Letter of response to comment on nhess-2021-18

Dear Jan Blöthe,

We thank you and appreciate your valuable comments on our manuscript. Your feedback has helped us to improve our work and pointed to areas which were ambiguous and therefore needed clarification.

Please find below the following colour coding for the review and your comments in black; our responses to the review are in blue and the changes made to the manuscript are in green.

General comments

A) Description of digital image correlation method and error assessment

In my view, digital image correlation is not a trivial method and deserves a more detailed description in section 4.3. Especially because the conceptual approach presented here grounds on the detection of significant movement (or even acceleration) from optical imagery, the authors should elaborate the exact processing steps and include a detailed accuracy assessment. This can easily be achieved by:

- The quantification of a level of detection between images, i.e. the residual mismatch of stable surfaces outside the landslide between consecutive images after image correlation, beyond which significant displacement can be detected with a given confidence.
- Excluding spurious matching results (displacement vectors) on the basis of a correlation threshold.

The description of section 4.3., Data Acquisition and Processing, has been modified by adding more details.

The attached Online Supporting Material (OSM) contains the variety of results which show our approach to selecting the appropriate combination of UAS input data (orthophotos, DSM and hillshade derivates) and displacement vectors (see OSM Figs. 7, 8 and 9). In addition, signal to noise results and volume calculations are provided (see OSM Figs. 3, 5, 8, 9 and 11, 12). The distribution of GCPs combined with DIC total displacement results of UAS are also presented (see OSM Fig. 1 and 4).

In terms of the selection of appropriate parameter settings, we decided to use:

- for a step size of one, as larger step sizes smoothed the velocity pattern, did not obviously improve the matching while decreasing the spatial resolution. Computation time would decrease if larger step sizes are employed.
- UAS 128 x 32, as an initial window of 256 returned a general decrease in velocity. Furthermore, the smaller initial window of 64 matching was only partially successful with very low velocities. The final window size is important to detect small scale features. If set too large, features could be smoothed out. In our case there were no distinct differences, which is why we selected the smaller final window option: to necessary small scale features.

However a detailed accuracy assessment requires comparable data which is not available such as in the verification process of DEM production based on stable surfaces. Therefore, we added the signal to noise results as you requested in the OSM. An accuracy assessment similar to Travelletti et al. (2012) having GCPs within the active landslide cannot be conducted, as in contrast our GCPs are located on stable positions outside of the active landslide (see OSM Fig. 1 and 4). Our approach to this study is to compare manual block tracking with the calculated velocities from DIC as part of the data evaluation.

B) Result of image correlation

As stated above, digital image correlation and the extraction of displacement from correlated imagery is not a trivial task and many pitfalls can lead to spurious results (the authors term these decorrelated). I will outline my doubts regarding the validity of the obtained displacement values referring to Fig. 5, but have given many detailed comments on the respective text positions in the specific comments below. In large areas, the image correlation returns areas that are "decorrelated", such as the western part of the landslide in (a) and (b), but also positions in (e) and (f) are affected by this. In my experience, such a pattern indicates that matching between images did not work, which should be visible by adjacent vectors having very different magnitudes and directions. Furthermore, the patchy nature of displacement values in the western part of (c) is very surprizing. Here very high total displacement of ~18 m is located in the vicinity of displacement on the order of 4-8 m. From an image matching procedure, I would expect a rather smooth picture here, such as in (d). But also from a geomorphic perspective, I am unsure how this pattern could be explained by a natural process. Finally, the results obtained from the downsampled UAS DEMs predominantly show high rates (16-18 m) that are interrupted by areas of no movement or very slow movement. My impression would be that these results are least reliable, because a) they show a completely different picture as (a) and (b), while being computed with the same data (just a different resolution), b) the displacement values are nearly the same for two very different time intervals (e = 376 days, f = 42 days), c) they are not matching the values obtained from manually tracking boulders (again, based on the same data), and d) I am unsure if such a pattern can be produced by a natural process.

Having outlined my reservations regarding the image correlation results, let me suggest a couple of strategies to improve the results:

- Use a hillshade not a DEM for tracking (not clear if this was done)
  Originally we used UAS orthoimages. Please see the OSM Fig. 8 for calculations using DSM and OSM Fig. 9 using hillshades.
- Resample the DEM to a slightly coarser resolution (0.5 m?)
  We have tried a 0.5 m resolution for the UAS orthophotos with different parameter settings showing overall better matching with still some decorrelation. However, with this input resolution and the best suited parameter settings of 128 x 32 the extent is already decreased in its size to a smaller displacement area.
- Try a different software for image correlation, there are many and all have their advantages and disadvantages
  This was done with DIC–FFT and IMCOOR (please see OSM Fig. 10 for results of DIC–FFT).

