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Discussions
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Interactive comment on "Analysis of instability conditions and failure mode of a special type of translational landslide using a long-period monitoring data: a case study of the Wobaoshi landslide (Bazhong city, China)" *by* Yimin Liu et al.

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NHESS review report on paper 2019-133 by Y. Liu, C. Wang, G. Gao, P. Wang, Z. Hou, Q. Jiao

I have been asked first to produce a ÂńÂădiscussionÂăÂż on your very interesting paper. You will find again this discussion below, which presents frankly my point of view particularly with respect to the limits attached to the application of limit equilibrium method in geomechanics/geotechnics.

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Discussion paper

Now for a review report, I will insist on the interest for the reader to understand well your FEM modelling and (if possible) to consider as instability criterion the so called ÂńÂăsecond order work criterionÂăÂż (well described in the recent book ÂńÂăFailure in Geomaterials, a Contemporary TreatiseÂăÂż by Wan, Nicot and Darve and published by ISTE/WILEY). Your paper will become a reference paper if you are able to compare (i) limit equilibrium method, (ii) FEM computations with Mohr Coulomb criterion and (iii) FEM computations with the second order work criterion. I am not asking you to do that in the present paper – probably too much work – but envisage such comparisons for a next paper. So for the present paper, I suggest to youÂă: (i) to add in the paper some comments about the limits of the equilibrium method, (ii) to give more details about your FEM modelling. In the present state of your paper, your numerical computations appear as a black box, this is not reasonable for the readership.

Previous published discussionÂă: The Authors discuss in their paper devoted to ÂńÂăinstability conditions and failure mode of a translational landslideÂăÂż a case study, whose main interest lies on its careful long term monitoring. The characteristics of this landslide are remarkably described and these results will certainly constitute a very valuable data bank for future studies. Moreover the basic mechanical analysis of the related boundary value problem has been conducted in a very convincing manner and the failure mechanism is clearly exhibited. So all the ingredients have been collected to form a perfect basis for the modelling. Let us insist here on the fact that a numerical or analytical mechanical model of natural hazards - to be fruitful - has to take into account as properly as possible the discriminant or critical features, exhibited by preliminary geological/mechanical/pluviometric analyses. These analyses have to be based on a well designed monitoring campaign and the issueing data bank. This is clearly the case here and the Authors have to be congratulated for having achieved that – particularly the correlation between rainfall intensity and block motion.

So what is interesting to be discussed now is the choice of the modelling strategy. The analytical model is based on limit equilibrium method, whose limits have to be recalledÂă: - this is a purely static method, - it ignores the influence of strain history, while it is well recognised that soils present an important hardening regime involving large plastic strains before failure, - a coulombian friction is taken into account, while the soil can fail before reaching Mohr-Coulomb plastic limit condition according to the second order work criterion, which is a more general material instability criterion. This is particularly important here, since the sliding surface has a very low slope angle of 6-8 degrees. Thus the instabilities are certainly appearing before Mohr-Coulomb criterion. - the hydro-mechanical constitutive relation of the sliding zone, which plays here a central role, can not be taken into account.

Having in mind these drastic limits, the reader is waiting for a deeper and more realistic finite element computation. However the finite element modelling is presented with very few details. According to the values of parameters as given on table 4, it seems that all involved geomaterials (rocks and soils) have been considered as associate plastic materials. Thus their dilatancy angle is assumed to be equal to their friction angle, what is clearly not verified experimentally. Let us note that the constitutive elasto-plastic matrix is symmetric for associate materials, preventing to describe all instabilities and bifurcations occuring before the Mohr-Coulomb plastic limit surface.This is probably the reason why it has been necessary to choose a so low friction angle of 11.2° for clay – what seems unrealistic.

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