Letter of response to report on nhess-2021-18

Dear editor Mahdi Motagh,

Thank you for your feedback. We now see clearer and understand the issues raised by reviewer #1. We followed the comments and suggestions carefully and addressed each one thoroughly.

We also thank you for your suggestions. We changed the title and extended the discussion by elaborating on how cloud-processing can contribute to shorten $t_{warning}$ by incorporating tools such as Geohazard GEP. Please see below.

Additionally, please find below the following colour coding for the review and the comments from you and referee#1 (Jan Blöthe) in *black italics*; our responses to the review are in blue and the changes made to the manuscript are in orange (following RC1) and purple (following the Editor). Reference to line numbers are based on the original preprint.

Comments to the Author by Mahdi Motagh:
Your paper was reviewed by two experts in this field. Reviewer 1 still noted a number of important shortcomings in the revised version and called for a 3rd review. I also noticed some issues myself. Although Reviewer 2 was satisfied with the revision, it is evident that the manuscript requires another revision. Please do not send us partially improved manuscript and consider all the comments of the Reviewer 1 that he has made on the annotated pdf and carefully address them in the revised version.

Also from my side, I think the title needs to be changed. In the discussion and results you carefully addressed all shortcomings that may arise when using optical remote sensing for early warning. Therefore, I would not call it a novel approach.
Rather you evaluated the potential that exists in optical remote sensing for early warning. Moreover, please elaborate on the discussion how the t_warning could be shortened using cloud-processing solutions such as Geohazard GEP. Although it is based on Sentinel-2, but the platform could be exploited for big landslides and reduces t_warning significantly.

Modifications according to the Editor:

[L1-2] **Challenging the timely prediction of landslide early warning systems with multispectral remote sensing: a ~~novel~~ conceptual approach tested in the Sattelkar, Austria**

[L565 ff.] For large and long–preparing slope failures, recent developments such as the ESA's Geohazards Exploitation Platform (GEP), developed and operated by Terradue, support on–demand services such as the Thematic Exploitation Platforms (TEPs) and have the potential to decrease $t_{warning}$: The GEP provides an archive of Copernicus' Sentinel–1 and –2, Pléiades and Spot 6/7 data, and access to cloud computing resources to support large scale geohazard mapping and monitoring (Volat et al. 2017; Foumelis et al. 2019; Lacroix et al.). Therefore, the time critical phases of time to collect and time to process, which in our example are attributed to the larger share of the total time requirement for

$t_{warning}$, could be significantly reduced as the data is directly accessible through high performance cloud computing. What remains is the third phase, time to evaluate, where a relatively short time is required, thus t_{lead} is extended.

Modifications according to referee #1

Dear Jan Blöthe,

We thank you very much for your time you spent once again reading through our revised manuscript, your response to it and the feedback on our previous letter of response (review no. 1).

Your detailed report clarified miscommunications from the first round and helped to further improve our manuscript. Thank you for your confirmations of our suggestions regarding your comments. We apologise for some misunderstandings (e.g. an overlooked spelling mistake) and are sorry for the difficulties due to the lack of line numbers.

Dear Authors,

As this is the second round of reviews, I will limit my comments to your answers on my earlier review in the document nhess-2021-18-AC1-supplement (https://doi.org/10.5194/nhess-2021-18-AC1) as well as the additional figures provided in the additional supplementary document nhess-2021-18-AC2-supplement (https://doi.org/10.5194/nhess-2021-18-AC2) .

Sadly, there are no line numbers provided, so I decided to attach a commented PDF file of the AC1-supplement. Sorry for the inconvenience, but I hope that this way my comments will be easy to follow and to relate to the specific lines in the text.

Very generally, I thank the authors for following some of my recommendations that in my view already improved the manuscript at this stage. However, regarding my very general comments, mainly raised under B) "Result of image correlation" in the first round of reviews, I still have major concerns. I outlined these concerns in a number of comments in the attached file but want mention the most critical point here once again.

In Fig. 5 the authors present the results of digital image correlation of a landslide in the Sattelkar (Austria) for two different time intervals (interval I/Ib (376/370 days): left panels; interval II (42 days): right panels). First, I am still not convinced that the challenging results for the western part of the landslide presented in Fig. 5 a-c, more precisely the random distribution of displacement vectors (a, b) and the patchy nature of vector fields shown in c), are highlighted clearly enough in the manuscript. Leaving these results as is (as the authors have done in the first round of reviews), in my view requires a thorough attribution of the limitations of this data – just labelling these areas as decorrelated is not sufficient.