- Have a detailed look into the correlation coefficients and the bearings of the displacement vectors and exclude spurious results.
  Please see OSM Fig. 3 (b) and (h) with displacement vectors and signal–to–noise maps in the OSM Fig. 3 (c), (i) and (d), (j), Fig. 5 (f) and (i) and a cross profile cutting the DIC total displacement for both intervals I and II, Fig. 6.

Yes, indeed a mismatch of the initial and final search windows, i.e. a decorrelation, is visible for many areas but especially obvious in the western part of our DIC results. The current literature states among others that there is an upper limit regarding velocities of ground motion (Delacourt et al. 2007; Travelletti et al. 2012). In this area very high motion clusters of this complex landslide exhibit debris slide characteristics. We observed that acceleration of the landslide body takes place here. In contrast, in the eastern part of our DIC results, there are correlated areas and smooth motion patterns indicating that matching took place and the method was successful with the applied parameter settings.

Additionally, in our case, the terrain surface is altered rapidly; big blocks with edge lengths of up to 10 m rotate and cause significant surface changes, which could be a further reason for decorrelation (see OSM Fig. 9 for results of DIC–FFT) (Lewis 2001; Stumpf et al. 2018). The geomorphic causes for the observed acceleration are unknown but could be related to permafrost degradation and increased infiltration of rain- and meltwater.

In the OSM we support the result from DIC with the corresponding displacement vectors (OSM Fig. 3 (b) and (h)).
With regard to the 3 m downsampled UAS orthophotos we are aware that these results are less trustworthy in terms of delineated velocities. Here, our purpose was to compare two different sensors in order to see how accurate PlanetScope data are for high alpine displacement calculations. Please see here our comment further below.

- L22/23: While this is certainly true, the authors should elaborate in the introduction that events instantaneously triggered by earthquakes or heavy precipitation are beyond what their proposed framework can deliver an early warning for. The necessity of gathering and evaluating data prior to issuing a warning limits the analysis to mass movements that indeed show a pre-failure acceleration on the order of days.
  Thank you for highlighting this. We totally agree that this has to be mentioned in the beginning to complement our explanations in the discussion, L561/562.
  L31: This definition of an early warning system (EWS) contains a time component but includes no exact time scale reference. ~~'Early' suggests that events are detected before harm or damage occurs and thus stands in contrast to events which are only detected once they have begun (e.g. snow avalanches). Thus, it is necessary to know sensor capabilities and limitations for pre- event mass movement observations (Desrues et al., 2019). The success of a warning requires that information is provided with enough lead time for decisions on reactions and counter measures (Grasso, 2014).~~ The success of an EWS therefore requires measurable pre-failure motion (or slow transport velocities) to allow for sufficient lead time for decisions on reactions and counter measures (Grasso, 2014). In this regard, knowledge on sensor capabilities and limitations is essential, as it determines which rates and magnitudes of pre-failure motion can potentially be identified (Desrues et al., 2019). Our proposed framework refers to mass movements with significant pre-failure motion operating over a sufficient time periods and thus excludes instantaneous events triggered by processes such as heavy rainfalls or earthquakes.

