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Interactive comment on "Development and validation of the Terrain Stability model for assessing landslide risk during heavy rain infiltration" *by* Alfonso Gutiérrez-Martín et al.

Anonymous Referee #2

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The manuscript describes a numerical approach to slope stability, and the corresponding original software. The model is two-dimensional, and its applicability is limited to a single slope; advantages are the software being freely available and inclusion of wet soil conditions, apparently missing in existing commercial software.

I believe that the manuscript suffer from several limitations, and in my opinion is not suitable for pubblication in NHESS. I will try and motivate my opinion in three different sections, as requested by NHESS reviewing guidelines.

__________________ General comments, main issues:

I believe that the material in the manuscript is organized in a rather confusing way, and

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Discussion paper

that key sections of the text do not contain the information they are supposed to.

The Title suggests that the paper deals with landslide "risk", while it describes a numerical model for slope stability assessment. The generally accepted definition of "risk" associated with a natural hazard is the product, or the combination, of the likelihood of an event of the given kind ("hazard") and "exposure", or "vulnerability", of human life and infrastrucuture to that kind of hazard. Morevoer, the generally accepted definition of "hazard" is, in turn, the product of spatial probability, temporal probability and magnitude of an event of the given type to occur. The model described in the manuscirpt deals with spatial and magnitude assessent of landslides; it is not clear to me whether a temporal component is included. Surely we cannot speak about "probability" here, because the model obtains a factor of safety, which is clearly NOT a probability. In order to obtain a probabilistic interpretation of the factor of safety, one needs to perform additional, non trivial steps. See, for example:

- S Raia, M Alvioli, M Rossi, RL Baum, JW Godt, F Guzzetti (2014). Improving predictive power of physically based rainfall-induced shallow landslide models: a probabilistic approach. Geosci. Model Dev. 7 (2), 495-514. https://doi.org/10.5194/gmd-7-495-2014

- S Zhang, L Zhao, R Delgado-Tellez, H Bao (2018). A physics-based probabilistic forecasting model for rainfall-induced shallow landslides at regional scale. Nat. Hazards Earth Syst. Sci., 18, 969–982. https://doi.org/10.5194/nhess-18-969-2018

- E Canli, M Mergili, B Thiebes, T Glade (2018). Probabilistic landslide ensemble prediction systems: lessons to be learned from hydrology. Nat. Hazards Earth Syst. Sci., 18, 2183–2202. https://doi.org/10.5194/nhess-18-2183-2018

Moreover, it is not true that the model itself includes an assessment of vulnerability, which must be taken into account separately and, most importantly, with additional (and often difficult to obtain) data. The Title also mention validation of the model, which was actually performed in a rather qualitative way. It also mentions the expression "during heavy rain infiltration", which is not actually substantiated in the manuscript since, again, no explicit time dependence is implemented as the word "during" would suggest, and no actual "infiltration" is considered, but only its effective result - namely, an effective value for pore pressure calculated at an arbitrary depth under the soil surface. At least, this is what I can understand after reading the whole manuscript. I will give more details below.

The Abstract contains unnecessary information (the first two, long sentences), a few inaccuracies (see below) and, most importantly, fails to properly and succintly introduce the methods, results and conclusions obtained in the manuscript. A GIS support is mentioned, while the whole code is implemented in Matlab.

From the Introduction, we understand that the scope of the proposed model is slope stability from the engineering point of view, which is a perfectly legitimate field for a NHESS pubblication. Nevertheless, given the range of expertise that the Journal is devoted to, I believe that the topic should be put in a broader perspective. The Authors made explicit reference to a number of commercial software programs that are supposed to have the same applicability domain. I believe that the existence of other, well-known models for slope stability assessment with a broader applicability domain should be acknowledged, and the relationship between these models and engineering of individual slopes should be elucidated. The following list of a few such models is certainly not exhaustive but it is a starting point:

TRIGRS:

- RL Baum, WZ Savage, JW Godt (2008). TRIGRSâĂŤa Fortran program for transient rainfall infiltration and grid-based regional slope-stability analysis. US geological survey open-file report 424, 38 https://pubs.usgs.gov/of/2008/1159/

- M Alvioli, RL Baum (2016). Parallelization of the TRIGRS model for rainfall-induced landslides using the message passing interface. Environmental Modelling & Software 81, 122-135 http://dx.doi.org/10.1016/j.envsoft.2016.04.002

