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

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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.

Responses to general comments

Comment 1: 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.

Response: Thank you very much for your comment. It is really true that the investigated slope can hardly fail considering the low height, a large amount of rock bridges along the slip surface. This is also confirmed by the high factors of safety. When it is subjected to significant environmental changes, such as earthquakes, rainfall, unloading, or overloading, the failure may occur following the potential failure mechanism.

The sampling window method (Kulatilake and Wu 1984) is used to collect fracture data in the present study. It is really true that the generated DFNs rely on and include the larger fractures since only fractures with the length larger than 1.5 m were measured in the field. The amount of fracture with the length smaller than 1.5 m are extremely large, which goes beyond the artificial measurement. Considering that small fractures have a little effect on slope stability, we take the cut-off limit of 1.5 m.

On the basis of the collected fracture data in the exposed rock surface, the DFN is a most possible representation of the real rock mass. However, it is really true that the DFN is more accurate and the numerical results are more reasonable if smaller factures are taken into consideration. In the revised manuscript, although there is no way to recollect fractures with the length smaller than 1.5 m, we reduced the strength of the intact material to remedy the lack of smaller fractures in DFN. Specifically, particle parameters which influence the strength of intact material, including the friction coefficient, tensile strength, and cohesion of particles are synchronously reduced by the same reduction factor (Bonilla-Sierra et al., 2015; Sun et al., 2014).

The new modelling results indicated that the factors of safety are indeed lower with the decrease of strength parameters, indicating the lack of smaller fractures indeed makes the factors of safety high. Nevertheless, the location of the critical slip surface remains consistent regardless of the strength parameter since the slip surface is always composed of pre-existing fractures and new-propagated ones. Similarly, the potential failure process is roughly identical to that of previous simulation since both are conducted in the natural condition (i.e., the gravity acceleration is 10.0).

In the revised manuscript, corresponding content of parameter reduction and the new modelling results are rewrote, which is modified a lot and thus not described in the responses. In addition, the scales were added in all required figures and the location of the mapping window has been added in Fig. 1c. We also carefully checked the whole manuscript and considered all of your line-by-line comments. Our responses to the line-by-line comments are as follows.

Responses to line-by-line comments

Comment 2: Line 40: authors should refer to "the non-persistent fractures in these works", rather than "these non-persistent fractures".

Response: Thank you very much for your correction. In the revised manuscript, we have changed "these non-persistent fractures" to "the non-persistent fractures in these works".

Comment 3: Line 51: SRM has been used also for underground applications, including mining and hydraulic fracturing.

Response: Thank you very much for your comment. We carefully searched the literatures regarding underground applications of SRM approach. It is really true that SRM approach has been widely used in mining and hydraulic fracturing. Therefore, we added corresponding description as "SRM models have been primarily used to simulate failure and deformation of fractured rock slopes (Bonilla-Sierra et al., 2015; Elmo et al., 2013), simulate hydraulic fracturing in naturally fractured reservoirs (Damjanac and Cundall, 2016), and estimate rock mass strength, fragmentation and micro seismicity in caving mines (Lorig et al., 2017)".

Comment 4: Lines 51-53: perhaps these two sentences can be merged. However, the first sentence requires rephrasing, as it seems some words are missing.

Response: Thank you very much for your suggestion. The latter sentence is the explanation of the former one; thus, these two sentences can indeed be merged. In the revised manuscript, we merged the two sentences as "DFN simulation included in SRM modeling program presents a significant variability, which means numerously possible realizations of 2D fracture systems exist given specified input parameters (Pine et al., 2006)."

Comment 5: Line 58: authors should state the country the investigated site is located in.

Response: Thank you very much for your suggestion. In the revised manuscript, we stated the country the investigated site is located in and described as "This study proposes a comprehensive approach that combines several well-established methods to conduct a stability evaluation and failure process analysis of a fractured rock slope in Tianjin City, China."

