Comments by Editor:
But the Editor will have the final decision on that.

Dear Authors,

You - as the contact author - are requested to individually respond to all referee comments (RCs) by posting final author comments on behalf of all co-authors no later than 13 Jan 2019 (final response phase) at: https://editor.copernicus.org/nhess-2018-192/final-response.

Comments by Anonymous Referee #2 (nhess-2018-192-RC2)

[Answers in blue]

GENERAL COMEMTS.

(1) 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 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 r_u 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.

SPECIFIC COMMENTS

(1) 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....

(2) In the abstract say:

"The geological stability of slopes is affected by several factors, such as climate, earthquakes, lithology and rock structures, among others.........."

That is to say in the abstract we talk about all the factors that affect the stability of the slopes:

Lithology, earthquakes, types of rocks and also the climate and within the climate we particularize it to the rainfall and in particular to the water infiltration.
We go from more to less, in the proposed code or algorithm we develop a slope optimizer with the possibility of integrating it with a hydrological factor r_u.

This coefficient was obtained with the following expression:

$$r_u = \frac{u}{\gamma h}$$

The parameter u is the interstitial pressure at the base of the segment calculation; assuming a homogeneous distribution of the pressure (as other authors suggested such as Spencer, Bishop and Morgenstern).

This model is especially useful for predicting the risk of landslides in scenarios of heavy unpredictable rainfall. A hydrological steady-state assumption was incorporated into this approach. The model, called Terrain Stability (TS), was developed and programmed in MATLAB.

We do not understand the lack of consideration of our investigation in view of the indicated. But when we are in a magazine that encourages scientific debate and the participation of researchers, com has been positive in this case due to the large number of entries to this manuscript.

Then it includes a series of references that we will include if the editor estimates us the publication of this manuscript, these references will be included in the introduction as existing models in a later revision; but that has nothing to do with our code, are probabilistic models and territorial level for the study of landslide hazard maps. Our code exhaustively studies the critical curve of landslide, something that the others that it indicates do not do to us.

(1) 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 ... scenarios of heavy unpredictable rainfall" is very bold. How can account for rainfall into a mathematical model, if the rainfall is unknown?!?

(2) We have already answered this same circumstance, which was already revealed in the General Comments.

(1) 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 than 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.

(2) We accept the suggestion and add new more current references, as we will show below in the section (3).

We also decided, as other reviewers have indicated, to use only one denomination to define the safety factor. In this case we have chosen the denomination Fs, which is the one that uses the code as a safety factor; we will eliminate the FOS denomination from the manuscript.

(3) "*Limit equilibrium types of analyses for assessing the stability of earth slopes have been in use in geotechnical engineering for las year. Currently, the vast majority of stability analyses* ***using this***

***method of equilibrium limit** are performed with commercial software like SLIDE V5, SLOPE/W, Phase2, GEO-Slope, GALENA, GSTABL7, GEO5 and GeoStudio, among others [Mousavi, 2017; Acharya et al., 2016a; Acharya et al., 2016b; Jiao et al., 2013; Gonzalez de Vallejo et al., 2002). Other models of slope stability based on the theory of limit equilibrium are still being studied, as is the case of the SSAP model (Borselli, 2016), but in this case a General equilibrium method model is applies."*

(1) 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 impossible 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.

(2) We have made a change at the proposal of the reviewer 3 and reviewer 2, and in view of its indication.

(3) **Enter on line 155:**

"When solving the normal and parallel forces at the base of the slice of the five acting forces, we obtain (Q), resulting from the forces between slices:

$$Q = \frac{\frac{c'b}{F}\sec\alpha + \frac{\tan\phi'}{F}(W\cos\alpha - ub\sec\alpha) - W\sin\alpha}{\cos(\alpha-\theta)[1 + \frac{\tan\phi'}{F}\tan(\alpha-\theta)]}$$