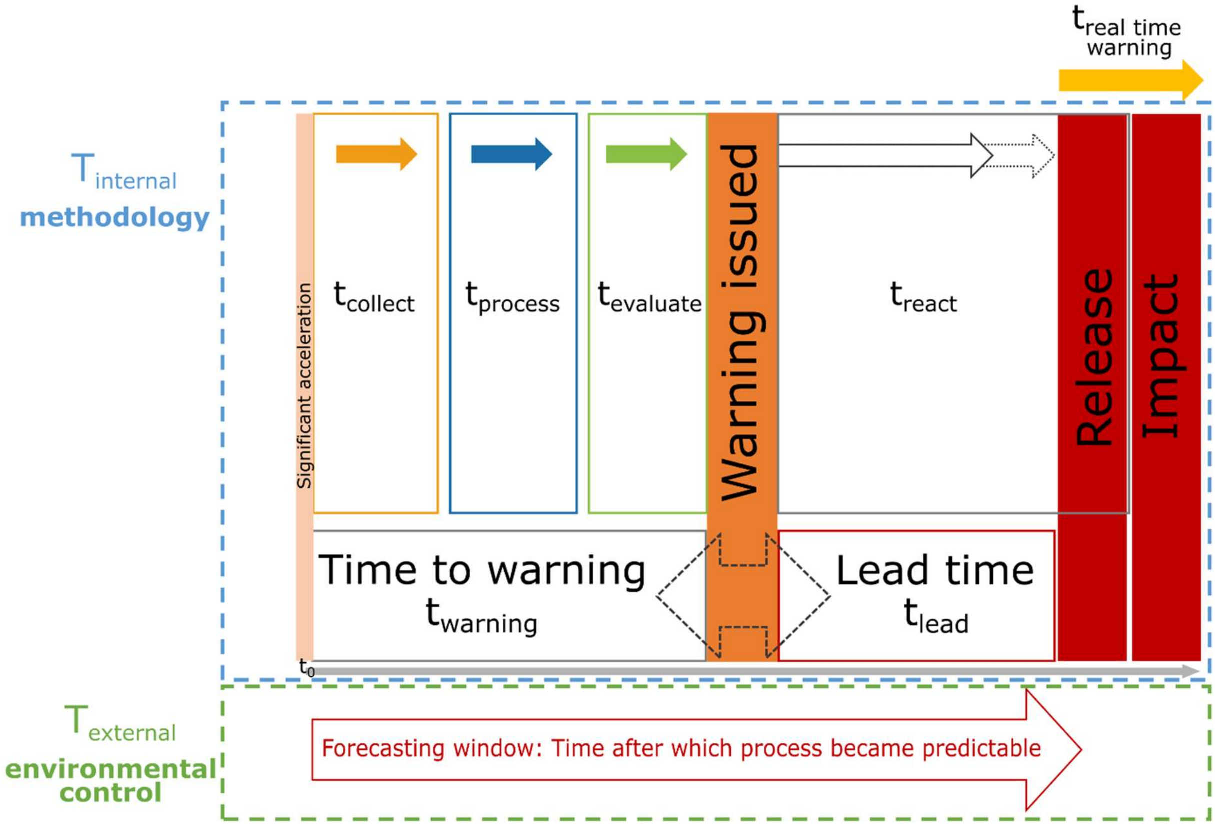
- L25/26: Is this really just attributable to the warming of the climate?
  To the best of our understanding and following Gariano and Guzzetti in their review (2016) the global climate warming directly and indirectly impacts natural and human induced factors which can again directly or indirectly condition landslide activity, abundance and frequency of events. Other reasons for landslide triggers are included in L22/23, earthquakes, rainfall events and human interaction.

- L47/50: I would think that also the rate of landslide movement defines whether or not it can be detected by optical imagery.
  Thank you for pointing out that detection is not restricted to sensor characteristics. This is very important to say, of course.
  ~~Until r~~Recently, the spatial and temporal resolution of optical satellite imagery has significantly improved ~~requirements for accurate early warning purposes have not been met by optical satellite imagery~~ (Scaioni et al., 2014) and has allowed substantial advances in the definition of displacement rates and acceleration thresholds to approach requirements for early warning purposes. This is essential since spatial and temporal resolution determines whether landslide monitoring is possible with the detection ~~allows defining~~ of displacement rates and the approximation ~~enables approximating~~ of acceleration thresholds, which both are lacking if information is based solely on post–event studies (Reid et al., 2008; Calvello, 2017).

- L79/80: This is the maximum revisit time at the equator, right? For the study area shown here, revisit time should be shorter.
  Yes, thank you for mentioning this. We will differentiate here between revisit frequency and repeat frequency, with the latter of importance for coherence.
  One advantage of optical imagery is its temporally dense data (**Fehler! Verweisquelle konnte nicht gefunden werden.**) compared to open data radar systems with sensor repeat frequency every six days and revisit frequency between three days at the equator, about two days over Europe and less than one day at high latitudes (Sentinel–1, ESA).

- L121: What do you mean by "natural developments" and how are these conditioned or different from natural processes?
  Thank you for this comment. We are sorry that this was not specific. We meant the development of natural processes.
  Natural processes and their developments constantly take place independently, thus dictate the technical approaches and methodologies researchers must apply within a certain time period.

- Figure 1: While I like the idea behind this conceptual figure, I would recommend the authors add a time axis and limit the area of "significant acceleration" to a vertical line that coincides with t = 0. In the present form, the conceptual figure contradicts statements in the text, such as "The forecasting window is started […] following significant acceleration […]" (L126), or "Simultaneously with the forecasting window, time to warning ($t_{warning}$) starts (grey outline)" (L128/129).
  Thank you, you are right. We changed it to our best understanding of your feedback.



- L133/134: This also does not match what Fig. 1 is showing
  "The lead time is the difference between the forecasting window and the time to warning."

  We want to express that t_{lead} is the rest/remainder of the subtraction as follows
  $t_{\text{forecasting window}} - t_{warning} = t_{lead}$
  Please let us clarify this as it seems to be some sort of misunderstanding here.
  As a suggestion, this could be replaced with L133/134, if you prefer "Lead time is the forecasting window minus the time to warning."

- L139: This also does not match what Fig. 1 is showing. In Fig. 1, tlead < treact.
  "An imperative for an effective EWS, the required time to take appropriate mitigation and response measures has to be within the lead time interval (tlead) (Pecoraro et al., 2019) with tlead ≥ treact".
  Please let us try to clarify this: in best case, the lead time is longer than the time needed to take responsive measures and react to the impending event (t_{react}), this is indicated by the shorter solid grey arrow. However, if the reaction time is as long as the lead time, see dashed extension of the grey arrow, then it is a coincident ending of both, t_{react} and t_{lead} prior the release and impact.

