

Interactive comment on “Performance evaluation of the national Norwegian early warning system for weather induced landslides” by Luca Piciullo et al.

Anonymous Referee #2

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This manuscript assesses the performance of a national early warning system for regional landslide occurrence that was established recently in Norway. To this end, a performance-evaluation method EDuMaP (originally developed in Italy) was adapted to the case of Norway where spatial warning units are not constant but variable in space from case to case. While overall the landslide early warning system (LEWS) seems to perform quite well, this study also revealed that such a performance analysis strongly depends on the criterion selection.

Assessing the performance of such a country-wide LEWS is of great interest to NHESD readers as such warning systems are still new and not well-established yet. The manuscript provides a good description of the warning system and shows an inter-

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esting approach how it can be evaluated in a systematic manner. In that sense, I see a substantial potential for publication in this journal. On the other hand, I have a number of major questions and comments that, I think, deserve some further work:

1) The analyzed data set (both the number of warnings and observed landslides) is limited. It includes only warnings of three warning classes (green, yellow and orange) and a relatively low number of landslide observations (in particular for case B) without any landslide event classified as “large” (line 388). So one of my main question is: why was this analysis restricted to Vestlandet only and not performed for whole Norway? And why does it only include data from two years? I’m afraid that with this limitation (in particular with the missing of red warnings) the performance analysis is not comprehensive enough to draw strong conclusions.

2) Coming from another research field than “performance analysis” I had substantial difficulties to understand the extended EDuMaP-method (section 3.3). In particular, I was missing the “rationale” behind this method. In simple words: What’s the rationale behind the assumption that an issued warning was successful or less successful. For example, is it more important that the location of an issued warning is correct than its intensity? Or is it most important that an warning is issued for day 1 even if the location and intensity is somewhat over- or underestimated? I suggest that the authors very clearly explain their rationale behind their technical assumptions.

3) I’m missing a benchmark for this performance evaluation. Is this landslide early warning system successful or not in comparison with other early warning systems worldwide? On lines 66 to 70 the authors mention a number of other such early warning systems – some of them are regional, others are local – and, in addition, there are also many flood early warning systems worldwide. I’m sure some of them have been evaluated in a similar way than this one. For the reader, it would be important to know (as a conclusion from this work) how the performance of this EWS compares with others.

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4) Fig. 9b seems to omit the category “no warning issued – no event observed” while Fig. 9a seems to include this category (True Negatives). Is this mentioned somewhere? On what basis did you do this? As a result, the green category (in Fig. 9b) seems to be underrepresented. Yellow seems to dominate (but this is only for cases with either a warning or an observed landslide.) I think this gives different messages if you include or exclude the category “no warning issued – no event observed”. From Fig. 9b the authors conclude that for Case B the EWS performs slightly better than for Case A. I would say the difference is very small . . . and I wouldn’t over-interpret Fig. 9.

5) That brings me to another issue: is it really necessary (and of added value) to conduct the performance analysis for the two cases? Why don’t you show only results for Case B (as you seem to distrust the data from Case A that you omit in Case B). Again – as stated above – I would suggest to extend the analysis to the entire country and to the entire period of the warning system, but exclude those landslide observations that you distrust.

6) The list of references includes many reports . . . some of them in Norwegian . . . please check which of these reports are really important for the understanding of this paper. (for example, do we really need all these references on geology and landforms?). On the other hand, I’m missing references to other authors (than Calvello and Piciullo) on performance evaluation of warning systems. There must be some of them!

Minor comments:

The abstract is not well balanced between introduction (background) and results (conclusions). There is too much background and introduction about the EWS. I suggest to shorten that substantially.

Line 47: “which are increasing with climate change”; I would say: “which are expected to increase with cc”

On line 75 the authors mention for the first time the fact that the Norwegian EWS issues

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“variable” warning zones. It is very important that the authors clarify what they mean with “variable”. I suggest to write “warning zones with a variable extent (or: area)”.

Line 110: “In contrary” should be “On the other hand,”

Line 216: “are observed described”; either observed or described The authors use the term “precipitation episodes” several times in the text. I’m not sure “episodes” is the correct term here. I would rather suggest “events”.

Line 254: “are shown” (not “are showed”)

Table 3 is not necessary because all this information is given in the text already.

Line 331: “the some” should be “the same”

Line 335: “in Day 1” should be “on Day 1”

Line 335: “appears” should be “appear”

Tables 5 and 6 are not necessary because all this information is given in the text already

Line 427: “Tab.2” should be “Tab. 4”

I’m not sure all of the Figures are really needed. Please carefully reconsider which of Figs. 1 to 6 (on the EWS and its application) are really needed.

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