

## ***Interactive comment on “Acoustic Emission investigation for avalanche formation and release: A case study of dry-slab avalanche event in Great Himalaya” by Jagdish Kapil et al.***

### **Anonymous Referee #1**

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The main goal of measuring acoustic emissions (AE) in snow is to predict avalanche release. This paper presents data from AE measurements from an array of sensors placed in an avalanche starting zone prior to the release of a natural avalanche. To the best of my knowledge, these are some of the very first AE measurements prior to catastrophic failure of a snow slope. The data presented in this work are therefore extremely interesting, and the paper therefore warrants publication. However, the paper is currently rather poorly structured, the description of the results is mostly qualitative, the presentation of the data is rather unconvincing and the wording used in the paper is very imprecise. As such, major revisions are required before the paper can be considered for publication.

The manuscript would be much more valuable if the authors focused more of their efforts on predicting / forecasting avalanche release based on AE signal characteristics. Indeed, the authors show there is increased AE activity up to 13 hours prior to avalanche release. However, it remains unclear if this increased activity can be exploited to predict catastrophic failure. In my opinion, a more in-depth analysis in this direction, as for instance in the work of Faillettaz et al. (2015) and the references therein or the work of Capelli et al. (2018), would be the most valuable addition of the paper. In this context, the authors should also consider deriving other parameters from the AE measurements.

The main result of the paper is the definition of five snow instability states based on the observed changes in AE towards avalanche release. The authors arbitrarily define these 5 snow instability states (Table 5) and associate certain threshold values for  $\beta$  with these. The definitions of these instability states are not provided. The method used to define the threshold values is also not explained. It thus remains unclear what the differences are between the instability states, and how useful the threshold values are in terms of predicting avalanche release, the stated goal of measuring AE in snow. The authors need to provide a much more convincing case for why five different stability classes are required, and what these mean in terms of failure prediction.

The most simple case would be to have two instability classes (stable and unstable) and evaluate a simple predictive system based on crossing a  $\beta$  threshold value, similar to the simple forecasting model suggested by van Herwijnen et al. (2016) (and references therein). Once the  $\beta$  value crosses the defined threshold value, an alarm is raised for a defined duration. If an avalanche releases during this alarm, the avalanche was correctly predicted. Such a simple alarm system with two variables ( $\beta$  threshold value and alarm duration) could then easily be evaluated in terms of predictive performance (e.g. Probability of detection and False alarm rate for a given threshold value and duration) to obtain the best suited alarm system. Of course, a more complex model could also be envisioned, provided the different instability states are clearly defined.

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Overall, the manuscript should contain more quantitative analysis of the data, beyond the rather qualitative description currently given. For instance, the authors suggest that an increase in AE count and signal strength were observed prior to avalanche release. While broadly speaking this is the case, when looking more closely at the figures it becomes clear that prior to the release of the avalanche, both the count and signal strength decreased drastically. This point is not addressed in the manuscript. Perhaps it is related to inaccuracies in the release time of the avalanche. Nevertheless, this should be discussed more thoroughly in the paper. Also, the authors did not analyze signal properties with distance to the avalanche, although this could easily be done with the array of sensors they installed.

Different sensor and arrestor types were used. For me, this complicates the interpretation of the results, as it is not clear to me if the observed differences between the sensors are caused by differences in instrumentation, distance to the avalanche, or related to snow instability? These issues should be addressed more thoroughly in the paper, preferably in a quantified manner. While it is clear to me that this is perhaps difficult to do, at the very least the authors should address this point in the discussion.

The overall structure of the paper is not ideal. There is no clear and succinct description of the site (e.g. Figure 3 should also include a map showing the location of the site), instrumentation and the methods. For instance, the authors mention that the AE sensors were deployed on an avalanche slope. However, it is not specified when and how this was done, and at what depth in the snow cover the sensors were placed. Furthermore, results and discussion are continuously mixed, and the text is often repetitive. The authors should restructure the manuscript and improve the readability. Furthermore, there are many grammatical errors throughout the paper (for instance the first two sentences of the abstract), and I would encourage the authors to seek the help of a native English speaker to go over the manuscript before resubmitting it.

Throughout the paper, there are numerous instances of inaccurate wording, poorly defined terminology and many unfounded statements. For instance, the authors write

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that changes in  $\beta$  correlated to progressive failure / damage (e.g. lines 437-438; lines 484-485; lines 486-487). However, failure or damage were not measured, and it is thus not possible to correlate both. In this context, it is also not clear how the stability threshold values in Figure 9 were determined. The authors should provide a description of the method/criteria used for this.

The presentation of the data and the results is not very convincing. For instance, the authors should show a figure with the relevant meteorological data (snow height, air temperature and wind speed) of the study period. The authors should also show the snow profile graphically (not a table), show stability test results and the location of the failure layer, and indicate the location of the avalanche failure layer. Figure 4 should show the avalanche that released on the study site. However, I do not see any avalanche debris confirming the release of the avalanche, and the quality of the image is rather poor. Finally, Figure 7 and 8 could be combined. Indeed, for each sensor, the large window average shown in Figure 7 could be plotted on top of the shorter window values shown in Figure 8.

Finally, the authors show data from one snowfall and the subsequent avalanche. It would be very interesting to show data from an additional snowfall without an avalanche, if these data exist. This would provide more insight into the robustness of the observations. Are the results presented in the paper associated with avalanche release, or are the same characteristics also observed for other snowfall events without an avalanche? At the very least, the authors should address this point in the discussion, and more generally the limitations of their results.

More detailed comments can be found in the annotated pdf

Capelli, A., Reiweger, I. and Schweizer, J., 2018. Acoustic emissions signatures prior to snow failure. *Journal of Glaciology*, 64(246): 543-554. Faillettaz, J., Funk, M. and Vincent, C., 2015. Avalanching glacier instabilities: Review on processes and early warning perspectives. *Reviews of Geophysics*, 53(2): 203-224. van Herwijnen, A.,

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Heck, M. and Schweizer, J., 2016. Forecasting snow avalanches by using avalanche activity data obtained through seismic monitoring. Cold Regions Science and Technology, 132: 68-80.

Please also note the supplement to this comment:

<https://www.the-cryosphere-discuss.net/tc-2020-38/tc-2020-38-RC1-supplement.pdf>

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-38>, 2020.

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