ESurf reviewer #1 — [anonymous]

Line	Reviewer comment	Our response
N/A	The authors should state more clearly what is the novelty of their study, especially in the introduction. I think that it would increase the impact of the paper. In the current shape, it is not clear what this study brings, compared to the references cited in the Introduction and Background sections.	A large component of the novelty of this study is the dataset — these are two very large events with very similar characteristics, *and* they were recorded extensively by acoustic and seismic sensors. We emphasize this in paragraph 3 of the Introduction section. We also stress the similarity of the events and accompanying benefits in the title, Abstract, and the first paragraph of Conclusions.
N/A	In the abstract, the authors are stating: "Seismic and acoustic signals from these often-remote processes, combined with other geophysical observations, can provide key information for monitoring and rapid response efforts and enhance our understand- ing of event dynamics". I was expecting more discussion on this point in the main body of the paper. What is this study bringing regarding to this statement?	We have added an additional section, §6.7, which explicitly addresses the feasibility of the force inversion method for rapid hazard response.
N/A	In general, the authors are having a nice discussion on their results, comparing them to other studies, discussing the limitations of the methods they are using. However, I think the paper would be improved by having a more fundamental discussion: what are these results telling us on the events, how can we use them to monitor this kind of events? Maybe the discrepancies between the inversion results for the two very similar events could be more discussed as well. (among other possible discussions)	Please see the above response to address your monitoring point. We hesitate to analyze the smaller-scale discrepancies between the two events, since these are more likely to arise from event-specific noise or station coverage factors (e.g., see the difference in vertical scaling between the 2016 and 2019 vertical trajectories). However — we have made a new section, §6.6, which incorporates an existing paragraph and now also discusses the observed difference in force-time function amplitudes between the two events.
N/A	Finally, it is not clear to me what is the use of the acoustic data in this study: the main results on the dynamics of the avalanches come from the seismic data. The acoustic data are occupying a large part in the title and main body. However, considering the output from this data, I would reduce their description, or emphasize better why they are new and important in this study.	We believe that the acoustic data reported on in this study validate their appearance in the title and main body. Firstly, acoustic recordings of large mass movements are rare; to have data from two highly similar events is even more so. We feel that it is important to present these data. Secondly, the acoustic data do provide complementary information to the seismic data. For instance, when both waveforms are aligned (Fig. 8) the absence of an initial acoustic transient to match the initial seismic transient suggests a sub- or inter-glacial source (as mentioned in the text) rather than a surface source such as a precursory rock or ice fall, since the latter would produce infrasound as well. Additionally, similar alignment of seismic and infrasound waveforms has been observed for other processes such as debris flows. Their alignment here may indicate something about the flow regime after fragmentation (stage B onwards). We've added additional discussion in §6.4.

