Overview of reviewer comments

In the following table the comments from the reviewers have been listed as they appear in the text along with out answer to them. We further refer to the manuscript with tracked changes.

	Reviewer	Reviewer comment	Author answer
		General comments	
	R1	I recommend the authors develop one strong research question they can address with their data, and the data is indeed very promising, given that the analysis goes beyond the currently very descriptive nature	We have made clear what the aim of the paper
	R2	Detailed comments given in “esrf-2020-32 R2_comments”	We have accommodated the numerous minor corrections and suggestions named in the document
	R2	Your figures need to be referred to succinctly in the text. You should not mention Fig 5 until you mention the ones ahead of it. The same for Fig 2e before a,b,c,d.	We have edited the figs (added new fig 2) and corrected the figure referencing
	R2	You should always cite references in text chronologically.	We have corrected this throughout the paper
	R2	Overall, the article has potential to show what can be done remotely, but it is not ready yet as it is not clear how well you have met the objectives.	We have made the aim of the paper more clear (to detect and locate unstable rock slopes and rock slope failures)
	R4	Mayor concern I have with the use of the landslide terminology	We have limited the terminology to “rock avalanche” and “unstable rock slope” as suggested, and exchanged the term "landslide" with "rock slope failure"
	R3, R4	the terms “historic” and “prehistoric” confusing.	We now describe the activity as "recent" or "older"
	R4	17 specific minor comments (see RC4)	We have accommodated all the changes
		Abstract:	
	R4	The abstract is badly structured.	We’ve restructured the abstract
		Introduction	
	R2	The intro needs something about what you knew before the landslide and what you didn’t and where else is at risk of these kinds of landslides/tsunami	We have elaborated on the state of knowledge before the landslide, this is also done in the start of the results section
	R2	The physiographic setting needs more than a couple of sentences on the bedrock geology. Need to present:	We have expanded the physiographic setting. We do not address the tsunami wave. This is done by Paris *et al*. (2019)

		climate, past glaciations, i.e, surficial geology, permafrost distribution, bathymetry, and why a tsunami wave would have made it that far	
	R4	The aims are poorly defined: [he suggests] It is a first step to develop one method that can help defining the threat/ hazard of rock slope failures in an unhospitable climate with very difficult access. The study also should contribute to the understanding of conditioning mechanisms including permafrost changes.	We have made clear what the aim of the paper in the abstract and introduction. We have elaborated on the conditioning mechanisms in the discussion but a detailed discussion is beyond the scope of this study as we do not contribute with dating on old rock slope faliures and with data on the permafrost conditions of the slope
		Methods and data	
	R1	There is no detail about the software and workflow used to perform the InSAR analysis.	More details on the InSAR processing have been added.
	R1	There is no information on the seismic data handling and analysis (e.g., signal preprocessing, detection of events, location of events, magnitude estimation, description/analysis of event signals; from the data presented in figure 5, it looks like the raw seismograms were inspected, without deconvolution, without filtering, without description of the spectral properties and their evolution, without inversion of the data for forces or other target variables)	We have added this to the method section and described why we use this approach.
	R1	There is only very diffuse information on how optical remote sensing data was interpreted to identify features, no software or workflows are described.	We have elaborated on the (very simple) workflow used for the optical images
	R1	InSAR data can yield so much more than just colourful pictures (without a legend by the way) and separating areas (of which size and with which degree of overlap to the failed sites?) of decorrelation from areas of coherence (by which measure actually?)	Detailed InSAR analysis is beyond the scope of this paper. The methodology has been described more clearly. We use InSAR data to identify individual events, not to map long-term deformation rates, thus the two-pair interferograms are the main data for our analysis. More detailed analysis, including multi-temporal InSAR will be presented in a separate paper.
	R1	Seismic data (see references of what other people have done with seismic data sets)	Detailed analysis of the seismic signal is beyond the scope of this paper. We use the

