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 will be used to substantially improve the manuscript. Please find the detailed response to each of your comments below.

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. Still, to showcase the strength and ability of the model, the field observations from 15/16th January 1992 presented in Conway & Benedict (1994) can be numerically reproduced using the presented model within the before mentioned limits and assuming a suitable set of parameters. Such a simulation will be added to the manuscript. This will also demonstrate the ability of the model to account for one-dimensional heterogeneity. 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. For future work, there is hope that with the presented work, further experimental research addressing potential thermal non-equilibrium situations in snow might be initiated and can subsequently be used to further constrain the model.

In general, the benefits of the presented model compared to conventional approaches are an improved process-understanding of the thermo-hydraulic processes within the snowpack, the possibility to investigate the influential parameters for local freezing/melting conditions and a consistent mathematical formulation of boundary conditions without the a-priori simplification of thermal equilibrium of all phases. I believe that challenging the simplifying assumption of instant thermal equilibrium between phases for rain-on-snow events is a valuable approach in itself based on the initial thermal non-equilibrium condition. As suggested, a comparison of the newly presented model to a thermal equilibrium model will be added to the manuscript to investigate relevant conditions of LTE/LTNE, which will help to clarify when the

explicit description of heat transfer is essential for the processes within the snowpack and when it can be omitted.

Please note that the proposed traditional method of M=PT/80 is assuming that the snow is close to melting point already. In the presented simulations that is not necessarily the case, so attention needs to be paid to apply this method only for the melting stage. Nevertheless, comparing model results and the analytical method will demonstrate the agreement of the novel model and traditional approaches for relevant macroscopic quantities and will be added to the manuscript.

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 will be included in the literature review and added to the reference list.

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 will be changed.

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. The references (Jordan et al., 1995; Yamaguchi et al., 2010; Yamaguchi et al., 2017) will be added to the manuscript and shortly discussed.

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 of the assumption will be extended.

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. Hence, ij represent the possible heat transfer combinations solid-liquid (sw), solid-air (sa), liquid-air (wa). The respective explanation will be added to the text.

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 will be 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 will be included in the respective paragraph.

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 will be added to the text and the references (Bertle et al., 1966; Marshall et al., 1999; Meyer & Hewitt, 2017; Barraclough et al., 2017) will be included

in the manuscript. Please note that the model itself does not account for mechanical compaction.

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). The investigation of varying rainfall intensities is an interesting point, that will be addressed in the manuscript by adding additional simulations with varying boundary conditions changing the top boundary condition from a constant head to an infiltration condition. A smaller rainfall intensity leads to smaller amounts of rainwater entering the snowpack delaying warming of the snow and melting.

Also based on this comment, the description of the top boundary conditions will be clarified in the text.

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 will be added to the manuscript.

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. This is an interesting point and will be addressed by adding simulations with a top boundary condition varying rainfall intensity to study the effect of different precipitation events.

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, will be added to the manuscript.

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