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 has been used to substantially improve the manuscript. Please find the detailed response to each of your comments below. Referenced lines refer to the manuscript with marked changes.

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 added the simulation of the field observation by 15/16th January 1992 presented in Conway & Benedict (1994) to the manuscript in section 3.1 because

   (1) The heterogeneous snowpack described by Conway & Benedict (1994) allows to showcase the model's ability to incorporate that.

   (2) The comparison to actual field data, a thermal equilibrium model of the same event and a simple analytical approach strengthens the trust into the model and the simulation results while at the same time demonstrates the improvement of the newly developed model compared to conventional approaches.

   It becomes evident that a warmed snowpack (close to 0°C) with no thermal gradient can be described just fine with a simpler equilibrium model. However, in the presence of thermal gradients within the snowpack, especially if layering hinders water infiltration, the developed model provides an improved representation of the thermos-hydraulic state of the snowpack.

   For the respective changes please see the added subsection 3.1 (starting l. 285) with three new figures (Fig. 1-3), as well as the revised Discussion (starting l. 525) and the new Conclusion section.

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. The discussion and the respective references have been added in section 2 in lines 243-248.

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. Please see my reply to comment #1 regarding the current model's ability to account for 1D heterogeneity.

Based on your comment, I extended the respective discussion of this important feature. Please see section 4.4 starting l. 587.

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.

   Please see my response above to the respective changes in the manuscript.

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Response to Reviewer Comments #2 (**Responses in bold**)

Thank you for taking the time to conduct this thorough review and for your constructive comments, which have been used to substantially improve the manuscript. Please find the detailed response to each of your comments below. Referenced lines refer to the manuscript with marked changes.

The author presents an up-to-date theoretical model of meltwater infiltration in snow and soil assuming a thermal non-equilibrium (TNE) between the vapor, water and ice phases. The modelling approach, tested in numerical experiments, is novel and interesting but experimental evidences to support the theoretical assumptions and the improvements in the modelling approach are missing. They would provide a significant added value to the research. Otherwise it is not very clear which is the added value of the modelling approach in terms of simulating the actual water and heat dynamics into the snowpack and the frozen soil. Therefore, if experimental data are not provided to support the model's hypothesis at least some simulations under the hypothesis of thermal equilibrium (TE), showing the differences between the TNE and TE assumptions, and simplified traditional hypotheses of advective heat transfer available for melt M (melt rate) as M=PT/80, with P being the Rain on snow intensity, T the air temperature and 80 the ratio of specific heat capacity of water and latent heat of fusion, are recommended. In this way the improvements introduced by the model would be more evident.

Reply: Finding a suitable experimental data set for quantitative comparison with the numerical model is difficult due to the current limitations of the model (1D, no preferential pathways) and its required input data (hydraulic parameters, thermal boundary conditions, etc.). Generally, measuring separate phase temperatures in snow seems experimentally challenging.
To showcase the strength and ability of the model, the field observations from 15/16th January 1992 presented in Conway & Benedict (1994) are numerically reproduced using the presented model and the results were added to the Results section as subsection 3.1. This dataset has been selected due to its well-documented rainfall conditions and the thermal as well as hydraulic propagation within the snow allowing to match needed parameters accordingly. Also, the relevant snow types are described as partly rounded or rounded grains fitting the theoretical assumption of the model.
The same field observation has been used to compare the model results with a thermal equilibrium model, the simple analytical approach you presented above and to study the effect of rainfall intensity (see your comment below).

Altogether, the simulation of this specific ROS events showcases the model's ability to incorporate 1D heterogeneity of the snowpack, strengthens the trust into the model and the simulation results while at the same time demonstrates the improvement of the newly developed model compared to conventional approaches.

It becomes evident that a warmed snowpack (close to 0°C) with no thermal gradient can be described just fine with a simpler equilibrium model. However, in the presence of thermal gradients within the snowpack, especially if layering hinders water infiltration, the developed model provides an improved representation of the thermos-hydraulic state of the snowpack. Please see the new results section 3.1 (starting l. 285), the

restructured discussion section (starting l. 525), as well as the newly added Conclusion section (starting l. 624).

Some key references are missing as suggested in the review.

Line 30 Literature in the 70s and 80s posed the bases for multiphase snowpack dynamics and meltwater infiltration into snow. I added some fundamental references (Colbeck, 1972, 1978: Colbeck and Anderson, 1982; Dunne et al., 1976; Morris, 1991; Akan, 1984a, 1984b) that cannot be neglected, also in view of the model's parameterization and verification with experimental data.
Line 35 About soil freezing and thawing I would refer also to Leuther and Schlüter (2021)

Reply: Thank you very much for pointing me to this relevant literature, which has been included in the literature review. Please see lines 29-37 & 44-45 in the Introduction.

Line 52 I would spell out LTE Local Thermal Equilibrium

Reply: Thank you very much for pointing me to this lack of introducing the abbreviation. Of course, this has been changed. Please see line 62.

Line 75 I suggest to give some more references about the capacity of the van Genuchten model (developed for soils) to explain water saturation-hydraulic head relationship also for snow.

Reply: Thank you very much for pointing out this lack of references for a critical component of the hydraulic model. Please see the extended discussion in lines 87-96.

