

## Point by point response to reviewers

### Reply to RC1

The authors would like to express our sincere gratitude to your thoughtful comments, which are very valuable for improving the quality of this work. The main concern raised relates to the consistency between results obtained from different methods used in this study. This indeed represents a key weakness in the originally submitted manuscript, partly due to insufficiently unified presentation of the study objectives across methods and partly due to the lack of directly comparable results. We have carefully addressed this issue and revised the manuscript substantially to improve clarity and consistency. Detailed point-by-point responses are provided below.

1. We fully agree that improving consistency between different components of the study is essential. In the revised manuscript, we now explicitly clarify that the ring-shear tests are designed to investigate the effect of impact level on landslide mobility under controlled stress conditions. In each test, the maximum applied stress is not fixed at 500 kPa but varies according to different impact coefficients. The corresponding stress states for each impact-loading condition are now summarised in Table 1.

For the numerical simulations, the objective is to investigate the effect of impact duration on landslide mobility. These simulations serve as an extension of the laboratory tests, which capture the shearing behaviour of a localised sliding zone but cannot reproduce full-scale runout processes. We appreciate the suggestion to include directly comparable stress paths; however, we note that a strict one-to-one correspondence is difficult to achieve. This is because the large-scale simulations involve complex interactions among a large number of material points, including material rotation, erosion, and evolving failure patterns, which are inherently chaotic. In contrast, the ring-shear tests idealise deformation along a prescribed shear surface under controlled conditions. Therefore, establishing a direct mapping between the two is beyond the original scope of this study.

Instead, the primary aim of the numerical component is to explore how material properties influence runout behaviour at the system scale. To provide a more quantitative link between the two approaches, we have now included an analysis of pore-pressure evolution at a representative location within the erodible layer (lines 299–309).

2. In the original manuscript, the experimental and simulation setups were only described in the main text, which may have made it difficult for readers to follow. We have now added summary tables for both the ring-shear tests (Table 1) and the numerical simulations (Table 3), in order to clearly present all scenarios and their configurations.

3. Details of the algorithm have now been provided in section 3.2. Specifically, the model follows Biot's theory of poroelasticity, in which changes in void ratio are represented as a consequence of volumetric deformation. Thus, the void ratio is not directly prescribed or manually controlled during simulations (lines 199-212). The variation of Young's modulus and Poisson's ratio is introduced to indirectly investigate the effect of impact duration on runout behaviour. Since impact duration cannot be directly prescribed in the runout model, it is effectively controlled through the elastic properties of the material.

4. Section 3 is now put ahead of Section 2 to make the paper more intuitive for readers to understand the background and motivation before diving into the experiments.

5. In the ring-shear tests, the three total stress paths are prescribed as input conditions. We now clarify that the corresponding effective stress paths should be interpreted as soil responses to these loading conditions, rather than prescribed inputs. Accordingly, we have added Fig. 13 in the Discussion section to illustrate three possible effective stress path evolutions based on experimental observations, together with detailed explanations (lines 333–343).

6. The sentence was originally intended to explain our observation that increasing impact load does not necessarily lead to faster liquefaction. However, we realised that the previous wording was incorrect as tests were conducted in undrained condition. We have removed this sentence.

7. This expression referred to our ring-shear tests. To avoid confusion, we have replaced the phrase 'previous physical experiments' with 'ring-shear tests' directly.

8. We acknowledge that the original manuscript did not clearly describe the parameter variation strategy in the numerical simulations. In the revised version, we explicitly state that all other parameters are kept constant when varying each individual parameter (see caption of Fig. 10). In addition, all simulation scenarios and their controlled variables are now clearly documented in Table 3.

## Reply to RC2

The authors would like to express our gratitude for your considerate comments. Detailed replies to each point are listed below.

1. It is very good advice for us to place more emphasis on the broader impact of the mechanical results from this study. As the impact effect is critical for analysing landslide mobility, we've now highlighted that material properties that determine a landslide potential impact level and duration could be significant for the hazard risk assessment. Accordingly, we further discuss that developing mathematical models that more precisely reflect the impact effect would be important for the future improvement of landslide numerical simulations. These points have been added to the Conclusion section to show the broader impact of our study (lines 370-376).

2. We fully agree that it would be super interesting to conduct comprehensive experiments across multiple soil types and environmental conditions, which would help achieve a more complete understanding of the impact-liquefaction mechanism. However, we must admit that conducting a large number of tests is challenging and resource-intensive. In our current study, we are preliminarily stay at proposing this possible mechanism, which is most likely helpful in explaining the observations from the LSB landslide case. We acknowledge that the current study framework is limited in material scope, and we remain open to further discussion and future investigations that extend this work.

