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Natural Hazards and Earth System Sciences
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Discussions

# *Interactive comment on* "Slope stability and rock fall hazard assessment of volcanic tuffs using RPAS and TLS with 2D FEM slope modelling" *by* Ákos Török et al.

**Anonymous Referee #2**

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Dear Editor, dear Authors:

General comment: This manuscript presents an analysis of volcanic tuffs instability along the southern slope of the Sirok Castel hill (Hungary) through multiple remote sensing, field and laboratory techniques. The topic fits the scope of the special issue and might meet the interest of researchers studying landslide hazard and cultural heritage conservation. Having say that, I think that the paper is not ready for publication and needs to be improved.

Specific comments:



1) Even if I am not an English-native speaker, I would recommend an English edit to improve sentence structure and terminology. The text is often difficult to read. Especially, the introduction and the study area description need major rewriting for sense and flow.

2) The aim of the paper is not clearly stated. In this way, also the conclusion seems to be too general and lacking of the result of the analysis.

3) The structure of the manuscript would be improved separating the Discussion section from the Result section. In the actual form, most of the results seem to be not fully described. The authors use too many figures for the description of the results but most of them are not self-explanatory.

4) The description of the study area is too general and not clearly organized. Please improve the description and add details about localization, distribution and geometric characteristic (e.g. dimension and geometry of the blocks) of the existent rock fall deposit at the base of the southern slope of the hill (e.g. page 2, line 26). Additionally, add details about the proneness to weathering of the material forming the slope. This might be a key aspect in long-term slope stability. Consider also to discuss this aspect in the text also in relation to the result of the stability analysis. Avoid to make comparison with other rocks (page 3, line 5), simply describe it in detail.

5) The authors define the RPAS as a tool that (in this case) allow to create a surface model of the study area. In my opinion, this statement does not reflect the real contribution that RPAS bring in mapping and monitoring application and might be interpreted like a "commercial description of the system". I would suggest, to underline that RPAS are simply "innovative and user friendly" platforms that offer a new sensing perspective (previously reserved only for small scale and/or very expensive investigation; e.g. airborne Lidar), reducing the time and cost of data acquisition. This perspective, or in other words the possibility to bring the camera (or the sensor) at specific positions above/around the object and to take images with specific geometries, as well as the

high repeatability, dramatically enlarged applicability of close to mid-range digital and Sfm photogrammetry and surface monitoring in general.

6) From the manuscript, it is not clear why the authors need to use both the "RPAS" photogrammetry and the TLS survey to reconstruct the topography of the slope. Especially, they state (see section 3.4) that the use of both techniques made the result difficult to manage and a specific post-processing is required to solve the redundancy of the result. Considering that the result of RPAS photogrammetry are comparable to that obtained using the TLS surveys, I would suggest use only topographic data derived from the RPAS photogrammetry for the analysis and eventually use TLS data to locally validate the reconstructed topography. In this case, they might consider change the title in: "RPAS photogrammetry for slope stability analysis in cultural heritage site, Sirok Castel hill, Hungary".

7) The method section needs to be improved adding more details about data acquisition and processing. Moreover, the authors often refer to the software used in the analysis. This is a good starting point, but it is important to specify the used criterion/procedure/equation. Please, separate the FEM global stability analysis from kinematic analysis or change the title of the section. In section 3.3, it is not clear: i) if the images were acquired using an image acquisition flight plan with a predefined frontal and side overlaps or in manual model, ii) if camera lenses were calibrated to reduce the effect of peripheral distortion that might affect/compromise the topographic reconstruction, iii) how image alinement was completed (e.g. automatic and keypoints based or picture centers coordinate based), iv) if/how the authors account for picture scale variation due to unconstrained relative elevation (in case of manual acquisition). In section 3.4, it is not clear if and how have you processed TLS point clouds for vegetation removal. Looking at figures 10a, 11a, 14a and 15a it seems that the vegetation was not removed. This compromise the topographic reconstruction of part of the slope creating local anomalies in morphological index maps.

8) In the Abstract the authors state that "joint system data were obtained from DTM



and used as input parameters. . .". However, in section 3.7, the authors state that "main discontinuity sets were measured manually on site" and TLS and UAV (RPAS) models "had been used also to determine the most hazardous part of the hillslope for block stability analyses" since "many parts of the hillslope cannot have been measured manually". From these sentences, it is not clear how the TLS and UAV (RPAS) contributed to discontinuity measurement and how the authors process models for discontinuity extraction. Please clarify this aspect.

9) In my opinion it is not clear which is the real contribution of morphological index maps to the study. If not supported by a specific description and comparison with field data the interpretation that the author made in the result section (i.e. "All resulting morphological maps strongly express the already eroded and potentially . . .") might be only considered a speculation. The improvement of the description of the study area (see comment 4) might make easier the contextualization of these maps for the understanding of the ongoing slope evolution processes.

10) The result of the stability analysis is not clearly described. Even if the author state that the critical global factor of safety is above 1, they then indicate that "the failure occurs in the weak layer". . . In this way, it is not clear what the reader should conclude looking at the analysis. Probably they would state that the slope is stable in the modeled conditions but a perturbation might induce its failure with the formation of a slip surface that should nucleate from the weaker layer. Please clarify this aspect. Additionally, from the text it is not clear if the authors account for discontinuities in the global stability analysis.

11) The number, orientation and typology of the major discontinuity systems is not stated. The graph of figure 18 is not self-explanatory.

12) Consider to delete figures 2, 11, 12, 13, and 21. In my opinion they do not add particular value to the analysis. It is not clear which parts of the slope is shown in figures 9, 10, and 14. Please add a specific map. Indicate also the localization of the

cross sections of figure 22. From the text, is not clear the number of tested sections and the width of the slope.

13) The use of references is generally appropriate. Please, thoroughly check consistency of both citations in the text and list of references.

With the above corrections, I feel the manuscript may be reconsidered for publication.

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