

Response to reviews and comments on the NHESS-manuscript

Dear editor and reviewers,

We sincerely thank you for the supportive comments and reviews related to our manuscript "Monitoring and prediction in Early Warning Systems (EWS) for rapid mass movements" (nhess-2014-281).

We are pleased to respond to the different comments and suggestions brought up by the reviewers and have implemented the corresponding answers into the manuscript.

Response to Jeff Coe, USGS (referee 1):

Major point 1: p. 7151, lines 19 and 20. Please define the types of rapid mass movements that are covered by the EWS described in this paper....Part of this definition should probably include a quantitative or qualitative definition of "rapid"

Our focus here is on landslides, debris flows and snow avalanches. However, we also add a few examples related to rockfall because many of the discussed observation techniques, deficits, and general statements also apply to rockfall.

-> A corresponding definition has been added to the Introduction, third last sentence: "Here in this paper we focus on landslides, debris flows and snow avalanches with speeds on the order of meters per second. The discussed items are general and apply also to other types of RMM, such as rockfall and ice avalanches."

Major point 2: p. 7155, lines 17-20

-> we have added a sentence about the role of the people maintaining EWS into the abstract, as proposed by referee 1.

Major point 3: p. 7156, line 2, it would be helpful to the reader if you defined the difference in the way that you use "alerts" and "alarms"...

The correct use of the terms "alerts", "alarms" and "warnings" is very relevant for this manuscript.

-> We have carefully gone through the manuscript and checked these terms for correctness (in accordance with the definitions of referee 1).

Major point 4: p. 7156, lines 14-29; p. 7157, all lines; p. 7158, lines 1 and 2. I suggest numbering each of these shortcomings/limitations so they can be easily referred to later in the text. Also, I suggest adding another shortcoming/limitation, that is, many current EWS don't include estimates of how mobile the predicted landslides will be (i.e., how fast and how far will the landslides travel?).

We are grateful for these suggestions of referee 1 and we have implemented them as described below:

a) -> we have numbered the shortcomings/limitations of EWS (p. 7156-58); b) -> we have added the shortcoming that current EWS rarely include estimates of RMM mobility. This shortcoming is related to insufficient application of international standards because assessment of RMM mobility is typically covered in the risk assessment component of EWS (see UNEP, 2012); c) -> we have added that statement about the missing precursors, or time lag between precursor and RMM

Major point 5: p. 7158, Section 4. I think the individual sub sections within section 4 should be linked back to the shortcomings/limitations that were just listed on the previous pages...

The structure of section 4 (and further) and the missing explicit link back to the shortcomings/limitations discussed in section 3 are also a concern of referee 2 (see his comment 1).

-> We have restructured section 4 by introducing sub-sections that explicitly link to each of the deficits of current EWS discussed in section 3. (A lot of the content of the new subsections 4.1 to 4.6 has been taken from the previous text of section 4 – complemented with some new text.)

Major point 6: p. 7160, lines 22-24, This sentence reminds me about a fundamental issue (lack of knowledge) that causes problems when running most models for regional forecasts of shallow landslides and debris flows. That is, an accurate determination of initial soil conditions (soil depth, soil moisture, pore pressure, etc.) within the region. This strikes me as something that could be added to the list of shortcomings/limitations of EWS, maybe within the precursor shortcoming (number 4)? Somewhere in the manuscript it would also be worthwhile mentioning the need for a remote sensing system capable "sensing" soil depth and moisture.

We agree.

-> The sentence in the previous version "The hydrology and saturation state at the catchment and regional scale must be represented appropriately in order to model the triggering of RMM." was replaced by an explicit statement (including references) about possibility and necessity to measure soil water content at larger scales by remote sensing or networks of low-cost water content sensor networks. In the case study in outlook section 6 we referred as well to the possibility to estimate soil depth distribution from remote sensing data.

Major point 7: p. 7164, lines 2-9, you could mention the potential usefulness of continued development of portable ground-based radar systems for determining local rainfall in shadow areas.

Good point.

-> We included the reference Jorgensen et al., 2011 in section 4.1.

All minor suggestions of referee 1 were implemented as suggested.

We hope that the amendments proposed above are to your satisfaction and look forward to finalizing the manuscript for publication in NHES.

Best wishes,

Manfred Stähli and co-authors