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Natural Hazards and Earth System Sciences
Discussions

# *Interactive comment on* "Cascade effect of rock bridge failure in planar rock slides: explicit numerical modelling with a distinct element code" *by* Adeline Delonca et al.

**Anonymous Referee #1**

Received and published: 11 November 2020

The paper deals with a topic which is of great interest to those involved in rock collapses and rockfalls: the influence of the rock bridges location on the shear and tensile strength along a discontinuity plane, as well as the associated phenomenology of rock bridge failure propagation, which were analysed through a two-dimensional distinct element numerical modelling.

In my opinion, the weakest point of the paper is its very poor connection with real case conditions. In particular, the selected strength properties of rock bridges, as well as those associated to open cracks (OC) are extremely low. This results in a high percentage occurrence of rock bridge with low strength, which is not representative of



real conditions, where we have a much lower occurrence of high strength rock bridges. About this, at line 129 Authors state that "Even if the values considered in the study are much lower than those found in literature, it is assumed that the failure propagation phenomenology will be the same as in reality". I personally disagree with this assumption, however I believe that it should be extensively discussed and adequately justified by Authors.

In addition, I found some inconsistencies and unclear point throughout the manuscript, therefore I recommend Authors performing an accurate revision before its acceptance, based also on the following specific comments:

L 26-32: Where Authors list the main methods for failure probability assessment. Please consider adding the kinematic analysis method.

L 33: "The main parameter. . .are", please check correct conjugation

L 35 and elsewhere: "W. S. Dershowitz and Einstein 1988". Please carefully check references and in-text citations according to journal instructions

L102-103: " During the computation process, the local stress distribution along the joint can lead to the rupture of some "rock bridges" regions, then becoming a region of "failed rock bridges" that behaves as an "open crack area"". Actually, if I correctly understood, for most of the models, the mechanical properties of failed rock bridges are the same as those of intact rock bridges (see table 3). Indeed, it is not clear what is the difference between RBF and RB in terms of behaviour within the model. RB have the same parameters of RBF, thus the behaviour of both should be defined as elasto-plastic (while in Line 120 RBs are defined purely elastic).

L130: "In the case of OC all the values are taken equal to 0 (Table 3)": please justify this fundamental choice, since in my opinion, especially when talking about shear resistance, the condition of zero strength along a discontinuity is impossible to reach.

L135 (Step 0): the initial step in Figure 3 is called S1 (not step 0), and the following

steps and figures 4, 9, 10 disagree too; please standardize the step numbering in the text and figures

L151-152: “This modelling protocol has the objective to analysis the rock bridge failure phenomenology. Based on this modelling protocol, different scenario have been considered” – please check english grammar.

L153-155: The introduction of a 30 cm long open crack corresponds to 5% of the total discontinuity length. Figure 4 starts from 10% of OC (which corresponds to 60cm), and in figure 4a 2% step are analysed (which correspond to OC increase along the discontinuity of 12 cm). Please clearly explain in the text the exact new OC length added at each step for the three scenarios.

L157: please, explain the difference (if any) between scenario 1 and scenario 2 (2) (open cracks introduced in the lower part).

L169: “steps S2 to Sn” - see previous comment L135: please standardize the step numbering in the text and figures

L179-180: “These modifications induce the failure of some rock bridges by increasing the shear stresses along the rock joint”. It could be interesting to add a figure to see which rock bridges fail with respect to the introduced OC contacts.

L184: 84% and 70% occurrence of rock bridge at failure in my opinion is too high (see general comment).

L198: if Authors refer to the slope of the steepest dashed line, I would say a rate of at least 10 (instead of 7) and 5 (instead of 3) for model 1 and 2 respectively.

L199: Authors should describe here the meaning of the “transition area” presented in figures 5-7

L199-202: the OC values of 8% and 17% corresponding to the beginning of the second phase and 17% and 27% corresponding to the non-convergence of the model seem to



not completely fit with figure 5 axes labels. I am wondering if some mismatch occurred between the axes ticks and labels.

L216-217: this sentence is unclear, please explain better

L225-233: I’m not sure to have correctly understood this point. By looking at Fig. 6, for %OC = 11% I expect a maximum proportion of the failed contacts of 25% (which corresponds to about 32 contacts). Why is the number of failed contacts between 40 and 70 in figure 8a? I have the same doubt for model 2.

L234: If 40 models were run, why do figure 8a and 8b report a higher number of points? (at least 70)

L240: Please comment results also for model 2.

L245 “must therefore plays”, please correct typo.

L255 please correct to “potential unstable blocks”

L258 if tensile strength is not considered in the previous analyses, why is it reported in table 3?

L265 and table 4: This continuous change of strength parameters is not scientifically sound, and it should be supported by a stronger discussion. Please explain why did Authors not choose a unique set of strength parameters for all the models

L269: How do Authors calculate this 6% value? Is it related to the number of green triangles with normal stress lower than -10kPa? Please explain

L271 and Figure 9 caption, please correct figure 09 to figure 9

L277-278: “It justifies that in reality, as the cohesion value of the rock bridges are 500 times higher than in the study presented in this paper, only a few portions of rock bridges allow a potential instable block to be in place”. I agree with this statement; why did not author decide to use more realistic strength (and rock bridges portion) values?

(see also general comment)

L281: please correct “though” to “through”

L285-287: from a geomechanical point of view it is not correct to consider failed rock bridges to have the same strength of intact rock, as well as it is not correct to consider them to have zero strength. Why did Authors not consider employing a continuously yielding model?

L295-297: Probably it could be interesting to see the propagation of rock bridge failure (like in figure 5) for model 4

Figure 2. The gravity direction is in my opinion misleading, I suggest adding the 40° slope model. Moreover, it seems to me that the alpha associations in figure captions are wrong, i.e. alpha is zero for 80° (not 40°) slope angle and 40° for 40° (not 80°) slope angle.

Figure 3. Expand the caption to better describe the figure.

Figure 5 and figure 7: please put the model 2 sketch on the right side, under the corresponding graph.

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