

Interactive comment on “Review article: Inferring permafrost and permafrost thaw in the mountains of the Hindu Kush Himalaya region” by S. Gruber et al.

S. Gruber et al.

stephan.gruber@carleton.ca

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Reply to comments made by Anonymous Referee #1 (doi:10.5194/tc-2016-104-RC1).

We thank Anonymous Referee #1 for their review and suggestions for improvement. Some of the comments were quite general and we hope our interpretation or implementation are to the point.

Referee comments indicated as “RC:”, author reply as “AR:”. Only sections requiring a reply are reproduced.

RC: The manuscript is not a review in a strict sense. It contains passages with textbook contents (e.g. p. 3 and pgs. 8/9), and general speculations based on knowledge from

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other areas. This makes the manuscript interesting reading, e.g. valuable for student courses, and a perfect introduction to a book about Central Asian mountains. I am less convinced about the value in a high-impact scientific journal. It is of course true that we can expect all sorts of impacts if permafrost thaws also in the Hindukush, it is only the question if this statement can be described as “review” or original research.

AR: We agree that the manuscript is largely not a Summary of permafrost research in the HKH. Because there are not enough local studies available, it reviews a larger body of literature and makes inference on what may be relevant in the HKH. We review existing knowledge and provide a synthesis for a new field of application. This, in our mind, is the essence of a review, which otherwise would be a mere summary. During the access review, the editor has already raised this point and we have subsequently changed the title by introducing “inferring”. This should make it more transparent that we are not reviewing work in the HKH but work relevant to the HKH based on current understanding.

RC: However, the manuscript of course contains lots of significant information. Important are the real review part, summarizing the work done for the area by the authors or other colleagues. And of course the discussion of the map by Gruber (2012), which is the only higher-resolution permafrost map for the area, providing a good image of the permafrost distribution of the area. This means, after my opinion, the manuscript is an important contribution, but could be much improved by: 1. Stick to the published investigations, and the map 2. Avoid/reduce substantially the text book passages, explaining basic permafrost/thermal processes etc.

AR: Both points (1 and 2) are best commented together: We believe the value of this text is exactly in the informed interpretation of existing knowledge from outside the HKH to phenomena in the HKH. Some aspects of permafrost science deal with local phenomena, others however, are transferable like other laws in earth science of physics. We believe that (a) the specific composition of these passages is tailored to the HKH and contains a mix of insight derived from both polar and high-elevation permafrost

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research, and (b) the argumentation helps to show what special phenomena we can expect in the HKH and what is already known from other locations. We hope this argument will satisfy this suggestion for improvement without changing the text and also point to the response of Anonymous Referee #2 who found this section useful and important.

RC: 3. Keep the “perspective-part”, which was interesting reading. Besides that “promising methodologies” (p. 13, l. 22) maybe also are developed other places than for the Alps. And, if you relate to other mountain areas (which of course is ok within certain limits), maybe other arid mountain ranges as e.g. parts of the Andes etc. could be included more.

AR: We have slightly modified the sentence and included two more recent and important non-Alpine references: “Promising methodologies for improved simulation in remote locations and mountain areas exist (Fiddes and Gruber, 2012, 2014; Fiddes et al., 2015; Westermann et al., 2015) and offer synergies with efforts in atmospheric sciences (Gutmann et al., 2016; Ménéguez et al., 2013), glaciology, and hydrology.”. In fact, however, most developments in permafrost simulation for mountains did originate from the Alps in the last decade. Concerning other arid mountain ranges, about 15 references to local studies in the Andes, the Rockies, and Central Asia are already used in the text.

RC: 4. I would suggest to add some more illustrations, highlighting important work. Now only derivatives from Gruber 2012 are shown more or less.

AR: It is difficult to add useful illustrations or even photographs without going into much case-specific speculation (Is there permafrost or not? Was permafrost really relevant for this vent?) – or showing just rock glaciers (and even there will be debate as to their status as permafrost landforms). Therefore, we have decided to keep the illustrations of this review rather minimal. We have expanded Figure 2 by three photographs (cf. response to Anonymous Referee #2).

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RC: In summary, I agree that the Hindu Kush Himalaya region is full of “white spots” in terms of understanding permafrost processes there, and that this of course justifies the author’s attempt to focus on this region. But I think the manuscript should undergo a thorough revision, focusing more on the “review” part and less on the “inferring” part.

AR: Thank you, we have done a thorough revision, also in response to the comments made by Anonymous Referee 2 and F. Salerno.

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