		can give so much more insight to the dynamics of mass wasting events (force inversion, volume estimates, duration and evolution,...)	seismic data to detect events, namely the exact timing.
	R2	The methods need a workflow diagram showing what you did first and what you did last	The methodology and workflow have been described more clearly. Composing a workflow diagram is not straight forward as there are multiple ways into the workflow.
	R2	The explanations for each method are too general	We have elaborated on the method descriptions
	R2	Eg DInSAR monitoring… how many images? Also, explain decorrelation, a variation of colour is shown but no one can tell how much change has actually occurred because there is no colour bar indicating the colours.	The methodology has been described more clearly, including information about the number of images processed. See also reply to R1 above.
	R3	It is stated that earthquake location uncertainties are up to 50 km, but what are typical "average" uncertainties? And what are typical magnitudes of the recorded events?	The typical uncertainties are dependent on the number of stations recording the event (tied with the magnitude), and with the station spacing. They are from under 10 km for large events to up to 50 km for smaller events. We record events in Greenland from under ML 1 to over ML 6.0 The events discussed in this paper (bar the mail 2017 rock avalanche event) are typically ML 1.2 – 2.7 – se table 2 in the ms.
	R4	details on each method are too limited. The methodology section has to be more precise in order that an independent scientist could reconstruct the same results	The methodology has been described more clearly.
	R4	seismology. It takes the reader to read to the discussion (line 407 – 418) to understand what was done	The seismology section have been restructured
	R4	How processing of data was done with InSAR keeps unclear.	The methodology has been described more clearly.
		Results	
	R1	are bedding characteristics (constrained from geological map, own mapping, UAV data,...?),	Bedding trends are from field measurements. We have clarified this in the results
	R1	Volume calculations (how constrained, how processed, what are the uncertainties)	The volumes are constrained by two DEMs subtracted from each other as (now more clearly) stated in the text.

	R1	sliding plane angles (how evaluated, what are uncertainties, and so on)	The sliding plane is covered by deposits but we infer it from the dip of the bedding in the area. We have clarified this in the text
	R3	Page 7: The 2009 and 2016 rock avalanches have similar volume, but quite different magnitudes (2.7 vs. 2.1). I would be interested in the authors' view on what may be the reason for this discrepancy	Both magnitudes are consistent within the stations recording the events. The plot (end of this document) of the spectra (8 min time and frequencies from 0-10Hz in both cases) of the vertical component from SUMG station show that the 2009 ML 2.7 (top) happened in a short time period and concentrated, while the 2016 ML 2.0 (bottom) was much more diffuse and took longer. So even though the two events happened at the same location and the volume and scar look similar, the timing within the events was quite different. A short concentrated fall gives rise to a higher magnitude in the seismic signature than a more diffuse events over longer time. Ideally, this should not be the case as magnitude aims at representing the energy released in the event. Here we use the local magnitude (ML) which is designed for tectonic earthquakes and thus does not represent the full energy release of a non-tectonic event. Very interesting observation from reviewer 3 - thank you.
	R4	I would suggest reorganizing the text blocks of each rock slide or rock avalanche by describing what is today visible, conclude on the process and then reconstruct the event / slide by remote sensing data. Here a bit more description becomes necessary.	We have made an introductory paragraph to the results section to describe more clearly what have been done
	R4	More descriptive documentation should be added which could be placed in a data repository. A detailed data repository would also be enormously beneficial to document the change in remote sensing data for each event. Figure 6 could be added in the data repository as it does not provide essential information to understand the manuscript	We did not find it necessary to compile the data in a repository as we clearly describe what data has been used to identify individual events and as all of the data is freely available through the sources listed in the method and data availability sections.
	R4	Some morphological features are described for the different events/unstable rock slopes. In general only the back scarp somehow easily visible in figures 1-3.	We have added a new fig 2 and updated the other figures to address this

