Natural Hazards
and Earth System
Sciences

Discussions

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

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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 dis-

cuss 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 comparison 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 account for all the layers would be virtually impossible, and the question is whether a detailed layer modelling is possible, when considering uncertainty in lateral continuity and layer thickness. The laboratory data is assumed representative for the variety in the soil mass. The soils are not completely different, mainly consisting of sandy silt and silty sand, and the hydraulic conductivity of these layers only varies with about one order of magnitude.

I will attempt to improve the discussion of the numerical modelling. The idea was to avoid numerical trouble with rather arbitrary layers of almost equal soils, for which the variation in soil parameters is not well-defined. Instead, the effect of assuming more or less homogeneous conditions for these soil types was checked.

Page 12, line 18. I wanted to be conservative and not utilise the full strength of the silt as measured, meaning that water content was for simplicity assumed to represent χ. At least such values are documented from the tests, both for sandy silt and for sand (Heyerdahl, 2016). It was also a choice available in the calculation software (Geo-Slope_International, 2015).

I will consider including a more precise strength function for the revised analyses.

Page 12, line 21-24, Figures 14 and 15. The soils will be presented more clearly, with information about how the hydraulic curves were estimated based on the WRC-

functions and measured saturated hydraulic permeability.

Page 12, line 25-28. I do agree. However, the start of line 25 points not to the infiltration capacity, but to the applied rainfall at the top boundary.

Section 6.2.1

Page 13, line 12. Agreed, I could rather use the term "critical shear surface"?

Section 6.2.3 and 7

The numerical analyses (unfortuneately..) show that unsaturated shear strength is not critical for landslide release here, since the slope saturates by groundwater rise from the clay, and due to the high measured cohesion. Unsaturated flow is however the guiding mechanism that destabilises the slope, and is connected to the title: "..extreme long-term rainfall and unsaturated soil properties…". In this way, I don't think the title is misleading.

I wish to present the unsaturated shear strength data in the paper, as I consider them a step forward for unsaturated research on Norwegian silty soils (whatever that may be worth). One way to make the unsaturated strength more relevant to the study, would be to apply the data to triggering of shallow surfaces, exposing the slopes to more intense 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).

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Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-410/nhess-2017-410-AC3-supplement.pdf

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