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Comment

## ***Interactive comment on “The influence of edge effects on crack propagation in snow stability tests” by E. H. Bair et al.***

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This paper investigates the influence of edge effects on crack propagation in snow stability tests.

The authors present results from a large number of field tests (168 rather than 158 stated in the first sentence of the conclusions). The field measurements and the analyses are generally well presented, although I do have specific comments/suggestions (below). Nevertheless, the data and findings make a strong case for revisiting current procedures employed for investigating crack propagation in snow.

The authors also introduce a FEM to help give insight into the results from the field. I would encourage the authors to more clearly separate the modeling component from

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the field measurements, or at least make it clear how the model is being used. Sec. 2.4 introduces the model but physical insight on how it works is not given. For example, what is  $\Delta r$ ? Is  $G$  the energy released as the crack extends  $\Delta r$ ? What assumptions are made when using the CFD ANSYS command SENE? Why is it simpler to model bending in PST than ECT? Is “simpler” a reasonable justification for not using some other command? What is lost in the by modeling PSTs and not ECTs? I am also confused by the three different snow profiles that are used in the model simulation. I assumed that they are they synthetic profiles, but then they appear to relate to different days – are these model days or days of measurements. If the latter, which ones are they?. Do you calculate elastic modulus  $E$  (eqn 2) for each layer? How does the assumption of elastic behavior affect the results? I like the calculation of  $G$  vs length (shown in Fig. 9), but it would be informative to know how these would change with different model assumptions. Similarly with results shown in Fig. 10.

Other specific Comments:

Page 2, line 17: Can you include more explicit information about how tests should be revisited. The problem is well stated in your conclusions.

Page 5, line 24: You might spell out names of states for those not so familiar with the USA.

Page 6, line 5: Do you mean “stipulated”? Could you state that “for the PST we restricted values  $r_c < 50\text{cm}$  so results were more comparable with ECTs. ....” I could not understand the next sentence – please clarify. In Fig. 3 it looks like “ $b$ ” is width; rather than “ $w$ ” used here; to reduce confusion it would be good to be consistent.

Particle tracking section is good. Minor edit Page 6, line 19: Black markers ..... side of the beam and were tracked to measure ..... Page 7 line 4. Provide reference for MATLAB.

2.4 FEM As stated above, for readability and flow of the paper, I suggest moving this

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section to the Discussion after the field measurements have been presented.

### 3. Results from field studies

Page 9, line 6: I am confused by these results. When I check table 1 and Fig. 4, I get: For 1-2m tests, 42 out of 88 show full propagation, which is 47% For 2-5m tests, 9 out of 27 show full propagation, which is 33% For >5m tests, 8 out of 35 or 23%.

Although the conclusion is the same, what am I missing?

Page 10, line 5: Presumably this should be 1-2m tests (to be consistent with the value given in the line

3.2.1. Collapse amplitude Page 10. It might also be informative to examine the spatial pattern of collapse in context of the length of the test.

3.2.2 Wavelength Is it possible that your calculations are contaminated by the effects of bending and/or surface erosion (mentioned briefly in section 2.3) could be expanded

Discussion Results are great, but discussion of them is sparse. As mentioned above, discussing them in context of FEM would help, including possible uncertainties in the model that are mentioned briefly (Page 12, line 6).

Page 12 line 18: Statistics shown in Fig. 5 are of interest for field workers. Perhaps this result would be worth emphasizing more in the conclusions too.

Page 12 line 25. Observations shown in Figure 8 are interesting; do you have ancillary information (eg. Snow settlement, temperature) from nearby study sites that could be discussed? If nothing else, the result does show clearly that “persistent weak layers” can and do persist over timescales of at least 2 weeks . . . .

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Interactive comment on The Cryosphere Discuss., 8, 229, 2014.

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