Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-410-RC1, 2018

Natural Hazards and Earth System Sciences Discussions
Open Access

Interactive comment on "Influence of extreme long-term rainfall and unsaturated soil properties on triggering of a landslide – a case study" *by* Håkon Heyerdahl

Anonymous Referee #1

Received and published: 8 January 2018

The manuscript deals with the interpretation of the failure of a slope in Norway occurred in 2000, taking into account the effect of soil suction on soil shear strength and its possible role in the slop failure evolution.

Such a topic is in line with the aims of NHESS. The English language is good and understandable.

However, the scientific content and the novelty of the manuscript are poor, while I believe that when a case study is presented for possible publication in a journal like NHESS, there must be novelty in the adopted modeling approach. Instead, as it will be detailed in the following specific comments, al the presented elaborations are based

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Discussion paper

on standard or even outdated approaches, so that the presented case study does not add any contribution to the understanding of triggering mechanisms in slopes like the one considered.

Therefore, I think the manuscript should be rejected.

Specific comments:

Section 2

The presented discussion of the effects of soil suction on effective shear strength in unsaturated conditions appears outdated (as are the models adopted afterwards to predict soil strength in the descirbed applications). Much work has been done in this topic. At least Lu and Likos (2006), Alonso et al. (2010), Lu et al. (2010), Nikoee et al. (2013), Greco and Gargano (2015) should be mentioned and maybe also applied, as the performance of the adopted models for the considered silt is extremely poor. Indeed, Greco and Gargano (2015) showed that silty soils are the ones for which the application of outdated approaches, like those adopted here for the evaluation of the contribution of soil suction to shear strength, leads to largely wrong results.

As the evaluation of the effects of unsaturated soil shear strength seems one of the main focuses of the manuscript, this is for me a very critical weak point.

Section 3.1

Page 5, lines 24-25. This is an example of the poor organization of the manuscript. The author touches here the possible effect on their results of the hysteresis in the water retention curve, without telling how he is willing to cope with it. Not even in section 4.3 ("Retention curve") he gives any information on this regard, and only in the sections devoteds to the case study, he comes out with other retention curves (and another soil, as well!), for which they modeled the hysteresis.

Section 4.1

Page 7, line 29. The numbering of tables should better follow their order of appearence in the text. Thus, Table 2 should be Table 1 and vice versa.

Section 4.2

Page 8, lines 3 and 5. The term "permeability", used here for the first time, and then repeated several times thorughout the manuscript, should be replaced with "hydraulic conductivity", which is the correct name of the variable the author refers to.

Page 8, lines 4 to 7. Another example of the lack of organization of the manuscript. Afetr referring only of silt samples (see Tables 1 and 2, as well as the entire sections 3.3 and 3.4), now sand suddenly comes out, and then also sandy silt and silty sand. The reader does not know anything about such soils.

Section 4.3

Page 8, line 12. Not only a graoh, but also the parmeters of the WRC according to Fredlund and Xing (1994) model should be provided. By the way, this curve is said to belong to a sandy silt.

Fig. 6. The expression proposed by Fredlund and Xing for the WRC relates suction with volumetric water content, whicle here the plot of gravimetric water cintent vs. suction is given. If a change of variables in the equation has been made (which is not totally correct, in my opinion, and not even necessary), this should be at least specified.

Section 5.3

Page 10, lines 15-20. It is not clear what the authors wants to point out. Is the water retention curve not representative of the tested soil? Or are the applied soil suction uncertain?

Section 5.6

It should be Section 5.5

Page 11, line 23. Other models, nowadays used at least as commonly as Vanapalli et al. (1996) take into account micromechanical effects.

Section 5.7

It should be Section 5.6

Page 11, lines 30-31. It is not true that the integranular forces exterted by suction act only along menisci, as they act also (maybe mainly, in most cases) as pressure exerted over the wteed portion of the external surface area of the solid particles (e.g. Greco and Gargano, 2015). See Lu and Likos (2006), which derive separately the contributions of menisci and pressure on the basis of thermodynamic considerations, and Nikoee et al. (2013), who propose a way to quantify them.

Page 12, line 7. The author seems to mix up two effects: the increased stiffness owing to overconsolidation does not imply an increased shear strength.

Section 6.1

Page 12, line 14. Now the simulations are carried out for two soils (silt and sand), after discussing only properties of silt. By the way, looking at the picture in fig. 2b, it appears that the soil profile at the considered site is layered, which may make things much different from the modeled cases. At least some discussion about this choice should be made.

Page 12, line 18. Probably this is a typo, but none of the three models presented in section 2.2 makes use of the product of water content times suction.

Page 12, lines 21-24, and figures 14 and 15. New hydraulic characteristic curves are here introduced, without any information about the "new" soils nor how the curves were estimated.

Page 12, lines 25-28. The "warm up" of the model with one year rainfall makes sense, but it has not only effects on the infiltration capacity (top surface boundary condition), as said, but it is rather a way to start the simulation of the triggering rainfall event from realistic intial conditions.

Section 6.2.1

Page 13, line 12. I see the author's point, but I do not think it is correct to talk about a failure surface in a situation in which the minimum factor of safety is >2.

Sections 6.2.3 and 7

As a matter of fact, the author makes his slope stability analyses considering a contribution of suction to soil shear strength much smaller than measured (page 12, line 20). This has no effect on the results, as the slope needs to become fully saturated for the model to predict a failure along the slope.

So, I have serious doubts about the conclusion that "evaluation of slope stability based on unsaturated soil proerties will be increasingly important (...) to understanding rainfall triggering of landslides".

I even miss the overall message of the presented case study, as it does not seem so important to correctly evaluate the unsaturated shear strength of a soil along a slope where saturation is necessary to observe a failure.

Furthermore, even a substantial reduction of soil cohesion, compared to the experimentally determined values, must be introduced in order to obtain low values of the safety factor. A possible interpretation is that the actual layered soil profile deeply affects the infiltration process (e.g. Damiano et al., 2017) and, in turn, the slide triggering conditions. Thus, I wonder what we learn from the presented case study.

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