We thank the reviewers for their time in reviewing our paper. We provide responses to each individual point below. For clarity, comments are given in italics, and our responses are given in plain blue text.

G. Zegers and colleagues enhanced GEOtop-3D, a well-known physically-based distributed hydrological model, to include the thermal effects of air convection and local thermal nonequilibrium (LTNE). Air convection exerts a cooling effect and critically shapes the permafrost distribution at landform scale, the scale needed for hydrological modelling in complex high-mountain catchments. The freely available model is (to our knowledge) the first geoscientific model that includes two-phase energy balance equations, four phases (rock-ice- water-air), and an explicit treatment of air and water flow. This is a valuable and important contribution and presented together with a very concisely written publication. The modelling is well connected to field observations and to the geomorphological literature where undercooling has been known for centuries. We have only few points to criticize.

We appreciate the supportive comments.

A major point is that the authors did not split in their paper results and discussion. This makes it difficult for the reader to see the results clearly separated from the interpretations. This is particularly true for Chapter 4.4 where the authors suddenly discuss the hydrological implications of permafrost on discharge. It would be nice to have the hydrological outcomes of this paper much better included in the whole paper, starting in the introduction and then also in the results and discussion section. We will give more details below (referring to L439).

We will address this concern in the detailed responses below and will add a paragraph to the Introduction to better introduce the hydrological aspects. We will also revise Section 4.4 to include additional detail and improve clarity in the interpretation of the results.

Another point is that below-ground heat transfer by thermal radiation is ignored, which can be significant for cobble to boulder-sized rocks without fine sediments that would clog the large pore space. This has been shown in geotechnical experiments by Fillion et al. (2011) ("Radiation heat transfer becomes significant for d10 higher than 10 mm and predominant at values higher than 90 mm") and in the field by Scherler et al. (2014) and Amschwand et al. (2024). Radiation increases the stagnant thermal conductivity of the solid composite material. It does probably not change your main findings significantly, though, because the effective thermal conductivity values of ~1 W m71 K-1 would be again closer to those of the standard GEOtop implementation. Nonetheless, because large pores for radiation and high permeability for strong convection are likely to occur together in rocky debris, the issue should be briefly discussed (e.g ., in the Section at L191-218).

We agree that incorporating thermal radiation could enhance the representation of the energy balance. However, it is beyond the scope of the present work. Accounting for thermal radiation in the current model setup is particularly challenging due to the presence of four distinct phases (sediment, air, water, and ice), all of which need to be considered when estimating radiative heat transfer. We will include a brief discussion of thermal radiation in Section 2.3.2, where thermal conductivity calculations are introduced.

A few minor points are listed below.

Minor points L13: Please consider rephrasing to " ... local thermal equilibrium approaches underestimate the impact of natural convection especially on short timescales."

We will accept the suggestion and rephase L13 to " ... local thermal equilibrium approaches underestimate the impact of natural convection especially on short timescales."

L28: Please change the reference from Riseborough et al. 2008 to a more specific reference like Van Everdingen, R. O ., 1998.

We agree, we will change the reference to Van Everdingen, R. O ., 1998.

L28/29: ... 'positive mean annual air temperature' this information was already explored far before the year 2000 as it is shown in your references. Already in the 1930s local researchers in Switzerland and elsewhere (e.g. Bächler 1930s) found such effects at certain places and in the early 1990s this was already scientifically described (e.g. Hoelzle 1992).

Thanks for the suggestion. We will include those references in the revised manuscript.

L40: You may add: https://doi.org/10.1002/esp.5998 (Wiegand & Kneisel ., 2024)

We will include the suggested reference.

L59: Suitable reference to add (and maybe to discuss) is Wicky et al. (2024), who also modelled air convection in a talus slope with a similar approach. Furthermore, please mention the works by Marchenko (2001) (a two-phase LTNE, 1-dim model where Rayleigh convection is parameterized), and Tanaka et al. (2000).

We will mention Wicky et al. (2024), Marchenko (2001), and Tanaka et al. (2000) in the revised manuscript.

L112: Please consider rephrasing: " ... reducing the amount of energy gained by the ground from the atmosphere (Gruber and Hoelzle, 2008)"

We will rephrase to: " ... reducing the amount of energy gained by the ground from the atmosphere (Gruber and Hoelzle, 2008)"

L127: "Fw as a sink term representing evapotranspiration": Please clarify by saying that Fw affects the surface cells only (correct?) and is derived from the surface energy balance.

Fw can act in the first layers of the sediments. Particularly transpiration can go into deeper layers of the sediments depending on the model configuration. We will clarify that in the revised manuscript.

L165: You could consider Amschwand et al. (2024, Fig. 4) for the closure of the snowpack.

