Nat. Hazards Earth Syst. Sci. Discuss., 3, C512–C517, 2015
www.nat-hazards-earth-syst-sci-discuss.net/3/C512/2015/

Natural Hazards and Earth System Sciences
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Discussions

Interactive comment on "Landslide early warning based on failure forecast models: the example of Mt. de La Saxe rockslide, northern Italy" *by* A. Manconi and D. Giordan

Anonymous Referee #2

Received and published: 26 April 2015

GENERAL COMMENTS:

The paper deals with a major problem in landslide risk management, i.e. the definition of the landslide time of failure in Early Warning perspective. This is a debated topic due to the gap between the availability and the real-world applicability of empirical or physically based methodologies (see for example Cloutier et al., 2015 for a discussion). Therefore, the contribution is very welcome and fully suitable for the journal NHESS.

The authors proposed a forecasting methodology built on the well-known "inverse velocity method", which is based on experimental evidence and theory of tertiary creep.

The idea is generally meaningful and has some interesting implications. Nevertheless, the authors fail to convince me regarding the overall robustness and practical applicability of their methodology in real applications. I list the most important points here, and further ones in the "detailed comments" sections.

1) I am not sure that the submitted contribution is new. The authors refer to their paper: Manconi and Giordan (2014) Landslide failure forecast in near-real-time. Geomatics, Natural Hazards and Risk, DOI:10.1080/19475705.2014.942388 for more details on the methodology, and propose the paper submitted to NHESSD as an application to the Mt. De La Saxe. Unfortunately I have no access to that publication, but the abstract of the published paper suggest that the contents of the two papers are pretty similar. Could the authors explain what's new in the submitted paper with respect to the published one?

2) The "inverse velocity method" is simple and based on a physically-consistent theory (Voight, 1988), thus it attracted the interest of many researchers and practitioners. When it works well (typically in engineered slopes in mining environments), it does not require many adds to be used as a powerful real-time early warning tool, provided that it is deployed and evaluated by expert staff. On the other hand, the applicability of this approach has several important limitations. They are related to non-steady loading conditions (changing stress state in the slope), non-constant empirical method "constants", and varying external loads (pore pressures, rainfall and snowmelt), and introduce significant biases in the forward application of the method. All these limitations also affect the method proposed here by the authors, but they do not discuss them despite paper conclusions outline substantial problems in the predictive capabilities of the method. I would suggest them to discuss this point carefully to explain the strengths and limitations of their approach. Which are the advantages to use their method with respect to the "ordinary" inverse velocity method in real conditions affected by the problems listed above?

3) the method proposed by the authors assumes that landslide evolution to collapse

corresponds to a progressively better fit of displacement data to the inverse velocity model (i.e. better correlation indicates closer failure). In fact, Figure 1 suggests that early warning thresholds can be defined based on the goodness of such fit. I have some serious concerns about the robustness of this assumption:

a) better data fit to Fukuzono's model toward failure is often observed in back analyses, but it is often not required for collapse to occur: "noisy" data can be observed even if a generally consistent 1/v trend is followed until failure. Conversely, very well-fitting trends can in turn deviate from the path to failure depending on changes in landslide behaviour or changes in boundary conditions;

b) in geotechnical engineering (also applying to slope stability), the concept of reliability is related to probabilistic analysis, and expresses how far a present state is from critical (e.g. limit equilibrium) conditions, normalized by the variability of the estimates (standard deviation). This is quite different from the concept suggested by the authors, and I am not sure that it is correct to interpret goodness-of-fit as reliability (and to use it directly to establish EW thresholds);

c) stronger data-model fit approaching failure may simply suggest that the "signal" of tertiary creep behaviour is stronger that the "noise" related to measurement errors and landslide physics. This is certainly an interesting supporting indicator for early warning, but I am not convinced that this is the major one, unless the physical nature of "noise" is not well known.

