Reply to Referee #1

We thank referee #1 for the thorough review and believe that these comments will be very helpful for preparing an improved revised manuscript. In the following we reply to the comments in detail and describe the changes we intend to make in the revised manuscript.

SUMMARY:

The authors monitored the temporal evolution of a weak layer-slab system during winter 2014-2015 in a field site located next to Davos. Typically, each week between 6 January 2015 and 3 March 2015 (8 days of measurements), they performed on the same site located next to an automatic weather station:

- *three propagation saw test (PST) on which they measured the critical crack length, the full or partial crack propagation and the slab displacement field (PIV measurements),*
- *around five SMP profiles,*
- *a classical manual snow profile with a density profile*
- *CT/ECT tests.*

The authors try to explain the observed temporal evolution of the PST critical crack length (general increase with a minimum the 28 January) by investigating the evolution of individual mechanical parameters of the weak layer and slab, namely the load on the weak layer, the weak layer fracture energy and the so-called bulk elastic modulus; and their interaction through the anti-crack model. They used previously developed methods to access these parameters from the measured data. They also used the SNOWPACK model to compute the critical length from the simulated snow profile with meteorological forcings from the automatic weather station. The authors show that monitoring the evolution of individual parameters cannot explain the observed critical crack length trend but that it is necessary to account for the complex interaction between these mechanical variables. The SMP metric is not able to reproduce the observed critical crack length. The SNOWPACK metric shows also an increase of the critical crack length.

GENERAL COMMENTS

The dataset collected by the authors is very interesting combining quantitative stability analysis (PST critical crack length) and highly resolved vertical hardness profile (SMP). Some of the results are of clear interest to the snow and avalanche community: the authors showed that both slab properties and weak layer cannot be individually monitored to understand the crack propagation propensity evolution; they also show that the previously developed SMP stability metric is not capable of capturing the evolution of the critical crack length. However, the methods are not well presented and appear as a black boxes where explanations on the basic assumptions are missing and the methods are mixed without an apparent logic. In particular, the SMP stability metric presentation is not clear in this form. Evaluating the stability metric of SNOWPACK from a modeled snow profile without showing that the modeled snowpack profile has something in common with the observations is not informative. The sensitivity analysis on a three parameters analytic function is based on four single cases. The trend analysis gives too much importance to a single day case that might be not statistically representative. Therefore, I recommend major revisions before publication.

- We agree that our description of the methods is minimal mainly referring to previous work. We will change this approach and provide more details on each of the methods we use.
- We will provide information on the SNOWPACK simulations so that the reader can assess whether the simulated stratigraphy has something in common with the observations (see below).

- The sensitivity analysis is exemplary and focusses on the temporal evolution. This paragraph was meant to illustrate how the various factors interact. We will change the title as it is obviously not a complete sensitivity analysis.
- With regard to the temporal evolution and the observed minimal values towards the end of January, we will discuss the representativity more thoroughly. We would like to point out that minimal values of snow instability tests are in general more trustworthy. In the case of the propagation saw test, any measurement and observation errors increase the cut length. Low values of the cut length therefore almost always represent the real conditions.

MAJOR COMMENTS:

1) The dataset collected by the authors is very valuable. Indeed, the authors present it as the first comprehensive time series of a weak layer, slab system. It uses state-of-the-art measuring techniques (SMP, PST) combined with "traditional" measurements (manual stratigraphy and density, CT/ECT). Since one of the objective and strength of the paper is this dataset, it appears logical to provide this dataset as supplementary files (Caaml file for stratigraphy, stability tests, text file for SMP and avi file for PST videos).

Whereas it is generally a good idea to provide the data, we believe sharing the data is not straightforward. We are not dealing with 'simple' weather data, but with data from various sources (SMP, PTV, SNOWPACK, profiles), which then have to be processed to get to the results. Furthermore, the processing is not trivial. Hence, we rather prefer to provide some of the data in the supplementary material. In particular, we will provide the manual profiles including an SMP profile for all days in the supplement and add a figure to the main text showing the SNOWPACK simulation (see below). And of course, we will provide the data on request to others who like to collaborate.

