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Earth Surface Dynamics Discussions

Interactive comment on "Reconstructing the dynamics of the highly-similar May 2016 and June 2019 Iliamna Volcano, Alaska ice–rock avalanches from seismoacoustic data" *by* Liam Toney et al.

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The paper by Toney et al. presents the analysis of seismo-acoustic data of two ice–rock avalanches that occurred on Iliamna Volcano, Alaska (USA) on 2016 and 2019. The paper is well written and reports a unique dataset of seismic and infrasound observations of large mass wasting events. The methods employed are not new but their application to two recent events makes the paper definitely relevant for the community dealing with natural hazards in mountainous areas. I would recommend publication of this paper after minor revisions, here following my main comments to the authors.

1) Structure: the background and methods sections are quite long and have many sub-

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sections. I would try to shorten the paper and simplify its structure. For instance, I would skip section 2.3 and move part of the text describing ice-rock avalanche to the Introduction. In addition, I suggest moving the text contained in the section 2.5 to the beginning of Data. Finally, I would skip the whole section 4.2.3 and move it all to a supplement file.

2) Dataset: did you consider extending your analysis to similar events that occurred at Iliamna Volcano before 2016? In Caplan-Auerbach and Huggel (2007) quite a lot of ice-rock avalanches are reported that produced seismic signals at Iliamna Volcano.

3) Methods: the inversion of low-frequency seismic data used to reconstruct the force history of the two ice–rock avalanches is a consolidated method. Given the large number of events (see point 1) and broadband seismic stations available, it would be possible to show and discuss the impact of the network geometry on the force history?

4) Event volumes: how did you estimate the value of 1.5 m deposit thickness? I would add the range of error to the event volumes. Ice-rock partition: is fifty-fifty consistent with field-based estimates of previous events? In any case, I do not expect that such an information on the volume uncertainty would explain the discrepancies between the masses inferred from the force inversion trajectories versus the ones calculated with satellite imagery. I suggest indicating where and when fragmentation and erosion-deposition processes occur, maybe adding some graphical features to figure 10 or some text to the description of stages A-E in section 6.2.

5) Results: quantitative results descend from the analysis of the seismic information. I appreciated the explicit acknowledgement of the limitations precluding the authors from assessing a complete infrasound source estimate. Actually, infrasound data are mainly used in the discussion to highlight the limitations of the force-history in describing the mass movements. However, I have the impression that section 6.4 can be extended mentioning that the transition from a block-type failure to a granular flow likely results in a higher frequency seismicity. Near-field seismoacoustic observations of debris flows

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can support this discussion, see Hürlimann et al. (2019) and references therein.

6) Stick-slip activity: although this is not the objective of the paper, this is an intriguing point. I am wondering if precursory tremors like those mentioned in the paper can be produced by small ice-rockfall events preceding the main collapse. Progressive rockfall activity is a common process during the first phase of motion of a large landslide. What do you think?

References

Caplan-Auerbach, J., and Huggel, C., 2007, Precursory seismicity associated with frequent, large ice avalanches on Iliamna volcano, Alaska, USA: Journal of Glaciology, v. 53, p. 128–140, doi:10.3189/172756507781833866.

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