All these points seem to hamper the robustness of the basic assumptions: in fact, in the conclusions the authors suggest that the methods is not performing very well: they start aiming at the evaluation of the landslide time of failure, but eventually give up and settle on the evaluation of "critical time ranges". The latter sound not very useful, since the method was invented to manage the final stage of early warning, which is a critical time range by definition. I suggest that the authors address very carefully the points listed above to support the validity of their method.

4) the paper is intended to be a case study application of a method presented elsewhere, but eventually the application is made on a single, poorly described event in the framework of a very complex landslide which is not explained at all. I understand that the authors provided some references, but I suggest that at least the most important information on the landslide model and on the monitoring network (techniques deployed, spatial distribution and significance of measurements, frequency of measurements, reasons why the authors used TS measurements, etc.) should be provided to the reader.

5) English could be improved with the help of a native speaking colleague

DETAILED COMMENTS:

1) Page 1512, line 15: "management of the territory" may better read "landplanning and management"

2) Page 1512, line 24: "may lead to". Early warning thresholds do not lead to landslide occurrence but are indicators of likely landslide occurrence. I would suggest "indicate the likely occurrence of landslides in a specific area with a specified degree of prediction uncertainty"

3) Page 1513, line 5: "single phenomenon". What makes the prediction of large landslides different from the prediction of small, fast rainfall induces landslides (e.g. soil slips) is not only their individual character, but notably their scale and long-term evolution

4) Page 1513, line 6: "instable" reads "unstable". Here and elsewhere in the paper, I suggest the authors to improve English phrasing and words, maybe with the help of a native speaking colleague.

5) Page 1513, line 8: "wide range of landslides" would better read "wide range of behaviours"

6) Page 1513, line 14: what is a "complex monitoring network"?

7) Page 1513, line 20: “problems on……these thresholds are well known”. If so, it is important that the author list and discuss them with respect to the validity and applicability of the proposed method (see general comments);

8) Page 1513, lines 23-24: “when the last threshold is exceeded, EWS end their efficacy”. If early warning thresholds are correctly established (physically meaningful, reliable, expressed in terms of suitable predictors), they should be the core of an early warning system and guarantee its efficacy. I understand that available methods to establish early warning thresholds are affected by many and severe limitations, thus near-real-time management of early warning is required

9) Page 1514, lines 6-9: see General Comment n.2.

10) Page 1514, lines 12: unfortunately I have no complete access to the cited paper by Manconi and Giordan (2014), but I went through the abstract and it is quite difficult for me to understand what’s new in the present manuscript submitted to NHESS (see general comments)

11) Page 1514, line 22: “usually”: the temporal evolution of landslides depends on landslide type, scale, and timescale. The authors should clearly state the conditions to which their method applies.

12) Page 1515, lines 5-6: “values based on the actual deformation measured do not provide any information about the possible evolution……”. Why?

13) Page 1515, lines 20-25: see General Comment n.3

14) Page 1516, line 10: “active mass movement”: “large active landslide”?

15) Page 1516, line 17: “continuous monitoring of surface modifications”: “continuous monitoring of surface displacements”. Which type of continuous monitoring is undertaken? Using which techniques? At which measurement rate? With which degree of spatial coverage (i.e. point-like or distributed)? All these points greatly affect the interpretation of the collected time series and their use in early warning. The authors

should provide some information on this;

16) Page 1516, line 27: “24 h”: is this a time window used to compute an average velocity to be compared to thresholds? Isn’t is quite a long period when approaching collapse very closely?

17) Page 1517, lines 4-15: see the General Comment n. 4.

18) Page 1518, line 2: “predefined value”: how set up?

19) Page 1518, lines 13-14: basically, the authors are stating that they are quite confident in their ability of predicting the failure when failure is already almost there. This is perfectly meaningful, but suggest that the method is not able to solve the issues of real-world early warning;

20) Page 1518, lines 29: “critical time ranges”. See the General Comment n. 3.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 1511, 2015.