Comment 6: 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).

Response: We are very sorry for our unclear description. Actually, all sentences from Line 71 to 73 contribute to the description of the study area (i.e., the quarry area). Therefore, it is true that the quarry area is higher in the north. The "mountain" in this place refers to the monoclinal mountains striking south-north. In the revised manuscript, we rewrote these sentences as "The Laohuding Quarry area is characterized by the low-mountain terrain, which is higher in the north than in the south. The highest and lowest altitudes of the quarry area are 160 m and 60 m, with a relative elevation of 100 m. A majority of monoclinal mountains striking south–north exist in this area. The average slopes of the mountains in the east and west of the quarry area are 25° and 30°, respectively (Fig. 1b)."

Comment 7: 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?

Response: We are very sorry for unclear description. Actually, we initially aims to say “The limestone is moderately weathered” rather than “The limestone is moderately thick”. In the revised manuscript, we corrected it. “Karst phenomena are not obvious” means that the karst phenomena are scarcely visible in the study area due to low precipitation and the lack of groundwater. In the revised manuscript, we rewrote this sentence as “Karst phenomena are scarcely visible due to low precipitation and groundwater shortage.”

Comment 8: 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.

Response: Thank you very much for your suggestion. It is really true that the term “intermittent” for discontinuities is rarely used. In the revised manuscript, we used “non-persistent” to substitute it as you suggested. Field observation demonstrated that no bedding planes, faults, folds, and shear zones are developed in the rock exposure, which we further interpreted in the revised manuscript. Therefore, the bedding plays no role in stability analysis. It is non-persistent fractures that play the most significant role in the slope stability and potential failure process.

Comment 9: 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 not clear whether this was kept into account.

Response: Thank you very much for your suggestion. When collecting fracture data in the field, the cut-off limit we considered is 1.5 m. The reason we chose this cut-off limit is that the amount of fractures with the length smaller than 1.5 m are quite large, which is beyond the artificial measurement; besides, the effect of small fractures on the slope stability is comparatively smaller than big ones. According to the cut-off limit, the number of eligible fractures is exactly 169 in the sampling window.

We are sorry for not mentioning the method we used for collecting fractures. Your assumption is right that the traditional field method, i.e., the sampling window method (Kulatilake and Wu, 1984) is used to collect fractures. The sampling window method mainly presents two limitations:1) orientation bias and 2) trace length bias. Orientation bias occurs because the probability that fractures with small intersection angles between the fractures and exposed rock surface can be collected in the field is smaller than those fractures with large angles. However, it is should be noticed that orientation bias only need to be considered when performing 3D DFN simulation (Terzaghi 1965). In the present study, a 2D analysis was performed and therefore the orientation bias can be ignored.

Trace length are biased due to two conditions: 1) only one end of a fracture is measured and (2) no end of a fracture is measured. In the present study, we corrected the trace length data using the method introduced by Kulatilake and Wu (1984). Table 1 lists the mean value and probability density function (PDF) of the corrected trace lengths for each fracture set.

In the revised manuscript, we mentioned the sampling window method and the limits of it as "Fracture characteristics, such as orientation, trace length, spacing, roughness, aperture, filling, and termination, in the exposed surface were systematically surveyed by the sampling window method" and "The sampling window method features two main limits of orientation bias and trace length bias. Orientation bias is ignored in the present study since it is only considered in performing 3D DFN simulation. The measured trace lengths bias occurs when the sampling window method is applied due to the following: (a) only one end of a fracture is measured, (b) both ends of a fracture are measured, and (c) no end of a fracture is measured".

Comment 10: Line 90: Reference here seems out of place, unless the result came from that specific work.

Response: Thank you very much for your suggestion. The reference aims to present that the grouping method used in the present study is suggested by it. It is really true that the reference should not be put in this place. In the revised manuscript, we put the reference in the right place by rewriting the sentence as "The fractures can be divided into three sets using the method proposed by Chen et al. (2005), as shown in Figure 3".

