Surface temperatures and their influence on the permafrost thermal regime in high Arctic rock walls on Svalbard" by Juditha Undine Schmidt et al.

Cicoira Alessandro (Referee)
alessandro.cicoira@epfl.ch

This manuscript presents a four-year time series of eight temperature loggers at rock cliffs in the surroundings of Ny- Ålesund. The authors use the model CryoGrid 3 in order to discuss the measurements and resolve the influence of the different components on the energy balance on the observations. In addition, the model is combined with three different representative concentration pathways in order to predict the evolution of rock cliffs temperature and active layer thickness throughout the next century.
The measurements advance our knowledge of the energy balance at rock cliffs in the Arctic, and the model is useful for their discussion. However, I have some concerns regarding the manuscript. In my opinion, the temperature measurements are not up to the state of the art, and the modelling work is promising but could be largely integrated with more simulations: adding the two parts is still not sufficient for a publication.

Answer to the author response:
I provide two main answer to the author response. One with regard to the measurements, the other one regarding the structure of the paper and the modelling study. The other minor comments have been implemented and I don't need to add on them. In general, I consider my comments answered and the manuscript suitable for publication.

Regarding the measurements: iButtons are, despite great limitations, a valuable tool for field studies in difficult environments and I have myself used many during the years. When I mention the "state of the art in the field" I was referring already to the use of iButtons. So, I am not commenting the use of other technologies. In my experience with the iButtons, more than a logger is needed for each location (ideally three to have a standard deviation), calibration is essential (with a minimum of two points, possibly using a cold bath with mixing devices for the 0°C) and the accuracy of the instrument can be set up (according to the model between 0.125 and 0.5 or between 0.06 and 0.5 °C). Having quickly stated what the standard would be for me, I understand that in other environments and geographical settings constrains of every sort can limit the possibilities during programming and deployment. Also, I understand that the measurements have been done already and cannot be modified anymore: this is why I suggest not to change the sampling strategy but to explain it in more detail. This has been done in the author answer to my comments, and partially in the revised manuscript. Personally I find this point essential and suggest to extend the discussion on this point in the manuscript.

Regarding the modelling: my comments have been integrated mostly in the appendix with some small mentions in the text of the manuscript. In particular, my major review has been accommodated by the sensitivity study, while my (personal) suggestion to reshape the study has been correctly argued against and the original structure and idea of the paper has been maintained – I believe improved from the reviews.

With these two short comments I suggest the paper for publication in TC.

Best Regards,
Alessandro Cicoira