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Interactive comment on "Snow fracture in relation to slab avalanche release: critical state for the onset of crack propagation" *by* J. Gaume et al.

Anonymous Referee #2

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In the submitted manuscript by Gaume et al. "Snow fracture in relation to slab avalanche release: critical state for the onset of crack propagation", a Discrete Element Model (DEM) of Propagation Saw Tests (PSTs) is used to examine the effects of varying material properties and other model parameters. Analytical expressions using a Mohr-Coloumb Cap model are derived and compared to model results. An analytical expression for the critical length is derived and compared with field PSTs along with expressions from other models. I applaud the authors for their efforts, as this is an important area of snow avalanche research with problems that have not been resolved, however I suggest that this manuscript requires major revisions to become publishable.

I am not a DEM expert, but the model set up and results appear sound to me. The analytical work is also sound. My main criticism is that the analytical model does not explain previous fieldwork with PSTs [Gauthier and Jamieson, 2008; McClung,

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Discussion paper

2009; Bair et al., 2012] showing that snowpacks with identical slab thickness (slope normal) and densities showed the same or increasing critical cut lengths as slope angle increased. Note that the same behavior has been shown in Extended Column Tests [Heierli et al., 2011; Bair et al., 2012] and Compression Tests [Birkeland et al., 2014], all with the same slab thickness and density. I realize those tests were not modelled here, but the fact that the three main stability tests all show the same trend cannot be ignored. Rather than addressing this discrepancy directly, the authors dismiss previous fieldwork with PSTs by stating that the snowpack properties could not have remained the same throughout the range of slope angles tested, thereby calling into question the accuracy of the reported values in each of these studies. As the author of one of these previous field studies, I find this particularly irksome given the trouble that I went to in ensuring uniform conditions throughout the slope angles tested. Further, the authors do not present any field evidence to support their modeled slope angle dependence, i.e. PSTs showing decreasing critical length with increasing slope angle given constant slab thickness and density. Finding constant slab thickness and density over a range of slope angles in the field is not that difficult, which explains why multiple studies have been able to do it. Perhaps the authors need to re-read these studies more carefully. For instance, they state that Heierli et al. [2008] only used three PSTs to validate the anticrack model. In fact, it was 44 PSTs over three different slope angles [Gauthier and Jamieson, 2008] used, with the means taken for each of the three slope angles. Likewise, one of the major criticisms of the anticrack model has been that it assumes a linear elastic slab. This model is supposed to be an improvement in accuracy, but the authors make the same linear elastic assumption without discussing pitfalls. This may explain some of the scatter in Figure 6 for both models. Further, the evaluation of the anticrack model is flawed since a constant (0.1 J m-2) specific fracture energy is used. Field data show this value varies by more than an order of magnitude [Schweizer et al., 2011]. A constant specific fracture energy in the anticrack model is akin to setting a constant value for the weak layer shear strength in the authors' model.

The manuscript requires extensive editing to correct grammatical errors. I suggest an

English language service. For instance, there were too many errors of tense for me to correct. I gave up. Likewise, symbols are used in graphs and equations without being defined until later in the text. I suggest deleting the “Application to simulated snow stratigraphy” section, as it is not convincing with only one measured and modelled profile for comparison. Other minor corrections are included as an annotated PDF.

Bair, E. H., R. Simenhois, K. Birkeland, and J. Dozier (2012), A field study on failure of storm snow slab avalanches, Cold Regions Science and Technology, 79-80, 20-28, doi: 10.1016/j.coldregions.2012.02.007. Birkeland, K. W., E. H. Bair, and D. Chabot (2014), The effect of changing slope angle on compression test results, in International Snow Science Workshop, edited, pp. 746-751, Banff, Canada. Gauthier, D., and B. Jamieson (2008), Evaluation of a prototype field test for fracture and failure propagation propensity in weak snowpack layers, Cold Regions Science and Technology, 51(2-3), 87-97, doi: 10.1016/j.coldregions.2007.04.005. Heierli, J., P. Gumbsch, and M. Zaiser (2008), Anticrack nucleation as triggering mechanism for snow slab avalanches, Science, 321(5886), 240-243, doi: 10.1126/science.1153948. Heierli, J., K. W. Birkeland, R. Simenhois, and P. Gumbsch (2011), Anticrack model for skier triggering of slab avalanches, Cold Regions Science and Technology, 65(3), 372-381, doi: 10.1016/j.coldregions.2010.10.008. McClung, D. M. (2009), Dry snow slab quasi-brittle fracture initiation and verification from field tests, Journal of Geophysical Research, 114, F01022, doi: 10.1029/2007JF000913. Schweizer, J., A. van Herwijnen, and B. Reuter (2011), Measurements of weak layer fracture energy, Cold Regions Science and Technology, doi: 10.1016/j.coldregions.2011.06.004.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-64/tc-2016-64-RC3-supplement.pdf

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