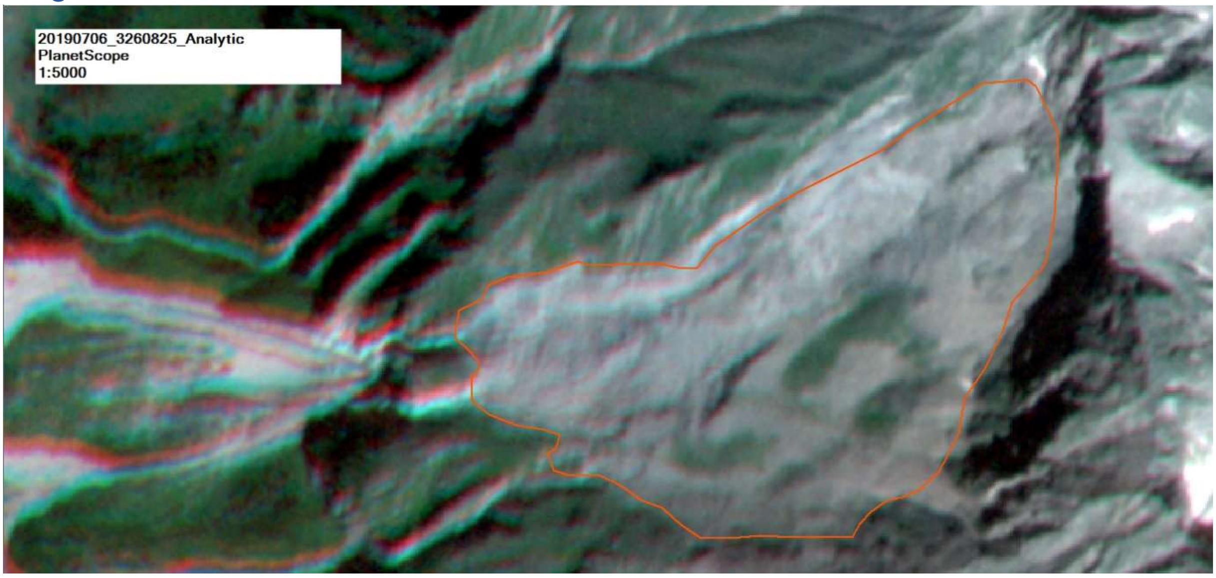
- L215/127: In theory yes, but as you show later (Tab. 2), the effective revisit time of optical imagery might in fact be very similar.
  Unfortunately, we do not understand what you are referring to in L127.
  L215: Sentinel–1 does have a revisit time of about every second day over Europe. However, the repeat frequency for coherence to generate interferograms is every six days. This is the shortest possible temporal baseline.

In terms of optical satellite images, yes, this is what the author team finally wants to lead to. PlanetLabs claim to have daily acquisitions and thus can provide daily imagery supply. But upon a closer look the practitioner knows the reality is different. This has to be kept in mind if this kind of data is employed for the purpose of a reliable monitoring and process observation. For this reason, Table 1 and Table 2 have different and contradicting statements, in this case for PlanetScope.

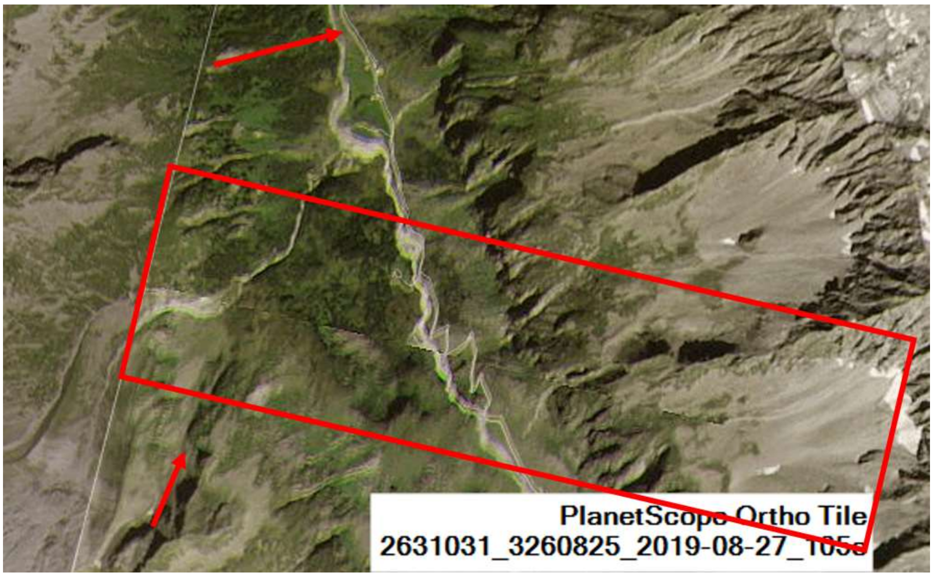
You are right in some way: free satellite images by Sentinel–2 are, at five days, very close to the six days for interferograms by Sentinel–1, given that both sensors are suitable for the given characteristics by the acquisition target (motion velocity, exposition). Apart from open data providers, there are many others providing even sub–daily acquisitions such as WorldView 3/4.

