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Interactive comment on "Evolution of events before and after the 17 June 2017 landslide at Karrat, West Greenland – a multidisciplinary approach for studying landslides in a remote arctic area" *by* Kristian Svennevig et al.

Anonymous Referee #4

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The manuscript describes a cluster of unstable rock slopes and rock avalanches on one slope of Karrat fjord, one of those the June 17th rock avalanche that caused a displacement wave and loss of life in the 30 km distant village of Nuugaatsiaq, W Greenland. The manuscript summarizes available remote sensing data from seismic stations, satellite and air and includes limited field observations. The manuscript summarizes data on published and unpublished events. In the discussion the authors ague that remote sensing data and databases on events are a good tool to predict large rock slope failures that might be more catastrophic. In addition, the authors discuss

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Discussion paper

conditioning mechanisms and suggest that permafrost changes might be one of the contributing mechanisms. I think the manuscript summarizes a large number of new and previously published data to enhance the understanding of rock slope instabilities on a fjord section in the Artic. In this way are the data presented unique for these climatic settings and contribute significantly to the discussion of how large catastrophic slope failures can develop. I think this manuscript is a very valuable contribution to the understanding of rock slope failure development and methods that can be used. However, I see several short comings that have to be addressed prior to publication that I would classify as “mayor revision”.

One mayor concern I have with the use of the landslide terminology. Throughout the manuscript there is a constant jump of terms between landslide, rock avalanche, unstable slopes, active areas, slump, slide etc. I think all phenomena described in the manuscript are in rock and none in soil. Soil slides in form of debris flows, soil creeps and others also exist in this environment. However as they are not part of the manuscript I would suggest to limit terminology to “rock avalanche” and “unstable rock slope” as no detailed descriptions (geological profiles) are given to classify the unstable rock slopes more adequately as “slumps”, translational, bi translational or other type of rockslides. Similar is my concern with the term “historic” that is not defined in the manuscript but frequently used and exchanged with “recent”. My definition of “historic” would be documented in written documents. Thus, although very recent are two of the rockslides not “historic” but reconstructed. In the following I go with my comments in sequential order two times throw the manuscript. In the first round by marking major issues that have to be addressed and in the second round focussing on small issues that can be changed / or have to be changed to improve the manuscript. Abstract: The abstract is badly structured, the newest relevant information come in the second paragraph. The first paragraph only summarizes information that is already published, or methods used. Introduction: The aims are poorly defined. This manuscript is more than a local case study to evaluate the threat or hazard of rock slope failures at Ummiammakku Mountain or the processes that lead to the Karrat 2017 rock avalanche. It is a

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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. Methods and data: This section gives a good overview of data used but details on each method are too limited. E.g. seismology. It takes the reader to read to the discussion (line 407 – 418) to understand what was done. The methodology section has to be more precise in order that an independent scientist could reconstruct the same results. This is also necessary for the section on InSAR. How processing of data was done with InSAR keeps unclear. Results: 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 than reconstruct the event / slide by remote sensing data. Here a bit more description becomes necessary. Why are the deposits of the "confirmed" events "deposits of rock avalanches" but not catastrophic rockslides? The deposits are visible in figure 1-3 but those figures are only tiny. 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. 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. Additional material is required, and landforms described should be marked. 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. 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. 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. 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. 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. Discussion: 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. 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. 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 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 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 discussion does not include any details on that.

Small comments: Line 52: Risk is hazard x consequences, this manuscript does not

include a consequence analysis rather use threat or hazard if you include volume, impact area and likelihood analysis. Line 131 Figure numbering is out of sequence. Line 195 What is “catastrophic”, do you use definition by Hermmans and Longva, 2012 or does your definition include “life loss” Line 210 “candidate” is jargon. Line 214 A “scarp” is a surface feature “back scarp, minor scarp, secondary scarp” but has no volume, better volume of the source area Line 230: This is not visible in figure 3C. Line 272: Define “initiation” do you mean first surface displacement? Or first failure of rock bridges? Line 294: Should be: “Other seismic activity in the Karrat landslide Complex area that can be related to rock slope deformation.” Line 319: Erosion is much more and you do not discuss landslide versus other type of erosion here better simple “rock slope failure activity” Line 334: Formulate a bit more cautious “with interpreted seismic characteristics” Line 340: seen in InSAR and seismic events that can be associated to it. Line 344: Use “likelihood” instead of “risk” Line 362: This is like multiple rock avalanches at Loen in Norway in the 20th century. Line 363: “could be at work” is jargon better: contribute Line 376: Magnin et al., 2019 would be an ideal reference here (published in the same journal :-)) Line 383: You described that structures and their orientations are important, are those known for slopes in neighbouring fjord systems? Line 441: “being alert to smaller events in a known landslide area is crucial” better “might be crucial” there are multiple failures that are not proceeded by small failures but rather by opening of cracks – see literature.

Please also note the supplement to this comment:
https://esurf.copernicus.org/preprints/esurf-2020-32/esurf-2020-32-RC4-supplement.pdf

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