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Discussions

Interactive comment on "Risk-based analysis of monitoring time intervals for landslide prevention" *by* Jongook Lee et al.

Anonymous Referee #2

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Dear Authors, your paper "Risk-based analysis of monitoring time intervals for landslide prevention" aims at demonstrating that different monitoring time intervals would reduce the landslide risk over wide areas. In this respect, "an optimized set of landslide monitoring time intervals for low temporal resolution methods, such as piezometer or inclinometer, was analysed". The thesis at the base of the paper, as well as the title, look awkward. It is not really clear how different monitoring time intervals may reduce the level of risk. Following the risk formula provided by Varnes 1984, (R = H x V x E) the risk can be evaluated as a function of the hazard, element at risk and vulnerability. Of course in landslide prediction the monitoring phase is fundamental, but, mainly the aim is to gather information on variables responsible for landslides triggering in order to define reliable thresholds or to reduce the number of incorrect predictions in landslide early warning systems (LEWS). Moreover the authors are analysing the occurrence of rainfall-induced landslides in a wide spread area (Pyeongchang County, South Korea), supposing to be fast slope movements (it is not clearly defined in the text), how these phenomena can be monitored using inclinometers? In this regard, it is important to have in mind the different scales of analysis we are dealing with. At a regional scale the position and sliding surface of a landslides is not known a priori. On the other hand, at a local scale, we could know the location of the landslide/s and its sliding surface. Thus, as a function of the type of landslide and scale of analysis the monitoring instruments change. Then, "to analyse monitoring time intervals for low temporal resolution . . . an equation for the probability of landslide occurrence was denoted by the concept of reliability, and the monitoring time interval was analysed quantitatively by calculating the average probability of landslide occurrence. To identity the frequency of landslide occurrence, a unit of relative temporal frequencies was adopted, and it was estimated by establishing rainfall threshold". A crucial point that needed to be clearly explained is how and why the concept of reliability can be applied to evaluate the probability of landslide occurrence. How the assumptions of the method can be generalized and applied to landslide? Which are the similarities? The same questions arise for the definition of the average probability of failure on demand. Is it possible to replace the time interval with the monitoring interval? Why? Some concerns arise for the rainfall threshold definition and the evaluation of the landslide occurrence frequency, too. For the first, the threshold is defined considering landslides triggered by one extreme rainfall event in 2006 (Typhoon Ewiniar) and the variables considered are dependent, as it possible to notice looking at figure 3. Using different variables would have helped. Moreover the threshold defined by the authors (150 mm for daily precipitation and 200 mm for 48-hours) have been exceeded 3 times in 11 years. But it seems that only 1 out of 3 corresponded to landslides occurrence (july 2006, Typhoon Ewiniar), it means that the threshold had 66% of false alerts, which is a quite high error. Then, the probabilistic period of landslide has been evaluated but it is not clearly explained how. However, it looks more as a return period of threshold exceedances than a probabilistic period.

Concerning the landslide occurrence frequency in table 1, the values seem to be incorrect. However, it the dimension of a spatial distribution of landslides than a frequency of landslide per each hazard zone. I would have expected a rate of the number of landslides occurred in each hazard zone and the total, in the period of analysis (11 years?). Finally, the authors state that different monitoring time intervals reduce the average probability of landslide (Table 2). The table proposes 4 different time intervals (1 – 3,6 – 12,8 – 38,7 years) which seem to be really too wide to me. Depending on the type of landslide, some slopes may require a monitoring frequency well below one year. Moreover the statement "we have shown that the same level of risk reduction effect can be achieved for lower landslide hazard areas with longer monitoring time intervals compared to the higher landslide hazard areas", reads a bit counter-intuitive. Other major issues are represented by the structure of the paper which results confused. A more linear structure would be: Introduction. 2 Method. 3 Case study. 4 Result. 5 Discussion. The English is difficult to follow, also because the sentences are often too long and not-well articulated. Finally the topic of the paper not really fit the aim of the Special Issue. My overall judge is that the assumptions are not well explained and justified and the paper fails in its crucial parts. For the above mentioned reasons, even if I acknowledge the effort in producing the manuscript, from my point of view the paper cannot continue the publishing process, and a fully reconsideration is needed before a new submission.

Best regards

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