Pre-collapse motion of the February 2021 Chamoli rock-ice avalanche, Indian Himalaya

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Response to minor revision

Editor comment

dear authors

after receiving the review reports on your manuscript, I am glad to inform you that your paper can be accepted by the journal should you be willing to apply some minor revision and modifications to it.
In the attached, you will find the two reports.
Please try to answer to all reviewers' comments and suggestions on a point-by-point basis, as well as please modify the original document accordingly and attach all required docs to the submission.
After your modifications, the manuscript will be subject to a final revision by the Editor, without passing a further review stage.

Looking forward to receiving your amended manuscript and replies,
best wishes
FC

We thank the editor and both reviewers for taking the time to read through our manuscript, and are glad that the revised version of our manuscript is suitable for publication in NHESS. We respond in detail to comments from reviewer 2 below (reviewer 1 recommended acceptance without providing any new comments), including a detailed note about the accessibility of very high resolution imagery. We write our comments in red, with reviewer comments in black.

Review of Van Wyk de Vries et al., 'Pre-collapse motion of the February 2021 Chamoli rock-ice avalanche, Indian Himalaya'.

Van Wyk de Vries et al. present an analyses of a variety of remotely sensed datasets to study the dynamics of the slope instability which produced the Chamoli ice-rock avalanche in Feb. 2021. Their results show how the broader Chamoli site has irregularly produced additional (smaller) ice and/or rock avalanches over the last few decades, and that the slope itself became mobile several years before the collapse event. Their results emphasize the strength of remotely sensed data to study processes operating over extended temporal baselines, and the authors nicely illustrate and discuss the limitations of their data where signal to noise ratios are less favorable. The authors present a synthesis of observations from other work on the Chamoli event and propose a trigger mechanism for the avalanche.

In my opinion the work is of high quality and the authors have carefully considered their evaluation of the application (and limitations) of remotely sensed data to study high mountain hazards such as ice/rock avalanches. I have a few minor comments about how the paper could be improved, which are listed below. Several of these comments relate to the over-emphasis of the capability of truly open access remotely sensed datasets in the study of these processes, which I think the authors would be wise to clarify, as this study certainly does not rely solely on publicly available datasets. Overall (noting previous reviewers comments) I find the work well within the scope of NHESS.

We thank the reviewer for their comments and for their positive overall view of our manuscript. We hope that the responses below and minor edits to our manuscript cover any remaining comments.

We respond to the comment about open access remotely sensed data below, but do not agree that its potential has been overstated. While this study also incorporates non open source data and demonstrates the value of merging multiple data sources, many of the core findings could have been reached with open source data alone (specifically, feature tracking of Sentinel-2 data clearly shows the pre-collapse displacement).

We do hope that comercial data becomes more readily available for the mitigation of natural hazards, and that new open-source monitoring missions provide comparable datasets in the future. Presently available open source data does, nevertheless, provide many new opportunities for hazard monitoring. We have added a comment about this in the manuscript lines 112-114

“However, most very high resolution imagery is not open source and is expensive to procure, limiting its use. Increased availability of this commercial imagery and/or new open-source stereo imagery satellites would provide many new opportunities for hazard monitoring.”

And further edits in the discussions section lines 333-334

“In addition, the DEMs and elevation change maps used in this study were generated from imagery not accessible in open source archives. Changes in the accessibility of commercial data or the launch of new, open access, stereo-imagery satellites would facilitate the use of elevation change in large-scale geohazard monitoring.”

Minor comments:

Title: ‘Indian Himalaya’ could be narrowed down to the ‘Garhwal Himalaya’ I think.

After some thought, we have retained ‘Indian Himalaya’, as the name ‘Chamoli’ already provides a narrower geographic area and additional information about the more general area is useful to those not familiar with the region.

L43- I think the authors need to be careful not to oversell the capabilities of truly ‘open access’ data here (and later in the manuscript). Many of the datasets the authors have used are not ‘open access’ as things currently stand (the high and very high resolution stereo imagery have not been sourced from open archives) and the analyses would not have taken the same shape without them. It would be inaccurate to suggest that the same kind of data could be easily acquired to study different events. It may even be a point for discussion for later in the manuscript to emphasize how the study of these events relies on high resolution, high precision data which are not abundant in the public domain.

We agree with the reviewer that not all data used in this paper is open access, but are not implying this in the sentence referred to here (“Growing archives of high-resolution, open access Earth observation data remain largely untapped for landslide monitoring.”). The success of Sentinel-2 derived displacements in this study is one example of this, and other studies have successfully leveraged Sentinel-1 derived InSAR maps for displacement mapping.

As Figure 7b shows, even change maps derived from very high resolution data may have limitations for large-scale analysis, and improvements in remote monitoring of slope stability need not only come from higher resolution imagery. We are not suggesting that the data used in our study could be used for any event in the world, but simply that a large volume of truly open source data is now available on a global scale.

