thickness in the Discussion? Or briefly mention here that this was related to the boundary conditions chosen?

You are correct and we will address this again in the Discussion section. We will explicitly mention that the boundary conditions, i.e. the simulation time step in SNOWPACK, limit layer thickness to approximately 3 cm.

Page 13, Figure 8: I found the text to adequately describe the results and comparison to Gaume et al. 2017, would consider omitting this figure.

For clarity, we prefer to keep this Figure. It explicitly shows that the best performance for Fwl is obtained with values of the exponents $x = y = 1$, i.e. the simple product of grain size and density.

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