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

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Author Response

Dear editor and reviewers,

Please find below a full response to each of the reviewers' points. We have addressed all of the points in the response and in the attached manuscript. Reviewer 1 suggested mostly minor edits related to the InSAR mapping section, which we have incorporated as discussed. Reviewer 2 suggested more major changes related to the framing and scope of the manuscript. We have substantially updated the introductory and discussion sections in response to these comments and sought to clarify and highlight the existing portions of the manuscript which also address the comments. In particular, we have updated the manuscript to better emphasize our use of the data rich Chamoli rock-ice avalanche to understand the potential of satellite imagery to identify signs of instability, and how our results are applicable to a wide range of environments. We have also updated our introduction to discuss a larger number of landslide-specific case studies from the literature involving each remote sensing technique.

We thank the editor and both reviewers for their time taken improving this manuscript, and hope that the present paper is suitable for publication in NHESS.

Sincerely,

Max Van Wyk de Vries, on behalf of the authors

Review 1

Dear Authors,

The manuscript is written well and address the research questions defined by you. However, I have some suggestions/comments before the manuscript is accepted for publication in NHESS.
We thank the reviewer for these comments, and for their positive assessment of our manuscript. We have responded to the comments in detail below, with the review comments given in black and our responses in red.

Himalayan terrain many times induces decorrelations. due to its topography as well as vegetation. The authors have tried to implement simple DInSAR methodology and were unable to obtain good interferograms. Loss of coherence is the main challenge to the InSAR application in the area.

I am afraid that simple PSI technique will hardly yield any significant result. Even the techniques such as SBAS, SqueeSAR etc may also fail to produce anything. As a suggestion you can try A-DinSAR techniques such as SBAS or techniques based on distributed scatterers such as Quasi PS (Perissin & Wang, 2011; Razi et al., 2018) or SqueeSAR (Ferretti et al., 2011) in this region.

Perissin, D., & Wang, T. (2011). Repeat-pass SAR interferometry with partially coherent targets. IEEE Transactions on Geoscience and Remote Sensing, 50(1), 271-280.

Razi, P., Sumantyo, J. T. S., Perissin, D., Febriany, F., & Izumi, Y. (2018, August). Multi-temporal land deformation monitoring in V shape area using quasi-persistent scatterer (Q-PS) interferometry technique. In 2018 Progress in Electromagnetics Research Symposium (PIERS-Toyama) (pp. 910-915). IEEE.

Ferretti, A., Fumagalli, A., Novali, F., Prati, C., Rocca, F., & Rucci, A. (2011). A new algorithm for processing interferometric data-stacks: SqueeSAR. IEEE transactions on geoscience and remote sensing, 49(9), 3460-3470.

A study has been done like this on the same study area. They have claimed that they have used PSI on the regional level.

Kothyari, G. C., Joshi, N., Taloor, A. K., Malik, K., Dumka, R., Sati, S. P., & Sundriyal, Y. P. (2021). Reconstruction of active surface deformation in the Rishi Ganga basin, Central Himalaya using PSInSAR: a feedback towards understanding the 7th February 2021 Flash Flood. Advances in Space Research.

Thank you for pointing out the difficulties of performing InSAR measurements in such challenging terrain. As we note in our manuscript, shadowing and decorrelation are problematic on this particular face. The primary issue is that the release zone is in radar shadow in the images of the ascending orbit and is affected by layover in the descending orbit. The additional tools that you mention (PSInSAR, SBAS, SqueeSAR etc.) are all time-series analysis tools. We did not attempt to generate a time-series for the Chamoli failure because the data quality is too low. In other words, too few coherent pixels are present in the failure area to allow for the extraction of a time-series or the generation of a stack, therefore we cannot apply time-series tools in this case. Kothyari et al. (2021) do indeed present an attempt to reconstruct regional displacements using PSI. However, their regional map of cumulative ground displacement (Figure 8b) does not show any exceptional signal at the site of the Chamoli rock-ice avalanche. Instead, their results exhibit ~40 mm anomalies across the entire study area, suggesting that noise dominates over any signal. Their cumulative displacement timeseries also show no long-term pattern consistent with an unstable block, instead fluctuating around zero (as expected for noise). This is compatible with our finding that the data quality is too low to detect the displacement signal.

