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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "A decision support system (DSS) for critical landslides and rockfalls and its application to some cases in the Western Italian Alps" *by* Davide Bertolo

Anonymous Referee #2

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The Manuscript deals with the issue of early warning systems for active slow moving landslides. A method is proposed for a "decision support system", claimed to be objective and reliable, which was applied to "some" (three) cases in the Italian Alps. I believe that the Manuscript is written in a rather confuse way, and that the above claims are not supported by the actual content of the text. Morevoer, it occasionally contains questionable statements.

Overall review of the Manuscript:

The Abstract of a scientific paper is supposed to concisely describe the framework, purpose, method, results and conclusions; the proposed Abstract hardly contains such

information. Then, from the beginning of the main text up to section 1.2, the Manuscript is written in a colloquial fashion and it does not introduce the general framework of the problem to put the proposed method into proper context. The description of the "decision support system" is given in a qualitative way, without motivating the adopted numerical values or, worse, by explicitly mentioning that values are "reasonable", without providing any insight into the consequences of using those particular values, or uncertainty, or any form of validation. At least these are not explained. The two following sections are supposedly devoted to the application of the same method in two different sites. It is not clear to me what is it that the Author learned from the first case, let alone that the information provided in the two additional sections does not provide any more insight. Thus, how can the Author state, in the first few lines of the Conclusions section, that the method was "successfully applied"? Where are evidences supporting that claim? The remaining of the Conclusions section fails to report actual "conclusions" of the effort made by the Author. What do we learn from reading the Manuscript? Why does the Author suggests not "installing useless and redundant instrumentation", where is evidence supporting that statement? The reason why would he like to revise the commonly accepted definition of landslides is also obscure to me. Eventually, the language throughout the text is sloppy; I am not an English native speaker myself, but I could spot several mistakes and, occasionally, repeated words and even a three-lines sentence (end of page 6) denoting careless writing.

Additional comments:

There are many expressions used in a confusing way. For example, the Author refers to "numerical methods" for landslide monitoring: "numerical methods" are usually devoted to computer-based problem solving, typically optimization or differential equation solving, which is not the case here. There are whole sentences that have little to do with the content of the paper (i.e. lines 29-38, page 2, about "human behaviour" and its supposedly relevant role in decisional processes), or the many references to "clinical practice", whose only overlap with the content of the Manuscript is the use of Bayes theorem. There are a few confusing sentences bringing conceptually different issues to the same level, for example: i) in line 39, the Author refers to "monitoring" and "bias" on the same footing, while monitoring does not contain biases per se, being a quantitative measure; it may have different degrees of approximations or confidence, but surely it does not represent a bias; ii) in line 6, page 3, the sentence "Such evidences, which are the so-called expert judgements" contains the same kind of flaw: an "evidence" cannot be a "judgement". Similar and even worse problems are represented by the use of the word "objective" throughout the Manuscript. Objectively defining values for parameters in a model, such as the various thresholds and percentages values used here, is not equivalent to select them arbitrarily. Of course one can follow a trial and error procedure, but in that case one would also have to make sure if initail (the trial part) values were properly selected, or should they be modified (the error part). It seems that the decisions system's parameters were rather arbitrarily selected; if not, the Author failed to describe the method properly, in my opinion. And, by the way, "parameters" are contstant values in a mathematical model, which are used to represent the relationship between the "variables" of the model itself; parameters can be tuned, not measured (at variance with line 5, page 10). Variables are the measurable quantities, and bear physical meaning.

The use of Bayes theorem is actually my major concern, here. Bayesian inference is a statistical method, i.e. it is meant to estimate a probability density function, based on the concepts of prior and conditional probabilities, and on the idea that posterior probabilty can be updated whenever new evidence comes about. It is understood that new evidence must concern the SAME variables used in previous estimates. Otherwise, we start describing a different phenomenon, or a different model. It seems to me that in the process described by the Author each different step (cf. Fig. 5) brings about NEW variables, and no probability is defined for the overall set of variables to simultaneously assume different values (conditional probability), and no prior estimate of such variables to assume those values was defined (prior probability). If so, I do not see how can one claim to enforce Bayes theorem, or I am missing something fun-

damental here. This add to the issue of how the numerical values of the thresholds were selected, discussed above. Moreover, in line 32, page 7, what does "Applying the corrections required .. [1] becomes" mean? Correction required by whom? How is Eq. (2) implied by Eq. (1)?

In line 11, page 8, the Author refers to susceptivity (usually referred to as susceptilibity) as the ratio of unstable area to total area under investigation, which is a bit of a semplification, given the body of literature existing on the topic of landslide susceptibility assessment... Also, the Author estimated such ratio to be 15% and mentioned that the value is consistent with the values found by (Malamud et al. 2004). In the mentioned paper, three different values are obtained in three different areas of the world, namely 0.24%, 0.3% and 0.6%, none of them is any near to the 15% calculated here. In my opinion, there is no reason why such numbers should be comparable, but still the claim seems to be wrong, if I am not mistaking. There are other examples in which the Author refers to literature in a sloppy way. For example, he mentions Voight model and inverse velocity method without spending a line explaining what they are; in Section 1.2.1 it is mentioned that "the inverse velocity method is embedded in a Bayesian DSS", without explaining what it even means. In the following line, he claims that (Manconi and Giordan 2015) stated the Se=75% "can be considered as a high reliable value" while, in the paper he refeers to, there is no reference whatsoever to Bayes theorem or to Se. In their paper, 75% is a "measure of reliability" given by a "normalized Pearson's coefficient between model and data": what is the relationship with Se, if any?

In conclusion, I believe that the proposed Manuscript is not suitable for publication in NHESS, both for the organization of the paper, the method used, and the conclusions (or lack thereof) drawn.

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