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Interactive comment on "Influence of weak layer heterogeneity and slab properties on slab tensile failure propensity and avalanche release area" *by* J. Gaume et al.

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We thank the two reviewers for their careful reading of our manuscript and their constructive comments.

We will revise our paper in order to account for their remarks and we provide below detailed answers to the various issues raised by the reviewers.

Answer to Referee #1

Comment 1). *The model, as well shown in figure 1, takes into account the weak layer heterogeneity: if a strong zone in the basal layer is met, the tensile failure of the slab is* *likely to occur. However I expect from the model that the energy release increases as the basal crack increases (i.e. as the crack propagates far from the original spot - red star in figure 1). In other words, I expect that, as the basal crack propagates, it is more and more difficult to arrest. Dynamical effects should also strengthen this observation. Please clarify: in figure 1 it seems that the distance from the position of the super weak zone has no influence on the position of the secondary tensile slab crack.*

Answer to comment 1). As the reviewer suggests, the position of the tensile fracture in the slab, if existent, also depends on the position of the initial failure in the weak layer. This is due to the fact that the stress concentration at the crack tip (or the fracture energy, equivalently) increases naturally with the crack size. Moreover, dynamic effects are likely to strengthen this effect. However, nothing in the paper is in contradiction with this observation. This effect is naturally taken into account in the numerical simulations and thus in the statistical distributions obtained.

In order to clearly mention this important point, a new paragraph will be added at the end of Section 3. It will also indicate that the shear stress difference $\Delta\tau$ and tensile stresses σ_{xx} do not depend only on the heterogeneity, but also on the size of the basal crack through the stress concentration at the crack tip.

In addition to this new paragraph, figure 1 will be modified accordingly to clarify this point.

Comment 2). *page 6038, line 7. When referring to the typical length scale of the system "lambda", we suggest citing [Chiaia BM, Cornetti P and Frigo B (2008) Triggering of dry snow slab avalanches: stress versus fracture mechanical approach. Cold Reg. Sci. Technol., 53(2), 170–178]*

Answer to comment 2). We will add this reference.

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Comment 3). *page 6040 line 14. Change "... with increasing tensile strength from 1 to 0." into " ...*

Answer to comment 3). This is not what we meant but we agree that the sentence was not clear. It will be modified as follow: "As expected, this probability systematically decreases from 1 to 0 with increasing tensile strength σ_t."

Comment 4). *page 6040 line 23. "Stress" is written twice*

Answer to comment 4). This will be corrected.

Answer to Referee #2

Comment 1). *In the formulation of the model section, slab and weak layer properties (and their respective ranges) are presented. It may be helpful to connect those properties to actual snow properties through discussion or reference.*

Answer to comment 1). We will add in section 2 realistic ranges of the parameters used in the model for the slab (density ρ, Young's modulus E, Poisson's ratio ν, tensile strength σ_t) and for the weak layer (cohesion c and its standard deviation σ_c and correlation length ϵ) as well as the associated references in the form of a table.

Comment 2). *Section 2, lines 3-4: that sentence isn't clear*

Answer to comment 2). This sentence will be reformulated.

Comment 3). *Section 3: Maybe consider carefully offering results prematurely. In lines 13-14 a result is mentioned, maybe consider building the approach followed by results.*

Answer to comment 3). This part presents qualitatively the results which are obtained

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in the simulations (two release types) and represents a preliminary paragraph to the more detailed parametric analysis of section 4) Quantitative results. To clarify this point, Section 3 will be renamed “Preliminary description of the results: two release types”.

Comment 4). *While a proxy for tensile strength and young's modulus with density is given in section 4.2, I believe discussion on the relevance of the snow properties used in the model would strengthen the paper. Some properties are given with no link or discussion on their realism. I found myself looking to other references to verify some values, adding it to the paper would help in realism and readability.*

Answer to comment 4). See answer to comment 1) above.

Comment 5). *Section 4.1.5: The results appear very sensitive to E in very common density ranges (≈200-300 kg/m3). The discussion addresses this, but it seems that the very stark transition with small changes in E with probability of tensile failure is very interesting and may be worth more discussion*

Answer to comment 5). We would like to recall that a sharp transition of the tensile failure probability is not only observed with Young's modulus E, but with most of the tested parameters (slab thickness D, tensile strength σ_t and coefficient of variation CV). In addition, the transition with E is not as sharp as it may seem since E is represented in log-scale in Fig. 5. Furthermore, note that for a real snow cover, when E increases, σ_t increases as well but with opposite effects on the tensile failure probability leading to more complex interpretations (as already stated at the beginning of paragraph 4.2). For these reasons we argue that this point does not need further discussion as it might disrupt the reader.

Comment 6). *Section 4.2, line 22: Large E (stiff) doesn't not imply “strong” snow. They*

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are not the same thing.

Answer to comment 6). We agree with this comment. The word “thus” will be removed.

Comment 7). *Section 5, lines 9-10: awkward sentence*

Answer to comment 7). This sentence will be reworded: “These simulations explained why hard and thick snow slabs are more prone to wide-scale crack propagation than soft slabs.”

Comment 8). *Section 5, lines 16-17: Could this be discussed more? The physical interpretation seems evident, why not expand the interpretation for the reader?*

Answer to comment 8). A sentence will be added to expand the interpretation that thick slabs induce a smoothing of the WL heterogeneity and thus lead to wider release areas.

Comment 9). *Section 5: When discussing release area, maybe remind the reader what the coincident probability implications are too.*

Answer to comment 9). We are not completely sure what the referee means by “coincident probability implications”. We think that the referee might refer to the fact that the exceedence probability of the release width from natural and artificial releases are almost the same. The interpretation of this observation is that the release area is mostly influenced by terrain characteristics (possibly smoothed by the snow cover distribution) and slab properties (density and thickness). This interpretation was already clearly mentioned in this section (page 6046, lines 4-5). Nevertheless, we will add a new sentence in this part to clearly mention that the width of the release zone does not seem to be influenced by the triggering mode, as supported by Fig. 7.

Interactive comment on The Cryosphere Discuss., 8, 6033, 2014.