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Natural Hazards and Earth System Sciences
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.

Ákos Török et al.

torokakos@mail.bme.hu

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Answers to the review of 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 Received and published: 15 April 2017 Reply: 10 June 2017

Answers to the reviewer #2: Thank you very much for your very constructive comments and suggestions. We have considered all of your comments and modified the manuscript accordingly. Please find the answers to your comments below.

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Pls also find attached file - in the SUPPLEMENT:where the asnwers are marked in red. Pls. also note that only some of the revised figures are attached to this answer as uploaded file and their numbering do not follow the numbering in the revised manuscript (i.e. Fig 1. here is Fig 14 in the revised mansucript)

Original comments - April 15, 2017

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.

Answer: The revised paper is checked by a native speaker, who corrects the text.

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.

Answer: The new version of the paper has been written after considering this review. We have reformulated our goals. Slope stability analyses are in the focus supported by remotely piloted aerial systems (RPAS) and analyses by finite element methods (FEM). The used terrestrial laser scanning (TLS) was only applied for validation purposes; the revised text is written accordingly. Several figures were removed in order to make more focused content on RPAS-based survey. The slope stability analysis was revised and additional data on the location of studied sections and on the links between the data set obtained by RPAS and used in stability analyses was emphasized. The Results and Discussions were separated. The Conclusion and outlook were rewritten.

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. Answer: The revised paper contains new structure: we accepted the suggestion of the reviewer. We tried to write more understandable Results and Discussion sections. In the Materials and Methods section we have swapped the TLS and RPAS sections, intended more focus on RPAS as applied data acquisition and less for TLS as a validation tool.

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.

Answer: The geological conditions of the study area are described in more details in the revised manuscript. The slope geometry is described in more details. The cross-sections where slope stability was calculated are shown in the revised paper. There are no rock fall deposits at the base of the southern slope. The proneness of the tuff to weathering was emphasized in the revised text, with added new data on the properties and with new references. The comparison with other rocks has been removed from this part of the text. However, it is necessary to emphasize that the studied tuff is very similar to other tuffs in terms of properties and in terms of slope stability.

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.

Answer: We have used RPAS technology to capture fine details about the rock cliff even about its generally inaccessible parts. We agree with the reviewer that this technology is “innovative and user friendly” as well as “it offers a new sensing perspective” which can naturally “reduce time and cost”. The acquired imagery was processed by Structure-from-Motion technology which became very common in photogrammetry nowadays. To be able to monitor terrain surfaces, some conversions and GIS modelling were necessary. One of the messages of our paper is that these platforms are suitable for similar tasks. We have reformulated the text in section 3.1 about RPAS.

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”.

Answer: Thanks for the valuable remark. We have reformulated the message in order to express that RPAS technology was the primary one and TLS was only used to validate the obtained surface data. The terrain was excellent to crosscheck these two technologies, this was the reason why we wanted originally to compare the methods. Following the suggested style, we changed the order of the sections, modified (decreased) the weight of TLS and have written hopefully clear statements about the data capture. We have changed also the paper's title, although we kept the original slope stability analysis and FEM modelling. We think that our pilot site (the Sirok Castle) is just an example how these two nice tools can be combined in geological practice.

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.

Answer: The Materials and Methods section has been improved as the reviewer suggested. We have deleted some figures about the equipment, as well as the duplication of presenting the results. Now the surface modelling based on RPAS observations is much clearer. More details (e.g. about flight control) is given about the processing of the imagery. There was no prior camera calibration, only simultaneous camera calibration, so this information was put into the text. GPS measurements were supported the georeferencing, which is documented in the section, too. Following the reviewer's suggestion, we have removed the TLS-oriented results to underline its validation role. With the deletion of TLS illustrations, the vegetation removal question is not relevant anymore.

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.

Answer: The main data capturing technology was based on an RPAS system. To be able to validate this dataset we performed TLS measurements. Both technologies were used to derive digital terrain (exactly surface) models (DSMs). After revising the paper, the TLS-based results were deleted and only the data quality check remained. The geological field measurements (i.e. all field works) were supported by the preliminary surface modeling results, so the manual inspections were "oriented" after the RPAS results.

