Review of the manuscript "Brief communication: 3D landslide motion from cross correlation of UAV-derived morphological attributes"

submitted by Maria V. Peppa et al.

for the NHESS special Issue "The use of remotely piloted aircraft systems (RPAS) in monitoring applications and management of natural hazards"

General comments

This paper describes how data derivatives of repeated UAV flights can be used to investigate the spatio-temporal development of a landslide. The presented workflow builds upon several established methods (SfM-MVS photogrammetry to produce orthophotos and digital elevation models, DEM differencing, image cross correlation to quantify displacements). As a novelty, the study evaluates the use of morphometric features (openness, slope and curvature) instead of (previously used) shaded relief maps to derive displacement magnitudes and directions. Exploring these alternative raster features as input for the cross correlation function is an interesting idea, and I think in general it is suitable for presentation as a "brief communication" in NHESS. For landslide monitoring such a test is relevant because there is the need for improved analysis methods using high resolution data from close-range and remote sensing.

In general, the manuscript is well written and clearly structured. However, there are a couple of points that should be improved (minor revisions) before I can recommend the manuscript for publication.

The most important issue concerns the objective of quantifying 3D motion. I do not completely understand if and how this is achieved. The results of the presented approach are (i) a horizontal (*x*-*y*) component of landslide motion (2D displacement vectors) and (ii) an elevation difference between two DEMs. These two components can, of course, be jointly used to characterize and interpret the landslide changes. However, I cannot find an explanation how these two datasets are combined to a true 3D data product. Is the elevation difference interpreted as the vertical component (*z*-direction) of landslide motion? The elevation difference at a raster cell is not necessarily caused by a vertical movement; (at least in theory) it could as well result from a purely horizontal displacement of objects/morphological structures. Please consider if this is a valid argument and if this can have implications for interpretation of landslide kinematics. If the presented study actually derived true 3D motions, please provide the required explanations to clarify this. If not, I suggest deleting "3D" from the title to avoid creating expectations by the reader which are not fulfilled by the approach presented in the paper. Instead of "3D motions", the motions may be rather referred to as "horizontal and vertical", or perhaps more appropriate (see above) as "horizontal motions and elevation differences". This should be corrected throughout the manuscript.

Another point that should be improved concerns the limitations identified in the study. A-priori knowledge of the displacement magnitude is required to select suitable parameter values for the tool that in turn derives the displacement magnitude. In areas with strong morphological changes between epochs the cross correlation method fails (which is to be expected). Moreover, surface structures for feature tracking must be identified and marked manually. Finally, spurious displacement vectors are produced and these must be identified and removed manually (so far). With regard

to an automated application of the analysis workflow, these limitations are clearly disadvantageous. The authors address these points properly in the discussion section, but in my opinion, a concise summary of the limitations (at least pointing to their existence) should be included in the conclusions as well.

Specific comments

Comment 1 - p. 1 line 13:
What do you mean with “effective”? Consider deleting this word if it has no specific meaning.

Comment 2 - p. 1 lines 16 – 17:
Consider inserting “characteristic” or “new”: “the formation of *characteristic/new* surface morphological structures”.

Comment 3 - p. 1 line 21:
This is the first use of the abbreviation “UAV” in the main text, so it should be introduced together with its complete name here.

Comment 4 - p. 2 line 6:
Consider replacing “implemented with” by “combined with”.

Comment 5 - p. 2 line 29:
How big are the targets you used? The size of the targets could be valuable information for inexperienced colleagues who want to apply the suggested methods for their own monitoring task and benefit from your experiences.

Comment 6 - p. 3 lines 10 - 11:
An estimate for the sensitivity level is important information. However, concerning the approach for obtaining the sensitivity level, “applying error propagation” is quite unspecific. A citation of Peppa et al. (2016) and maybe the respective reference therein (Wolf and Ghilani, 1997) should refer the interested reader to a description of the methodology.

Comment 7 - p. 3 line 20: “using a 3x3 pixel radial distance”
Assuming that you used a square neighbourhood (moving window) of 3x3 pixel, I suggest rephrasing this to “3x3 pixel window”.
More importantly, this point raises the question if other neighbourhood sizes have been tested and what impact this had on the results. This, however, is later announced as an objective for future work (in section 7), which seems to be a good idea.

Comment 8 - p. 4 lines 5 - 10:
What exactly is the output from the COSI-Corr function? Two maps, one for displacement in Easting and one for displacement in Northing? Are these components combined? What exactly is shown in Figure 3? The maximum displacement magnitude in any direction or only in Northing (because the slope gradient is directed roughly in N-S direction)?

Comment 9 - p. 4 line 10:
How have these “27 sample points” been selected/defined? Randomly? Or at distinct points (such as rock boulders) that can be visually well detected in successive epochs?

Comment 10 - p. 3 line 13:
This is where suddenly “3D surface deformation” is presented, but it was not mentioned before how this three-dimensionality was achieved (and stored in terms of data structure), since the described cross-correlation analysis and the DEM differencing resulted in “2D motion” and “elevation change” respectively. Are these two components (*x*-*y* displacement vectors and elevation change) combined to 3D vectors? If yes, how can these 3D movements be analysed and interpreted subsequently?

Comment 11 - p. 3 line 18: “characteristic surface structures were manually located”
Does this mean groups of pixels, which represent these surface structures, were manually selected and extracted from each epoch and then passed as input to the CIAS tool?

Comment 12 - p. 4 line 20:
Can you be more specific please, for which parameters could thresholds be used (for instance)?

Comment 13 - p. 5 line 27: “a threshold of 63°”
This threshold was empirically determined (by try-and-error or systematically by a specific search procedure)?

Comment 14 - p. 10 line 3: Figure 1 caption
Please consider providing the long name for SNR here as well.

Comment 15 - p. 11: Figure 2
Please provide a description for the grey margin (the sensitivity level).
Moreover, (in the text) you could report the number of samples with displacements larger than the sensitivity level (measured by at least one of the two methods or by both methods).

Comment 16 - p. 13: Figure 4 a) and b)
Please include the arrows (displacement vectors from NCC function?) in the legend or caption and maybe you can improve their contrast a bit.
Moreover, it is not clear what the dark red colour is. Does it represent parts where the 0° - 63° classes of both epochs overlap?