

Interactive comment on “The surface energy balance in a cold-arid permafrost environment, Ladakh Himalaya, India” by John Mohd Wani et al.

Giacomo Bertoldi (Referee)

giacomo.bertoldi@eurac.edu

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Review of the Article Tc-2019-286

The surface energy balance in a cold-arid permafrost environment, Ladakh Himalaya, India

by John Mohd Wani, Renoj J. Thayyen, Chandra Shekhar Prasad Ojha, and Stephan Gruber

The paper presents the results of a monitoring and modeling study to understand the surface energy budget in relation to permafrost formation in a little studied environment, as the high elevation dry Himalayan inner range.

C1

General comments:

This is a valuable and interesting paper, which shows an accurate modelling study of the characteristics of the surface energy budget in a poorly studied region, the inner Himalaya. Moreover, in this region permafrost is widespread and permafrost processes relevant for water resources and risks management. The topic is therefore relevant and the paper suitable for TC. The paper shows new observations and applies with good results a hydrological model, which considers explicit water and energy budget, in a cold and dry catchment.

The modelling study is solid and well done. Model validation convincing. Therefore the methodology appears to be sound (I have only one doubt related to water budget).

However, I have several major comments that, on my opinion, should be addressed before publications, regarding the paper organization and the results discussion. I suggest to move the model validation section before the discussion of the results. The reader before wants to understand the model's reliability, and then look to the results on the energy budget. The presentation of the results is rather long and with many repetitions. The main message of the paper is rather simple. In Ladakh mountain the environment is dry, cold and sunny. Therefore, this leads, compared to other sites, to little incoming longwave and more direct solar radiation which helps permafrost. Snow comes relatively late and major differences are related to the snow duration. This could be explained in a more concise way, leaving space for a more quantitative discussion (see specific comments). For the methodology, it is not clear to me if soil moisture is explicitly modelled or not (see specific comment at line 210). This has strong implications on the interpretation of the results. The paper is interesting, but the story is simple. I have the feeling that there are repetitions and details not needed. I think that the paper could be strongly improved if the model is used also for numerical experiments for quantitatively understand role of climate and possible changes for future permafrost development.

C2

Specific comments: see attached document

Please also note the supplement to this comment:

<https://tc.copernicus.org/preprints/tc-2019-286/tc-2019-286-RC3-supplement.pdf>

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-286>, 2020.