Referee #2

Authors would like to thank the referee for the stimulating comments and helpful suggestions that we hope will contribute to improve the paper in the revised version. In the following, authors discuss point to point the numerous remarks the referee raised and indicate the modification they have planned to implement in the paper.

Referee: The paper is mainly focused on the definition of basic features characterizing the predictive model. Then, three alternative models which could be implemented in the predictive tool are described by means of three case studies at a slope scale.

Authors: *Aim of the paper is to define the features of the predictive model of a water-related natural hazard within a general frame of its evolution stages, so to allow the identification of the most useful stage where the evaluation of the safety conditions should be carried out (made by means of what we call the predictive model), according to the characteristics of the considered phenomenon (mainly its temporal evolution, which is often related to its scale), as well as to the features of the adopted predictive model (empirical/physically-based, on-line/offline etc...). Aim of this framing is the setting up of an effective EWS. The focus of the given examples is not on the scale of the studied phenomena, but on the different modeling approaches that can be followed to predict the future evolution of a phenomenon, even with reference to similar phenomena (landslides triggered by rainfall infiltration into shallow granular covers along a slope). We understand that this message was not clear, and so we will modify the last part of the introduction in order to make it more clear.*

Referee: The manuscript is difficult to follow and awkward in many parts (the abstract is an example). The text would benefit from some language copy editing.

Authors: *A complete check of the entire manuscript will be made to improve the quality and clarity of the language.*

Referee: According to the title the main issue of the paper should be the description of basic features of the predictive tools employed in EWS for waterrelated. However, only examples of shallow landslides have been provided. Thus, I would suggest to modify the title according to these considerations.

Authors: *We beg to disagree with Referee #2: the title clearly states that the examples refer to shallow landslides, but the features of the predictive tool, as well as the framing of the evolution stages of a natural hazardous phenomenon, may be generalized to any water-related early warning system. This is confirmed by the Introduction section, in which the architecture of an EWS, the evolution of an hazardous natural phenomenon, the features of the predictive model are generally defined. Indeed, the core of the paper lies not in the Literature examples, but in the general concepts given in sections 2 and 3, in which original classification criteria are proposed, which may be useful for the design and setting up of Early Warning Systems. Therefore, we believe that the title should not be changed, to attract potential readers interested in water-related early warning problems.*

Referee: It is well known the different scales of analysis at which landslide early warning systems (LEWS) may be employed. I think could be useful to define in the Introduction, at which scale the Authors are referring to.

Authors: *The possibility of considering different scales is common to any EWS, not only for landslides. In the Introduction, indeed, it is discussed how the duration of the evolution stages of the phenomenon is often related to the scale of the considered system (e.g. page 2, lines 20-22), and nay affect the requirements of the predictive model (e.g. page 4, lines 2-4). So, on purpose the Introduction is general, without referring to a particular scale (not even to a particular kind of phenomenon, by the way), as the proposed classification criteria may be applied at all scales. However, all the given examples refer to slope scale, and we understand that this has resulted misleading. So, we will modify the relevant parts of the Introduction to improve the clarity.*

Referee: As stated at page 10 line 31 the examples are all referred to a slope scale. Does the paper refer exclusively to LEWSs at a slope scale? Many differences can be observed among LEWSs employed at a local or regional scale.

Authors: *As already pointed out in the answer to the previous comment, the choice of the examples all referred to slope scale resulted misleading. Besides modifying the Introduction, we will also add a paragraph at the beginning of section 5 to better clarify what is meant with the choice of the examples. Furthermore, we would like to underline here that, although increasing model complexity is usually adopted moving from regional scale to slope scale, the scale does not intrinsically relate to model features but, rather, to the spatial resolution of the available input data, which affects the entire structure of the EWS. In other words, any kind of model could be in principle adopted at any scale. In example, the well known I-D model is very simple and thus suitable to be applied at large scales. However, several examples exist of I-D thresholds derived for a specific slope, of known geomorphology and soil properties, on the basis of rainfall records collected near the considered slope. On the other hand, 3-D modelling of Richards equations is usually adopted at a slope scale, due to the huge amount of information (geometrical, geological, hydraulic) required to fully exploit its potential. However, given the computational power nowadays available at low cost, if a detailed GIS and a dense network of sensors cover a wide area, the 3-D modeling application may be extended to larger scales. So, as the scale is related with the features of the entire early warning system, and not directly and only with the model, we chose not to adopt scale as a classification element of the predictive model.*

Referee: Concerning the structure of the paper, Section 2 appears to be not coherent with the aim and the content of the paper. I would suggest to include the text concerning the uncertainties of prediction into Section 3 and to delete the remaining text, because it doesn't provide any evidences on costs connected to false and missed alerts nor how to minimize them.

Authors: *Although operational EWS in many cases do not account for the uncertainty of the predictions, it is our strong aim to underline that this is sometimes one of the major limitations to their effectiveness. We believe that cost-benefit analysis is crucial for all actions aimed at risk mitigation, and should represent the leading approach to all the decisions of the authorities in charge, for structural mitigation actions as well as for non-structural ones. In particular, the cost-benefit analysis is at the base of the correct setting up of an EWS, so we disagree about the deletion. In the revised manuscript, we will rather consider adding some text to better explain how the cost-benefit analysis should guide the choice of the sensitivity of the EWS.*

Referee: Section 3 introduces three evolution stages for a natural hazards. Is this an original proposal or it's derived by literature? Please provide some references.