- L242/248: It might be worth mentioning here that on average, only 11% of the images were usable, significantly reducing the theoretical revisit time, as you also outline in the discussion.
  Thank you, indeed this is worth to be mentioned and we changed accordingly.
  In this seven–month period, 43 images (20.1 %) had data voids or did not cover the AoI, thus the overall usability is limited to about 11 %.

- L267/269: Please elaborate how you filtered for "errors of location, shift and spectral colour problems" (are the latter spectral differences between images?).
  We used QGIS software to manually select the satellite images with the reference UAS images at the base and the visual "show/hide" of the satellite slave images on top. Similarly, the application Map Swipe Tool plugin was employed by dragging the slider across the images.
  Spectral colour problems are shifts in the individual r, g and b bands within one single image:



The other shifts which might occur cannot be corrected for. The first time these can be detected is in a GIS software with the visual check previously described:



Thereafter, a second selection (visually with the Map Swipe Tool plugin) from the downloaded images was filtered for errors of location, shift and spectral colour problems which were previously not clearly discernible in the online data hub.

- L281/285: Please specify the accuracy of dGPS coordinates as measured for the GCPs and also include an accuracy information for the DEMs and their derivatives that were produced from UAS surveys.
  The accuracy of dGPS coordinates, which were employed for the processing of UAS data and DEM/orthophoto generation, range between 5 cm horizontally and 10 cm vertically. All UAS model calculations are based on the same dGPS measurements.
  The RMS errors from UAS image processing in Pix4Dmapper range between 4 and 8 cm. If generation reports are necessary, they can be provided on request later (due to current office access difficulties).
  These were repeatedly (1000 measurements/position) registered with the TRIMBLE R5 dGPS and corrected via the baseline data of the Austrian Positioning Service (APOS) provided by the BEV (Bundesamt für Eich– und Vermessungswesen). Horizontal root–mean–squared errors (RMSE) range from 0.05 m to 0.10 m for vertical RMSE. These GCPs were employed for georeferencing and further rectification of all UAS surveys.

- L285/286: Please elaborate how image co-registration was achieved and state here the residual mismatch between co-registered images.
  DIC methods for estimating terrain movements require accurate geo-referencing of consecutive satellite images avoiding falsely detected systematic drifts. Although the investigated satellite sensors are equipped with high–quality geo-localization sensors, subtle deviations in the absolute geo-referencing rates are expected for different acquisition times.
  Therefore, a fine–registration between satellite image patches in the AoI was conducted based on a Matlab script (by Tobias Koch) applying a state–of–the–art image registration technique (Lowe 2004). Since radiometric differences between the different acquisition times and image distortions (e.g. clouds) could remain in the images, feature–based registration methods are preferable over correlation–based registration methods due to their ability to match local feature points instead of entire image areas.

To ensure that actual terrain movements in the AoI do not cause undesired shifts in the registration, the AoI was excluded from the feature point detection step. The remaining feature points were used for estimating a geometric similiarity transformation between the reference and all target images including a statistical outlier removal (RANSAC). This transformation was finally used to accurately register a target image towards the reference image.
Regarding the registration quality in the test site, a satisfying amount of feature matches of at least 500 after outlier removal could be found for all reference (master) and target (slave) image pairs and for all investigated sensors. The mean distance of transformed inlier feature points of the target image to their corresponding feature matches in the reference image ranged between 0.6 and 0.8 pixels, confirming the high registration accuracy (see OSM Fig. 14).

- L288/289: Usually matching between consecutive images is not achieved by matching "common pixels", but by maximizing the correlation between pixel-value distributions of patches of pixels (i.e. your windows of different sizes in Tab. 6).
  Yes, you are correct it estimates first the pixelwise displacement between two patches based on correlation peaks and second, the final correlation is performed to retrieve the subpixel displacement.
  We added this information and reordered the processing steps according to the COSI–Corr manual (Ayoub et al. 2009).
  There are two correlators; in the frequency domain based on FFT algorithm (Fast Fourier Transformation) and a statistical one. Applying the more accurate frequential correlator engine, recommended for optical images, different parameter combinations of window sizes, direction step sizes and robustness iterations were tested. Parameter settings include the initial window size for the estimation of the pixelwise displacement between the images and the final window size for subpixel displacement computation in x, y; a direction step in x, y between the sliding windows; and several robustness iterations (**Fehler! Verweisquelle konnte nicht gefunden werden.**).
  […]
  The results of each correlation computation returns a signal–to–noise ratio map (SNR) and displacement fields in east–west and north–south directions. These results were exported from ENVI classic as GTiff, and the total displacement was then calculated with QGIS.

