Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2020-73-RC1, 2020

Earth Surface Dynamics Discussions

Interactive comment on "Beyond 2D inventories : synoptic 3D landslide volume calculation from repeat LiDAR data" *by* Thomas G. Bernard et al.

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Received and published: 29 October 2020

This is a really exciting paper that takes a careful and robust approach to surface differencing in order to identify topographic changes associated with co- and post-seismic processes. In this respect the paper provides a novel and useful contribution both in developing a methodology for identifying the changes and in documenting the changes themselves. It is also (if correct) a potentially very important paper! It argues that the current form of landslide size distributions are a result of observational error for all but the right tail. Given the importance of these findings it is essential that the paper is very clear about its landslide detection process and the implications of each processing step for landslide detection (in terms of location, size and shape). Fundamentally I remain unconvinced at the end of the paper that the findings on landslide scaling and

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size distribution reflect those of real landslides in the study area.

The approach to surface differencing is rigorous, the writeup is clear (subject to a few minor comments that could be easily resolved). It is the step from change detection to landslide detection that I find problematic. Landslides are detected as connected patches of surface difference above a threshold. The validity of this detection method is not tested against any independent observations. I believe that the detection process is: 1) sensitive to topographic errors; and 2) prone to amalgamate some landslides and break up others. The resultant landslide inventory is then used to make claims about the size distribution and scaling properties of landslides, both of which are extremely sensitive to the errors detailed above. The conclusions of the paper are then built around these later size distribution and scaling findings, which I don't think the data are currently capable of supporting.

To me, the two main missing elements of the manuscript are: 1) a very clear definition of the range of processes and landforms that the authors would include within the category of 'landslide' (and therefore what set of processes their inventory represents); and 2) a detailed comparison of the landslide inventory generated here against independent observations within the study area, these are likely to include optical imagery but would ideally also include field investigation. These two elements are essential if the authors are to support their conclusions on landslide scaling and landslide size-frequency distributions.

The estimates of total landslide volume and of net mass loss from the study area are important contributions on their own. However, I think that more work is needed to post-process that volume estimate to account for counter-factual observations (such as deposition areas with no upslope erosion). It would also be very interesting to investigate the work associated with the event in terms of elevation reduction for the landslide mass.

Finally, examination of the properties of the landslides that have been identified would be valuable both because the dataset should enable interesting insight and because disagreement between findings from this dataset and more traditional inventories may highlight uncertainties or errors not only in the traditional approaches but also in this new approach.

Please also note the supplement to this comment:
https://esurf.copernicus.org/preprints/esurf-2020-73/esurf-2020-73-RC1-supplement.pdf

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