Authors: *To the best of our knowledge, such a framing of the evolution stages of a natural hazard is our original proposal, an attempt to make order on how existing or future EWS should be set up. Two further*

classification criteria are also adopted throughout the paper: the well-known one, distinguishing between empirical and physically-based modeling approaches; the not novel but (to our knowledge) never adopted in the field of natural hazards, which distinguishes between on-line and off-line predictions. These original concepts make us believe that the paper is worth to be submitted as a research paper. As suggested by the Referee in other comments, we will better clarify the originality and usefulness of this contribution in the revised Conclusions.

Referee: Then the content of this section is not coherent with the title. In particular the evolution stages of different natural hazards (seismic, snow-avalanche, overflow, rainfall-induced landslide) are presented. I think it's not necessary to mention them all, since the title and the abstract exclusively refer to water-related hazards.

Authors: *Although the field of application that we have in mind is that of water-related natural hazards (as confirmed by the examples we give), we believe that the framing we propose is suitable also to other kinds of natural hazards. Furthermore, the readership of NHESS is not interested only in water-related hazards, so we believe that this general multi-hazard perspective adds some extra value to the paper and should notbe considered as an out-of-theme issue.*

Referee: Section 4 should be the core of the paper,

Authors: *The core of the paper is represented by the two sections introducing classification criteria, namely sections 3 and 4.*

Referee: I suggest to clear describe what the authors consider as a predictive tool and where in the structure of a EWS it fits

Authors: *The predictive tool is defined in the Introduction, at page 3, lines 27-29, as the "interpretative model". This may be merely an empirical equation, or the output of a more sophisticated mathematical model allowing to evaluate the safety conditions of a system. In any case, once the model has run and given its output, this consists in a variable that can be compared with a threshold, so that a warning level can be defined (page 3,lines 30-33). The entire section 3 is then dedicated to the problem of choosing the most appropriate moment of the evolution of the hazardous phenomenon, when the predictions should be carried out. Possibly the misunderstanding is due to the use of different names (e.g. "interpretative model", "predictive model", predictive tool") throughout the paper. We will try to fix this in the revised manuscript, by also adding some examples and references, either in the Introduction or in section 3 and 4, to be more clear.*

Referee: Regarding the structure of EWS and, in particular for LEWS, there are many missing references that could be considered in order to improve the discussion (see References below). The same references needed to be taken into account for the text at page 3 lines 19-33. Furthermore, in literature there are many contributions of several authors concerning tools for the issuing of warnings which have been implemented in LEWS. I suggest the Authors to consider these contributions (see References attached), in order to improve the comments in Section 5 concerning the architecture of the predictive model.

Authors: *We agree that some more references to previous studies about the structure of LEWS should be cited, and we will add several more in the revised manuscript. We thank the Referee for his/her suggestions.*

Referee: Furthermore, in literature there are many contributions of several authors concerning tools for the issuing of warnings which have been implemented in LEWS. I suggest the Authors to consider these contributions (see References attached), in order to improve the comments in Section 5 concerning the architecture of the predictive model.

Authors: *We agree that some more references to previous studies about the models adopted in LEWS should be cited (see also the answer to the previous comment), and we will add some in the revised manuscript.*

Referee: Furthermore they can be used as examples to support the different classification criteria defined.

Authors: *We totally agree.*

Referee: In Section 5, performance evaluations (I mean the same analysis already performed forthe stochastic approach) of the empirical approach and of the physically-based model should be carried out to better express judgments and to compare their results.

Authors: *If we correctly understand what the Referee means, he/she is talking about the account for missing and false alarms which, for the case of the stochastic approach, is presented in form of tables, as in that case the sensitivity of the EWS can be regulated by choosing the probability of exceedance of a threshold at which the relevant warning messages should be issued. In the case of deterministic approach (as for the other two examples), the only way to obtain a more or less sensitive EWS would be changing the threshold values at which the warnings are issued. However, the two deterministic examples were not meant to show the sensitivity of the EWS, but the very different modeling approaches that can be implemented. Anyway, also for the two deterministic the performance of the hypothetical EWS is evaluated in terms of false alarms (in both cases the adopted thresholds would have not resulted in any missing alarm). We will rewrite the relevant parts in order to make it more clear.*

Referee: The conclusion ends rather abruptly, the authors do not clearly present their conclusions and text is too short. A big-picture summary statement or some comments on the importance and usefulness of defining basic features of the predictive tools used in EWS would be a necessary concluding remark.

Authors: *We thank the Referee for his/her suggestion, and we will modify the Conclusions to better highlight the contribution of the proposed model classifications and hazardous phenomenon framing for the implementation of effective EWS.*

Referee: In addition, the sentence of page 17 lines 22-24, is not supported by data and, the sentence of page 17 lines 27-30 is a repetition of the abstract and it is not appropriate for the conclusion.

Authors: *We understand that the given examples, all about shallow landslides, do not clearly show the conclusion of lines 22-24. Anyhow, we still believe that this is a message that should be given by this kind of paper, so we will try to modify the description of the examples to shed some light on this aspect. We agree that the last paragraph of the Conclusions is redundant and should be replaced with some remark about the usefulness of the proposed model classifications and hazardous phenomenon framing for the implementation of effective EWS (see the answer to two of the previous comments).*

Referee: Specific comments

Authors: *We thank the Referee for his/her suggestions about typos, unclear sentences, missing references and so on. We will try to fix them all in the revised manuscript.*