Two minor comments related to wording errors have been revised correspondingly.

### Reply to RC3

The authors sincerely appreciate your thoughtful review of our manuscript. To improve the reliability of our hypothesis on this impact-induced long-runout mode, we've added more detailed analysis on both numerical and lab tests. Please find point-by-point responses below.

1. The angle  $\alpha$  in Fig. 2 represents the angle between the impact direction and the surface of the basin sediment. Since the sliding surface is exposed clearly in the LSB landslide, this angle is defined based on the slope of the Maoyaba basin derived from drone-based digital elevation models, corresponding to the angle between the sliding surface and the basin surface. As suggested, we have added the input stress path (previously Fig. 2b) to Fig. 7. Regarding the laboratory tests and numerical simulations, they are designed to address different aspects of the impact and erosion processes. The laboratory tests provide a mechanistic understanding of the influence of impact force level under controlled conditions, but are limited to a specific material type and a prescribed shear surface. In contrast, the numerical simulations capture the full-scale runout process and allow investigation of how material properties control impact duration and subsequent mobility. We do not intend for these two approaches to directly validate one another. Instead, their combined use provides a more comprehensive interpretation of the controlling factors governing impact and erosion processes.

2. References including literatures which using the similar model we used and the documents of the model has been added. The constitutive models have now been listed in the Table 3. The propagation of the landslide in simulations is mainly governed through three conservation equations as Equation 1 to 3. The detailed model algorithm is now introduced in lines 199-212.

3. We have further discussed about this phenomenon and reached the following explanations based on the test results. According to the three different impact modes illustrated in Fig. 5, the tests produced three distinct responses. For  $k_d = 1.7$ , the impact peak (point B2 in Fig. 5) just reaches the failure line. The corresponding effective stress path moves rapidly leftward after impact and subsequently follows the failure line. This behaviour can be interpreted as structure-failure-induced liquefaction occurring at an early stage. During the unloading process, shearing continues, and pore pressure keeps increasing due to grain crushing, even though the controlled shear stress decreases. In contrast, for  $k_d = 2.5$ , the effective stress

path moves upward toward the failure line. This indicates stress-induced failure at the initial stage rather than liquefaction induced failure. The subsequent pore water buildup that occurred in unloading process is mainly driven by grain crushing, which requires time or shear displacement to accumulate. This difference explains why the case  $k_d = 1.7$  reaches complete liquefaction more rapidly. This discussion has been added to the manuscript (lines 333-343).

4. The disintegration process is indeed an important factor influencing entrainment. In our current simulations, this process is not explicitly modelled (e.g., through fracture or breakage mechanics), but is instead indirectly represented through variations in material stiffness and compressibility. The reason we mentioned this effect is because we would like to emphasis on that disintegration could lead to different duration of the impact process, therefore influencing the subsequent runout process. This is why we tried to vary the elastic parameters of both materials, as they indirectly controlled the disintegration and deformation of materials, also the impact duration. Of course, our current model does not consider a lot of processes that may exist in real landslide cases. This limitation in factors is not discussed in Discussion section (lines 344-353).

5. We agree that our initial description was imprecise, and we appreciate your correction. The misleading sentence is revised (line 327).

6. We apologise for this overgeneralised statement. As discussed in our response to Comment 3, the efficiency of strength loss during impact depends on the relative contributions of structure-failure-induced liquefaction and shear-induced liquefaction. We can only propose that an ideal impact level exists for fast leading to soil strength loss (lines 341-343).

7. The difficulty in deriving physically-based erosion models have been added to manuscript in agreement with your comment (lines 43-52).

8. Word has been corrected.

9. As described in lines 107-109 of the original manuscript, the stress states at point C and A in Fig. 2b (Fig. 5b in revised manuscript) correspond to stages C and A in Fig. 2a. These represent the conditions before and after the addition of an overlying material layer. Under the simplifying assumption that the surface of rapture evolves parallel to the sediment surface, both share the same slope angle  $\theta$ . The added layer increases both normal and shear stresses, following trigonometric relationships based on  $\theta$ , which correspond to the projections of

line AC in Fig. 2b.

However, in real cases, the rupture surface slope may vary spatially and differ from the sediment surface slope. Therefore,  $\theta$  in the two figures is not necessarily identical. In this study, they are assumed to be the same for illustrative clarity. This clarification has been added to the figure caption. We have also revised the caption to define all notations, including  $\phi_p$  (peak friction angle) and  $\phi_m$  (mobilised friction angle).

