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Natural Hazards and Earth System Sciences
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Discussions

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.

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We maintain that the manuscript is well within the aims of NHESS. We provide in the attached supplement, the constestations and contributions to the questions raised by you.

Thanks for your comments

regard greeting

GENERAL COMEMTS.

(1) The manuscript describes a numerical approach to slope stability, and the corre-

sponding 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 publication in NHESS. I will try and motivate my opinion in three different sections, as requested by NHESS reviewing guidelines.

(2) We maintain that the manuscript is well within the aims of NHESS. Undoubtedly, several papers on rainfall thresholds and landslides induced by intense rainfall events. But our novelty lies in the development of an original code with programming in Matlab, with the ability to predict well the slip failure of the curve and the area of the surface, taking into account the rain infiltration factor ru of the Spencer method.

We will take into consideration some of the assessments that we believe will improve the document. We have used a large number of the bibliographical references provided. Changes that we mention below.

The review say: "The model is two-dimensional, and its applicability is limited to a single slope. . .. ."

We cannot agree with this sentence because in the proposed example we have taken the topographic profile of the critical analyzed slope, although we can analyze all the profiles that we want of slope and landslide that we need with our code. The coordinates of the profile have been obtained from a topographic map of the slope and we have obtained it through a raster map and a GIS application.

The advantage over other 3D models and similar, is that this proposed code deals with the ability to predict a landslide failure curve and the slope factor of safety with a terrain stability (TS) analysis. Has the ability to well-predict the landslide shape and area.

(1) I believe that the material in the manuscript is organized in a rather confusing way, and 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 infrastructure to that kind of hazard. Moreover, 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 manuscript deals with spatial and magnitude assessment of landslides; it is not clear to me whether a temporal component is included.

(2) Our code does not include the temporary component that indicates us, so we understand that we should better adjust the title to the proposed code:

(3) "Development and validation of the Terrain Stability model for assessing landslide instability during heavy rain infiltration."

(1) 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:.....

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 succinctly introduce the methods, results and conclusions obtained in the manuscript. A GIS support is mentioned, while the whole code is implemented in Matlab.

(2) We disagree, a safety factor to the stability of a slope determines a number by which it indicates if there is a probability or not of being stable, if Fs> 1, it will be stable and there will be a great probability of stability, if it is less than 1, it will be unstable and there will be a likelihood of landslide on the slope.

Regarding the indicated references, we take it to include them in the introduction of our manuscript.

The rainwater infiltration is justified by a histogram figure 7 of our manuscript. The depth of calculation of the pore pressure is not arbitrary since our code calculates the surface and the critical sliding curve, so that the height of the slices in which We divide the land mass susceptible to sliding on the slope does not It is arbitrary with our code.

We have to say that the GIS support is only used to obtain the topographic coordinates of the critical profile to study in our code; hence it is only implemented in Matlab. But the support of the GIS system is there. There are other programs that have GIS support, but they have other different characteristics and are mostly used to analyze areas of slip instability normally shallow at the territorial level, using other models such as the slope limit. In our model we have the two options of stability calculation on slope, on the one hand we have translational or shallow landslides and on the other hand we can do stability calculation for rotational and deep landslides.

The versatility of this code lies in its engineering resolution, it is not a development in basic science but it is a very useful tool in engineering resolution, which in our opinion is a perfectly legitimate field for an NHESS publication. Our code has originality in front of the indicated commercial software, besides not being of payment, to raise a model of calculation with restrictions that the user imposes, by means of the Fmincon function of Matlab, in addition to the incorporation of a pore pressure factor by means of the application of the ru factor of the Spencer method. It is proposed in light of having raised other reviews, a more explicit explanation and development of the problem of this natural hazard and existing software's.

(1) 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 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 length 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.

(2) About the comments made in this section, in general we cannot agree with these

comments, by following:

The review, recommending the rejection of the manuscript, gives us to think that the reviewer has not understood the objective and the code developed, and explained within the manuscript. Probably by our fault, bus we think that all the changes conducted has improved the document a lot.

The reviewer criticized our local code and compare it with regional codes such as TRIGRS, SINMAP, SHALSTAB (based on GIS) among others, which are totally different codes, with totally different objectives and based on totally different fundamental ideas.

In any case, in the revision of the paper we intend to introduce in the introduction section at the request of the other reviewers the reference to these stability programs at territorial level. This software's are based on raster maps, with a resolution limited by their pixel quality, and with probabilistic calculations established by a previous slope catalogue. However, in our case, we analyze the field extracted data for a determined slope. Also our code allows us to use the number of slices that the user wants, on the contrary, the reviewer state that this is an aleatory thing.

This is a good feature allowing the user to adapt their calculations to their necessities in terms of precision and computational time. In addition, he/she suggested that the Spencer resolution is in some way random, and we believe that this does not deserve any additional comment from our side. The code that we have developed can be used in civil engineering to study the slope stability, with the capacity to predict the superficial landslides and deeper ones, with the addition of water infiltration, as we have been used in the UME (Military Emergencies Unit).

Also the reviewer asks for the MDE mode, while we only used it to extract the 2D topographical profile. These data, as referenced in the manuscript, can be obtained from the Geographical National Institute, You can download it on your website:

http://centrodedescargas.cnig.es/CentroDescargas/index.jsp

He also ask for the hydrological data which has been described in the manuscript and in the referenced bibliography in a previous paper published by the authors. The geotechnical and lithological data of the analyzed slope, say that they are random and difficult to find; this is not true, in our case study and analysis of our code, if geotechnical tests have been performed (table 1, table 2 and table 3 of the manuscript) to be able to demonstrate the calibration of our code, which, by the way, the critical curve coincides the reality of the real landslide occurred, is shown in the profile analyzed (figure 6 of the manuscript), a fact that could not be achieved with the stability programs probabilistic that you recommend.

In any case, the data that you say is difficult to obtain, it is not true, because you can obtain them from the Mining Geological Institute in this case from Spain. I attach the email address: http://www.igme.es/

In this page we have in the download area of raster maps where lithology by delimited leaves appears; that is, we can obtain the topographic profile and in the case of not having geotechnical tests, we can extract them from these raster maps that delimit the lithology. Once the lithology is delimited, we can obtain its geotechnical characteristics in existing tables in special geological engineering bibliography, among others:

González de Vallejo, L., Ferrer, M., Ortuno, L., and Oteo, C. (2002). Geological Engineering. Madrid: Prentice Hall.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-192/nhess-2018-192-AC2-supplement.pdf

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