For Figure 6b, I suggest the authors check all the available interferograms to confirm that the two areas marked with “Rock glacier motion” are always moving. From only one interferogram, it is hard to say the motion. The area marked with “Atmospheric noise” seems not so evident because there are some strange values in the surrounding area possibly caused by unwrapping errors due to the low coherence. If these areas can’t be confirmed from other interferograms, I suggest writing these areas are possible rock glacier motion or possible atmospheric noise. I suggest also writing “2.8 cm” in the caption of Figure 6 after “wrapped phase”.

Thank you for this comment. The rock glacier motion was indeed labeled as such after the analysis of all available interferograms, as well as the study of optical images that identified the landscape features as such. We have stated this more explicitly in the text. The area denoted as “Atmospheric noise” cannot be caused by unwrapping errors, since our figure shows wrapped phase which has not yet been unwrapped. However, the cause of this specific fringe pattern is somewhat unclear, and we agree that labeling it with "possible atmospheric noise" is preferable. We have also implemented your suggestion for the addition to the caption.

In figure 6b, I’m not completely sure about the “atmospheric noise”. I suggest to check the displacement time series, if available, because atmospheric noise can be identified as strange peaks.

We agree with this comment and have responded to it in more detail above.

Review 2

We thank the reviewer for their comments and suggestions on our manuscript. We have responded to the comments in detail below, with the review comments given in black and our responses in red. While we disagree with the reviewer’s assertion that our manuscript is outside of NHESS’s scope, we are grateful for the reviewer's constructive feedback and implement changes to our manuscript in line with these.

The manuscript focuses on investigating pre-failure surface displacement for the case study of the 2021 Chamoly avalanche. The case is undoubtedly interesting but, in my opinion, the manuscript should not be considered for publication in NHESS. In the aims and scope of the journal it is stated that "The following are generally considered out-of-scope or we do not encourage: [...] Localised case studies with no broader implications (in other words, ask yourself, what would someone else in another region learn from the case study that you have done; what is the broader context?)." While I recognise that the authors' work could have broader implications, these are not discussed at all in the manuscript. The manuscript is indeed completely focused on the case study and the authors make indeed conclusions (e.g., the unpredictability of the timing of collapse) only for the case study and do not discuss what they learned in terms of general implications.

We thank the reviewer for acknowledging the potential interest in our study, but disagree that this manuscript is outside the scope of NHESS.

Reviewing the scope of the journal, we see the following three points:

- the study of the evolution of natural systems towards extreme conditions, and the detection and monitoring of precursors of the evolution;

- the detection, monitoring, and modelling of natural phenomena, and the integration of measurements and models for the understanding and forecasting of the behaviour and the spatial and temporal evolution of hazardous natural events as well as their consequences;

- the design, development, experimentation, and validation of new techniques, methods, and tools for the detection, mapping, monitoring, and modelling of natural hazards and their human, environmental, and societal consequences;

We believe that our manuscript, through an in-depth study of the Chamoli rock-ice avalanche pre-collapse conditions and the methods used to assess this instability, contributes to all three of these points. Furthermore, we do not believe that the reviewer's statement "While the authors' work could have broader implications, these are not discussed at all" is accurate. Even in its present state, our objectives are broad enough to be of relevance beyond the Chamoli rock-ice avalanche (e.g. our objective line 110: "Would these pre-collapse datasets and tools be adequate to identify this hazardous slope without the prior knowledge of its failure?"), and we dedicate an entire subsection of our Discussion to the broader implications ("Future perspectives : remote-sensing based hazard monitoring").

