The Cryosphere Discussions

The Cryosphere Discuss., author comment AC3
https://doi.org/10.5194/tc-2022-15-AC3, 2022

Reply on RC2

Arnaud Caiserman et al.

Author comment on "Snow Avalanche Frequency Estimation (SAFE): 32 years of monitoring remote avalanche depositional zones in high mountains of Afghanistan" by Arnaud Caiserman et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-15-AC3, 2022

Dear reviewer,

We wish to thank you for your constructive and helpful comments on our manuscript about SAFE in Afghanistan. A few weeks ago, we had provided a first response to your comments. Now we address all your comments in the context of the modifications we plan to incorporate in the revised version of the manuscript (in green). Based on your comments, we have changed the terminology of the paper from *snow avalanches* to *avalanches deposit zones*. In addition, the figures were improved, as requested. We also believe that the paper shows a better understanding of the applicability of SAFE. Indeed, we have emphasized the interest of locals in models such as SAFE and its applicability in remote high mountains. Moreover, as pointed by both reviews, we have significantly improved the validation section. Please, find our detailed response below.

This paper "Snow Avalanche Frequency Estimation (SAFE): 32 years of remote hazard monitoring in Afghanistan" attempts to produce inventories of avalanche debris using Landsat optical satellite imagery in late spring when snow, bare ground and water are easily distinguishable. The concept of using a long time series of remote sensing data to identify hotspots of avalanche deposition zones and trends in their spatial occurrence is good, but there are many pitfalls with the overall implementation and communication of the work which reduces the impact.

1. The paper requires some major restructuring of the content, starting with the introduction. Throughout the paper I found that information was in the wrong order and/or wrong section. Results were presented already in section 2 (eg. Table 2) and discussions were being made in the results section. This makes the work difficult to follow, even with the flow chart provided. Moreover figures are wrongly labeled (Fig. 10) and have unsatisfactory captions or text to explain what is being shown or how they were produced, color scales are not constant making figures hard to compare (Fig. 6-8).

The authors are very thankful for your comments on our paper. As specified in our first response few weeks ago, we have restructured the paper accordingly. Now the validation section is at the beginning of the Results, in Sections 3.1 and 3.2.

Regarding Figure 10, we have improved the caption as follows lines 447-450: "Figure 10. a, Map of areas with significant increases in monthly land surface temperatures in the Amu Panj Basin based on MOD11C3 products from 2000 to 2021; b, Geographical shift of avalanche depositional zones: mean longitude and latitude of avalanche deposits each year since 1990 show evidence of a movement to the northeast due to increasing winter temperature in mountainous areas.". To explain how the map was produced, we also added the following lines 437-438: "However, the slope was calculated and a Mann-Kendal test was applied for each pixel of the land surface temperature images (MOD11C3)."

Figures 6-8 were redesigned based on your comment and the label colors are now consistent amongst all maps (sub-catchments, villages and roads).

2. It seems to me that the authors are basically identifying late season snow patches in valley bottoms close to rivers which they are assuming to be avalanche deposits. This is made quite straightforward by the fact that the regions of interest are snow-free and snow is easily distinguishable by higher NDSI in the Landsat images compared with bare ground or water. This just reduces the problem to a simple thresholding and classification of image pixels into 3 classes, and I fail to see what is state-of-the-art in this approach. Moreover the authors have employed MODIS data to identify the snowline in order to select the dates and regions which are snow-free. MODIS has poorer spatial resolution than Landsat, so why not just use the Landsat data to identify the snowline? I can't see any value in using MODIS vs. Landsat for this purpose.

As noted in our initial response, indeed, the NDSI reclassification approach of SAFE is straightforward, but the date as well as the region of interest are the key parameters in this model. To our knowledge, no previous studies have adopted this approach. In the introduction, we reviewed the literature related to avalanche detection using optical, radar, and Lidar data, but none of these studies used the NDSI as we did in SAFE.