SINMAP:

- RT Pack, DG Tarboton, CN Goodwin, (2001). Assessing Terrain Stability in a GIS using SINMAP. In: 15th annual GIS conference, GIS 2001, Vancouver, British Columbia, February 19-22. (and references therein; SINMAP is actually referred to at the very end of the paper, without any description or reference)

SHALSTAB/r.shalstab: -http://calm.geo.berkeley.edu/geomorph/shalstab/index.htm

-https://grass.osgeo.org/grass74/manuals/addons/r.shalstab.html

GEOtop/GEOtop-FS:

- R Rigon, G Bertoldi, TM Over (2006). GEOtop: A distributed hydrological model with coupled water and energy budgets. Journal of Hydrometeorology 7 (3), 371-388 https://doi.org/10.1175/JHM497.1

- S Simoni, F Zanotti, G Bertoldi, R Rigon (2008). Modelling the probability of occurrence of shallow landslides and channelized debris flows using GEOtop-FS. Hydrol Processes;22(4):532-545. https://doi.org/10.1016/j.proeps.2014.06.006

r.slope.stability:

- M Mergili, I Marchesini, M Rossi, F Guzzetti, W Fellin, (2014). Spatially distributed three-dimensional slope stability modelling in a raster GIS. Geomorphology 206: 178-195. http://doi.org/10.1016/j.geomorph.2013.10.008

Moreover, in the Introduction, the Authors state that stability models are limited to 2D approaches, while a few examples exist of 3D models. For example, among others:

- M Mergili, I Marchesini, M Alvioli, M Metz, B Schneider-Muntau, M Rossi, F Guzzetti (2014). A strategy for GIS-based 3-D slope stability modelling over large areas. Geoscientific Model Development 7 (6), 2969-2982 http://doi.org/10.5194/gmd-7-2969-2014

- ME Reid, SB Christian, DL Brien, ST Henderson (2015). Scoops3DâĂŤsoftware to analyze three-dimensional slope stability throughout a digital landscape (Version 1.0). Virginia: U.S. Geological Survey

- TV Tran, M Alvioli, G Lee, HU An (2018). Three-dimensional, time-dependent modeling of rainfall-induced landslides over a digital landscape: a case study. Landslides, 1-14 http://doi.org/10.1007/s10346-017-0931-7

About the resources required to apply stability models on large areas, these papers describe methods, including parallel computing, to cope with such issue:

- M Mergili, I Marchesini, M Alvioli, M Metz, B Schneider-Muntau, M Rossi, F Guzzetti (2014). A strategy for GIS-based 3-D slope stability modelling over large areas. Geoscientific Model Development 7 (6), 2969-2982 http://doi.org/10.5194/gmd-7-2969-2014

- M Alvioli, RL Baum (2016). Parallelization of the TRIGRS model for rainfall-induced landslides using the message passing interface. Environmental Modelling & Software 81, 122-135 http://dx.doi.org/10.1016/j.envsoft.2016.04.002

Sections 2 and 3, devoted to a description of the methodology implemented in the model, are confusing, and I cannot understand what are the assumptions and the relevant details of the method implemented in the software, and whether it is a novel enough approach. I will give more details later on.

Section 4.3 is devoted to the description of the results obtained using the proposed model. This section is very confusing, again. I believe that the comparison of the results of the proposed model with the another model, and with a real landslide scenario, are presented in an unsatisfactory way, since they are qualitative almost everywhere and it is difficult to understand what the quantitative comparisons refer to. Moreover, there is a large fraction of text which does not pertain to results but to the methodology itself.

Eventually, in Section 5, devoted to describe conclusions of the manuscript, again I do not find enough evidence of actual conclusions drawn from the results. In addition to repeating already mentioned concepts in a, in my opinion, misleading way (i.e., use of "prediction", of "time", etc.), there are a couple of expressions which, I believe, are not allowed in assessing the conclusions in a scientific paper. First, the Authors state that the proposed model "defines fairly well areas that intuitively appear to be susceptible to landslides and defined rigorously the failure curve". In this sentence, "fairly well" and "intuitively" are not good enough to assess the predicting performance of a quantitative model. Moreover, the "rigorous" definition of slip surfaces does not appear to be substantiated by the presented results, as I will explain at lenght in the following. Then, the expression "this model is probably the most powerful tool for determining slope stability", is again not substantiated by the presented results. Eventually, a reference to the SINMAP model comes out of the blue, in the second-last line, which is unjustified.

