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

H. Heyerdahl
hhe@ngi.no

Received and published: 11 March 2018

To both referees I want to express my thanks to both anonymous referees #1 and #2 for their efforts to give thorough and relevant comments and suggestions to the submitted paper. In my answers to the referee comments, I try to give answers to all comments. According to the procedure of the NHESS discussion process, a revised paper is not submitted at this time, which means that intended changes in the paper are only described principally in my answers. Some referee comments are general and not possible to answer/solve directly in the answers. At the start of my answer to each referee, I discuss these general comments, before answering the specific comments from each of the Referees. For specific comments, I intend to improve the paper in line with my answers, or I give a clarification where there may be misunderstandings. Finally, I hope that my response to the referee comments will give me the opportunity to present a revised paper at a later stage. Referee #1 General comments I am happy that Referee #1 considers the topic of the paper within the aim of NHESS, although the main conclusions from that point on are not so positive. Some of the general comments might not be fully answered before a revised paper is submitted, but I would like to underline that I have considered each of the comments given by Referee #1 very seriously. I will not argue with the general view regarding to what extent a case study should include novelty, existing or new methods etc. However, I intend to include some ideas on interpretation of the unsaturated shear strength in the reviewed paper (not included in the first version of the paper). I can’t be absolutely conclusive in my suggestions, since the laboratory tests were not designed to verify the method I will present. More work here could be interesting. The aims and scope of NHESS refers to "...a wide and diverse community of research scientists, practitioners, and decision makers...". In my opinion, this scope does not exclude case studies like mine. A practical case study may be useful for many of the groups listed above, by applying and combining aspects known from everyday practice, making the study within the reach of the reader (without being outdated). Still, I think it is true to say that the use of unsaturated soil mechanics is not so widely spread outside of academia, and NHESS is probably not the typical magazine for this special branch of geotechnics. That was also a reason for not diving too deep into testing methods and parameters of unsaturated soils. For many soils and countries there exists little unsaturated data. New data (of good quality) is therefore still an achievement and could give some credit in the scientific sense. Also coupling of local soil data with real documented landslide events has scientific value, and maybe contributes to making the study interesting (in my view, that is...). Specific comments Section 2 Many thanks to Referee #1 for sharing references of relevant and recent literature. I will include more discussion of recent work in the reviewed paper, and consider whether some of them should be included for compari-
son with test data. "Outdated" is not synonymous with "old". Well-established (and old) methods are widely used in unsaturated research and practice, and may be attractive to the reader. E.g. the forever young WRC-model van Genuchten (1980), often preferred to the better (in my opinion) WRC-equation by Fredlund and Xing (1994), simple but comprehensible models for prediction of unsaturated shear strength by Vanapalli et al. (1996) and Öberg and Sällfors (1997); the even simpler bi-linear model by Fredlund et al. (1978), although other models as Khalili and Khabbaz (1998) and Lu and Likos (2006) are superior. Just one example: Alonso et al. (2010) actually discuss their model for evaluation of shear strength based on effective saturation rate by use of (among others) the simplest WRC-function of them all, from Brooks and Corey (1964). Section 3.1 Page 5, lines 24-25. I don’t quite follow this criticism, as this section discusses ways to perform shear tests, by multi-stage or single-stage tests. Tests were run along a drying path for reasons discussed in this section. Thereby I did not see a reason to discuss how to deal with hysteresis here. In the infiltration calculations I do deal with this, by following the main wetting path of the WRC for the infiltration process. Section 4.1 Page 7, line 29. Numbering of Tables will be corrected. Section 4.2 Page 8, lines 3 and 5. The term “permeability” will be replaced with "hydraulic conductivity". Page 8, lines 4 to 7. The site description and presentation of soil data will be made more complete. References to sand and silt (abbreviated from "silty sand" and "sandy silt"), will be made consistent. Section 4.3 Page 8, line 12. Parameters of the WRC-equation (Fredlund and Xing, 1994) will be included. Fig. 6. The switch between water contents actually does not affect the equation or parameters in the Fredlund and Xing (1994) equation, as volumetric and gravimetric water content only differ by a constant for a given soil. I agree it is not necessary to do this switch, and will make this consistent in the reviewed paper (my apology is the geotechnician’s habit of preferring the gravimetric water content). Section 5.3 Page 10, lines 15-20. This is a good question! The easy way out would be not to mention it (or even check it). There is however inherent uncertainty in actual water content in the specimen in the individual steps of a multi-stage shear test, as water content in the specimen may not be verified during the test. only estimated from measurement of water flowing in/out of the specimen (which was done with a GDS pump, correcting for diffused air). I believe think deviating water content during tests (when comparing the WRC with applied suction) is not always reported; results are normally presented for applied suction without presenting this kind of uncertainty or mismatch in the data, but I have chosen to present it. As the silt layer was quite uniform (e.g. the void ratio varied within very narrow limits), and the determination of the WRC was quite thorough (Heyerdahl and Pabst, 2017), I have assumed that the measured WRC is representative. Concluding that water contents after tests are generally higher than expected from applied suction for the measured WRC, I need to make a choice for the interpretation. Correcting the suction values based on measured water content after the test actually makes the data collapse in a quite attractive manner. The answer to the question therefore is that applied (or "effective") suction is uncertain, and somewhat lower than the applied suction according to the controlling pressures (ua – uw) applied by axis translation. In a perfect laboratory test, these values would match. Section 5.6 The section number will be corrected. Page 11, line 23. I will include discussion of other interpretation methods for unsaturated shear strength in the theory chapter, and consider comparison with the test data. Section 5.7 Successive error... The section number will be corrected! Page 11, lines 30-31. My formulation is probably not good if it seems as if I generally mean that suction only acts in menisci at the grain contacts. That is of course not always the situation. What I wanted to express is that for low water contents the contacts at menisci will be important. I will also check with the suggested literature. Page 12, line 7 Here I do not agree. Experience generally shows that high stiffness caused by preconsolidation is related to higher shear strength than similar soils not exposed to preconsolidation. Reference is made to the classic work for OC clay (Ladd and Foott, 1974), and we know well that OC clay are stiffer than NC clay. The same goes for other soil types when compressed and then unloaded. The question still is whether suction alone causes a similar effect, not only on the volumetric response. Maybe at the moment only a hypothesis – but not a mix-up with stiffness. Section 6.1 Page 12, line 14. The site is definitely layered. To
short-term rainfall, which also occurs in these types of soils. (Again; it will be difficult to
trigger shallow landslides for the high measured cohesion, which means that such an
analysis will have to lean on a variation in this parameter).