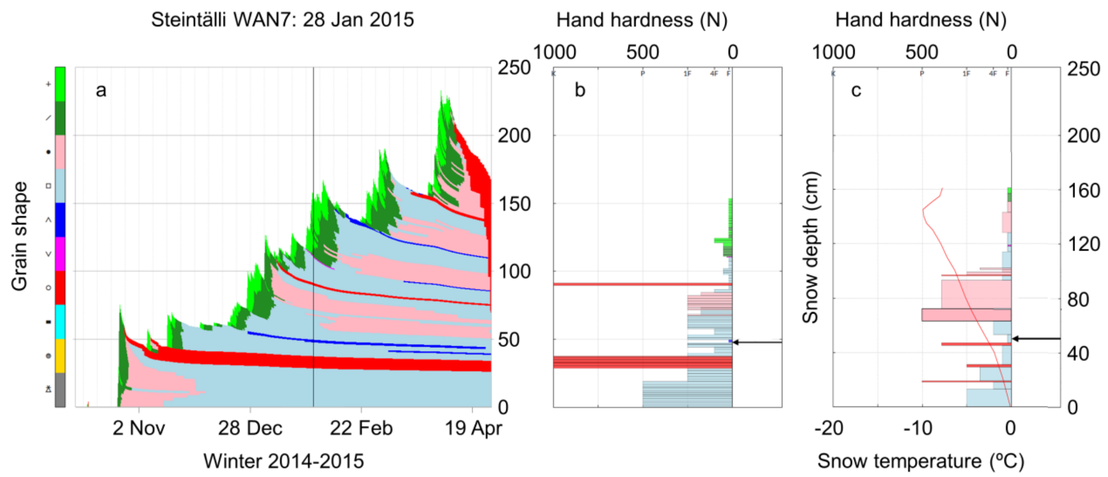

Figure: (a) SNOWPACK simulation for the location of the automatic weather station (AWS) WAN7 for winter 2014-2015 showing the evolution of grain size, (b) simulated snow profile for 28 Jan 2015, (c) manually observed snow profile at the location of the AWS on 28 Jan 2015. Arrows denote the weak layer.

2) The writing style on the mechanical background is often unscientific and requires precision and consistency. I have listed some of these problems:

- *about the elastic modulus. You used the following terms without proper definition:"elastic modulus", "bulk modulus", "modulus", "effective modulus", "bulk effective modulus", "mi-*

cro-mechanical modulus", "slab modulus", "stiffness", "elastic modulus with non-elastic parts of deformation". This vocabulary is misleading and is not suited for a scientific paper, where the mechanical concepts behind the used model should be precisely presented, which can be done in a simple way accessible to the snow community.

We acknowledge that the vocabulary on the deformation behavior of the slab may be hard to follow and will carefully revise the manuscript to account for this issue.
The problem arises from the fact that model assumptions, e.g. linear elasticity, do not fit what is actually observed and can be measured in the field; in addition, the slab is layered and not uniform. Therefore, there is some need for specific terms and it is not sufficient to just talk about the modulus. For example, the modelling approach by Heierli et al. (2008) includes the elastic modulus (Young's modulus), what we measure with PTV is an effective bulk modulus (bulk because layering is disregarded, effective because it includes not only purely elastic parts of deformation), what is derived from the SMP is the micro-mechanical modulus.
We will explain these subtleties in detail in the revised manuscript.

- *you use the terms "propagation propensity", "propagation criterion r_c_SMP" ,"critical crack length", "propagation propensity metric", "crack propagation propensity" to refer to the same parameter r_c, or maybe not but this is not clear. Why don't you use consistently the well-defined "critical crack length" and explain only in the introduction that the critical crack length is an indicator of the more general concept of crack propagation propensity?*
- *"initiation probability", "initiation propensity", "initiation criterion", "initiation indices", "skier stability index" ...*
- *delete vague and unspecific claims "reliable", "reliable in general", "distinct pattern", "relevant mechanical properties", "other mechanical properties"*

We will re-consider the use of the terms in connection with failure initiation and crack propagation. As we present measured as well as modelled values there is some need for distinction between the various measures.
We will thoroughly go through manuscript and remove vague and unspecific terms.

3) It is hard to follow the history of the weak layer-slab system. It is necessary to add a one-page figure with eight sub-figures (one for each day of measurements) showing the manual stratigraphy (at least snow type and density), a SMP profile and the position of the weak layer.

We will provide a figure showing the SNOWPACK simulation as well as modelled and observed profile for a specific day (see above). In addition, we will provide all the manually observed profiles including an SMP profile in the supplementary material.

