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.

Thanks for your comment. We added further information, citation 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