Comment 11: 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.

Response: Thank you very much for your suggestion. We accept your professional suggestion and rewrote this sentence as "The measured trace lengths bias occurs when the sampling window method is applied due to the following: (a) only one end of a fracture is measured, (b) both ends of a fracture are measured, and (c) no end of a fracture is measured."

Comment 12: Line 99: I recommend "and 'investigate the' potential failure mechanism".

Response: Thank you very much for your suggestion. In the revised manuscript, we rewrote this sentence as you suggested, i.e., “The cross section normal to the exposed surface was used to perform the 2D stability analysis and investigate the potential failure mechanism of the rock slope.”

Comment 13: 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.

Response: We are sorry for our unclear description. The slope is oriented at a trend of 200°; we rotated the slope 20° so that the slope exactly strikes in the NS direction prior to the deduction of the function. The information above is omitted in our paper considering it has been explained in the work of Zhang et al. (2017). However, the omission of this important information obviously results in the misunderstanding. In the revised manuscript, we added this information and interpreted this function as “We rotate the slope 20° so that the slope strikes in the NS direction and assume the fracture frequency measured along the mean normal vector direction of fracture set i is λi, and the acute angle between this direction and NS direction is ηi. The fracture frequency along the line parallel to the strike of the outcrop plane is λicosηi (Priest 1993), and the cross section plane is λisinηi. The fracture frequency of the latter is tanηi times that of the former, and P21 (2D fracture intensity) follows this result according to the concept of the integral.”

Comment 14: Line 106: P21 is “fracture intensity”, not “fracture density” (which is P20 in the 2D case).

Response: We are sorry for our wrong use of the term “fracture density”. It is really true that P21 is fracture intensity, which represents the length of fractures per unit area of rock mass (m/m2). P20 is fracture density, which describes the number of fractures per unit area of rock mass (m-2). In the revised manuscript, we changed “2D fracture density” to “2D fracture intensity”.

Comment 15: Line 117-118: The second statement seems to suggest that different input data (“fracture characteristics”) were used for the four DFN realizations.

Response: We are sorry for our unclear description. “Input fracture data” is different from “fracture characteristics” in our description. The former one refers to indispensably statistical fracture data for establishing the DFN, such as the distribution types of fracture locations, P21, the mean and variance values of the trace lengths. A majority of DFNs can be generated by Monte Carlo simulation on the basis of these statistical fracture data. Therefore, input data are the same for different DFNs, which explains the first sentence “More than one DFN can be generated with the same fracture data”.

The latter one represents the specific fracture characteristics that the generated DFMs present, such as the specific location, dip angle, and trace length of each facture. These fracture characteristics vary for different DFNs, which is described in the second sentence, i.e., “For example, Fig. 4 exhibits four DFN s with different fracture characteristics”.

It is really true that the statements of the two sentences are misleading according to your comment; thus, we rewrote the two sentences in the revised manuscript as “More than one DFN can be generated on the basis of the aforementioned statistical fracture data. For example, Fig. 4 exhibits four DFNs with the same statistical fracture data, but fracture characteristics, such as locations, dip angles, trace lengths, are different from one another.”

Comment 16: 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).

Response: Thank you very much for your suggestion. It is really true that the procedure is performed in 2D. In the revised manuscript, we changed “surface” to “profile” for being easily understood.

Comment 17: Line 130: even more importantly, SRM is used to simulate the brittle propagation of fractures, and thus the brittle behavior of rock masses.

Response: Thank you very much for your comment. It is really true that SRM is widely used to simulate the brittle propagation of fractures, which we mentioned in Introduction but ignored here. In the revised manuscript, we added this application and described as “SRM approach is widely used to reproduce the mechanical properties and behaviours of fractured rock masses, simulate the fracture propagation and brittle failure of fractured rock masses, and simulate the failure and deformation of fractured rock slopes”.