		Additional material is required, and landforms described should be marked.	
	R4	Some information on the rockslide is given, however the description by far do not allow defining slumps = rotational slide or other types of rock slope failure. So keep it to “unstable rock slope” or go in depth, produce shamtic sections of the instabilities and classify them correctly.	We have limited the terminology to “rock avalanche” and “unstable rock slope” as suggested.
	R4	Table 2 is confusing as it is unclear what goes into column 1 and 3. In column 1 is a mixture of “interpreted events” Karat 2009 rock avalanche, Karat 2016 rock avalanche and registered events “all seismic events” the Karrat 2017 rock avalanche. Column 3 summarizes, references, interpretation of some of the seismic events or repeats information given in column 1 with other words. This table has to be reorganized.	We have reorganized the table.
		Discussion	
	R4	Line 324-325 this should be mapped and shown somewhere in the result section. This is not a discussion but results of the mapping. Include in figure 1 and make an own figure for this or add into a supplementary data file. Out of the result section it also does not come clear if the remaining slopes in the fjord were mapped and no landslides were detected or if no sign of large landslides was seen rapidly and thus the slopes not mapped. This information is essential for the discussion	We have made a paragraph on “Field observations and sign of previous activity” in the start of the results section to accommodate this. We have added sentence about screening of the surrounding the KLC. We have added e new fig 2 showing examples of previous activity
	R4	Large part of the description on the seismological signature of a landslide should not go into the discussion but into the result chapter including figure 5.	We have rewritten this section and moved it to the method and result section
	R4	Line 343-346 This is rather a result and not a discussion. A nice figure could be added or this statement should be documented in a supplementary data file.	We have removed this statement form the discussion
	R3	I agree on the last sentence “: : : It is an effective tool for identifying and investigating active landslide areas, but actual field validation is necessary in order to further assess the risk”, but it needs elaboration. What can we	We have elaborated that field visits are necessary, especially to constrain the structural setting of the slope.

		obtain from field data, that we cannot see remotely? And how does that contribute to risk assessment? (and should it actually rather be hazard assessment?)	
	R3	Page 13, first paragraph: have you compared spectral plots of cryogenic seismic events and small landslide events? Could such plots be added to Figure 5?	Figure 5 (now fig 5) has been revised, and spectral plots added.
	R4	Work flow: this work flow is clearly new and it was developed based on the remoteness of the environment. However, it should be discussed against workflows form other environments and what is the improvement of workflow that could also give advantages in other settings.	We have added a discussion and references to accommodate this
	R4	Trigger/conditioning mechanism: Effects of static, dynamic conditioning factors and triggering have long been discussed e.g. Glade and Crozier 2005, Hermanns et al., 2006a and others and the discussion here could follow those classes as structural geology clearly is a conditioning factor here while permafrost changes is very likely one. The study does not contribute to the discussion of triggering and rock fatigue or a form of widening of the instability can be discussed. Look into the wide literature of progressive rock slope failure for references.	Based on our limited (mainly remotely sensed) data a detailed discussion on the trigger/conditioning mechanisms and contribution of permafrost degradation is beyond the scope of this paper. We have rewritten and expanded this part of the discussion with some of the suggested references but avoided going into a detailed discussion
	R4	The discussion on permafrost is relatively poor in respect with recently published papers on the topic. I think that the hypothesis of permafrost degradation is valid however it should be discussed based on other publications, e.g.: McColl, 2012; Ballantyne and Stones, 2013; Böhme et al., 2015; Hilger et al., 2018; Kuhn et al., 2019	See above
	R4	The same counts for repeated failure from the same slope. There is a vast literature discussing the relation between repeated failures: Grimstad, 2005; Hermanns et al., 2006b; Willenberg et al., 2008; Hilger et al., 2018	See above
	R4	The discussion starts with referring to the work by Krautblatter et al., 2013. This paper summarizes different effects of permafrost change on rock slope stability. The	See above

		discussion does not include any details on that.	
		Conclusion and outlook	
	R3	Page 14, 2nd paragraph: I do not agree that being alert to smaller landslide events will mitigate the risk of large, tsunamigenic events, though it may allow for evacuation of exposed populations before a large event. Consider rephrasing.	We have rephrased the conclusion

Plot of spectra: