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Author‘s response (egusphere-2025-2007)

Dear Dr. Wolfgang Schwanghart and Dr. Joris Eekhout,

Thank you very much for the positive evaluation of our submitted manuscript (egusphere-2025-2007) initially entitled “*Safeguarding Cultural Heritage: Integrative Analysis of Gravitational Mass Movements at the Mortuary Temple of Hatshepsut, Luxor, Egypt”.*

We have carefully considered all comment from two kind reviewers. In response to their suggestions, we rewrote the introduction and the synoptic discussion, expanded the methods and discussion sections, and updated figure 1 of the manuscript. We also propose to change the title to “*Safeguarding Cultural Heritage: Integrating laser scanning, InSAR, vibration monitoring and rockfall/granular flow runout modelling at the Temple of Hatshepsut, Egypt*.” to provide a clearer idea of the scope of our study in the title.
We thank the reviewers for their constructive feedback that greatly helped improving the quality of the revised manuscript.

Please find the replies to the reviewers below. We are looking forward to hearing from you.

Kind regards,

Benjamin Jacobs (corresponding author on behalf of all co-authors

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Reply to Referee #1

Dear Fritz Schlunegger,

we sincerely thank you for the detailed, constructive, and thoughtful assessment of our manuscript. We carefully addressed all comments and implemented substantial improvements throughout the paper. Specifically, we (i) completely rewrote and streamlined the introduction to provide a clearer narrative and a more precise statement of objectives; (ii) expanded the Methods section with additional references, methodological context, parameter justification, and discussion of uncertainties; (iii) integrated relevant literature on RAMMS modelling and added supplementary material (S9) to document parameter exploration; and (iv) revised and expanded the Discussion – particularly Section 5.5 – to offer a more holistic, cross-method synthesis and to better integrate geological context. We also implemented all specific line-by-line corrections and clarifications as suggested.
We thank the reviewer again for the valuable input, which has significantly strengthened the manuscript.

Sincerely,

Benjamin Jacobs (corresponding author on behalf of all co-authors)

Dear Editor, dear Authors

This paper combines various surveying techniques with the aim of estimating the occurrence and locations of potential hazards within a cultural heritage site. It has a strong applied component and, as such, offers a valuable contribution to the otherwise science-focused publications typically featured in this journal. The manuscript is well structured and thoughtfully conceptualized, and the results and interpretations are generally well supported by the presented data.

Given the interdisciplinary and applied nature of the work, I strongly support its publication after some moderate revisions. These include a clearer organization of the introduction, a more thorough engagement with relevant previous studies, and better consideration and discussion of the uncertainties regarding the applied methods, and an improved integration of the collected dataset.

At present, the introduction lacks focus and meanders through various methodological and thematic aspects without a clear structure. I therefore recommend a complete rewrite of the introduction, with the goal of streamlining the narrative and ending with a concise and clearly formulated statement of the paper's objectives. As currently written, the questions posed at the end of the introduction resemble statements typically found in a research proposal rather than well-defined scientific goals. They also come across as somewhat simplistic. Moreover, these questions are not adequately addressed in the body of the manuscript, leaving the effectiveness of the applied methods unclear. For instance, each of the methods employed demands a high level of specialization, involves the use of expensive equipment, and requires significant manpower. As such, their efficiency is questionable. I therefore strongly recommend restructuring the

introduction and refining the articulation of the paper's goals to better reflect the blend of applied and scientific objectives that are addressed in this work.

Thank you very much indeed, for your constructive feedback. We reworked the entire introduction with the goal of a clearer structure and better focus on the paper's aim. Also, we hope it becomes clearer now why the application of conventional, less expensive / complicated, intrusive measures are not necessarily a feasible option at this site.

Other common surveying methods, that are cheaper and less complex, especially the use of UAV-based photogrammetry, unfortunately needed to be ruled out a priori for administrative reasons (not in manuscript).

Section 3 outlines the methods employed in this study. While all of these techniques are well established in research, the chapter is quite sparse in terms of references. It should be expanded to provide a more comprehensive overview of relevant published work. Additionally, the manuscript lacks a discussion – either in the Methods or Discussion section – on the uncertainties associated with the applied techniques. What are their limitations? What is their level of accuracy? How does the selected survey impact these? This critical information is largely missing and should be addressed to better contextualize the results and support their interpretation. The same concerns the use of the RAMMS models. As noted above, this software has been extensively tested – for example, in Bolliger et al. (2024), where model parameters were calibrated using observed debris flow events at the Illgraben. There are also several other studies in which the applied methods have been thoroughly tested and parameter spaces systematically explored. The authors should therefore conduct a more comprehensive literature review and integrate relevant previous work on these modeling approaches to better contextualize their application in this study.

Thank you for your suggestions. We reworked and expanded the methods sections to include (i) general principles and more context of the applied methods, (ii) feature their applications and their trajectories and (iii) provide support for input parameters of our RAMMS models. We added a figure of the parameter space and its assessment of the granular flow model to the supplement (S9). This important figure should have been included in the first place.

We also edited and partially expanded the discussion to account for possible sources of errors and uncertainties and, in case of the RAMMS models, to evaluate our input parameters by comparing previous work and physical concepts.

Finally, the discussion section primarily focuses on the individual methods in isolation. However, it would be valuable to adopt a more holistic perspective on the insights gained from applying multiple methods to a single site. Specifically, the discussion could address how the combination of results contributes to our understanding of sediment transfer processes – from the rock face to the depositional areas – and how this is influenced by the geological pre-conditioning of the site. Although some geological context is provided early in the paper (e.g., a stratigraphic log and descriptions of fractures and faults), these aspects are not meaningfully integrated into the discussion. Incorporating this information would significantly strengthen the interpretation and relevance of the findings.

Thank you for this insightful comment. In the revised manuscript, we substantially reworked Section 5.5 (Discussion of safeguarding and hazard anticipation strategy) to provide a more holistic and integrative discussion of the multi-method approach. The revised section now explicitly synthesizes the complementary strengths of TLS, InSAR, vibration monitoring, and runout

modelling, and highlights how these methods jointly contribute to a more comprehensive understanding of site stability and hazard anticipation.

Thank you very much for your suggestion to put our findings in the greater context of sediment transfer processes. This is indeed a very interesting topic, given the vastly different environmental characteristics of the study site in comparison to Alpine sites. At this point, however, we think this exceeds the scope of the current study; but we will definitely keep it mind for the next stages of the project.

Specific comments:

Line 33: This paper does not really report on the impact of rockfalls and slope failure over 3 millenia – this statement appears to largely over-stated.

Line 44: research on the stability of the surrounding the Temple…. Something is missing here.

Line 50: ….lower mechanical strength in comparison of Alpine rock walls – can you make some more specific statements about the difference?

Lines 70ff: There is a large body of literature on the RAMMS software. In Bolliger et al. (doi.org/10.5194/nhess-24-1035-2024), we present an overview on where and how RAMMS::DEBRISFlOW has been applied in the past years. I apologizes for self-selling our work here, but I invite the authors to have a look at this paper and particularly focus on the articles that are mentioned there.

Thanks for the comments above. We completely reworked the introduction.

Line 90: The Dier E-Bahari…. ('The' is missing).

Thanks; Changed

Line 90: …. Opposite of Luxor city

Thanks; Changed

Line 110: What is the dip direction of these beds? This could be an important information if the scope is to assess the hazards related to mass failure processes.

Thanks. Changed to "*Almost horizontally bedded*"

Line 115: The … Formation is described in detail….. ('is' is missing)

Thanks; Changed

Line 115: …. By King et al. (2017), who subdivide (and not subdivides)….

Thanks; Changed

Line 114: A further stratigraphic subdivision…

Thanks; Changed

Line 120: ….the geological setup can be reduced to a typical brittle on ductile structure… What do you mean by this? What is the evidence for brittle and ductile deformation?

Thanks. We extended the sentence to provide more context. *In terms of geo-mechanics, the relatively many-layered geological setup (Dupuis et al., 2011) can be reduced to a typical "hard on soft" structure (Erismann and Abele, 2001), i. e. a mechanically unstable configuration in which a*

competent, brittle rock mass overlies a weaker, ductile substrate, promoting differential deformation and shear localization that predispose the slope to failure.

Lines 124/125: The sentence starting with 'Pawlikowski and …' sounds a little bit strange and needs to be rewritten.

Thanks. We rephrased the sentence so that actually makes sense and does not confuse the readers. *Pawlikowski and Wasilewski (2004) state that faults and fissures are the two main structural features that affect the region.*

Line 124: What are these structural features? Where do they occur in the surveyed area? Some information is given in the following sentences, but I cannot really get a full picture. Could the realted features be shown on Figure 1, for instance? Zones of mechanical weaknesses are very important for any hazard assessments, so fractures and faults would be one of the first features I strongly suggest to map. In fact, such information needs to be presented in this work as well, and the resutls of the survey should then be compared with such geological information.

Thanksfor your comment. We added further information and added a visualization of the main sets of discontinuities to Figure 1.

Line 136: Abdallah and Helal (1990)….

Thanks; Changed

Line 140: features that could…. (comma is not needed)

Thanks; Changed

Line 145: This might justify the statement in the introduction (line 33) about the survey over millenia. But nevertheless, the sentence in line 33 is an over-statement.

Thanks; We edited the Introduction (see above)

Line 145: The reference to Figure 5 is too early. Figures should be referred to according to their order. So far, Figure 1 has been mentioned; then next one would then be Figure 2 (but not Figure 5).

Thanks; Changed

Line 147: This entire section 2.4 can be deleated. The types of failure processes should be elaborated in the discussion and do not need to be listed as hypotheses. This would be ok for a research proposal, but not for a scientific paper. Alternatively, if previous research has already shown that these types of failure processes have occurred in the past, they can be listed as given information in section 2.2.

Thanks for your feedback. As kindly suggested, we moved this section to the end of section 2.2. as we reckon information on failure process types is crucial understand implication from our combined TLS / InSAR- deformation analysis and especially our modelling approach.

Line 234: These values need to be compared to what has been proposed in literature. In Bolliger et al. (2024) we found m-values that were one magnitude lower, but we found similar x-values as applied here. As mentioned above, a literature review on RAMMS::DEBRISFlOW is e.g., given in Bolliger et al. (2024).

Thank you, for your comment. We extended this section by providing an overview of previous applications of RAMMS::DEBRISFLOW, which show a trajectory to why we expect fairly high m-values. This is further discussed in section 5.3.2, as well.

Line 244: Figure 3 should be mentioned before Figure 6 can be referred to (same comment as above).

Thanks; Changed

Line 317: Figure 4.... ('4' is missing)

Thanks; Changed

Line 315 ff: How do the results depend on the input parameters? I guess that there is a sensitiviey analyses on this, but where are the related results presented?

Thanks for your feedback. We added a paragraph in the discussion (section 5.3.1 lines 494 ff) regarding this issue.

Line 335: How where the best internal friction parameters determined? Where is the corresponding information? Some data is given in the appendix, but it is not enough to fully appreciate the debrisflow modelling results.

Thank you for your comment. We clarified this in the last proper sentence of section 3.3.2 (Methods) and added a parameter matrix in the supplement (S9), which is also mentioned now in the discussion.

Line 360 ff: Shouldn't this be part of the Methods section?

Thanks for your suggestion. Changed.

Bern, July 22nd 2025

Fritz Schlunegger

Reply to Referee #2

Dear anonymous reviewer,

we thank you for the very positive and encouraging assessment of our study, as well as for the constructive comments that helped us improve the manuscript. We addressed all suggestions in detail. We clarified the TLS LoD calculation and error sources and provided additional context on the application and limitations of RAMMS::DEBRISFLOW for dry granular flows. We also refined the discussion of model calibration, revised the description of the retaining wall, and added missing methodological details (e.g., vibration monitoring duration). Furthermore, we rewrote the introduction, corrected terminology and figure references, ensured consistent use of abbreviations and BCE notation, and implemented all line-by-line corrections and clarifications suggested.
Overall, these revisions have strengthened the methodological transparency, contextual background, and clarity of the manuscript. We sincerely thank the reviewer for their helpful and constructive feedback.