*4) In Heierli's model, the total mechanical energy of a PST crack of length r is composed of two terms: V(r) = w_f * r + Vm(r) where w_f * r is the weak layer fracture energy and Vm(r) accounts for elastic deformation energy and changes in gravity potential energy of the slab. In case of a uniform slab, Vm(r) can be computed analytically knowing the density, thickness and elastic modulus of the slab. In case of a FE model of a multilayer slab (density, thickness and elastic modulus per layer known), Vm(r) can be calculated numerically. This is done for the SMP analysis. In case of a measured displacement/deformation field of the PST tests, Vm(r) can also be calculated.*
This is done in the PST analysis. In both cases (SMP, PST method), the calculated Vm(r) is used to fit the analytic mono-layer solution. The fitted analytic solution is then differentiated to obtain the critical crack length knowing the weak layer fracture energy (SMP method) or the

weak layer fracture energy knowing the critical crack length(PST method). I don't understand why the dVm(r)/dr is not computed directly from the calculated Vm(r)(or with smoothing of Vm(r)). This is not explained in the proposed references (Reuter et al, 2015 or van Herwijnen and Heierli, 2010). The bulk elastic modulus is a fitting parameter and it is unclear how physically-relevant it is. There is no clear reason why Vm(r) on layered material should fit directly the mono-layer analytic solution. Provide a proper explanation and discussion on that. Moreover, recall the main hypothesis (elastic linear, only the slab contributes to deformation energy) of Heierli's model.

We will provide a proper explanation, and also refer to the recent paper by van Herwijnen et al. (2016) where the PTV method is now explained in detail. We will use their refined approach, i.e. the adjusted mechanical energy to account for differences between the model of Heierli et al. (2008) and the FE simulations.
Taking the derivative of the raw data to derive w_f would not work, as there is too much scatter and this would result in very unreliable values of w_f.
We agree that the critical cut length can be computed with the FE model using the SMP slab properties and the SMP-derived specific fracture energy w_f, but would require an iterative approach to find r_c.

5) Section 2.4 describing the SMP signal processing is vague and unscientific. Many critical details are missing. It does not allow the reader to reproduce the presented method and appears as a black box. It requires a deep rewriting. It mixes method using different concepts that measures the same things differently e.g. Johnson and Schneebeli (1999) and shot-noise model used by Proksch, 2015. The window size for analysis, the SMP version, the adjustment parameters of (Proksch et al, 2015, calculated on a few alpine snow samples), the finite element layer mesh, etc. are missing.
There is additional linear scaling with no convincing explanation. The calculation of layer Young's modulus from SMP elementary failure element is known to be poor and is inconsistent with the one based on density (Scapozza, 2004) used by the snow cover modeling (p5 l30). The failure initiation criterion S is not detailed and it is hard to notice that it does not incorporate snow load in comparison to SK38 which does, ... The reference to other papers is far from being sufficient and clear explanations won't take more than 30 lines.

As mentioned above we will provide more details in general and in particular on the methods and not simply refer to previous work.
The additional linear scaling is simply introduced to obtain SMP-derived values that are comparable to PTV-derived values. This is clearly described as such. It is clear that SMP-derived values have some deficiencies, see Reuter et al. (2013), and we will discuss this more thoroughly in the revised manuscript. Furthermore, we will consider as an alternative approach, deriving density from the SMP signal, and then directly use a parameterization such as provided by Scapozza (2004) to obtain a modulus.

6) The authors used the snow cover model forced by a nearby automatic weather station as an input of a new critical crack length estimator (Gaume et al. 2014a, 2016). Without any clue on how close the snowpack simulation to the observed snowpack, it is impossible to exploit the results of this analysis. It is well-known that one point evaluation of a snow cover model on stability criterion is difficult. Note that the only variables missing in Eq. (1) is the weak layer strength that could be fitted to get r_c_snp = r_c_obs, similarly to what is done for the PST.

Additionally, it is not clear to me how the avalanche activity index (concerning the areaall around Davos?) can help to analyze the measurement done in this particular site.

