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Earth Surface Dynamics Discussions

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Interactive comment

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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We first thank David Milledge and Alex Densmore for the positive reviews and for the relevant and detailed remarks they raised that greatly helped to improve the manuscript. Following their comments, the previous manuscript has been subject to important revisions. In essence, our results are not changed, but we believe the MS now makes a more compelling and quantitative case for our previous conclusions. We propose here to give a general overview of the modifications brought to the manuscript but please see the rest of the document for a detailed answer by comment. 1. We clarified what we consider through the generic term of "landslide" which corresponds to the spatially coherent changes of several decimeters detected by our method on hillslopes. This definition has been added in the introduction of the manuscript. We have added a dedicated section that detail the different processes we observed in the discussion (section 5.1.2, L595). 2. Although it would be interesting to compare our landslide inventory to field observations, such data are not available for the studied area. However, we added a detailed comparison with a landslide inventory manually mapped from pre- and post-EQ aerial imageries (section 3.4 and 4.2, fig. 7). Given the impossibility of having the landslide extent mapped by Massey et al. (2020), due to the author's lack of response to our numerous requests, Dimitri Lague carried out a manual mapping. We used this new manually mapped landslide inventory to: (1) compare the two methods in terms of number and area of mapped landslide, (2) detail the origin of under-detection of landslides from both methods and (3) discuss the impact of landslide under-detection on landslide volume estimation using traditional approach. Following this analysis, the discussion now extensively discusses the origin of a rollover in the pdf(Area), and we provide a new analysis showing a systematic size dependent under-detection of landslides in the 2D inventory (fig. 12). This section also replaces the section about reactivated landslides present in the previous version of the manuscript for which we agree that the term "reactivation" was misused. 3. To further improve our treatment of potential errors and false detection, we have added two additional tests to our workflow and have added many elements on the discussion regarding current limits of the workflow. First, we explore in details the source of errors coming from misclassification of the original point clouds and imperfect flight line alignments. We therefore reprocessed the pre-EQ point cloud to remove incorrectly classified points and quantified residual errors for each flight line alignments (section 3.3.1). We now integrate the residual error from flight line misalignment in the estimation of the registration error. Second, we added a new analysis to deal with false detections after segmentation (resulting in a new step in the workflow, section 3.3.4). We define a new confidence metric for each segmented cluster (source or deposit) called mean signal-to-noise ratio (SNR). An optimal value of SNR, is defined to minimize the proportion of potential artefacts (new fig. 6 and 5b). This results in more than 746 sources and 748 deposits of low confidence removed from our inventory. The final inventory now contains 524 sources and 304 deposits of very high confidence (in the first version of the MS, there were 1431 sources and 853 deposits). 4. We have added further analysis of the impact of the main parameters of our workflow (registration, segmentation and SNR filtering) in the discussion, appendix and supplementary material. Our results, and in particular the lack of rollover in pdf(A) are not significantly affected by these parameters. 5. We have tested segmentation approaches based on density based clustering (DBSCAN and derivatives) which are considered state of the art for 3D rockfall segmentation. We demonstrate that these approaches (1) do not perform better than the segmentation approach we use and do not generate significantly different pdf(A); (2) may have the tendency to oversegment intermediate landslides into small ones or remove border points of large landslides; (3) are sometime much longer to run (45 min vs 3 sec) and have parameters which are not intuitive to set. Because the method part of the article was already dense, the result of this comparison exercise are in the supplementary (supplementary material S5), along with an explanation as to why density based clustering is actually not suitable to segment large landslides in our workflow. As in the first version of the article, we clearly state in the article that our segmentation has limitations, but we are not aware of other approaches that would perform better in segmenting complex 3D objects covering 3 to 4 order in size. 6. The balance between the supplementary materials and the manuscript has been revised, and many figures have been updated following the reviewers recommendation. 7. The title and abstract have been updated to feature the notion of rollover in the pdf(A) of 2D inventories. It reflects the additional data and analysis we provide that clearly demonstrate, at least in our study case, that the rollover in the pdf(A) observed in our 2D inventory but not in the 3D one, is due to a size-dependent under detection in 2D. We believe this is of critical importance for the geomorphic community working on landslide science.

We wish both reviewers a pleasant reading and thank them in advance for the time they may spent in evaluating this new version of the manuscript.

Please also note the supplement to this comment:

https://esurf.copernicus.org/preprints/esurf-2020-73/esurf-2020-73-AC1-supplement.pdf

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