Comment 18: Line 134: it would be good to provide a couple of examples (even in brackets) of the micro-properties that are used as input.

Response: Thank you very much for your suggestion. It is indeed better to provide some examples of the input micro-properties first. In the revised manuscript, we added some examples of micro-properties as “The SRM model in PFC2D is defined by many parameters, such as particle contact modulus, particle normal/shear stiffness ratio, and parallel bond modulus. These parameters cannot be directly identified via laboratory and field experiments”.

Comment 19: Line 160: perhaps “interpenetrate” or “compenetrate” is a better term than “pass through”.

Response: Thank you very much for your suggestion. The word “interpenetrate” is indeed much better than “pass through”; thus, we replaced “pass though” with “interpenetrate” in the revised manuscript.

Comment 20: Lines 186-188: this assumption is perhaps more adequate considering

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Discussion paper

the low stress conditions that characterize the real slope.

Response: Thank you very much for your suggestion. It is really true that the investigated slope is characterized by low stress conditions; thus, we added this reason as the support of the assumption in the revised manuscript. Specifically, it is described as "This process ignored the stress concentration at the tips of the structural fractures generated by tectonic stress, which was considered reasonable in this study since the investigated slope is characterized by the low stress conditions and the stress concentration was intensely reduced after the long-term stability of the rock slope."

Comment 21: 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.

Response: Thank you very much for your comment. It is really true this approach (one-time removal of the boundary) may cause excessive damage in the slope, especially for high slopes. However, the investigated slope was exactly formed by one excavation in the real condition; thus, the approach, i.e., one-time removal of the boundary, is practical. As for other high slopes, which may be more likely to be formed by progressive excavations, the progressive removal of the boundary is more appropriate. The specific approach to removing the boundary should be determined according to excavation methods of slopes.

Comment 22: 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.

Response: We are sorry for our unclear description. It is really true that the increase of gravity would induce the increase in shear stress, as well as the increase in normal stress. The increases in both stresses lead to increases in driving and resisting forces, which makes the change in factor of safety unclear. Therefore, the factor of safety cannot be reflected by only increasing the gravity. Only if one of the forces (driving or resisting forces) is fixed can the change of the other be related to the factor of safety. The driving force cannot be fixed because it is directly proportional to gravity; thus, the resisting force should be fixed. The resisting force is directly proportional to the shear strength, which is equal to c+σ tanâĄąφ (where c is cohesion; σ is the normal stress, and φ is friction angle). σ increases when the gravity increase; thus, tanâĄąφ is considered to be reduced for making resisting force constant. In PFC, tanâĄąφ is directly proportional to the friction coefficient of particle; thus, the decrease of the friction coefficient of particle can lead to the decrease of tanâĄąφ. In addition, the friction coefficient has little influence on cohesion; thus, making the amplitude of reduction of the friction coefficient is the same as that of the increase in gravity acceleration can ensure an approximate invariance of the resisting force. It is followed that the factor of safety is the ratio of the gravity acceleration in the limit equilibrium state (g') to that in the initial state (g), i.e., F= g' / g.

The details above are not described in the previous manuscript, which is indeed hard to tell the benefits the method. In the revised manuscript, we further interpreted the improved gravity increase method as "This method leads to the failure of a slope in PFC2D by slowly increasing gravity acceleration and reducing the friction coefficient of particles while keeping other parameters constant. Notably, the amplitude of reduction of the friction coefficient is the same as that of the increase in gravity acceleration. In this way, the resisting force can be fixed and therefore the factor of safety is directly reflected by the driving force".

Comment 23: 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.

Response: Thank you very much for your comment. It is really true that the investigated rock slope is extremely stable, which can be reflected by the high factors of safety. As you said, the amount of rock bridges along the rupture surface can also verify that the investigated slope is very stable.