The SNOWPACK simulation reproduced the snow stratigraphy reasonably well. We will provide the simulated stratigraphy in a new Figure (see above).
We certainly agree that stability predictions from simulated snow stratigraphy are challenging. We strongly believe that these stability predictions should be validated at the locations of automatic weather stations.
With regard to the comment on Eq. (1), we agree that the only missing variable is the weak layer strength, however, we are not sure we understand the reviewer's point. The shear strength cannot be determined form the measured critical cut length, otherwise the model would no longer be predictive. The shear strength is obtained from the parametrization implemented in the snow cover model SNOWPACK based on the work of Jamieson and Johnston (2001).
As we perform our measurements in a representative study plot commonly used in operational forecasting to extrapolate to the surrounding terrain (e.g., Gauthier et al., 2010; Jamieson et al., 2007), we added the avalanche activity data for comparison with the local stability evaluations.

7) The pattern of the PST critical crack length is a general increase with a local minimum for one measurement day (28 January). As discussed (p6 l20-23, p10 l3-6), the spatial variability can significantly affect the stability even a few meters away. Given the poor representativity of one day of measurement to define a trend, and potential spatial variability, it would be reasonable when speaking of trend to not focus on the minimum observed the 28 January but on the general trend (continuous increase ofr_c). Note that this does not challenge the fact that the SMP should reproduce the same trend (since measured a few cm away from the PST); but the comparison with SNOWPACK is challenged. The explanations "we deem it unlikely that the observed pattern is entirely the result of spatial variability and does not reflect the temporal evolution", "Previous studies performed in level study plots have shown that measurements in general are reliable and that the effect of spatial variations is relatively small" are not convincing, at least in this form.

We will re-consider the local minimum that we observed at the end of January 2015. In fact, low critical cut lengths were not only observed on 28 January but also on 5 February. On 5 February there are only two measurements with a large difference between them. However, low PST results are in general more trustworthy than high ones, if they concurrently occur, since any error while performing the test will increase the cut length. Furthermore, on 28 January 2015, for the first time, all cracks propagated to the end of the PST column indicating that the crack propagation propensity had increased. Finally, the additional loading towards the end of January 2015 resulted in many avalanches and shooting cracks were frequently observed also indicating increased propagation propensity. We will re-assess the issue of measurement accuracy and spatial variability, reword the corresponding statements and clearly denote them as interpretation.
The sentence "*Previous studies performed in level study plots have shown that measurements in general are reliable and that the effect of spatial variations is relatively small*" is supported by two references to previous work just following this sentence (page 10, lines 11-14).

8) The sensitivity analysis is poor and based on four different cases. To my opinion, this cannot be called a sensitivity analysis. Differentiating Eq. (2) with respect to E, sigma and wf provides a way to perform this sensitivity analysis properly.
Note that the general comments are general and require re-wording of several parts ofthe paper and additional explanations, and not only taking into account specific minorpoints listed below.

The purpose of this paragraph is to illustrate how changes of the modulus, the load and the specific fracture energy with time will affect the temporal evolution of the critical cut length. We will no longer call this a sensitivity study, but select a new title for the paragraph: Case studies.
We agree, that differentiating Eq. (2) with respect to E, σ and w_f would reveal the dependence of the critical cut length for a single parameter. However, these dependencies, considered independently are obvious: the cut length decreases with increasing load, and increases with increasing slab modulus and weak layer fracture energy. However, their interplay in course of time cannot easily be assessed – and the four examples we provide simply show that entirely different evolutions are possible.

MINOR COMMENTS:

abstract: the following terms are too vague : "distinct pattern", "other mechanical properties""some of the relevant mechanical properties"

We will clarify the terms or remove them.

p1 l25: "how much stress due to a skier is transferred".Misleading sentence. All thestress is transferred to the ground. But it is distributed on a larger surface. Reword.

We will reword the sentence: *"… the slab layers determine the magnitude of the stress due to a skier at the depth of the weak layer"*.

p1 l28: "with respect to the weak layer, a snowpack a weakness is" -> "the weak layeris"

We will reword the sentence as suggested.

p2 l2: "conceptual model". Describe this model in a few words.

We will add some explanations as suggested.

p2 l7: "though the strengthening may lag behind the loading". Sound unscientific.Delete.

We will reword to: *"… the rate of strength increase may lag behind the rate of loading."*

p2, l27: References to the model Surfex-Crocus (Vionnet, V. et al. Model Development The detailed snowpack scheme Crocus and its implementation in SURFEX v7.2. Geoscientific Model Development 5, 773–791 (2012)) and Mepra (e.g. 1. Giraud, G.MEPRA an expert system for avalanche risk forecasting. in International Snow Science Workshop 97–104 (1992)) are clearly missing.

