Response to Reviewer Comments #1 (**Responses in bold**)

Thank you for taking the time to conduct this thorough review and for your constructive comments, which will be used to substantially improve the manuscript. Please find the detailed response to each of your comments below.

1. Dr Heinze presents a local thermal non-equilibrium model for infiltration of water in snow. The motivation is to develop a numerical model to consider the thermal energy related to melting of ice or freezing of liquid water through the snowpack. The non-equilibrium model is interesting. However, it would be worthwhile to compare results from the same thermo-hydraulic scenarios with results from an equilibrium model. It would be useful to evaluate if there are conditions when a simpler equilibrium model is adequate.

   Reply: Thank you very much for pointing me to this lack in the analysis and the obvious reader's interest in the answer to this question. I will include a comparison to an equilibrium model and discuss the potential relevance of non-equilibrium models in dependence of snow properties and boundary conditions.

2. The coupling with the hydraulic conductivity of the snow is rudimentary in that thermo-hydraulic processes are investigated for influxes of water into snowpacks consisting of spherical grains. The author acknowledges that this is not realistic, but this condition could apply during infiltration through 'ripe' snow that has previously been wetted and subjected to grain growth (Colbeck, 1979; Raymond and Tusima, 1979).

   Reply: Thank you very much for pointing me to the condition of ripe snow. I will include this reference and change the text accordingly.

3. However, natural snowpacks are typically layered and heterogeneous; during infiltration, the snow structure and density, and flow fingering often evolve rapidly (e.g. Colbeck, 1979; Marshall et al. 1999; Marshall et al., 2014, Hirashima et al., 2017; Katsushima, 2020; Ohara, 2024). Forecasting impacts of ROS on flash floods and snow avalanches requires modeling thermo-hydraulic processes in natural snowpacks.

   Reply: I fully agree that the presented model is only a first step into applying LTNE models for simulating natural ROS events and related hazards. The focus of this work is to build the mathematical ground work and to develop the physical concepts. Hence, future extensions of the model towards two-dimensions to account for horizontal heterogeneity or even for dual-domain approaches (for rock: Heinze & Hamidi, 2017) are clearly envisioned. For showcasing the strength and ability of the model, the field observations from 15/16th January 1992 presented in Conway & Benedict (1994) will be numerically reproduced using the presented model. Such a simulation result and its discussion will be added to the manuscript. This will demonstrate the ability of the model to account for one-dimensional heterogeneity.

4. Dr Heinze mentions that different snow morphology and layering also need to be considered; you might be interested in a study using a water transport model, a dual-domain approach and a multi-layer SNOWPACK model to study infiltration of water in a layered snowpack. (Hirashima et al., 2018)

   Reply: Thank you very much for pointing me to this very interesting study. Combining the presented thermal non-equilibrium model with more realistic representations of snow hydrology and morphology, also in the context of a dual-

domain approach, are surely necessary for the simulation of realistic events. The reference will be added with a small extension of the discussion into the manuscript.

References:

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