N/A	The Introduction and Background sections are a bit long. They may be grouped?	We have removed a paragraph from the Introduction and moved one data-related section from the Background to Data, which makes both of those sections a more manageable length.
Fig. 1	In the legend of Figure 1: add the distances of the 2 closest stations	The distances for these two stations now appear in the map.
70	Is there a reference?	Two sentences later, we provide several references.
188–189	“The events also produced prodigious long-period energy with a dominant period of 35 s (Fig. 5)” What can be the source of this?	The source of the long-period seismic radiation is discussed in the first paragraph of §2.1.
Fig. 6	Figure 6, acoustic transmission loss: the patterns are pretty different, whereas the authors are stating that the sources are very similar. What can explain this discrepancy? (overall on the western part) I thought it could be due to the addition of acoustic stations in the western region, but these stations did not seem to detect any signal. I would like some discussion on this point.	In this response we assume that the patterns you refer to are the transmission loss patterns. Since the regional infrasound arrays all detected the signal, we focus on single infrasound sensors in the below discussion (inverted triangles and squares in Fig. 6). Our ability to detect an infrasound signal at a single infrasound sensor is strongly controlled by three factors: 1. The noise level at the sensor (indicated by sensor RMS pressure). Many of the “single-station” type infrasound sensors used in this study are part of the meteorological sensing package added to Transportable Array seismic stations. This means that the stations were sited primarily for seismic, not infrasonic, performance. Therefore, noise in the infrasonic band — for example, turbulence created by the interaction of wind with nearby topography (or trees/rocks/structures near the sensor) — can be large for these stations. Even for dedicated single sensor infrasound installs, such as those deployed for volcano monitoring, noise is an issue. Arrays can help mitigate this problem by determining coherent energy across the array. 2. The propagation conditions (indicated by transmission loss). For non-local (> 15 km) infrasound, propagation is especially important since entire portions of Earth’s surface can reside in “shadow zones” in between bounce points of the atmospheric waveguides (ducts, see §5.1.3). 3. Source strength. We assume that the source strength and directionality are very similar between the two events, based upon the similarity of the acoustic data and deposits. Fig. 6 is designed to display the first two of these factors on one map. In the case of the additional stations to the west in 2019, while we were better able to sample the wavefield, most of the added stations were noisy, so even with seemingly favorable propagation to the west, we have no additional detections. §5.1.2 discusses this. Finally, note that the transmission loss modeling is for a

		basic point source and is independent of source character between the two years — variability of the transmission loss pattern corresponds to variability in the atmosphere between the two years. Likewise for the RMS pressure calculation — this solely reflects differences in local site noise between the two years.
Sec. 4.1.2	what is the definition of the root mean square pressure?	For any time series signal, the root-mean-square is the square root of the mean of the squares of each data value. It is a relatively robust method for determining the average value — in this case, average pressure — of a waveform.
344–346	"We use the satellite imagery shown in Fig. 2 to estimate the mass for each event. First, we subtract the avalanche source area from the total area, ignore entrainment, and assume a uniform 1.5 m deposit thickness everywhere on the slope to obtain a volume." Is it not possible to deduce it from the DEMs?	It was not possible to deduce the avalanche volumes from DEM analysis. The SfM DEM was acquired in late July 2019, one month after the June 2019 event. Since the avalanche deposits have a large ice component, it is unlikely that the SfM DEM accurately captures the June 2019 event's deposit. More critically, we do not have pre-event DEMs for either event (nor a post-event DEM for the May 2016 event), precluding DEM subtraction. We've added a sentence explaining this limitation to the new "Mass estimation" section (§4.1) of the manuscript.
356–368	It is not clear to me how the authors choose the end point.	We do not in fact pick a COM end point. We pick a COM start point, and find the COM runout length that results in the best match of the force-time function features to the topography and flow features evident in the deposits. We've added some clarification to this end in §4.3.3.
Fig. 8	Seismic and acoustic signals are shifted to be aligned on the time 0 of the inversion. But I do not understand why they are shifted for travel time from different points? (point force location for the seismic signals, and avalanche path midpoint for the acoustic signals?) Can the authors explain this choice, since it has an impact on the interpretation (paragraph beginning Line 505)?	The selection of a source location to facilitate source-to-receiver distance calculations (and thus travel time removal) is difficult due to the moving source (COMs moved up to 8 km). We selected the avalanche path midpoint for the infrasound source location since this is likely the most acoustically energetic portion of the flow (see second and third paragraphs in §6.4 for discussion). The selection of a source location for the high-frequency (HF) seismic source is trickier, since we have identified multiple HF transients that correspond to different source locations. For example, the initial HF transient is associated with a failure near the crown of the avalanche, but the following spindle is associated with the fragmentation of mass further downslope (similar to the infrasound source location). For consistency with the infrasound travel time removal location, we've changed the location for shifting of the seismic signals to the avalanche midpoint. This does not change our interpretations (paragraph beginning on line 509 in the revised manuscript).
456,	"manifested as a high-frequency": Indicate the	Added "(> 5 Hz)" in these two places. See also Fig.

460	frequency here. (Same Line 460)	4.