**10.** Scales have been added to images a, b, and c.

**11.** Poisson's ratio and effective Young's modulus were selected based on common value ranges for sandy soils (<https://www.geotechdata.info/parameter/soil-young-s-modulus>). The initial porosity values can vary strongly depending on the degree of weathering and the deposition state. In this study, they were chosen as representative values for highly weathered granite (sliding mass) and loosely deposited sandy sediments (erodible bed).

**12.** We agree that bed porosity is an important factor. During the revision process, additional simulations were conducted with bed porosity varying from 0.2 to 0.6. However, these variations show limited influence on landslide mobility and runout patterns compared with the effects of elastic parameters. The reasons for this limited sensitivity are not yet fully understood and may be related to the spatial scale of the problem or the thickness of the sediment layer. This suggests that further investigation is required in future studies. This discussion has been added to lines 344–353.

**13.** The X label has been added, and the abbreviations TSP and ESP have been replaced with their full terms.

## Reply to RC4

We sincerely thank the reviewer for the thoughtful comments and for pointing out several shortcomings in the original manuscript. The revised manuscript has been carefully refined and up-to-date references have been added. Additional descriptions of the numerical model used in this study have also been incorporated into the Methodology section. In addition to wording corrections, point-by-point responses to all comments are provided below.

1. Line 9-10: The importance of material fragmentation has now been highlighted in the Introduction section (new line 173), and the suggested reference has been added.

2. Line 17-18: We appreciate the reference to the hypermobility model, which is indeed a powerful tool for predicting runout distances in large-volume avalanche events. In the Introduction, our intention was to first emphasize the complexity of long-runout mechanisms; therefore, not all advanced modelling approaches were discussed in detail. We would appreciate further guidance from the reviewer on whether a more explicit discussion of such models should be included in this section.

3. Line 29: We agree that the contributions of the suggested work (<https://doi.org/10.1016/j.ijmultiphaseflow.2020.103416>) are highly relevant to this study, and the reference has now been included (new line 28).

4. Line 41: We appreciate the suggestion regarding the role of shear stress–resistance competition at the impactor–substrate interface in controlling erosion efficiency. The corresponding reference has now been cited (new line 48).

5. Line 76: This sentence has been revised to: 'The findings of this study highlight the critical role of impact effects in the subsequent landslide erosion, which may govern runout patterns in alluvial basin settings.'

6. Line 78: This sentence has been revised to: 'This investigation of landslide impact-erosion processes not only contributes to a better understanding of long-runout landslide dynamics in similar topographies, but also emphasise their importance for future research aimed at improving runout prediction and hazard mitigation strategies.'

7. Line 89: The ring-shear tests were designed to represent the stress-state evolution within the sliding zone under impact loading conditions. The loading path is illustrated in Fig. 2b (now Fig. 5b). Fig. 2a is used to show how the laboratory loading conditions are derived from the

real-case landslide scenario.

**8.** Line 91: Thank you for this comment. Could the reviewer kindly clarify this point further? Specifically, we would like to confirm whether the suggestion refers to additional description of the impact model in the cited reference.

**9.** Fig. 2: The figure has been revised to show only the prescribed input stress paths for each test. The corresponding description has been clarified in lines 150–180. The resulting effective stress paths have been moved to the Discussion section, where they are interpreted based on the experimental results (lines 333–343).

**10.** Line 132: For the numerical simulation, we used a solid–liquid coupled model. The advantage of this approach is that it naturally captures pore pressure within the mixture and allows us to analyse its evolution directly from volumetric strain. The model you mentioned is indeed a powerful tool for spatially simulating the runout and inundation area of flow-like hazards, which we have also explored in other studies. However, since the focus of the present work is on the cross-sectional distribution and temporal evolution of pore pressure and its effect on landslide runout, we chose the coupled approach. Further details about the model are now provided in the revised version (lines 199–212).

**11.** Line 157: This sentence has been revised to: 'The longitudinal profile of the Luanshibao landslide shows a low angle of rest of  $11.7^{\circ}$ , while the rest angle in the depositional area is only  $0.9^{\circ}$ .'

**12.** Line 163: We described this landslide as having high mobility because the material travelled over an almost flat basin surface for approximately 2,250 m. We chose to retain this wording to reflect the observed runout distance. However, if the reviewer considers this characterization inappropriate, we are open to revising it and welcome further guidance.

**13.** Fig. 5 (new Fig.3): It was explained in new lines 113–112, describing the basic depositional features of the landslide.

**14.** Line 192: Thank you for the comment. The sentence in question was part of the landslide background and was intended as an observation from the field rather than a result. Specifically, the presence of saturated, confined-pore-pressure sediment suggested a potentially erosive medium that could influence landslide runout.

