

Interactive comment on “Modelling rock wall permafrost degradation in the Mont Blanc massif from the LIA to the end of the 21st century” by Florence Magnin et al.

Anonymous Referee #2

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The article by Magnin et al. entitled "Modelling rock wall permafrost degradation in the Mont Blanc massif from the LIA to the end of the 21st century" addresses an important subject relevant to various stake holders in high alpine environments. The authors have collected valuable data by undertaking considerable field work in a harsh environment. They also present 2D model results of long term simulations of the thermal state of three mountain top permafrost sites as well as one ERT transect of the north-west facing wall of the Grands Montets.

However, from my point of view the work does not make a significant contribution neither to permafrost modelling nor to the understanding of high mountain permafrost. The language is confusing throughout large parts of the manuscript. Further, the dif-

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ferentiation the authors seem to make between rock wall permafrost and high mountain permafrost in general - which is especially evident from the choice of the cited literature - is not convincing to me. Are there different hydro-thermal processes acting in rock walls than at less exposed sites or are they just active in different proportions? It is just stated that rock wall permafrost "is a relatively simple system" and it is further assumed to be homogeneous and saturated. The simplified assumption, that radiation and air temperature are sufficient for the simulation of such a system, especially if one of the relevant research questions in this context would be to gain insights on the triggering of future rock fall events (which are almost certainly not just a thermal phenomenon and would not occur in a homogeneous matter) - in my eyes - is to be considered as inadequate. It is also stated, that only air temperature was used to drive the model, which is in contradiction to this already simplified assumption. The question also arises where the water is coming from, or going to, when freeze-thaw processes occur in the model?

I would encourage the authors to rewrite and restructure large parts of the manuscript and to address these questions as well as to refer to additional relevant literature on the subject. The uncertainties arising from the chosen simplifications should be discussed in this light and the conclusions drawn should also reflect that.

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