Discussion of "Limit analysis of slopes with cracks: Comparisons of results"

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The discusser is the author of one of the two papers (Utili, 2013) compared by the authors of "Limit analysis of slopes with cracks: Comparison of results". The discusser wishes to highlight here that in his opinion there are a few misconceptions in the note and wishes to ask the authors to please clarify some ambiguities for the benefit of the readers.

In their note, (Gao et al., 2015) use the phrase "potential tension cracks" several times, e.g. in the Introduction "Predicting potential tension cracks in soil slopes", "Baker 1981 …incorporated the depths of the potential tension crack" and "the location and depth of the potential tension cracks are included". But they do not define what a potential tension crack is. In the literature the concept of potential tension crack is absent. The word "potential" is consistently used in Utili (2013), Utili (2014), Baker (2001) and Baker (2003) only for failure mechanisms, e.g. 'potential failure mechanism' or 'potential slip surface' whereas it is completely absent in Michalowski (2013). But with regard to cracks, either a crack exists before the formation of a failure mechanism, called 'pre-existing crack' or 'open crack' in Michalowski 2013 or just 'crack' in (Utili 2013), or it is formed as a result of tensile failure occurring in an intact slope. In the literature the closest phrase to "potential tension crack" that one can find is 'potential tension crack zone' that is used in some soil mechanics textbooks, e.g. Ishibashi and Hazarika (2010), to indicate the zone in a slope subject to horizontal tensile stresses where cracks may form. However, the kinematic approach of limit analysis does not require stress equilibrium but only a kinematic admissible failure process, so could the authors please clarify the meaning they attribute to "potential cracks" in the note?

Then in the following sections of the note the authors make a series of statements that need to be clarified or contextualized:

- "…. by neglecting the energy dissipation during crack opening when conducting the limit analysis according to Utili (2013), larger stability numbers are obtained, and the stability of the slopes with a tension crack is significantly underestimated for steep slopes". The solution provided in Utili (2013) is for slopes possessing pre-exiting cracks. In this case, the solution does not underestimate at all the stability of slopes. The solution underestimates slope stability solely for the case of intact slopes (no pre-existing cracks) subject to a failure mechanism involving the formation of a crack in the failure process.

- "Moreover, these results were compared with those presented by Utili (2013) to demonstrate the effects of ignoring the dissipation factors during crack opening on the position of the potential cracks. As shown in Fig. 3, the trends in the critical locations were significantly different between our study and the study conducted by Utili (2013). … More reasonable results regarding the locations and depths of the potential cracks were presented in chart form according to the method used by Michalowski (2013)." Please can the authors state why the locations and depths of the potential cracks obtained according to Michaloswski's method are considered more reasonable? Here, and in other passages of the note, a misconception emerges: the idea that the location and depth of the so called "critical crack" for the case of unknown crack depth and location (Utili (2013)), would imply that this crack is likely to occur. Actually, Utili (2013) says nothing about the likelihood of occurrence of the critical crack. The analysis is meant to provide guidance to the geotechnical engineer when no information is available on existing cracks by providing a measure of slope stability (values of $\gamma H/c$) for the most unfavourable scenario, i.e. the presence in the slope of exactly the crack most adverse to the stability of the slope. Indeed, this worst case scenario will rarely occur since it is more likely that other pre-existing cracks, with a location and depth other than the most critical one, will be present and that the failure mechanism taking place will involve one of those.
- "The l/H ratio approached zero for vertical slopes according to Utili (2013). Thus no cracks will form on vertical slopes." The authors are invited to clarify the reasoning that has led them to conclude that according to Utili (2013) analysis no cracks will form on vertical slopes. In fact, in Utili (2013) it is simply shown that the most unfavourable pre-existing crack for the stability of a vertical slope is a crack as deep as the slope height and located at infinitesimal distance from the slope face. This situation (vertical slope with a full height crack at infinitesimal distance from the slope face) was first analysed by Chen (1975) who believed this situation to be realistic and provided the critical value of $\gamma H/c$

for such a case. Then, the authors say “the critical slip surface of vertical slopes cannot be obtained using the limit analysis method of Utili (2013): therefore, a slope of β =89 is used.” Certainly for β = 90 the minimisation of the energy balance equation may present problem of numerical convergence, however the theory allows us to identify the critical slip surface without any doubt (see Chen (1975), chapter 3).

In summary, the authors of the note aimed to make a comparison between the features of the failure mechanisms predicted by limit analysis for the case of slopes subject to a pre-existing crack and the case of intact slopes subject to a failure mechanism involving the formation of a single crack in the incipient failure process. However, this comparison risks being grossly misleading for the readers due to the way it is presented in the note and to the language used. For instance in Figures 2, 3, 4, 5, different quantities are presented (e.g. stability number, normalized depths of the cracks, etc.) with the legends inside each figure invariably saying “limit analysis method of Michalowski (2013)” and “limit analysis method of Utili (2013)”, as if there is one problem being tackled, namely finding out $c/\gamma H$ and the features of the failure mechanism for slopes subject to tension cracks, by two different methods: limit analysis of Michalowski and limit analysis of Utili. Indeed, this is not the case and such a representation is misleading as it can be ascertained by the following statement from Michalowski’s reply to (Utili, 2014)’s discussion (Michalowski, 2014): ‘stability analysis with pre-existing cracks is applicable to slopes with pre-existing cracks, whereas the analysis with a crack forming as part of the collapse process is applicable to intact slopes. One can only state that for two slopes of the same geometry and built of the same soil, one intact and one with cracks, the critical value of the dimensionless group $\gamma H/c$ for the intact slope will not be lower than that for the slope with cracks. However, the statement that one mechanism is more critical than the other cannot be made, because the two mechanisms are used to solve two different problems (one mechanism is used for a slope with a pre-existing crack and the other for an intact slope)’.

Therefore, I would like to call upon the authors to address the issues mentioned above.

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