__________________ Other specific comments:

In the Abstract, in addition to unrelevant information in the first two sentences already mentioned above, I believe that a few other ambiguities exist. It is stated that "Climate is one of the main factors [affecting slope stability, Ed.], especially when large amounts of rainwater are absorbed in short periods of time". The paper does not discuss climate effects on landslides, or correlations between the different factors determining the climate of a given region and landslides. Thus, this should not appear in the Abstract, which must contain a short description of the specific topic discussed in the paper; maybe in the introduction, if a sufficiently clear link is made with the topic of the paper. A quantitative relationship with climate (actually, climate change), rainfall events and slope stability including actual time-dependent account for rainfall infiltration can be found in, e.g.:

- S Gariano, F Guzzetti (2016). Landslides in a changing climate. Earth-Science Reviews, 162, 227-252. https://doi.org/10.1016/j.earscirev.2016.08.011

- M Alvioli, M Melillo, F Guzzetti, M Rossi, E Palazzi, J von Hardenberg, MT Brunetti, S Peruccacci, (2018). Implications of climate change on landslide hazard in central Italy Science of the Total Environment 630, 1528-1543. https://doi.org/10.1016/j.scitotenv.2018.02.315

The Authors claim that the model is "supported by a GIS", which is not true. The only step in which a GIS can (can) be used is when they mention that the terrain profile was obtained fom a DEM. The model is coded in Matlab, and not within a GIS. The statement "the model is especially useful for predicting .. scanarios of heavy unpredictable rainfall" is very bold. How can accout for rainfall into a mathematical model, if the rainfall is unknown?!?

In the Introduction, in addition to what I have written above. At lines 35-36, the Authors refer to a 2002 paper commenting "nowadays"; I believe that more recent papers exist, other that a 16-years-old one. FOS is used but not defined. Moreover, throughout the Manuscript, the Authors refer to factor of safety using both "FOS" and "Fs", apparently for no good reason.

Section 2, where details of the models are described, is rather confusing to me. After the relevant equations are introduced (missing the definition of a few quantities here and there), it is stated that these coupled equations must be solved to obtain the factor of safety Fs and the angle theta. Then, theta is assumed as constant, for no apparent reason, other than the sentence that "it provides optimal results", with no further explanation or justification. The whole meaning of theta should be explained in a better way, in my opinion. Then, the role of pore pressure is introduced. I believe it is imposisble to understand why they Authors refer to a "pore pressure distribution" and then use a single value (effective coefficient?) for it, or if they actually use a distribution. Moreover, it is impossible to understand if a time dependence - according to actual rainfall infiltration as a function of, well, rainfall intensity and varying water content in the soil, was taken into account or not.

Line 120, "precise" should be "accurate"

Lines 125-126, "lower than 1, or stable if it is higher than 1" miss the (mathematical) possibility that Fs=1. Moreover, the statement that Fs "tend to be higher than one" deserves an explanation in addition to Burbano et al. (2009), since it represents the whole point of the paper.

Lines 133-136: I do not understand the seentence from "However" to "exhogenous factors of the slope". Moreover, as for the sentence "Fs>=1.3 can be considered stable by most standars", please se my detailed discussion below.

Lines 141-142: whay does the initial curve depend on the "data introduced"? Does the user specify the whole curve, or what? What is the dependence of the results upon such an initial, arbitrary choice?

Section 3 tries to describe in somewhat more detail the operation of the software, but this is also done in a confusng way, in my opinion. First, I find confusing to name a terrain stabilitt model as "terrain stability", but this might be my personal taste.

Figure 2 shows a terrain profile, obtained from a DEM, and it is mentioned that the profile was splitted into 500 slices. How does the profile emerge from the DEM? Which DEM, with what resolution? Also, why 500 slices? Is this the number of DEM cells along the profile, or is it less, or more? If it is less, why is it so? If it is more, how do we interpolate the DEM and why? Is the use of a circular shape for the slip surface a limitation, which I believe it is, given that other engineering-like models use "trial" (and not "first", since there is no hierachy in the different "trial" surfaces) surfaces of any shape? For example, in the SSAP model, which has apparently an applicability domain similar to the model presented in the manuscript and it is free as well, slip esurface can be of any shape, to my knowledge; I might be mistaking: https://www.ssap.eu.

