Review of G. Zegers et al. (2024)
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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. 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).

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 d_{10} 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 m^{-1} 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).

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."

L28: Please change the reference from Riseborough et al. 2008 to a more specific reference like 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).

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

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).

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

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

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

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).

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

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?

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

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

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?

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.

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). 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?

References

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