Sincerely,

Benjamin Jacobs (corresponding author on behalf of all co-authors)

Dear Authors and Editors,

this manuscript presents the application of established mountainous geomorphology methods (Terrestrial Laser Scannig (TLS), InSAR, RAMMS modelling and vibration analyses) to assess rockfall and granular flow activity at the Temple of Hatshepsut in Luxor, Egypt. TLS, InSAR and vibration analyses identify hotspots of geomorphic activity at the cliffs next to the temple, while the modelling approaches aim to predict possible future events and their impacts on the temple and its visitors.

The study is clearly written, well structured, and uses methods well suited for such an application. Its novel adaptation of geomorphological approaches to an archaeological setting is particular commendable, offering possible opportunities for interdisciplinary research between geomorphology, archaeology and geology. I therefore recommend this manuscript for publication in ESurf.

Prior to publication, however, some points should be considered.

Main points:

- The uncertainty/error estimation of the TLS point clouds is not entirely clear. While the registration errors for both point clouds are provided and a LoD of 0.03 m is stated, it is not explained how this LoD was calculated. Is it based on a 95% confidence interval? Was it calculated based on stable areas only? Please clarify. Additionally, could you elaborate on why the M3C2 algorithm was chosen over a basic DoD approach?

  Thanks for your comment. In conjunction with the second review, we edited section 5.1.

The LoD is the instrumental error, controlled by the error propagation from raw data quality and registration process. This includes the scanner's target accuracy (5 mm @ 100 m range), precision (3 mm @ 100 m range), laser beam divergence (0.35 mrad) and atmospheric correction (RIEGL, 2014). In our analysis the LoD is produced as significant change calculated by the M3C2 algorithm (Lague et al., 2013), and also corresponds to the 95th percentile of model distances (similar to Abellán et al. (2011)).

We did not consider a DoD approach in this study. While it can be very intuitive and straightforward in some applications, it reduces the data analysis to a 2.5D problem (which actually is its charm is some cases). In the case of Deir El-Bahari, however, this approach would lead to reducing the 90° steep cliff to a few pixels (projection in Z-direction) or reduce the topographic complexity and introducing large sources of error by projecting the "cirque-like" shape of the terrain to any flat plane, e. g. parallel to the Z-direction to account for the cliff. *M3C2 is a well-established method for straightforward 3D change detection. It compares raw 3D point clouds and avoids gridding artifacts, interpolation errors and loss of detail in rough or vertical terrain* (added in 172 ff). Therefore, we chose this method.

- As noted in your discussion, RAMMS::DEBRISFLOW was developed for debris flows with significant water content. To me, it is unclear whether this is the first study to apply the model to dry granular flows. If so, the statement that is "[...] a simple and geomorphologically accurate simulation tool for dry flows (granular flows)" (lines 448-449) appears overstated, given that the validation is based only on a single photograph from 1892. I recommend addressing this limitation in more detail and discussing the restricted validation options in greater depth. If there are previous studies using the model in a similar way, please cite them.
  Thanks for bringing this up. In conjunction with the second review, we changed parts of the methods section (3.3.2) and the discussion (5.3.2) to provide more context on the use of RAMMS::DEBRISFLOW and the parameters used. We elaborated a bit on the trajectory of RAMMS applications in different environments and compare out input parameters (especially Coulomb friction) to other studies and physical concepts.

  We also dialled down the aforementioned sentence to *"However, in the case of this study it turned out to be a simple and geomorphologically sound simulation tool for dry flows (granular flow), too."*

  Furthermore, the presented granular flow simulations are only reconstructions of two historical events. Why did you not apply the calibrated model to the rock tower in A 02, for example?

  Thanks for the comment, very good point. At this point of the project, we focussed on historic events and their benefit for calibrating models, that can and will be used in case larger instabilities become imminent in the future, hence the term "areas of increased monitoring demand" in Figure 3c. However, public availability of such models needs to be carefully considered is subject to approval by the Egyptian Ministry of Antiquities.