Thanks for pointing this out. We are certainly aware of the French model forecasting chain and rate it highly. However, it is unclear to us how the temporal evolution of strength is modeled, and in general how the strength is derived. We are not aware that in the various publications about MEPRA this is described in detail. As far as we know, in the paper by Vionnet et al. (2012) the words "strength" and "cohesion" do not even appear. In Giraud (1993) there are also no details given on how the strength is determined. If the reviewer can provide the reference where the function is described we are more than happy to include it.

p3 Section 2.1: Is the snowpack completely dry during measurement period?

Yes, the snowpack was completely dry – apart from some melting at the surface in early January resulting in a thin crust (see Figure above).

p4 l1-2: “The weak layer . . . December 2014”. Explain how you know that.

We know as we closely follow the snowpack evolution and are in the field several times a week. This was the decisive weak layer at the end of December 2014. As mentioned on page 4, lines 2-3, there are no profiles available that were performed at fracture lines to support this assumption, but the particular weak layer consistently showed up as the primary failure layer in snow instability tests in the days following the avalanche cycle.

p4 l2-3: “While no fracture . . . January 2015”. I don’t understand. Reword.

See reply above. We will reword the sentence in the revised manuscript to clarify the argumentation.

p4 l7: “The manual snow profile served as a reference”. Do you mean that you performed-manual stratigraphic matching to adjust the other snow profiles to the manualprofile?

The manual snow profile served as a reference to, for example, indicate the depth of the weak layer or other prominent layers.

p4 l10: “at least three PST”. It appears from Figure 1a) that there two other dates where less PST were performed.

As mentioned on page 4, line 15 some test results had to be discarded since the cut was not performed consistently close to the interface which we only realized once we analyzed the videos. For that reason, we only have two test results on two days.

p4 l14: "we cut the layer of faceted crystals at its upper interface". One of the main difficulty of the PST is to follow the weak layer of interest. As explained in Section2.1, there was another FC layer just above the weak layer of interest. Showing the SMP profiles (see main comments) could help the reader to evaluate the likelihood of deviation of the saw cut in the weak layer.

As we filmed all tests we can easily assess whether the tests were properly performed – and have of course done so. As mentioned above, we will provide one SMP profile per measurement day in the supplementary material. However, the SMP profiles are less suited to assess a potential deviation while cutting the weak layer.

p4 l18: Give version of SMP.

We used SMP version 2.

p4 l25: "the displacement of the markers was used to estimate the mechanical energy Vm (r) with increasing crack length". As far as I understand, at this step, you also need the load, i.e. the density of the manual profile. Add explanation if this is correct.

Thanks for pointing this out; we will add that the density of the manual profile is used to evaluate the mechanical energy.

p4 section 2.3: The critical crack length of the modeled PST is inherently equal (or very close) to the observed critical crack length since the observation is used to fit w_f. This might not appear clearly to the reader. Please add this kind of explanation.

The critical crack length is modelled from the weak layer fracture energy w_f as derived from the SMP. It is independent of the observed critical crack length. We will clarify this in the revised manuscript.

p5 l28: "the shear modulus of the weak layer which was estimated". How ?

Following Gaume et al. (2016) we used a constant value of the shear modulus G_{WL} = 0.2MPa according to the laboratory experiments performed on snow failure by Reiweger et al. (2010); for the Poisson's ratio of the slab we assumed a value of 0.2.
We will reword this statement to: *" … , for the shear modulus of the weak layer a constant value (0.2 MPa) was assumed, based on the laboratory experiments by Reiweger et al. (2010)."*

p5 l30: I suggest to explicitly indicate the power law relation used here.

We will provide the relation as suggested.

p6 Eq2: To my opinion, this equation in this form does not give any information to the reader. Delete or give detail on all terms.

We will add the terms as suggested.

p6 l23-26: "By then, the weak layer of ... resulting in a load of almost 4 kPa." Belong to the load section 3.2?

This part of the sentence simply makes the link between slab thickness and density on one hand and load on the other hand so that the reader can better relate load values to commonly used parameters such as slab thickness.

p7 l29: "0.3 J m -2 to about 1.5 J m -2". Recall that this range results from a linear scaling between w_f_SMP and w_f_PTV.

We will mention the scaling here again.

p8 l3: S = shear_strength / skier stress should be described in Methods. Adding two lines of description is not a big deal and would clarify the message. See main comments.