Comment 24: Line 232 (and after): Perhaps it will be better to refer to the “slip mass” as “failed”, or “detached” mass/volume/material.

Response: Thank you very much for your suggestion. It is really true that “failed mass” is better than “slip mass”; thus, we carefully checked all the manuscript and changed “slip mass” to “failed mass”.

Comment 25: Line 255: I recommend referring to the “model geometry” or “morphology”, rather than “shape”.

Response: Thank you very much for your suggestion. It is really true that “morphology” is better than “shape”. We carefully checked the word “shape” describing the same meaning and then replaced it with “morphology” in the revised manuscript.

Comment 26: 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.

Response: Thank you very much for your comment. It is really true that the variability between factors of safety is very high. In the revised manuscript, we reduced the strength of intact materials to account for the smaller fractures, which we explained in the response to comment 1. In the recalculation of factors of safety, a lower variability is indeed observed and absolute values are also lower. We are conducting the recalculation of factors of safety of all 100 SRM model; thus, the final result is not yet available at present.

Comment 27: 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.

Response: Thank you very much for you comment. It is really true that the base of the model is constituted by strong rock, which is represented by a rough rigid wall in PFC. The distribution of the failed mass over the distance is not analysed since this result cannot been proved a statistical significance. The accumulation results vary for 100 different SRM models, which can be verified in Fig. 13. The only thing common is that the final deposit is composed of relatively intact rock blocks and crushed particles, and the blocks pile up above the crushed particles, presenting an inverse grading phenomenon.

Comment 28: Lines 301-303: this sentence is unclear and requires rephrasing.

Response: We are very sorry for our unclear description. In the revised manuscript, we rewrote this sentence as "The factor of safety of the investigated slope is extremely high but reasonable. In the field investigation, weak interlayer and through-going discontinuities are not observed. The non-persistent fractures are very developed, which therefore play a vitally important role in the stability of the investigated slope. The safety factors of this type of slopes (i.e., slopes are characterized by non-persistent fractures) are always high".

Comment 29: 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.

Response: Thank you very much for your comment. On the basis of our previous results (small fractures are not considered), the factors of safety are accurate and can represent the real situation. This is because we also calculate the factors of safety by the traditional methods (i.e., the ratio of the resisting force to the driving force), which presents the same result as the simulation ones.

The DFNs in Fig. 4 are totally introduced into the simulated slopes, which is reflected by comparison between Fig. 4 and Fig.7. As for very large and intact blocks you mentioned, maybe you refer to the blocks of the boundary sections located in the bottom and right sides of the slope section. The boundary section won't affect the slope stability, which mainly contributes to overcome boundary effect. In the revised manuscript, we added the description regarding the boundary section as "The bottom and right sides of the slope section were expanded by 10 m as the boundary section, which aims to avoid boundary effect and does not affect the slope stability (Fan et al., 2004)"

Responses to comments on figures/tables

Comment 30: Figure 1c: a scale and possibly a north arrow is required

Response: Thank you very much for your suggestion. In the revised manuscript, we added the scale and the strike of the slope in Fig. 1c.

Comment 31: Figure 7: a legend bar (stress) and scales are needed for clarity

Response: Thank you very much for your suggestion. In the revised manuscript, we added the legend bar and scales in Fig. 7 as you suggested.

Comment 32: 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.

Response: Thank you very much for suggestion. Different color bars and legends in Figs. 8 and 10 aim to make displacements of particles clear in each pictures, which is indeed inconvenient for the comparison of different states. In the revised manuscript, we unified the color bar and legend in each sub-figure as you suggested.

Comment 33: Table 4: I recommend using the same order for micro, numerical, and lab parameters: friction, normal, and tangential stiffness.

Response: Thank you very much for your suggestion. The same order for micro, numerical and lab parameters is more beneficial for comparing the results of parameter determination. In the revised manuscript, we changed the order of parameters to ensure they are orderly arranged.

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:

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