- L304/305: What is the uncertainty of these east-west and north-south displacement estimates? Did you check whether the bearing of the displacement matches the general slope of the Sattelkar?
  In the OSM we are providing the results of the correlation computations for our published results (east–west and north–south displacement fields as well as signal–to–noise maps). The results are consistent. We further provide total displacement results of other parameter combinations.
  Yes, we checked the overall orientation of the correlation based on computed directional vectors (with SAGA GIS software). We provide these vectors in the OSM, too (OSM Fig. 3 (b) and (h)).

- L307/308 and L440/442: This seems a bit arbitrary. How did you determine a cutoff–value of 4m displacement? How did you distinguish outliers from non-outliers? What is the confidence of your estimates?
  We determined the cutoff–value employing several criteria. First based on field experience we know the landslide extent and displayed the results in combination with the demarcation displayed as 'Active area' in Figs. 2, 3, 5 and 7. Then we checked the value distribution in the histograms for both the calculated total displacement as well as the signal–to–noise maps. These maps were further used to visually compare the total displacement results. This allowed us to identify outliers and unlikely displacement. Based on the histograms and the acquired experience for the results, the thresholds were tested and set for transparency and to display values. Please see the OSM (Fig. 13).

- L308/309: This contradicts the descriptions of Fig. 5a, where you point out that "ambiguous, small-scale patterns with highly variable displacement rates" (L332/333) dominate the western part of the mass movement.
  Here we would like to differentiate between inconsistencies which we understand as artefacts and noise due to snow, vegetation, clouds, cloud shadows and terrain shadows. De–correlation with its salt–and–pepper appearance due to velocities exceeding the correlation capability of DIC have a different origin and reason.
  However in the results, section 5, we described the appearance of these ambiguous signals, while in the discussion section they are explained.

- L311/312: I am not convinced that manually tracking boulders in the same images that were used for image correlation can verify the results of this correlation. You can use these data to check if manual and automated tracking give consistent results. Comparing manually tracked boulders from UAS imagery could however be used to compare against the displacement estimates from satellite imagery.
  We are certain that the direct measurements of travelling distances from blocks of 10 m size for consecutive orthoimages, which were also employed for the DIC method, are a valid method to underpin the total displacement results by the DIC.
  Comparing these tracks with satellite imagery might be useful keeping in mind that the difference between UAS orthoimages of 0.16 m and PlanetScope satellite images of 3 m spatial resolutions is substantial and sensor type, image processing etc. can introduce further inaccuracies.

- L320: As you present total displacement for different time intervals here, not rates in distance per unit time, I would suggest changing the title here. Same is true for L326, L346 and L361.
  Yes, thank you for pointing this out. We changed the section title (see below) and in the text accordingly (L326, L346, L350, L354, L357 and L361).
  Section Title: 5.1. Total displacements ~~Displacement Rates~~

- L335/336 and L366: Did you check the direction of displacement for the areas of smallscale patterns of ambiguous signals? I would suspect that these are very heterogenous here as well. It would also be worth looking into the quality information (correlation coefficients) for these regions.

Yes, this is a good point. Indeed, we checked the direction based on displacement vectors as well as signal–to–noise maps. They both give the same indication of heterogeneous and ambiguous signals with no correlation for exactly the same areas with ambiguous signals in the total displacement calculation. Please see our OSM (OSM Fig. 3).

- L397: For a comparison (and also for a better readability) you could convert your total displacement to average rates of m yr-1 or cm d-1.
  Yes, converting them into averaged rates is a good suggestion for the discussion section, see below. If you recommend this conversion for the results section 5 too, then the section title should be kept "Displacement rates" as before (see your previous comment for L320). For the (old) L417, 418 and 420, the values were added with yearly rates in brackets:
  trajectories up to 4 m (34.8 m yr^{-1}) (d); a 16 m (139 m yr^{-1}) trajectory (a); approximately 10 m (86.9 m yr^{-1})

- L398/399 and L402/404: Given the large differences in total displacement between sensors and resolutions used for image cross-correlation, I do not think that you can make this claim. Please use an appropriate measure to quantify the agreement between manual boulder tracking and the three different approaches used for digital image correlation.
  These lines refer to the results of the total displacement derived from UAS orthophotos. Regarding L398/399 the parameters were tested and selected independent of others' recommendations, but we arrived at the similar conclusions. With regard to L402/404 we believe that the travel distance measurements of field mapped boulders based on the same data (UAS orthophotos) are comparable to DIC derived total displacements.

