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Comment on nhess-2021-168

Anonymous Referee #2

Referee 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.,
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The authors performed an automated method of landslide detection based on NDVI difference method in earthquake affected areas using Google Earth Engine, and compared with manual delineation. Overall analysis and extensive use of EQIL inventories for validation is impressive. I, however concerned with the methodological aspect of this study.

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.

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.

3. 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.

4. 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.

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.

Minor comments

1. Xu et al 2014 inventory is having serious problem. It would be better if authors used the Fan et al., 2018 inventory for Wenchuan.

2. Figure quality should be improved, A grey background or another color would hel to distinguish the ALDI 0 from positive ALDI values.

3. Delete the Xu et al inventory from Fig 8. The offset in Xu et al inventory make the visualization difficult.

4. Title can be a more relaxed one than current one. Automated mapping still have problem of delineating source and accumulation zones. Further, in automated method, separating two landslide in adjacent slopes (special case – Hokkaido) is difficult.

5. Shorten the paper a little for better reading

I suggest moderate revision of the manuscript considering above comments.