

Interactive comment on “Laboratory and 3-D-distinct element analysis of failure mechanism of slope under external surcharge” by N. Li and Y. M. Cheng

Anonymous Referee #1

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General comments:

As a whole, the paper has a good scientific relevance. Methods and procedures are described in a clear way and the work can provide an interesting insight regarding the understanding of the failure mechanism of the slopes. However, at some points the authors need to be more rigorous in the interpretation of physical test and numerical model and also more critical respect to some results they provide. In the following, some critical points are proposed and need to be addressed by the authors.

Specific comments:

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1. Page 9: the authors argue that there are significant differences between the cases with 6 N (named as Case 1) and 60 N (Case 2) particle bond strength. However, Figure 11 shows that although a lower downslope movement occurs at the top of the slope in the 60 N case, it seems that a clear extrusion at the toe of the slope and forward movement of the slope body are also visible for this case. Moreover, it is not clear if in case 2 the slope remains stable or not and the sentence "it is demonstrated that larger bond strength is followed by slower and smaller failure, or the soil is more stable under external load" is too generic and needs a clarification. Therefore, in general the whole point needs to be clarified.

2. Page 10: based on the numerical results, the authors state that the slope crest corners are properly involved in the failure process (Figure 12). However, pictures in figure 6 (or figure 14a) seem to indicate that the crest corners are not involved in the failure mechanism and remain practically stable. A comment on this discrepancy is required.

3. As regards the previous point, probably the whole failure mechanism is strongly conditioned by the side effects both in the physical model and also in the numerical model, since the model boundaries along the three axis directions seem to be very close to the process volume and there are clear constraints in the development of the failure mechanism due to the presence of the lateral boundaries. This should influence the failure mechanism, which is probably different from the actual failure mechanism in case of no side effects. A comment on this point is also required.

4. Page 11: the authors state that a very good description of the failure process is not possible with the Finite Element SRM or LEM methods. Why? From the reviewer's point of view, this sentence is really questionable and should be proved with appropriate analyses to be used as comparison with DEM analysis or at least justified from a theoretical point of view. The failure process can be well simulated with a finite element analysis, better if 3D, especially if the initiation of failure is considered.

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5. In general, from a quantitative point of view, the authors focus on the results in terms of loading forces, while there is no matching between physical and numerical models in terms of slope displacements. This makes the whole analysis more qualitative than quantitative. As an example, they should calibrate more the numerical results in terms of vertical displacement histories (as in Figure 12 and 13) against the measured trends. Moreover, the force-displacement curves obtained from the numerical model do not match the measured ones for the post-failure stage. In fact, DEM method should be theoretically suitable to follow in a proper way the post-failure stage of the soil material, which affects the failure mechanism of the slope. This point is already discussed by the authors in the conclusions, but they should try to make a stronger effort in order to address the problem in a more rigorous way.

Technical corrections:

- figure 3 and 4 captions should be exchanged - figure 15: the authors should provide a clearer interpretation of the DEM results by adding schematically the failure mechanism on figure 15b and 15c, since in the current version of the figure it is not very clear.

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