We will include Amschwand et al. (2024) in the mentioned references.

L170: Please consider rephrasing (see major point): "The energy transport equation in the air phase combines advection and conduction processes, but it does not account for the thermal radiation of coarse sediments". We suggest not to mention thermal radiation in the paragraph on the air energy equation (because air is essentially IR-transparent at such short distances), and to discuss it instead in the paragraph on the composite-medium conductivity (Sect. 2.3.2).

We agree with the suggestion. We will mention thermal radiation in the paragraph on the composite-medium conductivity

L278: In the description of the Babylon talus slope, please mention the typical block size.

We will include in the description that:

”The talus is predominantly composed of cobble to boulder-sized rocks and lacks fine sediments”

L377: " ... air temperature, incoming longwave radiation, and cloud fraction (estimated from incoming shortwave radiation data (Long et al ., 2006))". It is not clear here how your input data is organized. Which radiation components have been measured, short-wave as well? Why did you use cloud fraction instead of the shortwave incoming radiation directly?

Because incoming solar radiation is not constant in the domain and it depends on the shadow effect (Section 2.3.4), measured income solar radiation cannot be used directly. Instead, incoming solar radiation is used to estimate hourly cloud fraction and estimate incoming solar radiation to each pixel of the model based on the hourly sun position and estimated cloud fraction. This will be explained in the revised manuscript.

L439: While Chapter 4.3 follows naturally from the previous discussion (permafrost distribution), Chapter 4.4 about the effects on discharge appears a bit sudden. A statement at the beginning of Chapter 4.4 or in the introduction about the links between ground thermal regime, ice content, and hydraulic properties (permeability) would make the transition clearer.

We will include a paragraph in the introduction referring to permafrost and subsurface water flow:

“The presence of permafrost directly influences subsurface water flow. As soil reaches freezing temperatures, water becomes ice, hindering or blocking flow depending on the ice content. Permafrost generally acts as a confining layer, creating two main flow paths: near-surface flow above the permafrost (supra-permafrost) and deeper subsurface flow (sub-permafrost) (Giardino et al., 1992; Harrington et al., 2018; Langston et al., 2011; Woo et al., 1994. However, when permafrost is discontinuous or contains low ice content, intra-permafrost flow may also occur. Regarding the contribution of ice melt to streamflow, several studies have shown it is not highly significant in alpine landforms (Arenson and Jakob, 2010; Harrington et al., 2018; Hayashi, 2020;

Krainer and Mostler, 2002). Thus, the primary function of permafrost in alpine hydrogeology is to control the subsurface water flow paths.”

While the interpretation of water temperatures (Fig. 12B) follows straightforward from the previous discussions, the discharge (Fig. 12A) is hard to interpret solely with the concepts presented in this paper. For example, the discharge is flashier in the CG-AO (no permafrost) model run than in the CG (permafrost) model run ("in the case with permafrost, discharge minimums tended to be higher, and the discharge maximums lower ,... "), which was attributed to refreezing and lower hydraulic permeability (L454). First: is this a conjecture or a model output?

