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Reply on RC2

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Author comment on "Landslides caught on seismic networks and satellite radars" by Andrea Manconi and Alessandro C. Mondini, Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-34-AC2, 2022

The paper shows, thorouhg a case study, the potentiality of integrating seismic and EO data to improve landslide mapping capabilities. The proposed approach uses broadban seismic networks to detect landslide events and SAR imagery to spatially locate the event.

The paper is well written and well organized. The results shown are promising. I think that the parper is worth to be published. However I would propose to improve the discussion section. I agree with referee 1 on clarifying stron and shortcommings.

REPLY: We thank reviewer 2 for the positive feedback. Here below we reply point by point to the comments and concerns.

COMMENTS

1) Proposed approach: It is understood that the method strongly depends on the quality/density of the seismic network. That means that nowadays it is hardly scalable to other places where landslides are a major issue. I wonder if there could be the possibility to analyze the network requirements". I mean have you tested not to use all the seismometers and just see how much the preliminary location decrease as a function of the number and density of the used seismometers? This could be a good output of the paper.

REPLY: We thank the reviewer; this is a very valuable comment. It is difficult to provide minimum requirements for the seismic network, because the capacity of detection depends on many aspects, including size and type of the event, number of stations operative at the moment of the event, signal/noise ratio of the seismic data at the moment of the event. Considering the suggestion of the reviewer, we have calculated how the area of the function LLL>0.95 (preliminary location) would change if the nearest triggered stations would not be considered in the calculation. We have included a table and additional discussion for this specific point.

2) As stated by the authors, LQ5 and LQ6 detection is ambiguous and strongly depends on the user. I understand that the authors are refering here to Sentinel-1 data. How important is here the resolution or the number of images important? It would be nice to mention it in the work.

REPLY: This question is highly relevant: the adequacy of the resolution of the images used to detect landslides is probably one of the main discussed topics in geomorphology, also when optical imagery is used. In our case, the resolution of Sentinel 1 hampers for sure the possibility of a certain detection of LQ5, and LQ6 (LQ6 in particular) because the two events are very small, and the changes left, in terms of size are, quite similar to the changes left by the salt-and pepper noise (LQ6), or other changes occurring not too far from the 'Bondo valley' (LQ5). In these cases, the landslide detection cannot be entrusted to the sole use of the area but, when possible, it must make use of other geomorphological constraints, including shape and geoenvironmental factors (e.g., slope). It should also be remarked that, when SAR is used, there are other factors that should be taken into account, in particular the relative geometries between satellite and slope where the landslide occurs: big landslides in very unfavorables geometries can be seen much smaller, and/or remain undetected. The quality of the detection in LQ5 would probably benefit from a multitemporal analysis because the environment in between the two images used to measure the change of the backscattering was quite dynamic (snow?), giving origin to several clusters of changes. Arguably, in our work we decided to use a bi-temporal approach in which, systematically, the pre-event image was the last image acquired before the landquake, and the post-event image was the first acquired after the landquake to privilege the rapid detection. In this case, results could have been improved with a shorter revisit time of the satellite to limit the occurrence of changes not related to the occurrence of the landquakes. Other, and more effective mapping techniques might be linked in cascade where our procedure flags areas of changes like in LQ5 & LQ6. Further information can be found in table 3 (pro. & cons) not reported here, and in the modified results section. The discussion has been also changed accordingly.

3) I misss an analysis on the reliability of the proposed approach. Are LQ1-LQ6 the unique seismic signals with this characteristics? Or there area false positives or negatives? I think it would be nice to comment this in the work to understand how it works. Same happens with the location of the event. Is it the unique detected change? Or there are more? If there are more, how the authors atribute to the one selected? How many of the detected areas are landslides?

REPLY: This point has been raised also by reviewer 1. The idea of the paper is to show the feasibility of the combination of seismic data and remote sensing to have a better accuracy on the location and estimation of rock slope failure events. We have not performed a continuous processing of the seismic data to evaluate the proportion of false positive/false negatives, this beyond the scope and will be performed in further investigations. This is now specified in the text.

Minor comments

- Line 147: "The outlier segment that identified covers ..... " This sentence sounds strange to me.

REPLY: Thanks, corrected.