- The manuscript briefly mentions the retention wall at the temple only twice. From my point of view, the existence of this wall means that the rockfall problem has already been recognised and countermeasures have been taken. As this is directly related to the study, I would like to suggest that you provide some more detail about the retention wall. For example, when was it built? Does it serve its purpose based on the rockfall model?

Thanks for your comment. The retaining wall is indeed somewhat helpful regarding possible rockfall hazards but was not necessarily meant for this. We edited parts of the discussion (section 5.3.1) to address this: *This becomes especially obvious in the case of the effect of the retaining wall behind the Temple of Hatshepsut. Originally built in 1968 to stabilize the soft Esna Shale formation immediately above the festival courtyard and the entrance to the Amun shrine (Lipinska, 1977), the retaining wall serves as a natural rockfall collector for smaller and intensely fragmented rockfalls.*

We also unified the term "retaining" wall throughout the manuscript, as this is the correct technical term. Before "retaining wall" and "retention wall" were both used.

Minor points:

- I would like to suggest going through the manuscript again in detail and checking the sentence structure and consistency (e.g. figure numbers). Below, I have listed a few of such points that caught my attention.
- Please double check all abbreviations in the manuscript. These should be defined when first mentioned, but not again in subsequent references.
- In accordance with the ESurf guidelines, please use the neutral BCE (before the common era) instead of BC (and avoid a mixture of both).

Thanks, we changed this and hope we caught all Abbreviations and Figure references.

Lines 31-32: Double use of the word "worldwide"

Line 32: "[...] has been emphasized in many publications [...]" – please name some.

Line 40: Use BCE instead of BC

Line 44: A word is missing here

Line 45: It is the Valley of the Kings, not the Valley of Kings

Line 50-53: It is not clear whether this "benchmark field study" is the present study or another one. Please clarify

Line 86: Please define LiDAR

All the above: Thanks, we rewrote most of the introduction and included you remarks where applicable.

Line 90: Typo: It is the Deir El-Bahari Valley

Thanks, changed.

Line 115: A word is missing here, please refine

Thanks, changed "hanging" to "overlying".

Line 125: "to" is missing here

Thanks, changed.

Line 136: The brackets of this in-text citation are incorrect

Thanks, changed.

Line 150: Maybe even mark the rock tower in figure 1 for better visualization

Thanks, changed.

Line 162: Please define MSA as Multi Station Adjustment

Thanks, changed.

Line 201: "A02" should be "A03"

Thanks, changed.

Lines 201-202: The block sizes seem to be quite random at this point in the manuscript. Please consider referring to S1 here

Thanks, we added an extra sentence on this: *Here, 0.01 m^3 corresponds to the five distinct rockfalls in our 1 one-year TLS data, and 25 m^3 to the largest single rockfall reported for the last century (Abdallah and Helal, 1990) as well as larger distinct blocks in the cliff (S1, S2).*

Lines 206-208: Why did you not just analyse the block sizes and shapes behind the retention wall?

Thanks for your question. The block sizes above the retention wall are limited to very small sizes. To account for varying magnitudes of single rockfalls in our simulations, we expanded the range of block sizes. As volumes of up to 25 m^3 are reported in the literature (Abdallah and Helal, 1990) as well as visible as distinct blocks in the cliff (S2) – even in our AOIs – we found this approach appropriate.

Section 3.4: Information on the time / duration of data acquisition is lacking

Thanks, we added: *We used a Trillium Compact 120 s seismometer on March 6th, 2023 (09:37 to 23:43) with a sampling rate of 200 Hz.*

Line 243: HVSR was already defined

Line 244: SSR was already defined

Line 247: SSI was already defined

All above: Thanks, changed

Line 264: What does "significantly" mean here? Is it based on a statistical parameter?

Thanks, yes, we added: *For better visual accessibility, we subsampled the change detection to a point spacing of 0.25 m and 100 significantly changed points (from M3C2 analysis) in a radius of 1 m.*

Line 270: How was the total rockfall volume calculated? With or without the LoD?