Indeed, one of our primary aims in this manuscript is to use the complex, but also well-studied and information-rich Chamoli rock-ice avalanche to evaluate the potential of Earth observation datasets to detect unstable slopes prior to their occurence. When discussed from this perspective, we believe that detailed individual case studies can contribute to our understanding of hazard monitoring techniques and processes well beyond the study area. We discuss below how we have clarified and in some cases expanded on this aspect of our manuscript.

In summary, our detailed analysis of the Chamoli rock-ice avalanche draws conclusions that are both novel and more widely relevant for the investigation of the pre-collapse conditions of other unstable slopes, particularly in complex high-relief or glaciated environments.

A review of recently published papers in NHESS shows many examples using a detailed local case study to draw wider conclusions, for instance:
Submarine landslide source modeling using the 3D slope stability analysis method for the 2018 Palu, Sulawesi, tsunami,
The Cambodian Mekong floodplain under future development plans and climate change,
Spatiotemporal evolution and meteorological triggering conditions of hydrological drought in the Hun River basin, NE China,
Geo-historical database of flood impacts in Alpine catchments (HIFAVa database, Arve River, France, 1850–2015), or
Correlation of wind waves and sea level variations on the coast of the seasonally ice-covered Gulf of Finland (all published within the past month at the time of writing).

Finally, we also note that our manuscript passed the editorial access review editor which suggests that it is potentially of interest for the NHESS readership "Manuscripts submitted to NHESS at first undergo a rapid access review by the editor [...] to identify and sort out manuscripts with obvious deficiencies in view of the above principal evaluation criteria. [...] the paper should contribute something new and interesting to the community." (https://www.natural-hazards-and-earth-system-sciences.net/peer_review/review_criteria.html)

As mentioned above, we do believe that valuable points were made in this review. We therefore make edits and build on the questions listed below to improve our manuscript:

Could the timing have been predicted if more images were available (e.g., 24 prior to the collapse, 8 hours prior to the collapse, etc.)?

For this particular question, we do not believe that the data are available to provide a meaningful answer. This question could perhaps have been answered if any image had been collected in the hours preceding collapse, but no such image exists. We do not detect any anomalous pre-collapse increase in velocity in our Sentinel-2 feature tracking timeseries, with the final pre-collapse image pair being the 31st of January and 5th of February 2021 (8 days and 2 days prior to collapse), although the precision of displacements derived from any single image pair is low.

Furthermore, even if the collapse timing were detectable in pre-collapse imagery, this would not in itself be sufficient for the methods to be applied for hazard monitoring. For methods to be useful in hazard monitoring, the timing of collapse must be identifiable without special attention to the imagery immediately preceding collapse, as this timing will not be known.

We agree that this is an important point, and will further develop it in the discussion of our revised manuscript through a discussion of what is visible in the pre-collapse imagery, and what would need to be visible to yield useful insight into the timing of collapse.

Is it just a matter of noise or, even without noise, no trend to failure could be seen? Is it a matter of resolution, instead?

Noise and resolution are not independent in the case of feature tracking or DEM generation and differencing. For instance in feature tracking, the choice of a larger window size will generally reduce noise levels at the cost of lower resolution. A related question could therefore be ‘For a spatial resolution sufficient to study a given unstable slope, is the signal-to-noise ratio low enough for the hazard to be monitored?’ or conversely ‘Is the minimum spatial resolution achievable while maintaining a certain signal- to-noise ratio sufficient for monitoring a given hazard?’. The use of different satellites with higher resolution can provide an improvement, but does not always do so: for instance, the 3 m resolution Planet imagery often produces velocity maps with higher noise levels than the 10m resolution Sentinel-2 imagery due to greater differences in imaging geometry between image pairs.

We discuss some of the limitations of current noise levels in the final section of our discussions and in Figure 8.

Is it a matter of mechanism of failure (e.g., a very steep tertiary creep that causes orders of magnitude of acceleration in a matter of minutes/hours?). Is this mechanism rare or typical in such a type of failures?