**15.** Fig. 7: The figure has been updated to include input stress signals, and font size issues

have been corrected.

**16.** Fig. 8: The figure has been revised to improve clarity of material boundaries and to present results in a column-wise layout, as suggested. The corresponding description is provided in lines 265–270.

**17.** Line 248: In the simulations, we primarily focused on the elastic parameters of the two materials, as they control stiffness and compressibility. These properties directly influence impact duration and pore-pressure development, which in turn affect runout behaviour. The relationship between material properties and impact response is further discussed in lines 244–254.

**18.** Line 277: This model is based on the classical Newton's impulse theorem which is explained more clearly by 'According to the theorem of impulse, the change in the impactor's momentum is equal to the product of the impact force and the impact duration, while the magnitude of the impact load depends on the landslide mass and its velocity at the moment of impact.'

**19.** Line 276: We apologise for the confusion caused by our wording. What we intended to describe was the time at the beginning of impact, rather than during impact. However, upon reconsideration, we found the velocity at the beginning of impact may also change due to the effects during the propagation after initiation, so we have revised this sentence in new line 327.

Other minor revisions based on your kind comments can be found in the attached pdf.

## Reply to RC5

We thank reviewer for your positive comments and your suggestions in improving the quality of this work. Detailed responses are listed below:

1. The density of the soil specimens in the laboratory tests was controlled through consolidation under stresses corresponding to a burial depth of approximately 10 m prior to impact, in order to approximate field conditions under the assumption of normally consolidated soil. The densities were kept consistent across all tests. We acknowledge that direct extrapolation of the results to field-scale stress conditions is challenging, as the stress states varied strongly in the real case, whereas the laboratory tests represent an idealised situation.

2. In the numerical simulations, we fixed the impact load factor by using the same landslide geometry, and focused on the influence of impact duration on the runout pattern. This duration is related to the disintegration behaviour of materials, which is determined by the two elastic parameters. In contrast, the laboratory tests were designed to investigate the influence of different impact load levels. Together, these two approaches can provide complementary insights into the mechanism.

3. Indeed, it is quite difficult to distinguish them in the trench due to their similar mineral composition and long-term geological processes affecting this ancient landslide. Thus, the sampling was not conducted within the trench, but at the frontal boundary of the landslide as shown in Fig. 4, where the deposited landslide material is relatively thin and the alluvial material can be clearly identified in the active stream in front of the landslide.

4. Line 15: The sentences have been revised to “Landslides are one of the most frequent and dangerous natural hazards in mountainous areas.”

5. Line 24: Yes, this refers to the fact that basin sediments are typically deposited by fluvial processes without significant consolidation, resulting in loose deposits with a relatively high void ratio. However, as this sentence describes a general condition and the exact void ratio also depends on particle size distribution, we prefer to retain the original wording. We are open to revising this if the reviewer considers it necessary.

6. Line 45: In this sentence, we intended to refer to real landslide cases occurring in similar geomorphic settings to the LSB landslide, particularly the Oso landslide, rather than focusing on the specific model used in the cited study. To clarify this, we have added an additional

reference related to Oso landslide (new line 38).

7. Section 2.1: The initial void ratio of all laboratory tests is approximately 0.861, corresponding to a contractive state as indicated by monotonic shear tests provided in the Appendix. The saturation conditions and verification procedures are also documented in the Appendix.

8. Section 2.2: We apologise that the previous section 2.2 did not explain enough what we did and how those parameters are considered. In response, we have now added table 3 to clearly present the design of the simulation program, along with a more detailed description of the methods used, including the governing equations (lines 199-213).

9. Please see our response to Comment 3.

10. These sentences have been moved to the Method section.

11. We fully agree with this comment. As also noted in our response to Reviewer 1 Comment 6, we identified this as an incorrect interpretation of the laboratory observations and have thoroughly revised the explanation.

12. The purpose of varying the impact-related parameters (i.e., impact coefficient in the laboratory tests and elastic parameters in the numerical simulations) is to systematically control the impact intensity and impact duration, which together govern the impact and erosion process. In this framework, path liquefaction is interpreted as a result controlled by material interactions. The kind of sensitivity analysis therefore demonstrates that path liquefaction is not governed solely by the magnitude of the applied stress, but also by the impact duration and material deformability. In this sense, path liquefaction should be understood as a dynamic phenomenon, where the triggering efficiency depends on how quickly the system reaches the failure envelope and whether pore pressure is generated predominantly through structural collapse or progressive shear deformation.

13. This part has been revised following additional analyses on the influence of porosity and permeability (lines 344-353). We agree with your suggestion and have moved the original figure to the Results section.