

Interactive comment on:

“The role of thermokarst evolution in debris flow initiation (Huttekari Rock Glacier, Austrian Alps)” by Simon Kainz et al.

By anonymous referee

General comments:

The manuscript investigates a cascade of processes in the Radurschl Valley in the Austrian Alps. The outburst of a (supra-) glacial lake initiated the event, which continued with the failure of a rock glacier front with subsequent debris flow initiation. The deposit of the latter caused the blocking of the valley and the formation of a lake, which was drained to manage the risk related to its possible outburst. The series of events and their description are very interesting and, in my opinion, would enrich the scientific literature. However, I have some concerns regarding the manuscript, which in my opinion could profit from some revisions.

The methods are described at a level of detail that is not sufficient for understanding the analysis and reproduce the study. In this sense, they need to be revised and improved. The results and the discussion are mixed at several points in the text, with several repetitions and omissions. The logic structure is at times difficult to follow.

As a general suggestion, I would welcome a major revision of the text. Firstly, the authors could more effectively highlight the original analysis and results of the study, avoid substantial repetitions, and increase the logic structure of the manuscript. Secondly, the use of technical language should be always preferred to common-language expressions (e.g. “temporal changes” vs “dynamics”, see detailed comments later) in all cases where this is possible and the methods should be described in more detail. Some minor analysis and addition to the data presented might improve the manuscript and its relevance in the community.

In the following, I provide some more specific comments, that I believe could improve the manuscript. I hope that the authors can receive the feedback in the in the same spirit in which it was written and for what it is: constructive suggestion to improve the paper.

Some other comments (non-comprehensive):

Page 1, Line 1: Is the front of the rock glacier failing or are the debris accumulated in the gully (below the rock glacier) that are triggered and flow downhill? I write the comment here, but it applies to several other sections in the manuscript. I think that there is a distinction between rock glacier failure, active layer detachment, and debris flow initiation at a rock glacier front. Neglecting the differences between all these processes is in my opinion a first-order approach, which might well be justified in certain contexts, but should not be used in this manuscript, where the focus is on this specific topic.

Page 1, Line 3: What does “multivariate permafrost degradation” mean? What is “retrogressive debris flow”?

Page 1, Line 5: What do you mean by “environmental forces” and what are the “ambiguous conditions”?

Page 1, Line 6: The paper is an application of the methodology presented in Glade and Crozier, 2005. If I understood this correctly, the authors do not establish a basis for multi-hazard assessment but apply an existing method. If contrarily the authors argue that they implemented the methods for hazard assessment, I would welcome a clearer explanation of the original methodology, particularly with regards to the concept of hazard.

Page 2, Line 55: I would argue that destabilizing rock glaciers are defined by the combination of abnormal velocities and the presence of surface features such as cracks and scarps. Several recent papers in the literature provide a more accurate description of rock glacier destabilization.

Page 3, Line 63: reading this line, the expectations to this paper sky-rocketed. Unfortunately, I must say that they were not satisfied by the manuscript, especially with regards to hydrogeological and mechanical aspects (some comments later). With this I don't want to suggest major revisions, but simply to be more precise in the description of the goals of the paper.

Page 4, Line 96: This sentence is in my opinion very controversial. Are the authors stating that the rock glacier originated 170 years ago as a result of the LIA glacier advance? The next sentence quickly describes the surface morphology of the terrain influenced by recent glacier advances. Why not considering the simplest and most-supported hypothesis that the rock glacier pre-existed and was simply overridden by a glacier during the LIA? Anyway, I would simply avoid this argument, which is irrelevant for the discussion of the results and the topic presented in the paper. If the authors want to keep the statement, I would welcome more extensive explanations of their theory.

Page 5, Line 105: how do the authors know that the channels are eroded into the ice core? Are the authors referring to water flowing on top of the permafrost table or to water infiltrating into the ice-core? This is a very interesting topic and if observations are available, they could be presented and discussed in more detail. If no more details are available, I would still welcome a clearer description of the observations (if considered relevant).

Page 6, Line 106: what is a "retrogressive debris flow"? I see here some confusion between "retrogressive erosion" and "debris flow".

Page 6, Line 111 and following: This paragraph is extremely interesting, and I suggest to extend the description (here) and discussion (later) of the observations, the numbers presented, the methodology used to derive them, and their uncertainties. These points might be extended in several locations in the manuscript, maybe replacing some of the repetitions and digressions that are currently present.

Page 7, Line 123: Concurrently means at the same time. The question of the timing is essential for the process described. Please explain in more detail what is known (and how, see previous comment) and what are your hypothesis. Later in the manuscript, they should be tested and discussed.

Page 7, Line 126: it is interesting that local knowledge is taken into account. Still, I suggest to perform a systematic analysis with modern remote sensing data (Sentinel 2 for example) to quantify the evolution of the lake in recent times.

Page 7, Line 139: Are the authors talking about dynamic forces here? Impact from external mass movements? Earthquakes?

Page 8, from Figure 4:

Very interesting observations of the rock glacier front. What's the height of the shear horizon depth here? I would be very happy to see in the published version of the manuscript a detailed description of this area (possibly before and after the event?), which is highly relevant to the debris flow initiation.

A figure showing a simple comparison before-after the event (maybe in appendix?) would be very useful.

What is the thickness of the debris around the lake? And around the front? Panel g and h seem to indicate a debris covered glacier. Would it be possible to provide a more detailed geomorphological assessment of the landform and a map of it?

Page 9, Section 4.1: some of the text presented here might be more suited for the introduction. Also, the analysis of the presented factors on slope stability is not clear to me. I don't see any stability assessment, or somehow I missed it. Maybe a clearer description of the methods might help.

Page 9, Line 165 and following: This is in my opinion an interesting approach, but cannot be introduced so lightly. Please provide more details, assumptions, rationale, and potential limitations. As known, the internal structure of rock glacier is very different from the active layer (also not homogeneous in the active layer!). Assuming the the latter is representative of the first is not valid in my opinion, especially given the photographs presented in figure 4. Were the boulders only collected at the surface? Only evaluating the boulders at the surface creates a bias towards boulders. How were the samples collected at the front? Who went there? Where exactly were they collected? How representative should this be of the internal part of the rock glacier, especially the shear horizon if you suggest that water was flowing there (erosion/deposition at the surface)?

Page 9, line 177: this is not a sufficient description for the data used.

Page 10, line 183 and following: is the use of climate relevant here? Are the authors not limiting the analysis to meteorological forcing? Also, what is meant by "progressive change of climatic factors"? In general, the description of the method is not sufficient for reproduction, and in this sense, it should be improved and extended.

Page 10, Line 210: are the calculations about snow cover validated in any way?

Page 11, Line 218: The statements about the hydrology seems to be results. What observations are available? Please expand the description of the data and methods here. E.g. why until the 13th of August?

Page 14, line 308: Data of surface velocities would be very interesting. Is it possible to present them? Especially relevant if the authors speak about destabilization. Possibly you could also see some signal in ice melt (vertical component – once subtracted subsidence due to dynamics).

Page 17, Figure 8: great figure. I suggest to add the equations to the lines and expand the discussion with regards to failure processes (Lou and Similaun different from this case, no?)

Discussion and Conclusions: I would welcome a more detailed discussion of the original results of the manuscript, limiting long review of previous literature. Same for the conclusions. Maybe some of the comments above can help in the process.