In this expression, u is the pore pressure (permanent interstitial pressure) at the base of the slice and the weight of the slice is determined by W. If we *assume that the soil is uniform and its density (γ) also, the weight of a slice of height h and width b can be written:*

$$W = \gamma b h \qquad ”$$

Enter on line 164-172:

The factor r_u is a coefficient of pore pressure (interstitial pressure coefficient), which determines the rain infiltration factor on the slopes. As it is well known, the water that infiltrates the soil may produce a modification of the pore pressure, affecting its resistant capacity. This factor may vary from 0 (dry conditions) to 0.5 (saturated conditions). In the article of Spencer (Spencer, 1967), assuming a homogeneous pore-pressure distribution as proposed by Bishop and Morgenstern (1960), the mean pore-pressure on the base of the slice can be written like the equation 7.”

(1) Line 120, "precise" should be "accurate";

(2) we accept change

(1) Lines 125-126, "lower than 1, or stable if it is higher than 1" miss the (mathematical) possibility that Fs=1.

(2) The following change is introduced point (3), to have covered the Fs = 1.

(3) “According to equations (4) and (5), the slope FOS (FS) can be considered unstable if its value is lower than 1, or stable if it is equal o higher than 1.”

(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.

(2) As the sentence of the manuscript indicates:

“It should be noted that, when applying the factor in the engineering and architecture fields,.............”

In all the countries of the world in their technical regulations of application for civil engineering, architecture and geological engineering, include for the analysis of stability of slope the security coefficient , which include among other application regulations: The CTE (Technical code of The Spanish Building, Eurocode, Guide for Anchors and Stability of slopes of the Ministry of Development), among others........................ ......

In addition to the aforementioned reference.

References:

CTE. Technical building Code, 2007. Basic Document Structural Safety Foundation DB-SE-C. Ministry of public works of Spain.

General Directorate of Roads of Spain, 2009. Foundation guide on road works. Madrid.

General Directorate of Roads of Spain, 2001. Guide for the design and execution of ground anchors in road works. Madrid.

Eurocode Building: https://eurocodes.jrc.ec.europa.eu/

We include that clarification in point (3)

(3) *"It should be noted that, when applying the factor in the engineering and architecture fields, the limiting value tends to be higher than 1, with common values being 1.2 or even up to 1.5 [Burbano et al., 2009], security coefficients that include The European technical regulations and, specifically, the technical regulations of Spanish application (table 2.1, of the DB-C of the CTE, or Technical Code of the Building) among others."*

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

(2) As the sentence of the manuscript indicates:

No engineer or architect can think of calculating the stability of a slope, without taking into account a safety factor greater than unity, apart from the fact that it is prevented by the technical regulation of the states. A coefficient of 1.30 is not exaggerated; there are European regulations that raise it to 1.50.

In civil and geological engineering these coefficients what they do is foresee these exogenous agents, examples:

1. Construction on the side of a road, unforeseen overload ....
2. Excessive infiltration not taken into account in other hillside calculus models ....
3. Construction of a building, subject to the slope of unforeseen overloads ....
4. Modification by man of the morphology of the slope, which makes it unstable ....
5. Do not contemplate all the characteristics of the hillside or choose a bad model or calculation software ....

Among others.

(1) 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?

(2) There is nothing arbitrary in our code, basically what the user of the code does is to introduce a cut point of the slope and a center point of the breaking curve and the code with those two points traces an initial circle of breakage, (yellow line) that corresponds to the initial break initial circle. Both initial data, are a common values to be introduced into a slope stability code.

The user also determines the number N of slices in which he wants to divide that slippery soil mass and the code programmed in matlab, by means of the Fsolve function he automatically calculates the Fs (safety factor) by the Spencer method of that initial curve.

The code does a non-linear calculation of the equations of the Spencer method. The originality of this code is that it automatically searches from that initial curve, the curve and the critical center with the function of Matlab Fmicon, with the restrictions imposed by the user and that come in the manuscript.

The user introduces the infiltration r_u factor defined in the Spencer method, if $r_u = 0$, there is no infiltration and if we define a $r_u > 0$, we are introducing infiltration in the slope.