As mentioned above, we will introduce the SMP metrics in more detail in the Methods section of the revised manuscript.

p8 l10: SK38 = shear_strength / (skier stress + weight_stress) should be described in Methods. See main comments.

We will introduce the SK38 in more detail in the Methods section of the revised manuscript.

p8 l22-24: The CT/ECT tests could be better used to evaluate the initiation criteria(SMP, SK38).

Thanks for this suggestion; we will also discuss the CT/ECT results with respect to the initiation criteria in the revised manuscript.

p9 l14-18: I don't understand this paragraph. The rc_obs is used to compute w_f_PTV. That w_f_PTV as input in Heierli's model gives the same trend for r_c does not appear to me as a finding ??? Clarify.

This paragraph is to illustrate that under certain assumptions for the temporal evolution of E, σ and w_f the critical cut length can at some times decrease and at others increase. The values of E, σ and w_f were taken such that they overall about mimic the observations, but were not identical to them. These are, as mentioned above, just case studies to illustrate how the various parameters interplay.

p9 l27-28: "Only when the load had reached 2 kPa, all cracks fully propagated towards the end of the column. This finding suggests that the slab was initially not strong enough to support the propagation". I don't understand the logic link between these two sentences (load/strength ?). Clarify.

We suggest that the tensile strength of the slab was initially not large enough so that cracks did not propagate to the very end of the column, but slab failures occurred. Slab density generally increases with increasing load, and tensile strength also increases with density. We will clarify this in the revised manuscript.

p10 l7 "5.9 cm". This is not a range.

Thanks; we will change range to difference.

p10 l15-19: "The errors associated with the parameters ... the dots in the PTV analysis)."This a new info that belongs to Methods and Results sections.

We think it is common practice to discuss errors and uncertainties in the Discussion section, but we will re-consider where to best put this information in the revised manuscript.

p10 l19-22: Adding error-bars on the figures 2a, 3a would help to illustrate this discussion. Moreover, you might go further in this discussion. Indeed w_f depends only onone layer whereas E is an integrated value on the slab layers and might thus be less sensitive to the spatial variations of one layer.

We will indicate these typical errors in Figures 2a and 3a. Thanks for the suggestion; we think the difference in reproducibility is not related to spatial variations.

p10 l26: "validated" -> "evaluated"

Reuter et al. (2015) in fact validated their SMP-derived metrics with independent observations. Hence we prefer to keep validated.

p11 l3: "is in line with the observations in particular when considering the CT and ECT scores.". What are the others ?

We refer to avalanche activity and will do so explicitly in the revised manuscript.

p11 l10: "– suggesting that the propagation propensity decreased". Delete

We agree that this statement is redundant, but we think it helps the reader to digest the message.

p11 l10-11: "This behavior follows from the fact that two of the essential variables, the bulk modulus and the weak layer shear strength also increase with time." From your sensitivity analysis (figures 6a,b) and the fact that you get the same results for Eq. (1), this is not a sufficient explanation.

We explain in detail in the following lines why we think that r_c^{SNP} shows this behavior. We will further clarify this in the revised manuscript.

p11 l14-15: "However, it seems premature to rate this metric as it has to be considered as being still in an experimental state." I agree this is a very valuable criterion to help to synthesize the data of snowpack models. However, the explanation is evasive. To my opinion, evaluation of this metric on one point stability observations with potential errors in meteorological forcing and SNOWPACK modeling is the main problem. See main comments. Delete or reword.

We are not aware of any more appropriate way of validating parameters derived from modelled snow stratigraphy other than with measurements in study plots surrounding an automatic weather station. We strongly believe that snow instability predictions from a numerical snow cover model need to be validated with fracture mechanical experiments, or in-situ snow instability tests in general, directly at the location of the weather station. The model of course needs to be driven with these local data otherwise there is already an unknown spatial bias.

p11 l19: "The parameter most strongly influencing the critical cut length seems to be the load". Not shown in results. Can be quantified. See main comments.

We agree that this statement is not supported since we missed to previously mention this in the Results and Discussion sections. We will refer to this result earlier in the revised manuscript.

Figures: what is the running median smoother (kernel size?)

It is a running median with window size 3.

Figure 1: a) give r_c in m for consistency. b) indicate in the figure what is the black solid line.

The black solid line is described in the figure caption: load as provided by SNOWPACK.

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