Reply to Referee #1 (E.A. Podolskiy) RC C2912

I enjoyed reading this well-written and carefully prepared manuscript proposing an objective instability assessment technique, which is certainly addressing a problem at the core of snow avalanche forecasting. The authors analyze tens of Snow-Micro-Penetrometer (SMP) and Propagation Saw Tests (PST) against finite element (FE) predictions supported by previous analytical solutions in order to justify the proposed methodology, which is making an important step out of observer-dependent instability evaluation.
To me, clearly presented rationale, methods and results, supporting the developed approach, seem convincing and valuable for a wide community of snow avalanche professionals and snow scientists. Below I am listing only several minor remarks and points requiring, in my opinion, some more details or explanation.

Abstract
Since the failure initiation criterion is a function of additional stress due to skier loading, this should be mentioned in the Abstract. E.g., L18: ". . . method for estimating snow instability {under skier loading}." Doing so in the title is indeed your own decision.

> *We agree that the mass of a skier is considered for the failure initiation criterion. However, the crack propagation is not linked to any kind of external loading. As we present two independent criteria, we do not prefer to introduce this limitation in the Abstract.*

p. 5827, L15
Provide a reference reporting such field observations.

> *We will insert a reference to Perla (1977).*

p. 5827-5829
Somewhere in your review I advise you to mention a work by McClung (2009), which is strongly related to the domain of your paper.

> *We will refer to the work by McClung (2009) as suggested.*

p. 5829, L29
"force-distancesignal" - missing space

> *We will change as suggested.*

p. 5833, L1-3
Here you describe derivation of the penetration depth and I could not follow which one do you mean. For example, in Fig. 3 the x-axis shows Depth, so that Force=f(Depth).
So, before plotting, you need to cut off air signal from snow signal to get the snow surface? I suggest to specify what are you talking about here. - To indicate better my confusion: you mean that the penetration depth, let's call it D, is obtained from raw force-distance signal: 0.0036=Int(D,0) F(z) dz so this D stands for what? Does this penetration depth correspond to air/snow interface, or is it somehow related to the weak layer through wf? The lower boundary is fixed or sliding?

> *To improve clarity we will insert the formula and specify that the integration starts at the snow surface.*

p.5834, L18
What was the skier penetration depth and how was it evaluated?

The skier penetration depth was derived as in p. 5833, L1-3, but we will improve its description (see reply to above comment).

p.5834, L19-20
I am afraid that without more details it would be hard to reproduce this step of snow compaction in someone's model. You could better explain how it was done. So, the density below the strip of a width a was assigned with a new density 300 kg m−3 and thus new modulus 16 MPa until some penetration depth (which was not shown explicitly). However, I am not sure how do you realize it numerically - does it involve some 2D changes in the field of density? Since the penetration of plate into snow with corresponding compaction is by itself a topic for a research paper (e.g. Mohamed and others, 1991) more details would help.

We will insert "i.e. density and thickness of slab layers were manipulated" to make it clear that we changed the layer properties before FE modelling.

p. 5834, L28
A matter of taste, but nevertheless: would not it be informative (if meaningful) to indicate the range of theta giving you the maximum shear stress? Any skiing reader will be interested to learn these numbers.

Theta is not constant but varies between 53° and 60° for slope angles ranging between 0° and 50° (Schweizer, J., 1997. Contribution on the skier stability index. Internal report, 712, Swiss Federal Institute for Snow and Avalanche Research, Davos, Switzerland).

p.5834, L21-22 & p. 5839, L1-3 & p. 5840, L13-15
"A fixed value of the Poisson's ratio . . ." - I am wondering if this could also contribute to predicted values of critical cut length (Sect 3.2). Some studies proposed to use the ratio as a function of density (Sigrist and others, 2006; Sigrist and Schweizer, 2007; Mellor, 1975; Teufelsbauer, 2011). Usually, constant value works well and has little influence, however, usage of some analytical solutions (e.g., Heierli and Zaiser, 2006) shows that the critical crack length can vary for several % as a function of the ratio. Same may be said about roots of your expression (Eq.6). Perhaps, this will be of minor importance in improving the agreement, but nevertheless is worth checking with sensitivity tests for dropping away insignificant factors in future work.

We agree, the influence of the Poisson's ratio is rather minor. The influence of the Poisson's ratio will not affect the results as the uncertainties involved with SMP derived snow properties are within 10-20%.

p. 5835, L5
"slab larger than the ski{er} width (0.2m)." -> ". . . the ski width. . ."?

We will change to "width of skier load".

p. 5836, L 6
What is gamma and how did you select it (same for Eq.7)? It appears to be even more important than Poisson ratio for high slab thickness if varies between 0-2.

We will specify gamma which is the mismatch parameter (Heierli et al., 2008).

p. 5837, L13
"Eq. (6) is then solved" - with help of what? I recommend to specify your media for this here.

We will clarify as suggested.

p. 5839, L4
Since the values of the critical cut can vary from 10 to 60 cm it is important to indicate here the relative mean % to highlight how good the agreement actually is.

We will also provide the mean absolute percentage error.

p. 5841, L14-18
Here, I recommend to direct a reader to a work where some steps in this direction have been previously made (e.g., Mahajan and Joshi, 2008).

We will add the reference to Mahajan and Joshi (2008).

p. 5842, L17-19
Even if the approach and its quantitative nature are indeed novel and original contribution, I nevertheless suggest to put some reference here. Because, as far as I know, the necessity of holistic view to snow avalanche release has been in the air for quite some time (e.g. McClung and Schaerer, 2006 or McClung, 2009). So that this sentence reads as something like: "Whereas previous authors noted a need of holistic approach to avalanche initiation [. . .refs], and we anticipated this finding (i.e. that both conditions have to be fulfilled), we are not aware that it has been demonstrated before."
Also, I think it would be honest to mention in Discussion or Conclusions one of the difficulties which may limit a direct utilization of your approach by snow professionals - a need to rely on FEM in order to evaluate skier-induced stresses at weak layer depth, deltatau , for a given snowpack stratification for obtaining the S. Perhaps, you could also share you vision or idea what to do without this deltatau ; say, make compromise and rely on analytical solution for a uniform slab (Fig. 4) which is, however, not a good approximation for predicting rc?

We agree that a "holistic" approach is needed for avalanche forecasting, i.e. that one should not make any decisions based on a single in situ test – as pointed out e.g. by McClung (2009). However, we refer to the two important processes in avalanche release – failure initiation and crack propagation – to be considered in point stability estimation.
To this end, McClung and Schaerer (2006) clearly stated that the appropriate measure for assessing snow slope instability is considering the balance between a shear stress intensity factor and shear fracture toughness (p. 80). In other words, they consider propagation as the only decisive factor since dry slab avalanches initiate by propagating shear fractures.

We are of course aware that it is always a long way into practice but we are convinced that today it is time for more sophisticated approaches that include numerical simulations – and many people carry a smartphone with sufficient computing power these days. So maybe we're able to model the deltatau right away in the field in future.

Cited references

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Mahajan P and Joshi SK (2008) Modeling of interfacial crack velocities in snow. Cold Reg. Sci. Technol., 51(2–3), 98–111, doi: 10.1016/j.coldregions.2007.05.008
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