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Interactive comment on "Numerical modelling of convective heat transport by air flow in permafrost-affected talus slopes" *by* Jonas Wicky and Christian Hauck

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Dear Jonas and Christian,

First I'd like to offer my apologies for the delay in reviewing your manuscript. Unforeseeable circumstances did not allow me to review your contribution earlier. Once I finally started, I enjoyed reading your paper. I understand that there are significant limitations in modelling actual air movements and related ground temperatures caused by convective heat transfers in coarse talus slopes, the paper provides a good overview of why supercooling is observed in such materials. The paper is understandable and overall only requires some minor modifications. I have added comments and suggestions in the annotated version you find as a supplement file below. Also, please make sure

that you differentiate between natural (density driven) and forced (pressure difference) driven air convection. In such a slope, both types typically occur, however, your paper specifically focuses on gravity driven natural convection. Additional, more general comments:

- Page 3, Line 17: Make sure you add the limitation of modelling natural convection in 1D, i.e. that it isn't possible because you need at least 2-D to model the development of convection cells. It may help to describe why convective cells form and what determines their form.

- In Section 5.2. the Rayleigh number is mentioned for the first time without explanation. Please add it in the introduction and explain its significance when analysing convective heat transfer. You may want to link this to the point above.

- It is not clear why the air space has been included in the model, instead of applying an air pressure boundary conditions and air entry value (to allow air to move into the talus slope or not)

- In terms of material parameters chosen, it is understood that those may not have physical meanings per se, and shouldn't necessary be compared with what one would measure, however, in relation to each other some care must be taken. The following observations were made that may require an explanation by the authors in terms of how those values may affect the results:

o Thermal conductivity of air is much higher that actual thermal conductivity of air.

o Conductivity in air is in essence infinite, i.e. the model setup, where the value for the talus material was chosen to be the same, may affect the result.

- Page 8, line 27: It is not clear what is not homogeneous in this surface layer. If it is pure conductive heat transfer, the results should be homogeneous. If not, it is typically a sign for numerical instabilities.

- In the results and discussion section it would have been helpful to show the results of the convective models in relation to the pure conduction model. This clearly shows where the air convection is the dominant process and to what extent.

- In the limitations a discussion on the snow cover would be helpful, i.e. the limits on how this is model, in particular when holes form that affect the overall air flow through the talus cone.

- Finally, in the conclusions, do not forget the hydrology, i.e. infiltration of water, in particular snow melt. There is additional advection from the infiltration of melt water that can affect the ground temperatures, but is also key when generating a frozen core.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-227/tc-2016-227-RC2-supplement.pdf

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