Second, my main concern is the treatment of results shown in Fig. 5 e) and f). Here, the authors use the identical data set as for Fig. 5 a) and b), but resampled to 3 m resolution. Yet the results shown in Fig. 5 a) and b) compared to e) and f) are very different. While the former show rather smoothly distributed displacement of ~2-8 m (a) and ~2 m (b) in the eastern part of the landslide, the latter show displacement *predominantly exceeding ~18 m for both time intervals, respectively. Furthermore, the appearance of these displacement fields is very patchy and shows no systematic pattern (Fig. 5 e and f). Yet in L371-378 (revised version of the manuscript) and L479-498 the authors present and discuss the data shown in Fig. 5 e) and f) as if these were adding robust information to the study, without any interpretation of why these results are completely different from Fig. 5 a) and b), nor discussing a potential explanation for this observation.*

There are a number of points indicating that the data presented in e) and f) is problematic here and that a) and b) are rather plausible. I have listed these concerns, numbered a) to d) and highlighted in the attachment, in the general comments of the first round of reviews and these still stand.

Kind regards

Jan Blöthe

General comments

Page 1
Comment: 1
Thank you for adding some more details here.
Thank you for your suggestion. We have added more details.

Comment: 2
This is now a very complete supporting material that in large parts documents the approach that you have taken in your study.
Thank you, we are happy to provide some further analyses and research on the OSM.

Comment 3
I do not think that this is the case. Tracking stable surfaces outside the moving area is very well possible and the figures in the OSM (esp. Fig. 13) indicate that the tracking was done for the entire images. Quantifying spurious displacements in stable regions should therefore be possible. Please also see my comment on this matter below (L285/86).
Yes, you are right. We fear that we have had a misunderstanding in terms of residual mismatch outside the landslide area on stable surfaces. Please see here our profiles for interval I and II in the OSM, Fig. 6. There is zero total displacement in the stable regions outside the active landslide area.

Page 2

Comment: 1
This is a good apporach, but in my view is not sufficient for the data at hand.
Thank you for your comment. We have extended the OSM and hope that it is now sufficient.

Comment: 2
Please, see my comment below

Comment: 3
Let me again try to illustrate this aspect in a bit more detail. The changes made to the manuscript and foremost the inclusion of additional figures into the OSM helps to shed light on this issue. As the velocity

profiles given in Fig. 6 a) nicely show (please add x- and y-axis labels) is that in the area of decorrelated values, the displacement values in the "salt and pepper" domain of the western part of the landslide, are very heterogeneous. Taking the highest peak as an example, the problem of this data becomes apparent: the location at ~325 m x-axis is moving with ~100 m downslope according to the results of the DIC here. Just ~25 m along the profile line in both directions, movement of less than 10 m has been tracked. How should this be possible, if the tracking did not simply produce random correlations with erroneous locations? What I am trying to say here is that the presentation of the results in the manuscript allows the reader to take this data for granted. Yet the authors show in their rebuttal to a number of my comments that they themsevles do not view these data as reliable. Please indicate clearly in the figures and the text that these data are erroneously matched pixels, or provide a different plausible explanation for the pattern. Furthermore, in my view the term decorrelated is misleading in the following way: technically, the patches that the DIC matches are correlated to another patch (see your reply to my comment L288/289), else no vector should be produced by the matching procedure. The pattern in these "decorrelated" areas of the landslide indicates that the matching did not find the corresponding patches and therefore does not produce a smooth displacement pattern, but random noise. In other words, despite the correlation the DIC finds, it correlates the wrong locations (images patches), i.e. produces wrong displacement vectors.
Thank you for your detailed feedback. We have revised the manuscript and defined *decorrelation* based on the current literature and our understanding. Accordingly, we have further indicated the ambiguous signals and explained their source (caption Fig. 5, caption Fig. 6, section 6.1.: L387f., L423ff., L430ff and section 6.2.: 470ff..). Please see also our comments further below.