Line 125 Explain better the assumption about a similar flow velocity for air and infiltrating water. Water is forced by gravity and capillary forces that cannot be treated in the same way for air.

Reply: This simplifying assumption of equal flow velocities is a consequence of other assumptions made, such as the incompressibility of water and air, the capillary tube model, and the exclusion of mixture flow within one capillary tube. Hence, if water replaces air during infiltration, conservation of mass requires the same flow velocity if the tube diameter does not change. Naturally, in a 3D reality the flow paths of the air are complex and through various capillary tubes which cannot be represented here. However, due to the negligible thermal influence of the air, this simplifying assumption has no impact on the simulations' outcome.
Based on your comment, the explanation has been extended. Please see lines 145-150.

Line 130 Specify the meaning of subscript ij (the 3 phases of water?) for Qij, hij and Aij

Reply: Thank you for pointing out the missing explanation. The subscripts ij indicate the involved phases, which exchange heat. The respective explanation has been added to the text and usage of subscripts was checked and modified again for consistency. Please see lines 154-156.

Line 212 In Table 1. Ice density is assumed 917 kg/m3 a value generally adopted in the literature. Why is ice density assumed 940 kg/m3 at line 212?

Reply: This is indeed a mistake, which has been corrected. Throughout the manuscript, the ice density has been set to 917 kg/m^3 and used accordingly.

Line 212 The assumption of a spherical shape for snow crystals with low density as 0.1 kg/m3 is not very realistic as for that density a dendritic shape of snow crystals is more appropriate. Which are the implications of this assumption for the model proposed?

Reply: The assumption of spherical snow grains is obviously a strong limitation of the model but enables a consistent mathematical formulation also accounting for growth and decline of snow grain diameter. The spherical shape is used in the model to calculate the surface area of the snow for the heat exchange terms. The linear dependence of this is shown in equ. 13. Hence, more surface area increases the heat transfer across that surface. Also, estimations of the infiltration behavior (vanGenuchten parameters) taken from literature consider spherical grains. As these are empirical equations, implications on the hydraulic side are difficult to assess.

Based on your comment, a respective explanation has been added. Please note a mistake in the original manuscript. Snow densities considered are 100 – 800 kg/m^3 not 0.1 – 0.8. Please see lines 242-249 for the changes.

Line 230 The explanation of the mechanical compaction of snow needs to be better explained.

Reply: The mechanical compaction is partly based on the weight of the snow and the rainwater infiltrating but there are also changes to the crystal and grain structure (Marshall et al. 1999). Melting might occur, as seen in the simulation result discussed here, in deeper layers of the snowpack and not necessarily at the surface. Hence, changes in the snow pack structure might cause collapse due to the load above. An extended explanation has been added to the manuscript. Please see lines 264-270.

Line 250 A rainfall depth of 0.1 m is assumed but over which time period does rainfall occur? Then it seems that in the modelling approach a constant hydraulic head of 0.1 m holds at the top boundary (see figure 1, 2, 3, 4, 5, 6, 7). Is this the head of a constant water depth (totally unrealistic) or does it include the capillary head?

Reply: The boundary condition of 0.1m constant water head was chosen in analogy to laboratory experiments on frozen soils (e.g. Hansson et al., 2004). This constant pressure head boundary condition leads to a non-linear infiltration pattern into the unsaturated snow which might not be fully representative for a natural rain event and might lead to infiltration rates of more than 40mm/hour, which are comparably high for rain-on-snow events (cf. Juras et al., 2021) but were used in rain-on-snow experiments (cf. Yang et al., 2023).
Based on your comment, the investigation of varying rainfall intensities has been added to the manuscript in the results section 3.1.
Also based on this comment, the description of the top boundary conditions has been clarified in the text. Please see lines 274-277 & 372-373.

Line 254-292 This numerical simulation is interesting. But how would the melt

Reply: Sadly, your comment was abbreviated in your review.

Discussion. Some discussion about perspectives of the modelling approach to test its results for instance testing its results with measurements of snowpack properties and passive microwave monitoring of the freezing/melting processes as in Cagnati et al. (2004) would be useful.

Reply: Thank you very much for this comment and pointing me to the respective references. Such a data source, ideally at a high spatial resolution of 5 cm or less, would be greatly valuable to further constrain the heat transfer processes in the model. However, for comparison with long-time monitoring data, the model would probably also need to include more processes on the boundaries as well as internally (compaction, snow metamorphosis). Based on your comment, a systematic discussion of arising possibilities to compare the model with respective monitoring techniques has been added to the manuscript. Please see lines 572 – 586 in the new respective subsection in the discussion.

How would the infiltration fluxes change if a hydraulic head of 0.001 m is assumed at the top boundary? The top boundary hydraulic head conditions are not very clear (see comment to line 250).

Reply: Please also see my reply to your comment above. The effect of varying rainfall intensities has been added to the Results section 3.1.

Line 408. If experimental data are not provided to support the model's hypothesis at least some simulations under the hypothesis of thermal equilibrium (TE), showing the differences between the TNE and TE assumptions, and simplified traditional hypotheses of advective heat transfer would be useful.

Reply: Please see my reply to your earlier comment regarding this suggestion. A comparison to field data, to a thermal equilibrium model and to a traditional analytical model, as suggested by you above, has been added to the manuscript in results section 3.1.

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