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Discussions

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

Anonymous Referee #2

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The paper describes the application of the synthetic rock mass (SRM) approach for the investigation of a rock slope within a limestone quarry. The authors briefly summarize the input data used for the creation of discrete fracture networks (DFNs) which are then implemented into a bonded particle code (i.e. PFC2D) in order to investigate the potential failure mechanism of the slope. The numerical analyses are undertaken using a modified gravity increase method, and high factors of safety (>25) are computed for all the DFN realizations. The study represents a starting point for future studies focusing on the analysis of higher slopes, also in order to evaluate and simulate potential support methods.

The paper is well written, easy to read (except a few sentences throughout the paper requiring some rephrasing), and overall, well structured.

I have a few general comments about the manuscript. Then, I will also provide line-to-line observations/comments/feedback/criticism as needed.

The first general comment is that the investigated slope is quite low (20 m in the simulated models). It is true that in certain conditions brittle damage can develop even in low stress conditions, especially when tensile strength is exceeded or in case of significant stress concentrations. However, in this case it seems that the amount of rock bridges along the critical path(s), would most likely stabilize the slope, except perhaps for small blocks at surface. It is very unlikely that the real slopes will ever fail with the simulated mechanism, unless smaller fractures are included in both the data collection and DFN. This is clearly confirmed by the high factors of safety computed in the simulations.

This brings me to the second, important comment: the model input data, which seems the "weak point" of this manuscript. It is not clear what technique was used for the initial data collection, but the DFN used in the models seems to rely on and include only the larger discontinuities. This results obvious by visually comparing figure 1c and figure 2 (which seems to depict a much more fractured rock mass). Considering the input DFN, the high factors of safety computed makes sense. However, the question is: "is this DFN a realistic representation of the real rock mass?". The issue with the input data may have significantly impacted the numerical results, in terms of factor of safety, and location of the critical path. A "work around" would be to decrease the simulated strength of the intact material, to account for the smaller fractures that are not implemented as discrete discontinuities in the DFN.

In conclusion, in its present form, the manuscript seems quite conceptual, in that there is a seemingly weak connection between the model and the actual rock mass. While the simulations may indeed reproduce a realistic fracture propagation mechanism, the actual process is very unlikely to occur in the simulated slope, or any slope with similar height, lithology, and structural configuration. In view of this, this paper should be significantly improved (major revisions) with regard to the model input data and their description. Either by a) including the fractures that seem to be missing in the DFN, or b) demonstrating in a clearer manner the similarity between the DFN and the rock mass. This could include better pictures, with close-ups and, importantly, scales. Showing the mapped traces onto the photo of the slope could also greatly improve the clarity.

In the following, I will provide line-by-line comments. Line 40: authors should refer to “the non-persistent fractures in these works”, rather than “these non-persistent fractures”. Line 51: SRM has been used also for underground applications, including mining and hydraulic fracturing. Lines 51-53: perhaps these two sentences can be merged. However, the first sentence requires rephrasing, as it seems some words are missing. Line 58: authors should state the country the investigated site is located in. Line 71-72: this sentence can be improved. Perhaps the slopes are higher to the south, rather than the quarry area itself. Also, from this sentence it is not clear whether the “mountain “is a ridge oriented north-south, or the if the bedding are dipping to north (or south). Line 79: is the formation thick, or the limestone layers? Either way, how thick? Karst phenomena are not obvious, meaning there is not any, or that they are not or scarcely visible? Lines 80-83: the last two sentences could be merged. The term “intermittent” for discontinuities is somewhat inaccurate (or simply very rarely used, to my knowledge) – perhaps simply “non-persistent” is more appropriate. Additionally, one would expect that bedding would be very persistent. Will this play a role? Although it is true that observing the bedding trace does not necessarily imply a fully persistent plane with no tensile strength. Line 86-92: A 62 by 6 m is a large area to perform systematic discontinuity mapping (i.e. using traditional field techniques or short range remote sensing methods), and 169 discontinuities seems a low figure – what is the cut-off limit you considered (i.e. the smallest fracture that was considered). Looking at figure 2, it seems that the location of the mapped discontinuities is slightly biased towards the bottom of the window. Because of this, I would assume that the mapping was performed using traditional field methods, rather than remote sensing techniques. Either way, this