[L404] Here we follow Leprince et al. (2007) describing a correlation loss as '*decorrelation'* with signal–to–noise values of low/null (i.e. no convergence of the correlation algorithm) and/or large offsets, either unrealistic in nature or beyond the valid matching window distance. Decorrelation in our understanding exhibits a salt–and–pepper appearance in the DIC result with random displacement vectors, related to inconsistently tracked features. The software is not able to find the corresponding, correlated surface pattern, leading to a misfit (i.e. misrepresentation) and/or mismatch (i.e. blunders) of the matching windows and finally resulting in noise (Debella-Gilo, 2011; Guerriero et al., 2020). Nevertheless, this decorrelation signal is still a valuable observation that might be related to surface processes and not only to erroneous limitations of the DIC method. There are three main reasons that might cause these effects: (i) significant temporal change of the surface, i.e. revolving and/or rotational deformation, (ii) high displacements exceeding the matching window size being smaller than the offset, (iii) land cover changes such snow cover, vegetation cover and alluvial processes, among others, and (iv) changes related to illumination (e.g. shadow) or image errors (e.g. orthorectification, shifts in individual bands) (Leprince, 2008; Debella-Gilo, 2011; Lucieer et al., 2014; Stumpf et al., 2016).~~Leprince (2008) describes snow cover, vegetation cover and alluvial processes, among others, as potential explanations for these decorrelations.~~ In our study, the decorrelated ~~areas~~salt–and–pepper areas include to a large degree the landslide head (a), the drainage channel (h) (Fig. 5a, b), a larger patch south of the active area boundary (g) (Fig. 5a), and some smaller ones in little depressions (g) (Fig. 5a) and (j) (Fig. 5a, b). ~~Most~~ The patches (*j*) and east of (*j*) are identified as snow fields in the orthophotos and the noise results from decorrelation.

Comment: 4
These are the points a) to d) I mentioned in my review statement.
Thank you, you are absolutely right. We removed the results and discussion of the 3 m DIC UAS analysis from the manuscript. Please see our detailed answer to comment 2, page 12 further below.

Comment: 5

Thank you very much for following a number of suggestions that sadly did not really improve the results of the image correlation.
Thank you for your suggestion to include pre-analysis calculations in the online supplementary material which helped to shed light on the complexity of the Sattelkar.

Page 3

Comment: 1
Please, see my above comment on the idea of decorrelation.
Thank you, we have incorporated your comments #2 and #3, page 2.

Comment: 2
This is a crucial point. While you acknowledge here that in the area of decorrelated values, matching did in essence not work, this is not stated as explicitly in the manuscript. In my view, it would be necessary to clearly highlight the decorrelated areas as such in the Fig. 5 and explicitly mention the division in reliably tracked regions and those regions that are unreliable (i.e. random) and cannot be interpreted. In my impression this is not done in the present revised manuscript.
Thank you for pointing this out; now we understand your intentions in the first review round. In our revised manuscript we have clearly stated how these decorrelated areas should be interpreted. Please see also our previous comment (#2, page 2) with indicated manuscript changes. In addition, we have included a definition of "decorrelation" based on the current literature and our understanding.

[L422ff] However as field observations provide evidence that the rock masses are deforming, and the surface is altering due to the high mobility and rotational behaviour of some boulder blocks. This~~, which~~ leads to changed pixel values and spectral characteristics of the block surface and the surrounding area, which can also result in poor correlations, and even random errors and mismatches (Debella-Gilo and Kääb, 2011). This finding is similar to observations in a rock glacier study by Debella-Gilo and Kääb (2011).

Comment: 3
This is very true and from a image processing point of view, it is the same reason why shadow and snow cover effects induce erroneous correlations. The changed pixel values inhibit rigorous matching. Please include more than just the speed of the landslide in your discussion of errors in matching between images - I also state that in another comment further.
Thank you for identifying this in our first letter of response and the first revised manuscript. We have modified the text accordingly. Please also see our answers below.

