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## Reply on RC2

David Graham Milledge et al.

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Author comment on "Automated determination of landslide locations after large trigger events: advantages and disadvantages compared to manual mapping" by David Graham Milledge et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-168-AC2>, 2021

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Thank you for your thoughtful comments which have helped us to considerably improve the manuscript. We hope that the responses below, together with the modifications in the manuscript address all your concerns. We give brief responses to your major comments (in bold) below. In the attached response document we then provide detailed responses to all comments (including new or altered manuscript text).

**1. NDVI based differencing approach is not new which authors also clarified in the method section. For novelty part, author have incorporated the cloud score, NDSI and temperature into the existing method. While I agree that the incorporation of NDSI and cloud score is necessary in snow covered areas (here, Gorkha), but in other areas such as in Haiti, this make things complicated. In previous studies, it has been found that vegetation recovery in earthquake affected areas take minimum 2 (Kashmir case) to more than 10 years (Wenchuan). Thus, cloud free composites of either Landsat8 or Sentinel2 images within the first or second year of event can easily be prepared in GEE platform, and should be used in cases other than Gorkha. This essentially makes two different algorithms, but I believe things will be less complicated.**

We agree that: 1) NDVI differencing is not new; 2) there are some cases where snow is not a concern; 3) cloud free composites can easily be prepared in Google Earth Engine. However, we disagree that: a) it is less complicated to present two different algorithms; and b) our algorithm converges on a comparison of before and after cloud free composites. Finally we note that even this comparison of pre- and post-event composites would require decisions about stack lengths to generate each composite.

Therefore we argue that our findings are novel not only in presenting a new algorithm (albeit following closely from previous work (e.g. Behling et al., 2014; 2016; Marc et al., 2019; Scheip and Wegmann, 2021) in a way that is outlined in Sections 1 and 3.1); but also, in identifying 'behavioural' (i.e. well performing) parameter values for the key parameters that must be defined for this type of analysis; and in demonstrating that even an algorithm as simple as the one we present here can identify landslide affected pixels with comparable skill to manual mapping.

**2. Further, to improve the performance of NDVI based difference approach in areas such as in Haiti, I would suggest authors to take minimum NDVI approach rather than average NDVI (i.e., minimum NDVI of the pixel of interest in the last, say, 5 years preceding the event). This approach will make sure that fresh barren surface caused by landslides have lower NDVI values than pre-event, and can be easily detectable and also helps in reduce the false positives.**

This is a good suggestion. Developing a minimum NDVI based algorithm could be a fruitful avenue for future research. There is certainly a rational theoretical basis for such an algorithm and it would be interesting to compare the two approaches but doing so would involve developing a second algorithm. This would take the paper in an entirely different direction and (we feel) broaden its scope beyond that which is tractable for a single paper. Thus we do not pursue it here.

- **Although the authors have validated their method with manually delineated landslides, readers would like to know where the new approach stands when compared with other automated approaches such as HazeMapper (Scheip and Wegmann, 2020), supervised classification or machine learning techniques. These should be incorporated in discussion section.**

We have added a new section (5.5 in the discussion) comparing ALADIN to these alternative approaches (see attached response document for the new text).

- **Among all the inventories applied in this study, the Wang et al, 2019 (Hokkaido case) is the most recent one, and is mapped from 3 m Planet imageries. There is one more inventory available for Hokkaido case (see Dou et al. 2020) which was mapped from aerial images (less than 1 m). I would like to see this results in table 3.**

We could continue to add inventories indefinitely but chose to stop at five study sites. We feel that this is sufficient. Hokkaido complicates the analysis because it opens the possibility of a Sentinel based analysis. This would require re-calibration and a more complete introduction to the properties of the Sentinel satellite and we feel that this is out of scope for the current study. We have used datasets from USGS sciencebase throughout our quantitative analysis in order to ensure consistent and traceable analysis. We include the Hokkaido dataset as an exception for illustrative purposes but do not use it in our quantitative analysis.

**5. More explanation is needed on how the ALDI pixel are converted to landslide objects. I can see that in Kashmir, Aisen and Wenchuan cases, the large landslides identified by ALDI are more than manual methods (Fig. 7). Comment on this.**

We now explain that the continuous ALADIN index is thresholded to generate the same FPR as the comparison inventory. We then explain that landslide pixels identified by ALADIN are converted to landslide objects based on a connected components clustering. We have also added reference to specific examples in Fig. 7 in the results section to describe and explain the increased frequency of large landslides in the ALADIN-based distributions (see attached response document for the new text).

Please also note the supplement to this comment:

<https://nhess.copernicus.org/preprints/nhess-2021-168/nhess-2021-168-AC2-supplement.pdf>