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Earth Surface Dynamics Discussions

Interactive comment on "A new methodology exploring the record of snow avalanches in lake sediments" *by* Laurent Fouinat et al.

Anonymous Referee #2

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Review of Fouinat et al., "A new methodology exploring the record of snow avalanches in lake sediments" for Earth Surface Dynamics

The manuscript of Fouinat et al., seeks to provide a new approach to reconstruct the past occurrence of snow avalanches from lake deposits. The introduction explains the challenges of documenting these events in many locations, and the importance of having historic context of their relative frequency. The paper argues that the CT-scan approach allows for a non-destructive and continuous (throughout a core) quantitative analysis of sedimentary records, in terms of grain size, number of grains, and assessing this for materials of different 'relative density'. This is quite interesting, namely in the potential to look at macro-organic debris (lower density) and clastic material separately, and isolate large clasts from the matrix in situ. Most of the time we try and get rid of the organic matter, and this study shows how it could provide interesting in-

formation. The data are well presented, the core chronology seems robust, and the data are interpreted alongside other records of avalanche occurrence. There are also links to the erosion of soil and vegetation (and organic matter/nutrients/carbon) which could interest a wider audience. The paper should be considered for Earth Surface Dynamics.

However, there are some issues which need to be addressed in my opinion:

1. The paper is set up as a 'new method', with the abstract and parts of the manuscript suggesting that the CT-scan method allows for 'avalanche deposit reconstruction'. But to do this, surely you need one (or more) known avalanche input events which have been cored to examine their sedimentology and make up of organic and clastic debris? (such as the one shown in May 2015 in Fig. 1?) I didn't see this being done clearly. Therefore, this paper does not provide a test of a method, but an interpretation of the sedimentology in terms of the process operating.

In my opinion, I think the paper would be better set up to illustrate how CT-scanning can be used to provide new, quantitative information on sedimentary deposits of a high altitude lake basin. This is in the context of interpreting fluvial events, debris flows, and avalanche deposits. I suggest refocusing the piece on the record itself, and interpreting it in terms of geomorphic and sedimentary processes. A paper with that focus would have to rely less on this being a transferable method just for 'snow avalanches', which is a weak part of the manuscript. Also, by doing so one might actually conclude that avalanches are pretty difficult to reconstruct in this deposit (probably because of frozen vs ice-free lake conditions when an event happens). A revised version could then focus more on better explaining and justifying the approach shown in Fig 2b, which seems to allow information on the distribution of organic matter (and its size) and certain clastic grain sizes.

2. More information needed on the CAT-scan methods and results: The '256 grey-scale' values are central to the approach here, providing a 'relative density'. There

needs to be more discussion of this – is this calibrated to anything? A standard of known density? Is it part of the 'soft tissue and bone kernal'. Why is the range 95-125 selected for organic matter – it seems to be justified on picking a single twig – what about finer organic debris, or leaves etc? There is a large second 'hump' in the histogram of values at 160 (Fig. 2a) – what is this value? How does the CAT-scan deal with a voxel which is made of air/water+silt – does this end up with a value <255, or lower? For this (and the subsequent counting methods) to be more widely applied, this needs more discussion/justification, and an awareness of caveats of this classification (i.e. quartz vs mica could have different density?).

3. The processes which deliver sediment/debris to the lake basin: The paper mentions that the lake is frozen for several months in the winter. Surely this makes it almost impossible to identify an avalanche deposit, because one entering an open lake will look fundamentally different to one which enters the lake after the thaw of the surface ice? The paper discusses this (at the start of the discussion), but to me this is the major caveat of the approach, and a flaw of focusing the paper solely on tracking avalanche deposits. It seems these can have a non-unique signature in the record.

4. Number of rocks as a proxy: This wasn't clearly justified as a proxy when introduced line 291. The link between the count data and the historical EPA dataset (which is regional??) shown on Fig. 4 is not convincing. I would use it to argue that it is extremely challenging to fingerprint these events at all in the record. The reason may well be that they do not have a unique sedimentary deposit associated with them. Why would >4 rocks reflect an avalanche and not a fluvial flood or debris flow? I'm not sure you can justify this.

5. The paper has numerous grammatical errors which need to be addressed.

Other comments:

52+54: snow vs wet avalanches – be clear on terminology and what it means.

56: 'Elements with different densities', perhaps 'assessment of the relative density of clasts in the deposit'

58: I wasn't convinced that the data showed that organic matter macro-remains characterise wet avalanches.

59: would perhaps be better to cast this in terms of new insight on sedimentary archives which can be used alongside existing approaches?

108: did you really test it?

117: do you mean using 10Be exposure ages – spell this out please.

146: what is the noise – this needs more explanation (as do other elements of this approach – see comment 2 above)

149: why is this grainsize data not shown?

180: after isolation – explain better what you mean here.

199: units are needed after density.

258+269: these sentences seem to contradict one another?

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