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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Stability evaluation and potential failure process of rock slopes characterized by non-persistent fractures" *by* Wen Zhang et al.

Wen Zhang et al.

ccao@jlu.edu.cn

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Dear referee:

Thank you very much for your valuable comments and suggestions. These comments are all valuable and helpful for revising and improving our paper, as well as the important guiding significance to our researches. The main corrections and the responses are listed as follows.

Comment 1: L 69-70: Authors state: "These slopes may become hidden dangers (e.g., geological disasters) and pose potential threats to people and nearby equipment". This

sentence is in strong contrast with the results from the stability analyses, since the safety factors range from 25 to 75. I'm wondering if the investigated slope is really too stable or the safety factors could have been somehow overestimated. Nevertheless, no event seems to have occurred in recent times from the investigated slope; this is a pity, since real events could have been extremely useful for validating the performed analyses. Why did Authors choose to test the proposed procedure to this slope?

Response: Thank you very much for your comment. It is really true that the investigated slope is stable according to its safety factors; thus, a large-scale rock slide can hardly occur. Only some small-scale rock falls may happen. Therefore, we corrected this sentences as "These slopes may become rock falls and pose potential threats to people and nearby equipment. Whether rock slide will happen requires calculating and evaluating".

The investigated rock slope is highly stable according to the following reasons: 1) weak interlayers and through-going discontinuities are not developed in the field; 2) the amount of rock bridges along the slip surface is about 30%-40%, which will stabilize the slope because the rock bridges are multiple orders of magnitude stronger than fractures.

It is really a pity since no real event can be used for validating the performed analyses. We have investigated many destroyed slopes. The results shown that the stabilities of the slopes are always controlled by weak interlayers or through-going discontinuities. This type of slopes is disadvantageous to our analysis of the influence of non-consistent fractures on rock slope stability. Therefore, we selected a slope whose stability is completely controlled by non-persistent fractures. However, the safety factors of this type of slopes are always high. We will select the rock slope with more fractures to test the proposed method for future researches. Nevertheless, we believe the proposed method really make sense for engineering projects related to fractured rock slopes.

Comment 2: L 92-93 and figure 3: the division of the detected discontinuities into sets is rather questionable, since data dispersion is indeed too high. Yet, this should not affect model reconstruction too much, as dispersion is also considered in the generation of artificial discontinuities within the DFN model.

Response: Thank you very much for your comment. It is really true that the data dispersion is high. An important reason of this phenomenon is that small-scale structural fractures with highly dispersed orientations predominate in the exposed rock surface. Subsequently, some fracture poles are also dispersed for each set. However, the distances between these fracture poles and the centre of the set is indeed closer compared with that between them and the other two sets, which was validated using the method proposed by Chen et.al (2005). Therefore, the grouping results are reasonable.

It is really true as you said that the data dispersion is high, but it is not caused by grouping. Even though fractures with similar orientations are divided into one set, the orientations of fractures in the same set still present dispersion to some extent.

We considered the dispersion of all fracture orientations in the field. The final DFN model is formed by combining three fracture sets. Therefore, the data dispersion will not affect the establishment of the DFN model.

Comment 3: L184: I suggest not to take for granted what a “fish function” is; please explain for those unfamiliar with numerical modelling.

Response: Thank you very much for your suggestion. It is really true that fish function may be hard to be understood for those unfamiliar with numerical modelling. Therefore, “fish function” was interpreted in the revised manuscript as “Then, an embedded scripting language in PFC, i.e., FISH, is used to write user-defined functions for extending the functionality or adding user-defined features in PFC. In the present study, we used the FISH functions to add the DFN into the model of the slope section by reading the location data of fractures. Subsequently, the SRM model composed of the BPM and DFN is established”.

Comment 4: L255-259: The modelling results are nice and look quite reasonable, but without validation they seem to be an end into themselves. Did any real event occur from the investigated slope in recent times? Are there any detached blocks to compare their volume and shapes with the simulated ones? What about their runout? In my opinion the reliability of the proposed procedure needs to be demonstrated.

