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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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This is an exciting manuscript that reports on a promising way forward in detection and analysis of landslide inventories. The authors use the M3C2 point cloud analysis approach, previously developed by Lague and colleagues, to detect change in pre- and post-earthquake Lidar point clouds from the Kaikora earthquake area in New Zealand. This is absolutely the right thing to be doing, and the authors are clear about how their approach gets around some vexing issues with current best practice (which is to map using 2d imagery and to use volume-area scaling to get at landslide volumes). The topic is fully appropriate to the journal and I anticipate that this work will attract a good deal of attention from the journal readership.

I do have some questions and suggestions for the authors to consider before the manuscript is published. Many of these are fairly minor and are detailed in the attached PDF; I will not repeat those here. But there are a couple of wider issues that are related to the clarity of what the authors have done, and in part to the division of material between the manuscript and the supplemental information. In brief, I don't think it's possible to follow the authors' approach from the manuscript alone, and there are key parts of their analysis that can only be understood by going to the SI. I don't think that's right for a manuscript that purports to document a new methodological approach. These issues come under three headings (but please see the PDF for more detailed comments):

First, because the authors are documenting a new approach for mapping landslides, I would expect to see some quantitative comparison of their results with a landslide inventory or inventories prepared in the more traditional way. The authors show some amalgamated results (Fig 5, Table 2), but I think it would be useful to show a more systematic comparison with the Massey et al. results. Given that there are only 27 landslides in the Massey et al. inventory within the study area, this should be pretty straightforward - but I think it is important to demonstrate the extent to which their approach can match (or not) landslides detected by the alternative approach, as well as the additional landslides that they claim to be able to map. The only place I could see the Massey et al. landslides was as barely-visible centroids on a perspective view of the study area in Fig. 6.

Second, and somewhat related, the text is very unclear on how individual landslide sources and deposits are segmented and identified. Because of this, all of the resulting statistics of the area and volume distributions are uncertain in the reader's mind. This is more clearly explained in the SI... but again I don't think it's fair to require the reader to go to the SI to understand a methodological advance that is being proposed. I deliberately read the manuscript without going to the SI to see if I could follow it, and there are places (e.g., section 3.3.3) that are very difficult to understand and don't really address what has been done. I'd really recommend that the authors review the balance between the text and SI and try to flesh out the explanations in the main text. The details of the sensitivity analysis (e.g., to the distance threshold D_m) can be left for the SI, for sure.

Third, I found the authors' use of the term 'reactivation' potentially confusing, and I'd suggest that they choose a different way to express this. Assessing reactivation (e.g., of coseismic landslides in post-earthquake storms) is definitely a big issue in multi-temporal landslide inventory analysis, and a few different approaches have been put forward, none of them very satisfying. Reactivation implies some renewed landslide activity that partly or wholly overlaps with a landslide from a previous epoch - e.g. further erosion within a pre-existing scar, or headward/lateral progression of the scar edges, or erosion within a scar coupled with deposition on a pre-existing deposit... But that's not what the authors are actually talking about here, because they only have two point clouds spanning one epoch, and there's no independent dataset of pre-existing landslides. A better way of framing this part of the analysis might be around the following question: are there landslides that would not have been recognised using the classical approach, either because they occurred on bare bedrock or because the vegetation contrast was too low, but that they can see with their method? That's an important question - but it's not the same as reactivation.

Finally, a fairly minor point: I agree that the approach outlined by the authors is the way forward and that, where suitable Lidar data are available, this should be further pursued and developed. But I think it's equally fair to recognise that (1) suitably accurate and high-resolution Lidar data aren't always available, and (2) there are problems and applications for which this approach simply isn't (yet) feasible. For example, for multi-temporal inventory creation over the full landslide-affected area, where the goal is understanding patterns of landslide occurrence and hazard but volume estimation is a secondary concern, then a traditional 2d image-based approach might be fine. New Zealand and a few other countries can fly repeat high-density Lidar surveys; this ca-

pacity doesn't (yet) exist in most landslide-prone regions of the world. I think it would be fair of the authors to acknowledge this - it doesn't detract from their analysis but perhaps places it into a better wider context.

To summarise, this is a really exciting piece of work. Once the authors have dealt with these issues, then I look forward to seeing this published.

Please also note the supplement to this comment:
https://esurf.copernicus.org/preprints/esurf-2020-73/esurf-2020-73-RC2-supplement.pdf

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