[L403ff] There are three main reasons that might cause these effects: (i) significant temporal change of the surface, i.e. revolving and/or rotational deformation, (ii) high displacements exceeding the matching window size being smaller than the offset, (iii) land cover changes such snow cover, vegetation cover and alluvial processes, among others, and (iv) changes related to illumination (e.g. shadow) or image errors (e.g. orthorectification, shifts in individual bands) (Leprince, 2008; Debella-Gilo, 2011; Lucieer et al., 2014; Stumpf et al., 2016). ~~Leprince (2008) describes snow cover, vegetation cover and alluvial processes, among others, as potential explanations for these decorrelations.~~

Comment: 4
I am very surprised that the authors try to find arguments for the validity of the data shown in Fig. 5 e and f. The results from the downsampled UAS give completely different displacements as the high-resolution UAS data, yet the data and time span are the same. How can the authors interpret these results as "less

trustworthy"? In interval II for example, the landslide either moves ~2 m (Fig. 5 b) or ~6 to >18 m (Fig. 5 f), but not both at the same time. These values are not similar in any kind of way, i.e. one of these results is simply wrong. I have been outlining this in detail above (highlighted lines of the general comment B raised in the frist round of reviews) and want to add an additional aspect to point the authors to the contrasting results these data suggest, if taken for granted: If you calculate a velocity from the displacement for e) and f), this translates to an average velocity of 17.67 m/yr for interval I as opposed to an average velocity of 156.5 m/yr for interval II, i.e. an acceleration of factor 8.9. The data for a) and b) however yield velocities of 9.71 m/yr and 17.39 m/yr, respectively, translating to a much more plausible acceleration of factor 1.79.
We thank you for emphasising this point. We discussed this again in detail and follow your arguments. Therefore we have removed these results and revised the manuscript accordingly.

<u>Page 5</u>

Comment: 1
Sorry for the typo...

Comment: 2
Exactly. That is why I commented that in theory, planet offers a big advantage here with daily data availability. But very practically, as you show with Tab. 2, only ~10% of the planet scenes were usable. Effectively you end up with rather similar revisit times between planet and sentinel 1, if you want to use these examples here. But don't get me wrong, optical imagery has a lot of other advantages...
Thank you for this suggestion. We have modified the text accordingly.

[L510-514] Regardless of any meteorological constraints, the promised daily availability by PlanetScope is unrealistic, due to data gaps and provider issues; our study showed that for the Sattelkar from April to October 2019 only 11 % of the captured images during this time were usable. Hence, PlanetScope data ends up in a similar temporal availability to Sentinel–1 with a 6-day revisit time. In time–critical early warning scenarios, when time is running out, all available even partly usable images will be utilised and fieldwork may be conducted, even if the prevailing conditions are suboptimal but will increase data availability.

<u>Page 6</u>

Comment: 1
Thank you for clarifying this. Please, include this in the manuscript and replace the ambiguous term "spectral colour problems" with the much more precise spatial offset or "shifts in the individual bands".
Thank you for this suggestion. We have modified the text accordingly.

[L267-269] Thereafter, a second selection (visually with the 'Map Swipe Tool' plugin) from the downloaded images was filtered for errors of location, inter-tile shift and shifts in the individual bands ~~spectral colour problems~~ which were previously not clearly discernible in the online data hub.

<u>Page 8</u>

Comment: 1
Thank you for the detailed answer. My comment was intended to motivate the authors not only to provide me with these details, but to include them in the revised version of the manuscript. Please include this explanation, or a short version of it, in the text. Regarding the residual mismatches stated here and shown

in Fig. 14 of the OSM: this already gives a measure of the amount of significant displacement, i.e. beyond a level of detection, that can be detected with DIC between these images. If your residual mismatch after coregistration (as shown in Fig. 14 OSM) averages to 0.6 - 0.8 pixel, everything below that cannot be treated as significant motion. But still, this would be much lower than your arbitrary 4 m and would be based on a preproducible quantification approach.
Thank you for this good suggestion. We have modified the text and added the information in the methods section of the manuscript.

[L462-464] This is clearer for the time interval I (376/370 d) (Fig. 5a vs. c) as for the longer temporal baseline the total displacement accumulation is higher, thus better captured by COSI–Corr for PlanetScope 3 m resolution. Due to the shorter interval II (42 d) (Fig. 5b vs. d) with less accumulated total displacement, the rear of the landslide is not represented; no signal is shown as the total displacement for PlanetScope was restricted to values above 4 m. Values below 4 m had to be discarded for PlanetScope DIC results as they were lost in noise, i.e. for the entire DIC results there is total displacement between 0 m and 4 m (cf. the online supplementary material (OSM) Fig. 13).