L70- The 80-20 composition statement could do with a citation, otherwise it just reads as some kind of informed guess.

The calculations behind this compositional breakdown are provided in Shugar et al., 2021. We have added a citation on this sentence.

L85- As with my comment on L43, this is at odds with the assertion that high-resolution, open access imagery is available to study these events.

We agree with the reviewer that very high resolution imagery is not open access in most cases. We have added some additional discussion about this in other sections as shown in our responses to the other points.

The majority of our feature tracking results are derived from Sentinel-2 imagery, which is truly open access. The calculations made with the other datasets primarily serve as validation of this data (e.g. Figure 4 d-g). Given that our main feature tracking conclusions can be derived entirely from open-source data, we consider the text to be appropriate in this section.

L98- Might be worth tailoring this point about sensing in conditions inhibitive to optical imagery to emphasise the benefits of InSAR in the monsoon period, perhaps?

We have added a note about the monsoon to this sentence, and agree that this is particularly relevant in the context of rainfall-triggered landslide motion.

L108- Again, high-resolution imagery suitable for tasks such as this come at a high price. I think its worth mentioning that these very high resolution stereo imagery were specifically acquired for the AOI, not just plucked from an open archive.

We have added an extra note to this sentence about the high cost of this UHR imagery. Not all of the imagery we used was specifically tasked over this AOI (only the 2021 ones), and there are large public datasets derived from UHR imagery (e.g. ArcticDEM). However, these are not always available and we agree that the cost is useful to note.

We have also, as noted above, added a comment about this in the manuscript lines 112-114

“However, most very high resolution imagery is not open source and is expensive to procure, limiting its use. Increased availability of this commercial imagery and/or new open-source stereo imagery satellites would provide many new opportunities for hazard monitoring.”

L154- delete ‘is’ after AutoRIFT.

We have removed ‘is’.

L174- this --> these velocity data as they are plural?

We have changed ‘this’ to ‘these’ as recommended.

L175- Is there a Figure the authors can refer to in this paragraph?

We have added a reference to figure 1 which shows this bedrock ridge.

L217- Should this be ‘as a real sign’ rather than ‘is a real sign’?

This should be ‘as’. We have corrected the typo.

L227- A trivial point, but it can’t have reached its max. width by the end of 2018 if it ‘widened further’ between 2018 and 7th feb, 2021, can it?

We have changed this to read ‘The crack grew until the end of 2018’.

Figures 2-4- really nice!
Only thought…do you need the inset map of India again on Figure 4? Just seems to hinder your formatting slightly.

We thank the reviewer for their positive comment on our figures. We agree that the inset was not necessary due to Figure 1, and have removed it to improve the formatting of this figure.

Figure 6- A few minor points:

-Are the elevation change maps in panels B and C derived using the SPOT, Cartosat or composite 2018 DEM in combination with the pre-event and post-event DEMs?

The composite DEM was used. We have added this to the figure caption for clarity.

-Is there any way that the same elevation change range can be used in panels B-E? +/-25, +/-100 and +/-40 are all used here and it’s not that easy to interpret.

We selected these different elevation change ranges as they are necessary for the details to be visible in each case. We could use +/- 100 m for all four panels, but the details of the pre-collapse change would be difficult to visualize. We have added a note to the caption that the elevation change bounds are different.

-The differences over stable ground areas seem to have opposite signs in panels B and C. Is this the result of some sort of minor bias in the 2018 DEM?

The systematic differences over stable ground are likely related to differences in snow cover between the three time periods. These differences are, however, small enough to not affect the signal of interest.

Figure 7- Is it worth adding some glacier outlines to this figure? The preceding text refers to elevation change signal over glaciers and it'd be easier to pick this out of the elevation change map on the right hand side with the help of outlines.

Our primary objective in this figure was to show the difficulty in separating the block displacement signal from the 'noise' (both glacier elevation change and DEM errors). We therefore do not add glacier outlines, as we consider that these may distract from this point.

L330- The modeling here is described very briefly and I doubt could be replicated with the details provided. Is this modeling based on an established method?

This 'model' is very simple and involves a simple translation of one elevation raster (collapsed block thickness) over another (post collapse topography). We have added more details to this paragraph (L335-340) to clarify this point.

L350- What period is referred to as 'longer term' here? Can the authors provide an estimate of the warming that has occurred over the last 2-3 decades in the region? This might provide a little more context for the discussion of freeze-thaw fracturing and permafrost degradation which takes place in the paragraphs below.

We have added more information to this sentence, including an estimated total warming over the past 3 decades. This sentence now reads:
"On the longer term, this region has warmed an estimated 0.014 K (Zhou et al., 2021) to 0.033 K (Shrestha et al., 2021) per year since 1980, for a total warming of 0.4 K to 0.9 K over the past three decades."

L360- Refer to Appendix B in here somewhere?

We have added a reference to this appendix here (L360-361).