There is no geomechanical data available in the case of the Chamoli collapse, and any assessments of the mechanisms of failure must be conducted using remotely sensed data (including seismic data; https://www.nature.com/articles/s41598-022-07491-y) and post-event field investigations, complemented by numerical modeling or analogy to other, better instrumented, areas. Here, we propose a collapse mechanism compatible with the available data, based on a combination of permafrost thaw and loading in the headwall crack. The exact collapse mechanism is, however, secondary to our main research question of whether this collapse could have been detected or forecast from remotely sensed data.

What is the general conclusion in terms of remote sensing capability in predicting failures/timing of failures? What types of landslides could be predicted, instead? Are there examples in the literature of successful/unsuccessful predictions of other case studies based on similar data sources? Will we ever be able to predict the timing of failure based on satellite remote sensing alone?

The final two sentences of our conclusions provide a general conclusion in terms of remote sensing capability in predicting failures/timing of failures. For convenience, we include them here:
"Finally, we assessed the potential of these datasets and approaches for monitoring other unstable slopes. While they were effective at identifying precursory signals at a known collapse site, it remains very challenging to predict such collapses with sufficient levels of confidence in high-mountain areas. "

We agree that the above questions are important, but also note that many are outside of the scope of our paper. We cannot and should not conclude in general terms about whether remote sensing is able to predict the timing of failure, as each individual site is subject to unique conditions. We found that dentifying the timing of collapse at Chamoli was likely not possible. It may, however, be possible in other locations using the same datasets.

We believe that the following paragraphs in our discussions section (lines 376-385) provides some answers to these questions, and are be further developed in this revised version of this manuscript:

"Overall, forecasting the 7 February 2021 Chamoli rock-ice avalanche prior to its occurrence from remotely sensed datasets would have been very challenging, and certainly not routine work using well-established methods. Current image resolution, characteristics, and processing algorithms result in noise levels on a similar order to the signal itself – although joint interpretation of feature tracking results, DEM differences, and satellite images does reveal clear precursory signs of slope instability. In addition, none of the data in this study are able to adequately forecast the timing of collapse. As such, current archives of satellite images do not currently appear to be practical for forecasting individual events. At the same time, this should not prevent remote monitoring of hazardous zones, particularly when adjacent to vulnerable areas. Every slope failure will exhibit a different range of pre-collapse signals, and new instabilities might be recognized in some cases. Even though the forecasting of individual events remains a challenge, these data have value for identifying zones of highest risk for in-situ monitoring or the installation of early-warning systems (Cook et al., 2021)."

Feature-tracking, DEM difference, and InSAR datasets can be processed and analyzed on a regional or even global scale – and in many cases pre-processed datasets are already available online (Morin et al., 2016, Gardner et al., 2018). While these pre-processed datasets are not generally produced for slope stability monitoring, they can be used to improve hazard maps and reduce landslide related damage. Future advances in Earth observation satellite capabilities and processing algorithms will improve the quality of such products. ”

In addition, with reference to the specific case study, I found the discussion speculative, for instance, when it came to the safety factor as all discussions on driving and resisting forces were based on general knowledge/speculation and not supported by, e.g., geomechanical data of the case study.

As we note above, no geomechanical data or other ground-based assessments of the conditions of the avalanche block are available. We are aware of this limitation, and note in our Discussion section that: “In the absence of in-situ instrumentation and observations, it may not be possible to determine the exact cause of the failure at Chamoli.”. Nevertheless, our proposed collapse mechanism is compatible with our remotely-sensed data and is able to explain the mid-winter timing of collapse.

Also, the introduction is too generic, describing information that is very well known to researchers in the field. Perhaps the introduction could have focused only on recent advances in remote sensing techniques for natural hazards that are perhaps closing a knowledge gap and enabling the type of analysis conducted by the authors, albeit with remaining limitations.
We appreciate this comment about our introduction. We had aimed to provide a broad overview of each technique we had used such that our paper was more accessible to researchers beyond this specific field of study. We acknowledge that more specific information would be useful, particularly with regard to how these techniques are currently used for landslide monitoring and will revise this section accordingly.