Response: Thank you very much for your positive feedback of the modelling results. No rock slide has occurred in recent times from the investigated slope, which is clearly confirmed by the high factors of safety computed in the simulations; thus, it is hard to compare the volume and shapes of the real and simulated ones. It is really a pity since no real event can be used for validating the proposed procedure. We will select the rock slope with more fractures to test the proposed method for future researches.

Comment 5: L260: Please change section 6 title, since “stability analysis” has already been used for section 4. Maybe, “Statistical analysis”?

Response: We are very sorry for our carelessness. It is really true that the title of section 6 should be “Statistical analysis”. In the revised manuscript, the title of section 6 has been changed to “Statistical analysis”.

Comment 6: L281: please change are 43.5 to is 43.5

Response: We are very sorry for our improper word. In the revised manuscript, we changed “are 43.5” to “is 43.5”. In addition, we carefully checked the revised manuscript to ensure that all vocabulary and grammar errors were corrected.

Comment 7: L327 and 334: To trigger instability, Authors used the gravity increase method proposed by Meng et al. (2015). It is not clear to me if Authors made some amendments to this method, or if they used it as is. If so, in my opinion, the method cannot be defined as “innovative”.

Response: Thank you very much for your suggestion. In the present research, we directly applied the improved gravity increase method proposed by Meng et al. (2015)

to trigger instability. We are very sorry for our wrong use of the word “innovative”. Although this method is relatively new compared with traditional gravity increase method, this study cannot refer this approach as an innovative one. Therefore, we deleted the word “innovative” and rewrote this sentence as “The factor of safety is determined on the basis of the improved gravity increase method”.

Comment 8: Figure 1: Fig 1b: image is not clear; I suggest deleting the text superimposed over the image. Fig 1c: if the image contains the window reported in Figure 2, please add the box limits.

Response: Thank you very much for your suggestion. In the revised manuscript, we deleted the text superimposed over the image in Fig. 1b as you suggested. In addition, we enhanced the contrast to improve the image clarity. It is really true that Fig. 1c contains the window reported in Fig. 2; thus, as you suggested, we added the box limits corresponding to the sampling window in Fig. 1c.

Comment 9: Figure 7 In my opinion, the picture in picture representation is misleading. If possible, add all the drawings in a single image, otherwise, split in two different figures.

Response: Thank you very much for your suggestion. It is really true that one image composed of two pictures is misleading. In the revised manuscript, we added all drawings in a single image. In addition, we added detailed description in the caption of Fig. 7, which would help further understand the meaning of each image.

Comment 10: Figure 8: to improve image resolution, I suggest cropping the images to the fractured sectors.

Response: Thank you very much for your suggestion. It is really true that the resolution of Fig.8 is low. In the revised manuscript, the boundary sector without fractures are cropped and only remained the fractured sectors to improve image resolution.

Comment 11: Figure 9: sometimes the timestep count is represented in decimal nota-

tion and sometimes in scientific notation, please uniform.

Response: Thank you very much for your suggestion. It is really true that the expression of the timestep count should be uniformed. In the revised manuscript, we changed the timestep count represented in decimal notation to scientific notation.

We tried our best to improve and make changes to the manuscript. We sincerely appreciate your work and hope that our revised manuscript will be met with approval. Once again, thank you very much for your favourable comments and suggestions!

Relevant references:

Chen, J. P., Shi, B. F., and Wang, Q.: Study on the dominant orientations of random fractures of fractured rock mass, Chinese Journal of Rock Mechanics and Engineering, 24, 241–245, 2005.

Meng, Y. D., Su, Q. M., Lu, W. P., and Xu, Z.: Research on improved grain flow gravity increase method, Chinese Journal of Water Resource and Power, 33, 149–151, 2015.

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