Fonts are way too small in any figure in the Manuscript.

The introduction of wet conditions seems to be performed as a separate step, is it true?

This is relevant, and really hard to understand. Why is the modification only considered "at the basis" of the terrain slice? This would account for a modified shear stress at the bottom of the slice, but where is the contribution of water weigth along the whole slice? Is this approach rigorous, or is it an approximation? The curves in Fig. 3 are described to be different because of the "different data introduced": what kind of data did the Authors change? Introducing pore pressure effect is not "different data", it is a different physical mechanism, thus a different model model. The statement "after the outcome here, it can be stated that the rainwater infiltration factor is necessary to predict instabilities of the slope" contains two logical mistakes, in my opinion. Firstly, for a "prediction" to be performed, one needs to have spatial and time dependence, or at least specifiy what is it that one is trying to predict, which is not the case here. Secondly, to establish that infiltration is a "necessary" factor, it is not enough to show that results with and without inclusion of the pore pressure correction are different: one must show that the case with inclusion is closer to reality than the other case!

Lines 247-249: I do not understand the sentence "if this infiltration factor is small enough, taking into account the safety coefficients, the design may still be adequate, but there was a lack of critical information for calculating this parameter" is not only difficult to understand, it also poses severe doubts on how is it possible to develop/test a model in which rainfall infiltration is supposed to be one of the key ingredients, and then the test case is taken as one in which not enough data exist to apply the model itself??

Line 300: "dimensions" shold be "sizes", or something of the like.

Information in lines 320 to 325 seems to be trivial enough not to be highlighted with a bulleted list. Moreover, the statement ".. after the event, accodring to the histogram" is rather misterious, since I can't find any event in Fig. 7.

In Section 4.2, Figure 7: when did the landslide considered in the paper occur, in the timeline? This is relevant information, is it not? The Authors refer to "level 2 and level

3": what are the levels the Authors refer to? They also refer to "infiltration calculations", when and how did they perform the mentioned calculations? This is probably described in Section 4.3, but this comes out of the blue and I do not understand how the calculations were done, and why they were not described in the methodological Section, instead of the "input data" Section.

Section 4.3 is devoted to describe the results obtained using the proposed model with a real landslide scenario. This section is very confusing, again. First of all, the Authors compare theirs results with the results obtained from a different model/program; so far, so good - even if this should have been mentioned briefly in the Introduction and/or methology sections, since it is part of the reseach method. Then the refer to "previous calculations", about which the reader is not aware, and they discuss curves that are non existing in Figure 9 (yellow and red curves?). They pretend that the "curves are similar", without any attempt to quantify the extent to which they are similar. Of course they are similar indeed, since all of them are circles arcs, but that does not seem to me to be enough, as a comparison. The same goes for the comparison with the real landlside failure curve, which I do not understand if it was actually measured or not, or if it is measurable at all. Then, the Authors refer to measures in square meters of the "surface area that corresponded with the profile", which I do not understand. What does "correspond" mean? The software is supposed to provide a two-dimensional failure curve on a vertical plane, there is no corresponding surface area. Or, at least, I don't see what it is, particularly I do not see what is the "real situation" the Authors refer to.

In the same Section, the Authors refer to a "very stable" slope as one with an Fs much larger than unity. I believe this is a conceptual mistake. A model in which slope stability is assessed with an Fs defined as the ratio of destabilizing forces to stabilizing ones, there is no such thing as "more stable". A slope, or a DEM cell, or a slice, is unstable if Fs<1, and stable otherwise. Different degrees of stability are not defined in the model, since no attempt whatsoever exist (in this and in similar models) to quantify the sensitivity of Fs results to the large number of parameters and assumtpions utilized to obtain the result, nor to give a measure of the uncertainty. We do not know if an enormous rainfall would change Fs by a tiny bit or by a large amount, nor what is going to happen if an earthquake comes about. In other words, values of Fs different from the exact value obtained from the calculations do not have different degrees of probability, thus different degrees of stability are undefined within such a model. At least, if no further analysis is performed. Lastly, in the same Section, six points are listed, which contains methodological remarks and no results, As such, these do not belong in this Section, but to a previous one.

____________________ Technical comments:

English seems fairly good, but I am no native English speaker.

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