Comment: 2
Though in the current state the details of Fig. 3 b) and h) are difficult to identify, it is clear that the regions with smooth displacement value distributions show consistent bearings, while the regions with decorrelated displacements also show random bearing. In my experience, this is a clear sign of errors during the matching procedure, i.e. the correlation found highest agreement with the original feature. I would recomment to include the bearing information more prominantly in the manuscript itself, as this is a very important information on the data quality.
Thank you for this good suggestion. We have modified Fig. 5 by including the bearing information in the manuscript. The interpretation to this is discussed method text and caption. In addition we have added another map in the OSM showing the displacement vectors only (see. Fig. 17).

Page 9

Comment: 1
I am unsure how the active area extent is related to the definition of the threshold value here?
Thank you for your comment. We apologise for our vague explanation. Besides the value distribution for both, the total displacement results and the SNR including their visual comparison, we used the demarcated active area as visual guidance to distinguish between values within the active area and aberrant displacements outside of it. Please see our comment below.

Comment: 2
It would be appropriate to include this description into the manuscript. As I pointed out in my comments in the first round of reviews, the error assessment is very important and just setting an arbitrary threshold of 4 m without elaborating the calculation of this values is insufficient.
Thank you, this is a good suggestion and we included it in our revised manuscript.

[L307ff]
Displacement below a 4 m threshold was discarded from the PlanetScope datasets due to aberrant values (noise, outliers). The threshold definition was defined on (i) the value distribution in both the total displacement and the corresponding SNR result, and (ii) a visual comparison of the maps for the total displacement and the SNR. This definition allowed us to identify outliers and unlikely displacement. Apart from this threshold; no other filters were employed, and we ~~maintained~~ kept the output raw (see for raw DIC on PlanetScope OSM Fig. 13).

Comment: 3
I strongly doubt that you can make this statement. The image processing algorithm matches pixel value distributions from patches in consecutive images. When the pixel value distribution changes between images, the algorithm does not find the "true" corresponding area, but matches to the most similar patch it finds. Whether the pixel value distribution changes in reaction to snow cover, shadows, or vegetation, or if large displacements induce the spectral differences is not discernable for the DIC software.
We completely agree that the software cannot differentiate between the different reasons for the resulting displacement; hence the software returns matches for pixel changes of rediscovered reference patches independent of the reason for these changes. And yes, you are right we used the same input data here (UAS orthophotos) and compared two different orthophoto derivations to check for result behaviour and consistency.
What we intended to say is that the user may be able to determine if the orthophoto shows differences in illumination, vegetation or snow cover, or if volumetric calculations show significant surface elevation changes (as visualised in the OSM Fig. 2 and manuscript Fig. 6).
We have revised our manuscript and described this in more detail.

[L469-473] Measured ground motion of block tracking and PlanetScope results indicate and support existing high ground motions. In addition there are morphologically significant volumetric turnovers with areas of large gains and losses between ± 5 m (see OSM Fig. 11). These~~is~~ observations might ~~be the~~ explain~~ation~~ ~~for~~ the observed~~resulting~~ decorrelation at the finer resolution of 0.16 m for the landslide head: the matching window is smaller than the offset and texture surface changes are too complex to be re–detected, i.e. matched, and thus correlated, leading to decorrelation and noise. Homogeneous correlated patches are in the front of the landslide body for the shorter time interval; there may have been some displacement just below the detection threshold for this high ground motion or some boulders and their surroundings might have been matched, or both (Fig. 6a (a)). . ~~For~~In this case for the complex ground motion with high spatial resolution data ~~this reason~~, the previous assumption ~~using~~ based on a shorter time interval likely leads~~ing~~ to improved detection of inherent process behaviour (see Sect. 6.1.).

and

[L430ff] In sum, though the results contain heterogeneous, noisy, decorrelated areas, the combination with homogeneous displacement areas still offers valuable insights into this and other internal landslide structures and complex behaviours.

Comment: 4
While I do agree that visually tracking boulders in orthoimages gives reliable results that underpin the displacement that was obtained with digital image correlation in the same images, the term verification implies that these displacements come from independent data.
Yes, you are right. To call it “verification” two independent data types would be necessary. Thank you, we understand what you meant and therefore changed the term accordingly to a better suited description. Please see here also our answer to the previous comment (no. 3, p. 9).
Visual tracking of 36 single blocks, identifiable in the UAS orthophoto series allowed deriving direction and amount of movement; this supported the ~~verification~~ confirmation process ~~of~~ for (i) the total displacement and (ii) the results of automated and manual tracking. In the next section we present this approach only for time interval II.