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.

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Answer: We have considered the reviewer's opinion and have reduced the indices. Since the catchment area figures excellently express the similarity of the RPAS and TLS measurement, they are kept as quality comparison. The topographic wetness index can suggestively demonstrate the geological situation, the RPAS-based index image was solely kept. We want to repeat our analysis a couple of years after the first data capture to check the potential of this technology to measure the volume and map the erosion. This is not part of the current paper.

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.

Answer: The slope stability analysis was modified in the revised paper. A modified figure that shows the "weak layers" in the slope stability model was added to the revised manuscript, clearly marking the calculated slip surface at the weak layers. A new figure that describes the studied sections is now part of the revised manuscript. The difference between this model and the planar failure and wedge failure were described in more details. A figure that shows the joint orientation (and DEM model) explains better these types of potential failures.

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

Answer: Former Figures 18 and 19 (now new Figures) show the strereographic projection of the measured discontinuities as a lower hemisphere projection. Each point on the stereonet represents a normal vector of a discontinuity plane. Based on the projections six main joint sets (85/156, 88/312, 79/110, 81/089, 82/064, 61/299) can be separated.

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.

Answer: Fig. 2, Fig. 11, Fig. 12, Fig. 13 and Fig. 21 were deleted from the text. A new Figure was added to show which parts of the slope are shown in new Figures. An additional Figure describes the location of cross sections was added to make it more clear. Out of 55 tested cross-sections 5 were chosen to analyze the global stability. Figure 22 shows two examples for the results of the analyses: Section 1 and 3 (see new Fig). Local stability analyses were not constrained to specified sections. Areas of the possible failures were determined with kinematic analyses.

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.

Answer: We would like to thank to the anonymous reviewer for his/her valuable time spending with our manuscript. We have considered the suggestions and prepare a revised form of the paper.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-56/nhess-2017-56-AC2-supplement.pdf

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Fig. 1. New Figure, Fig. 14 Location of some studied cross sections

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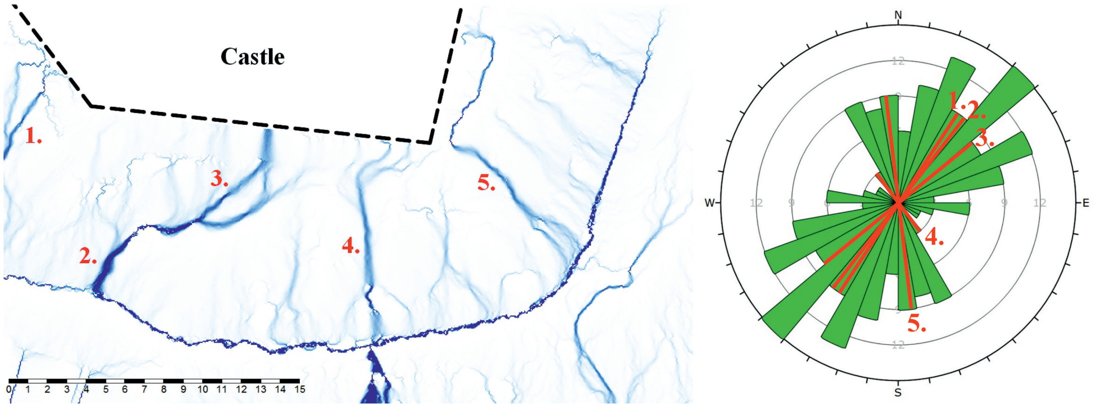

Fig. 2. New Figure: Fig. 12. Joint system obtained by RPAS (marked on catchment area DEM) and also measured on the field. Numbers refer to major joint systems marked on DEM map and on rose diagram