- L419/422: This might be the case, though you tested larger patch sizes (Tab. 6) that should have given you consistent results for this region then.
  In the OSM we provide results of our parameter tests for larger final window sizes (see OSM Fig. 5 and 7).

- L433/434: This should be backed by a statistical measure. From a close look to Fig. 5, I rather get the impression that the only patches you can make this statement for is location a in Fig. 5 (b) and (d) and location c in Fig. 5 (a) and (c), but to a lesser extent.
  Thank you for pointing this out. In our opinion the first time interval with slightly more than one year of accumulated displacement, the frontal area and core body of the landslide are reflected in both DIC results of UAS and PlanetScope (locations (*a*) and (*c*), as well as (*d*) and slightly (*e*) and (*f*) in Fig. 5 a) for UAS and c) for PlanetScope I). In contrast to the second interval of 42 days, it seems that there is not enough accumulated displacement to be captured by PlanetScope DIC, as the middle to rear landslide body are only reflected in the UAS DIC result (locations (*b*)–(*d*) Fig. 5 c) and remain free of signal for these locations in Fig. 5 d) for PlanetScope.

- L445/447: The size of the snow patches does not play an important role. The presence of snow in one image hampers correlation between images and leads to false patchmatching results.
  Yes, we absolutely agree and this is also described by Leprince et al. (2007; 2008), noting that variations, thus the difference in snow cover, limit the technology. In addition, they say that in images with high gains, the areas of snow coverage are saturated too, and as a result, do not allow for any correlation (Scherler et al. 2008).

Regarding the displacement for (j) as identified in both sensor combinations (see Fig. 5), there is a patch of snow (1–2 m height, length ~ 25 m, see OSM Fig. 10) in the UAS and PlanetScope images on 24.7.2019 while for the images on 13.7.2018/19.7.2018 (UAS/PlanetScope) and 4.9.2019 (UAS and PlanetScope) there is no snow (see OSM Fig. 2 and 11). Thus, in this case, the existence of snow in one image but not in the other explains this false correlation and indication of displacement.

Minor snow fields as visible in the images from 24.07.2019 for both, UAS and PlanetScope, likely explain the big cluster of incorrect displacement southeast of the lobe (*j*); nonetheless, in the satellite image they are smaller than the resulting DIC displacement.

- L457/462: To be frank, I do not see much similarity between Fig. 5 (c) and (e) nor (d) and (f). I would be very cautious in interpreting these results as is. This is especially true for the resampled UAS results.

  Thank you for pointing this out. Yes, we agree in some part. Our purpose was to compare our high accuracy UAS orthophotos to PlanetScope satellite images, in order to estimate the goodness of fit and limitations of the latter.

  We are aware that this downsampling factor is large, and therefore the resulting displacement rates and inherent velocities have to be viewed with reservations.

  However, in terms of noise outside our defined active landslide area and the overall detection to the landslide boundary as delineated based on the 0.16 m UAS data: for the first, the noise is low to moderate, and there is generally a good fit for the 3 m downsampled UAS data similar to DIC results of UAS at 0.16 m, respectively. In contrast, DIC results of PlanetScope neither show likewise noise–free areas outside the active landslide regions nor do they reach the same extent total displacement extent as the downsampled UAS data.

- L463/464: As the GCPs for referencing the UAS data are probably located close to the landslide, it is not surprising, but neither disturbing, that false displacement clusters appear outside the area of interest.

  Please see our map of GCP distribution as well as images thereof in the OSM (OSM Fig. 1). Some GCPs are close to the landslide area, but installed on stable bedrock and to best of our knowledge, they are not moving and thus provide continuous usability and comparability.

  False displacement is indicated for a cluster outside of the boundary to the image border in the east for UAS interval I (Fig. 5e) and in the north western area (*h*, *i*) for interval II (Fig. 5f) contributing to changes in shading and illumination.

- L468/470: Again, I would not trust the displacement estimates of the resampled UAS data. While it is true that your manual boulder tracking identified 2 boulders with displacement of 10 or more meters, the remaining 34 boulders show something different.

  Yes, you are right that not all of the 34 boulders are exactly reflecting the DIC total displacement result. However there are more than two which are in the same range of displacement, and others are very close to it, keeping in mind that there are some uncertainties and limitations when it comes to the threshold of identification of small ground motions in the DIC method. Please see here the section 6.1, discussion. We are happy to revise this further.