With these parameters entered in the code, this automatically calculates the safety factor of the slope and draws me the critical sliding curve of that topographic profile introduced. What we have shown in the case study, with the geotechnical tests carried out in situ, that the code works.

(1) Section 3 tries to describe in somewhat more detail the operation of the software, but this is also done in a confusing 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.

(2) We believe that this section is largely answered with the previous ones. As for our model to define a circular form of sliding, we understand it is not a limitation as you say; because depending where this is the center of the critical circle of the slip curve, the shape of the surface will vary, our code being able to simulate both rotational and translational landslides.

With this methodology and the circular form you can simulate almost all existing forms of landslide. The SSAP model that comments, https://www.ssap.eu., Does not consider the hydrological factor of infiltration r_u and the resolution of the equations by the Spencer method as our code.
It is different in its operation to ours and also only allows circular shapes; but as we have already mentioned, the breaking shapes of the landslide depends on where the center of the critical circle is.

When the center of the critical circle is further away from the slope, the shape of the break will be more like a translational landslide and if the center of the critical circle is closer to the analyzed slope, we will be in a deep or rotational landslide.

(1) 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 atthe 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 specify 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??

(2) The operation of the proposed code has been explained in the previous section. The introduction of wet conditions as we have already mentioned is introduced with the r_u factor, it is not true that it is a separate step; we in the example have made the comparison of introduction or not of the pore pressure, but it can be done directly. Precisely the originality of the code is that which is a factor integrated into the software itself. It is a rigorous approach and not approximate.

In case the analyzed slope is partially saturated as you indicate, the weight of the water in the slope is considered in the code when entering the saturated density of the soil; instead of entering the dry density.

The difference of figure 3 with respect to the calculation of figure 2, is that a value of $r_u = 0.3$ has been introduced in the calculation, in figure 2 the infiltration of rainwater is not taken into account and in the Figure 3 the infiltration is considered. That is why there is a change in the safety factor and the shape of the critical curve. In figure 2 we have a stable slope Fs = 1.45 and in figure 3 we have an unstable slope when introducing the infiltration of rainwater into the slope (Fs = 0.95), in this comparison the interest of this is shown code in the stability analysis of slopes and landslides.

We do not agree that to predict must incorporate the time variable, since what is essential for landslide occurrence related to water infiltration, depends not so much on time, but on rainfall and also the lithological characteristics of the soil of the slope, as it can be among others the coefficient of permeability (K).

It is not true that the infiltration factor has not been introduced in the case study, a value of $r_u = 0.35$ has been introduced in the calculation and the code gave us a value of the slope safety factor of Fs = 0,95 (unstable), when in the dry state the code calculated a safety factor of Fs = 2,300 (stable).
The calculation of the safety factor in the STB2010 program; that lacks the analysis of infiltration in the calculation, offered a result of Fs = 2.063 (stable).
Using the STB2010 program, we would not have been able to previously detect the landslide of the case study of the manuscript, calculation that is not normally done in the stability calculations, with the calculation with our code we could have avoided the collapse of the building.

(1) 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" isvrather 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.

(2) The date of the landslide is shown in figure 7, it is precisely indicated in the histogram the month of February and March of 2010 in the plot, date of the landslide, after the accumulation of so many consecutive days of rainfall.

The levels come in table 1 of the manuscript, as a consequence of the geotechnical tests made by Geolen S.L.

A topographic profile of the slippery soil was made, using MDE and the results of the geotechnical tests of the Geolen laboratory (figure 6) and then compared with the curve shape calculated in our code (figure 10), giving a satisfactory result.

We refer to what has already been explained and clarified in the previous sections, for example when we talk about the safety coefficients, the higher the safety coefficient the more stable the slope.
As for the rain, we have already spoken in previous sections of the operation of the code and the possible uncertainties. We understand that this type of code is again confused with other stability programs based on probability.
As for the points of the section indicated, also by recommendation of other reviewers we will move to section 2 where appropriate.