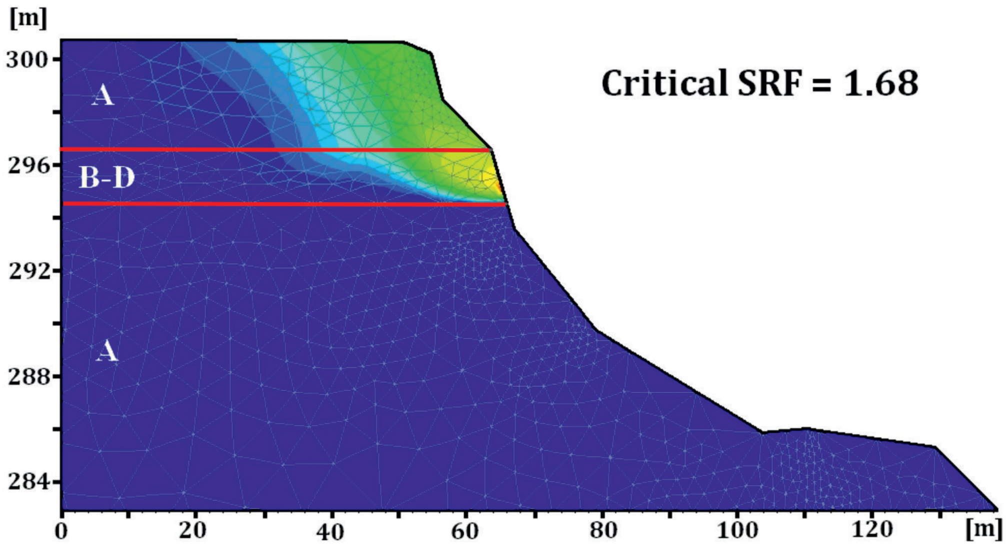

Fig. 3. The results of the global stability analysis of the slopes (sections 3 and 4 on Fig. 14), total displacements are marked in blue to red (lithology is indicated by letters A-D, note the weak zone)

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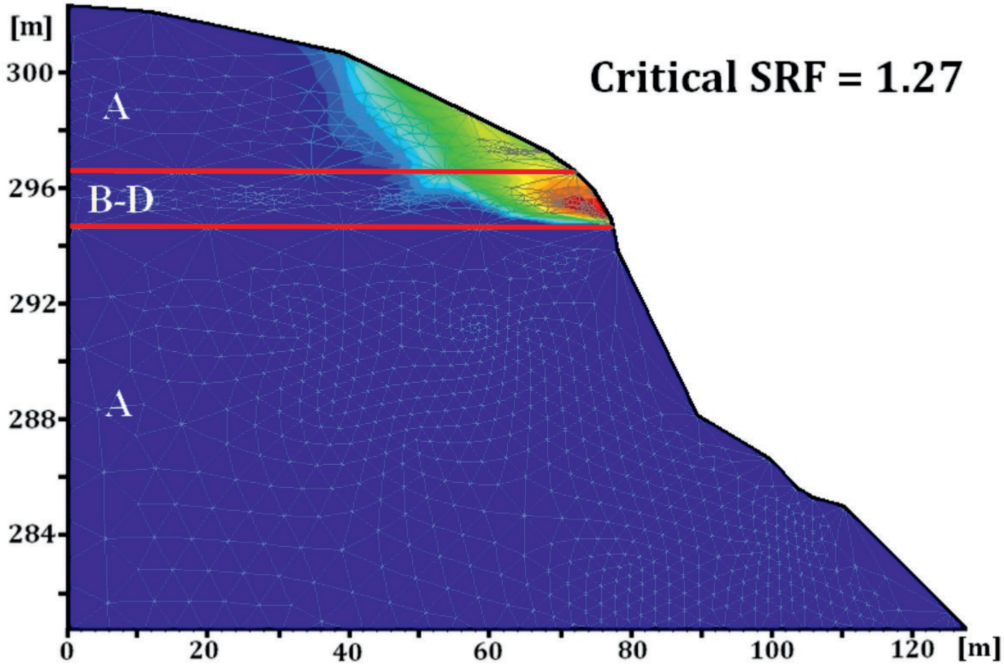

Fig. 4. The results of the global stability analysis of the slopes (sections 3 and 4 on Fig. 14), total displacements are marked in blue to red (lithology is indicated by letters A-D, note the weak zone)

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