Colorado State University, Ft.Collins, Colo. Hydrology paper No.3, (March). Fredlund,
Xing, A., 1994, Equations for the soil-water characteristic curve: Canadian Geotechnical
in GEO-SLOPE International, L., ed., Volume August 2015 release Calgary, Alberta,
Canada. Heyerdahl, H., 2016, Rainfall-induced landslides in Quaternary soils in Nor-
way, in Proceedings 3rd European Conference on Unsaturated Soils (E-UNSAT 2016),
ison of experimental and predictive approaches for determination of water retention
curves of intact samples of Quaternary soils Journal of Geotechnical and Geological
for the determination of the shear strength of unsaturated soils: Géotechnique, v. 48,
no. 5, p. 7. Ladd, C. C., and Foott, R., 1974, New design procedure for stability of soft
clays: Journal of Geotechnical Engineering, ASCE, v. 100, no. GT7, p. 24. Lu, N.,
and Likos, W. J., 2006, Suction stress characteristic curve for unsaturated soil: Journal
of Geotechnical and Geoenvironmental Engineering, v. 132, no. 2, p. 131-142. van
Genuchten, M. T., 1980, A Closed-form Equation for Predicting the Hydraulic Conductiv-
Fredlund, D. G., Pufahl, D. E., and Clifton, A. W., 1996, Model for the prediction of shear
strength with respect to soil suction: Canadian Geotechnical Journal, v. 33, no. 3, p.
379-392. Öberg, A. L., and Sällfors, G., 1997, Determination of shear strength param-
eters of unsaturated silts and sands based on the water retention curve: Geotechnical