Page 10

Comment: 1
This is a good idea. Sadly, but maybe owed to the reduced quality of the review files, I find it very hard to identify vectors shown in Fig. 3 b) and h). Also for the signal to noise ratio figures, shown in Fig. 3 d) and j), the transparent grey colours on top of the greyscale hillshade image are difficult to identify.
We are sorry that the results are difficult to identify and hope that the quality of the uploaded material is now higher.
In addition, we have added displacement vectors to Fig. 5 for the UAS DIC result interval II (13.07.2018–24.07.2019).

Page 11

Comment: 1
In my view, it might be advisable to plot the manually tracked boulder displacements against the mean displacement obtained by your DIC for the surrounding pixels (and not just one pixel). As image correlation for tracking is based on matching of value distributions, i.e. matching patches of pixels, adjacent pixels should show similar displacement magnitude and bearing. With this, you could strengthen the argument that the boulder tracking actually backs the DIC tracking - maybe not for all regions, but for the majority.
Thank you for this good suggestion. We have calculated the mean total displacement from COSI-Corr of 0.1 m buffers around the manually tracked boulders and plotted them against the distance the boulders travelled (please see the OSM Fig. 15 and Fig. 16). The residual standard error is 0.7137 (on 31 degrees of freedom) with a multiple R-squared of 0.7425.

Page 12

Comment: 1
I don't think that the comment on L457/462, where you limit your answer to areas outside the landslide area is related to the issue raised here.
Thank you for this comment.

Comment: 2
It is my impression that the authors have understood my above comment differently than it was intended. Figs. 5 a) and e) as well as b) and f) show the results of the first and second interval, respectively. While a) and e) result from DIC of 0.16 m resolution orthoimages, b) and f) result from the same images, but resampled to 3m. Yet the pattern and magnitude of e) and f) do in no way match the data in a) and b), while it is the same data over the same time interval. My point is that this does not make sense from a technical point of view when automatically tracking features in the same images, with only a different resolution. I would think that the authors need to decide, which of the two solutions is closer to the truth, but in my view it has to be made absolutely clear in the manuscript that these data do not match. When pointing out that I do not see a plausible explanation for the data presented in e) and f) from a geomorphic point of view, I was hoping for a critical evaluation of this data.
You are right. We discussed this matter and agree with the points you mentioned. Therefore we deleted the row in Table 6, modified Fig. 5 including its caption and revised the methods, result and discussion section accordingly: L313-314, 317-318, 322-323, L352-359, L457-460 and L467-470 were deleted, L460-462 were revised and extended.

Comment: 3
Again, I fear this misses the point. In the text you state that false-positive displacements were observed and I was just interested in the analysis conducted to identify these. Please elaborate.

Thank you for emphasising this. We have investigated the orthophotos and satellite images in detail. Please see our analyses in the OSM Fig. 2, 11 and 12. We have modified the text accordingly.

[L407 ff] ~~Most~~ The patches (j) and east of (j) are identified as snow fields in the orthophotos and the noise results from decorrelation. In Fig. 5a, the large southern patch (g) shows clear displacement values for the rear part and decorrelation for the front region resulting from ~~significant~~ morphological changes within ~~an~~ the image pair of interval I (see OSM Fig. 12). This is due to a gain between 1 and 2 m for an area of about 250 m^2.

Further, the second comment refers to the general statement you are trying to make here from your analysis. As mentioned before, there are a lot of approaches to enhance tracking results and I would urge the authors to be very cautious in attesting planet imagery a limited use here, given the challenging results obtained.
Thank you for clarifying this.

<u>Page 13</u>

Comment: 1
I have to express a certain degree of frustration with this review, as the author's tendency to rebuttal seems higher than their inclination to improve their manuscript: in Line 1 (the tiltle of your manuscript) the word LANDSLIDE is misspelled. And it is still misspelled in the title of the revised manuscript and on the NHESS webpage.
We are very sorry for this spelling mistake. None of us noticed it through revising the manuscript multiple times prior submitting it to NHESS. We immediately notified NHESS about this typo and asked for it to be corrected on the website and corrected it in our manuscript, too.
We apologise as we misinterpreted what you meant by ‘landslide’ in your first review, assuming you wanted an explanation and better description of our understanding of the Sattelkar landslide.