should be mentioned, also to address or acknowledge the potential limitations of the methodology used, in terms, for instance, of orientation bias. Speaking of orientation bias, the fracture set 1 is suggested to be less represented – this might be due to its orientation very similar to the slope, while sets 2 and 3 are almost perpendicular. It is nt clear whether this was kept into account. Line 90: Reference here seems out of place, unless the result came from that specific work. Line 94-95: This sentence requires rephrasing. I suggest starting from the issue of the trace length bias, and then stating the reason, rather than the contrary. Line 99: I recommend “and ‘investigate the’ potential failure mechanism”. Lines 105-107: It seems the authors suggest that the fracture intensity in a section is a function of the orientation of the set, with respect to the north. This is a bit counter-intuitive, as the orientation of the rock face (and specifically the angle with the fracture set) is surely more relevant than the azimuth (angle with the North) of the fracture – which in fact should not be that important. More detail on the method should be provided to improve clarity. Line 106: P21 is “fracture intensity”, not “fracture density” (which is P20 in the 2D case). Line 117-118: The second statement seems to suggest that different input data (“fracture characteristics”) were used for the four DFN realizations. Lines 120-123: Perhaps a sketch would help the reader understanding the procedure. Also, I believe this procedure is performed in 2D. If so, I suggest to use slope “profile” instead of “surface” – this would make the procedure easier to understand for the reader (especially if no figure is provided). Line 130: even more importantly, SRM is used to simulate the brittle propagation of fractures, and thus the brittle behavior of rock masses. Line 134: it would be good to provide a couple of examples (even in brackets) of the micro-properties that are used as input. Line 160: perhaps “interpenetrate” or “compenetrate” is a better term than “pass through”. Lines 186-188: this assumption is perhaps more adequate considering the low stress conditions that characterize the real slope. Line 190: Just a comment here. As the authors know, this approach may be “risky” in other conditions (i.e., high stress/high slopes) as it may cause a “shock” in the model, causing excessive damage in the slope, compared to a progressive excavation (or a progressive removal of the boundary), which generally is more representative of a "real world" situation. Lines 194-197: It is unclear what are the benefits of decreasing the friction coefficient while increasing gravity. Intuitively, the gravity increase already would induce an increase in shear stresses compared to the initial state (very much like increasing the density). How is this double effect (increase in shear stress, decrease in shear strength) accounted for in the FoS calculation? And why just the friction, and not the cohesion? The paper would benefit from a more detailed explanation of the method employed. Line 202: this seems a very stable slope. Expectedly, in view of the amount of rock bridges along the rupture surface, which may be estimated at about 30-40%, according to figure 7. Line 232 (and after): Perhaps it will be better to refer to the "slip mass" as "failed", or "detached" mass/volume/material. Line 255: I recommend referring to the "model geometry" or "morphology", rather than "shape". Line 272: A variability 25-75 is indeed very high. Perhaps this variability be lower if a more realistic DFN (i.e. inclusive of smaller fractures) was to be simulated. Absolute values would be lower, for sure. Line 290: again, just a comment. The limitations of this estimation is that is assumes that the base of the model is constituted by strong rock, likely with high coefficient of restitution, and the distribution of the failed mass over this distance is not considered. Lines 301-303: this sentence is unclear and requires rephrasing. Lines 303-305: I agree with the authors here: rock bridges are multiple orders of magnitude stronger than discontinuities, and this justifies the high FoS. The questions, however, is: are these estimations accurate and representative of the real situation? Figure 4 shows a rock mass significantly more fractured that the DFNs employed in this study, where the slope is formed by very large, intact blocks.

Comments on figures/tables Figure 1c: a scale and possibly a north arrow is required Figure 7: a legend bar (stress) and scales are needed for clarity Figures 8 and 10: the use of an uniform color bar and legend would enhance the comparison of the states depicted by each sub-figure. Table 4: I recommend using the same order for micro, numerical, and lab parameters: friction, normal, and tangential stiffness.

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