- L471/476: While it might be true that the results obtained from image correlation of resampled 3m UAS data are better (internally) correlated and show a more homogeneous deformation pattern, this does not mean that the result is correct. As I outlined above, I have serious doubts regarding the interpretability of this data, as there is no agreement with the manually tracked boulder velocities (except 2 boulders). Also, from a geomorphic perspective, I am not sure how you would explain a velocity pattern where high velocities dominate throughout the entire landslide, but are speckled with lower to zero movement within (Fig. 5 e and f).
  We agree with the 3 m resolution to some extent. Please see comment above for L457/462 the comparability of manual block tracking to UAS DIC result.
  The 'speckled' pattern, is due to decorrelation resulting from velocities too high to be captured with the DIC method; this combined with an observation period of 42 days delay (Delacourt et al. 2007; Travelletti et al. 2012) may be exceeding the accumulated displacement to be captured by the method, which could contribute to this pattern and explain the resulting limitation to some extent. In addition, we know that the surface changes significantly in the frontal part and these strong alterations also limit the DIC method (Lewis 2001; Travelletti et al. 2012). For more please see section 6.1, discussion, too. If this is not clear enough in the discussion, we would be happy to further revise this.

- L485/488: Did you evaluate the proportion of false-positive displacements to truepositive displacements and if so, how did you do this and can you please include this data? Based on the image correlation results shown here, you can make this statement, but I would be cautious to make a general claim on the usability of the data.
  We approached our results by testing of different parameter settings and combinations based on visual comparison as is common in the field (Bontemps et al. 2018). The PlanetScope DIC results presented here are the most suitable master–slave image combinations. We could provide the other intervals of DIC results which are not meaningful for comparison if wished.

- L552/554 / Table 7 / Figure 9: I do like the idea behind this, where the authors show that their proposed workflow would enable a timely warning in the case of historic landslides. However, in the case of Vajont, I think you should include a critical factor. While it is theoretically true that a "forecasting window" would allow for your workflow to be completed well before the failure, the slow deformation of Vajont (35 mm d-1) in the 30 days will be well below the level of detection of your image correlation analysis, if you collect an image directly after the onset of "significant acceleration". In order to be detectable, movement must have accumulated a critical distance before data collection of your workflow can set in (30 days = 1.05 m total displacement) – a factor that in my view would be important to include here.
  Thank you for mentioning this, you are absolutely right. We added the following sentence below to emphasise this critical detection capability limit of the DIC method.
  We assume that approximately 30 days before failure Vajont would have displayed a signal exceeding the noise at modern standards and would have become predictable.
  For Vajont, the 1/velocity plot by Petley and Petley (2006) (based on data from Semenza and Ghirotti (2000)) shows an increase in movement at about day 60 along with a transition from a linear to an asymptotic trend at

approximately day 30, defined as a transition from ductile to brittle. Therefore, we assumed 30 days of forecasting window for twarning and tlead until the impact of the hazardous event on 09.10.1963. However, it has to be kept in mind that velocities of about 35 mm d^{-1} are still low and at the minimum of the displacement recognition capability for the digital image correlation method.

Technical corrections:

- L1: Landslide
  Here we are referring to landslides in general, not to a specific landslide.
- L103/105: Check grammar
  We did not add a comma as the text is in BE; in AE, however, a comma could be added (In this investigation,…). We added quotation marks to improve readability.
- L185: Is this really the source the authors need to cite for the location map?
  Thank you, we modified in response to comment by RC1 (J. Blöthe) by changing Vienna to Wien. Otherwise this is according to the publishing company and the copyright statement from the online map.
  Figure 1 (**a**) Overview map Austria (Österreichischer Bundesverlag Schulbuch GmbH & Co. KG and Freytag–Berndt & Artaria KG, Wien).
- L229: beginning of April
  Thank you, we inserted missing word.
  span from the beginning of April to the end of October in 2019
- Table 3: Here you use a different date format than in the text
  Thank you, we corrected the format. In addition to that we also reformatted the dates in Table 6 accordingly.
- L257: UgCS-Software?
  Further information on the flightplanning Software UgCS can be found here: https://www.ugcs.com/photogrammetry-tool-for-land-surveying
- Table 4: Unit for GSD missing
  Thank you, we added the GSD unit.
- L273/274: Add this information to Table 5 and delete here
  This is a good suggestion and we followed it.
- L299/300: I guess this is only relevant if you explicitly mention the image–processing times.
  Thank you, however we think this is relevant as the duration of image processing and DIC calculation are an important part of our temporal concept in the results section 5.3. and discussion section 6.3.
- L398: can be compared
  We think that the repetition of 'compared' is not necessary; it follows from the logic of the sentence.
- L409/410: resulting from significant morphological changes?
  Thank you for pointing this out. After a detailed verification of volumetric calculations, we can confirm changes of about 1 m. Please see our calculations and visualisations in the OSM.
  In Fig. 5a, the large southern patch (*g*) shows clear displacement values for the rear part and decorrelation for the front region resulting from morphological changes within the image pair of interval I.
- L443: bracket missing?
  Yes you are right, thank you.

- L 460: check figure reference
  Thank you for pointing on this auto–correction mistake.

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