Thanks, we slightly modified this sentence: *For an area of ca. 6,5 ha of exposed rock wall (S6), we calculated a total volume of five distinct rockfalls of 0.589 ± 0.05 m^3 (S1), which translates to a rock wall retreat rate of ~0.009 ± 0.001 mm/a.*

You are absolutely right, that considering the LoD when calculating rockfall volumes is generally relevant. As shown in S1 we went without the use more advanced volume computation (e. g. concave alpha-shape or cut / fill) at this stage of the study, as (i) there are only five distinct rockfalls in our data, (ii) they are very small and (iii) more accurate estimates would not substantially change the message. Nonetheless, we added the error effect from the LoD (+/- 0.03 m) to the stated values.

Line 290: I do not understand why table 1 shows that velocities exceeding +/- 5 mm/a are significant

Thanks, we slightly modified this sentence*: Based on the statistical characteristics of the dataset, velocities exceeding ±5 mm/a, approximately corresponding to double the standard deviation, are considered significant (see* ***Fehler! Verweisquelle konnte nicht gefunden werden.****).*

Lines 293-295: Based on S8 I assume that these locations represent A02 and A03. If so, please state

Thanks for your suggestion. In fact, this sentence refers to all three parts of the cliff where the InSAR analyses shows activity, that are later defined as AOIs A 01 to A 03. However, this interpretation is done in conjunction with the TLS results (see dedicated section 4.3). In this section only the results from the InSAR analyses are presented and we feel that mentioning the AOIs by name before they are defined in the manuscript may be confusing to the readers.

S8 is restricted to A 02 and A 03, as the InSAR-results are too patchy in A 01 to be considered one possible, mechanically linked area of instability.

Line 301: AOI was already defined

Thanks, we reworked section 3.2. Therefore, this is now the first mention.

Lines 306-309: Here, the locations are called “A 01” etc. But before they are called “A01” etc. Please decide on one spelling

Thanks, we did this for all AOIs and rockfall scenarios. It is now A 01 etc.

Line 317: Figure number is missing

Thanks, changed

Lines 322-324: If the rocks hit the retention wall – is it a “hit” or a “miss”?

Thanks, this would be a “miss” as they do not reach the visitor perimeter (compare Figure 4). The flat top of said retaining wall is regularly cleaned from debris to prevent any built-up of material.

Lines 324-325: Please refine this sentence

Thanks, we wrote: *The number of rocks deposited in the Temple of Hatshepsut visitor area increases with the size of the released blocks, as larger blocks tend to travel farther.*

Line 332: Figure number is incorrect

Thanks, changed

Line 377: Typo: “basis”

Thanks, changed

Lines 382-383: Please refine this sentence

Thanks, we wrote: *Our data show that environmental conditions at Egyptian heritage sites can have a major impact on data quality and thus on the LoD.*

Line 431: Typo: “scarce”

Thanks, changed

Line 441: Type: “it” is missing

Thanks, changed

Lines 452-454: You could discuss using a monoplotting method to better assess information based on a single historical photograph

Thanks, great idea! We thought about this, however, all the “interesting bits” are either obscured by temple debris in the old photo or by the reconstructed temple (= terrain) in the view of the TLS data. Good line of site / perspective and the amount of terrain alteration (i. e. change in surface elevation change between the date of the monoplotted photo and the DSM generation) is crucial for good results. Both of which are problematic in this case.

We also tried terrestrial SfM with several historic images from the ground and even from post WWII aerial shots. However, we did not get anywhere with this since and therefore left it out. As mentioned in the manuscript and the comment above, the entire area of Deir El-Bahari was subject to very intense terrain alterations over decades. The aera of the Tutmose II temple (event A), for example, was used as a sediment dump between ca. 1910 – 1960, and the area of event B was cleared stepwise from ca. 1930 onwards. As Photography was expensive and complicated at the time of the earliest digs, the number of photographs is too limited for SfM convergence.

We added this to the methods section (3.3.2): *To better constrain the volume of the failures we set out to reconstruct the deposition geometry with terrestrial and aerial photogrammetry and monoplotting. However, due to the intense and repeatedly anthropogenic terrain alterations and limited number of usable historic photographs, this approach lead to little to no success.*

Line 494: Use BCE instead of BC

Thanks, changed

I would like to thank the authors for preparing and submitting this research article, and I congratulate them on a well-executed and valuable study.

Thank you very much indeed!