Regarding the snowline extraction, we used MODIS because of its coverage and its ease of application. Landsat archives can certainly provide snowline maps with higher resolution but the amount of data to be extracted for this purpose is much greater. Moreover, the cloud coverage on Landsat images presents a greater challenge compared to MODIS because there are more tiles to merge from different times, and the coverage is smaller than MODIS. MODIS was used to separate highlands from lowlands across the entire study area and a coarser resolution was acceptable for this purpose.

3. Throughout the paper the authors emphasise that the approach is based on Landsat data and the use of the google earth engine because it should be used in areas where internet connection is poor. However, they also highlight that the main end-users of such a dataset are stakeholders and decision makers. Are these stakeholders and decision makers likely to be located in remote mountain villages or the main cities (where internet connection is presumably good)? Are local villagers in these mountain environments really likely to be making use of

this dataset? I find it hard to believe that knowing where a large avalanche deposit has occurred several months prior to its detection is likely to be of interest to these people.

Thank you for those comments. In our first response, we noted that there is indeed local interest and concurrent local knowledge related to running SAFE (see RC2). To emphasize this point, we have added the following sentences to the conclusion lines 561-565: "Moreover, villages of high mountains such as in Afghanistan are strongly highly dependent on roads connections to provide necessary food, energy, medical supplies, and life-support items, especially in winter. It is therefore critical for local decision makers to assess the frequency of road blockage by avalanche deposits. Thus, open-access and user-friendly tools such as SAFE are highly applicable to interests of local stakeholders even with medium to low power computers since SAFE uses Google servers."

4. As pointed out by reviewer 1 the classification of avalanche size seems quite arbitrary and does not have much meaning when it is being detected late in the season after it has already partly melted out. It would be more meaningful to show for example a histogram of the avalanche size to show what is being detected rather than applying some random size classification to the detected deposits.

It was indeed stated in Review 1 that snow avalanche sizes must be classified by volume; however, the two datasets published on EnviDat (https://www.envidat.ch/#/metadata/satellite-avalanche-mapping-validation) were actually classified by area (m^2), therefore we retained our size classification based on surface area. However, as noted in our response to Reviewer 1, we changed 'size' to 'surface area' wherever needed, for more clarity.

5. Inconsistent terminology. Avalanche debris/deposits are referred to as "snow packages", "snow patches", "avalanche depositional" in the paper. The authors should use the correct term and use it throughout.

Thank you for this comment. In the paper we had used the term "depositional zone", but we now made it more consistent in the revised manuscript.

6. Poor validation. In section 3 the authors state that over the 32 years of data analysed they identified around 810,000 avalanche deposits using their dataset. However for the calculation of POD and PPV as shown in Table 2 they have ony used 158 deposits observed using Google Earth images. Moreover they do not describe how the validation data were identified (was this done visually or was there some other algorithm used to detect them in these images?). Overall this

does not come across as a satisfactory validation dataset with which to evaluate their detections.

Thank you for your comment. We used 158 snow avalanches because those were visible in some regions during specific years of Google Earth images as explained on line 243: "A total of 158 snow avalanche depositional zones were easily identified in the riparian buffer zones on Google Earth images in 2001, 2003, 2015, 2017, and 2019". And Google Earth images were indeed used to verify the locations of the avalanches predicted by SAFE. The lack of Google Earth imagery over this 32-year period restricted the number of avalanche deposits we could assess. For more clarity on this matter, we have added the following lines 268-272: "No other Google Earth images were available during the last 32 years in Afghanistan, therefore the comparison between SAFE and the true events was conducted with those available 158 deposit zones. These 158 deposits were extracted from Google Earth and stacked with SAFE outputs. SAFE deposits were considered as valid when the two datasets were overlapped at the same location.".

Moreover, to improve the validation, we have completed the comparison between outlined snow avalanches using SPOT-6 images and SAFE results in Switzerland, as recommended by Reviewer 1. Results are now in section "3.2 SAFE outputs comparison with outlined avalanches using SPOT-6 images".

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2022-15/tc-2022-15-AC3-supplement.pdf