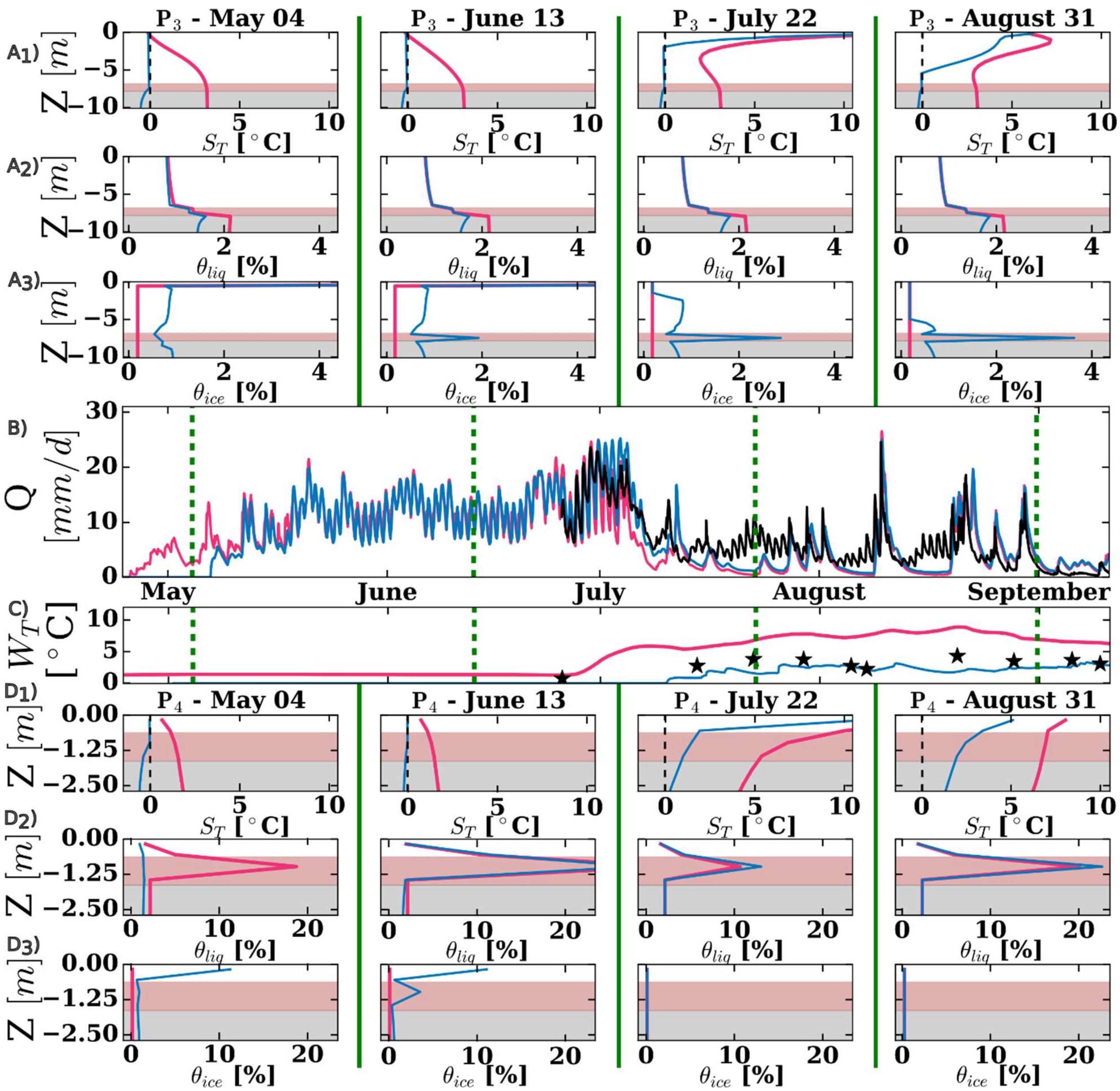
Do these findings refer to deeper intra-talus water pathways? Please clarify, perhaps explaining (or showing) the different water pathways in the frozen/non-frozen talus.

Second, often the opposite is true, supra-permafrost runoff being more rapid and flashy than runoff in a non-permafrost catchment.

This conclusion is based on an interpretation of the model results. The findings specifically refer to intra-talus water pathways. In the analyzed case, there is no supra-permafrost layer because the simulated permafrost is discontinuous with low ice content rather than a continuous body. As a result, there is no supra-permafrost flow. Instead, groundwater within the talus flows from more conductive layers toward less conductive but more saturated zones near the bedrock.

During June for the CG case, the snowmelt and infiltration led to an increase in ice content within the lower conductivity layer at the middle section of the talus (P3 profile). This increase is illustrated in the following figure (provided in the Appendix).

We will clarify in the text that we are referring to intra-talus flow.



L597: The links for the Leontini reference do not work.

We apologize for the error in the original reference. The correct reference is "Su, Y., & Davidson, J. H. (2015). Modeling approaches to natural convection in porous media. Springer." https://link.springer.com/book/10.1007/978-3-319-14237-1

L699: Please briefly comment on: How strong is the influence of moisture on the air density, and would taking the virtual temperature (or even the virtual potential temperature) improve the modelling?

Moisture in the air has a minimal effect on air density at temperatures below 20 °C; therefore, including it would not significantly impact the simulation results. This will be clarified in the revised manuscript.

Figure 2: The figure is helpful. Please clarify that the CM energy balance is 1D vertical, or maybe draw 3D cubes instead of 2D squares.

Yes, the CM energy balance is 1D vertical. We will change the figures to 3D cubes to show a clearer description.

Figure 3: It is not clear why you do not use your GEOtop model to do the shortwave incoming radiation calculation as you explain also in the text (L311).

You are right that the radiation calculation can be done directly with the GEOtop model however, the PISR values in this figure are shown as a qualitative analysis and cover a larger extent than one the employed GEOtop model. Nonetheless, the radiation calculations done by using SAGA GIS do consider the shadow effect in incoming radiation.

In addition, your BTS-class - 3°°C<BTS <- 2°C is not shown in the figure, but maybe there are no measurements within this BTS- class, please explain?

Yes, there are no measurements within the BTS-class - 3°°C<BTS <- 2°C.

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