Anonymous referee #1

We thank the referee for his careful reading of the manuscript, which has been modified according to his remarks. In the following, the referee comments are in bold characters, the modifications made in the manuscript are in italic.

I am generally happy with the changes made by the authors. I think the paper is more accessible, the approach is clearer and explained in a more pedagogical way. I have only very minor suggestions that I would like to take into account, but otherwise I think the manuscript is generally ready for publication.

L132 "effective properties"

Does this mean model variable? Is it a common denomination?

"Effective properties" refer to the physical properties to be considered in the simulation. The term "effective" means that they may depend on some physical assumptions made in the governing equations (e.g., the Boussinesq approximation or the specific model used for the change of state). This is a common terminology. We modified the sentence:

"No subscript indicates the effective physical properties to be considered in the governing equations of the water domain (solid, liquid and diphasic)."

L174: Maybe repeat the reference of Comsol (2018) in the figure caption.

Done.

L193: Here again I would insist, "[they]investigated with an experimental approach" or something similar. To remind the reader that it's a comparison with experimental observations.

Corrected:

"Virag et al., (2006) investigated with an experimental approach the effect of free convection on …"

L209: "4.1 Stagnant liquid water (SLW) versus free convection (FC)" not formatted as a title

Thank you for pointing this out. We corrected it in the manuscript.

L218: "an extreme slope" the wording is a bit peculiar, it suggests something crazy is happening. Maybe reword.

The corresponding paragraph has been reworded as follows.

"When convection is disregarded (SLW), the melting front is nearly horizontal except close to the walls, where a steep slope is observed. The higher thermal diffusivity of the rock ($\alpha_r \approx 8.8 \times 10^{-7}$ m^2/s) compared to that of the liquid water ($\alpha_l \approx 1.3 \times 10^{-7}$ m^2/s) results in faster heat propagation in the rock, and enhanced melting of the ice closer to the rock. When free convection is considered (FC), the advection of heat by the flow results in faster propagation of the melting front, with an inversion of its curvature (the meting front propagates faster in the center of the cleft than close to the walls)."

L231-232 “This shows that the circulation of water inside the cleft results in a thermal bridge between ice interface and top atmosphere.”

Isn’t it exactly the point of including free convection? It’s in the model in order to do that no?

We modified the sentence: *“This confirms that the circulation…”*

Figure 9: It is a relatively common practice to "turn" graphics which have depth as an axis so that depth is vertical.

We followed the reviewer’s suggestion and modified the figure accordingly.

L258: “heat flux propagates perpendicularly to the cleft”

So horizontally? I don't visualize.

We tried to clarify this point as follows.

“For the SLW case, heat transfer is mainly driven by diffusion in the rock, which has a greater diffusivity than water (see the temperature contour in Fig.8a). At a given depth, the rock is warmer than the water. The ice directly in contact with the rock thus melts faster. This explains the larger specific melting rate obtained with the smallest aperture (A_p=2 cm)”

L319: I would add “similar to the soil temperature increase” when describing the T forcing

We modified the sentence as follows:

“To assess the melting rate, we considered the same conceptual model as in Fig.1. A linear temperature rise from 0 to 5°C during 4.5 days was assumed at the top boundary (black dashed-dotted line in Fig.12b). This temperature rise is similar to the temperature evolution measured in the soil (solid black line in Fig.12b).”

L327: “The cleft is subject to a linear temperature rise” redundant to line 318-319 if I’m correct.

We deleted this redundant sentence.

L333-344 do not discuss anything, they wrap up the study like a conclusion I feel.

This paragraph has been moved in the conclusion, which has been reorganized as follows:

“A quantification of the melting rate of ice-rich permafrost in heterogeneous media is essential to assess the speed of permafrost degradation. Our model relies on a 2D approach coupling free convection (buoyancy-driven flow) in a vertical cleft with conduction in the surrounded homogeneous rock. Increasing the temperature of the ground surface can generate free convection cells because water-density increases between 0 and 4°C (a property specific to water). The convection cells generate a thermal bridge between the atmosphere and the melting front, resulting in the formation of a mixing

zone with quasi-uniform temperature in the water column. This dramatically enhances the melting rate of interstitial ice when compared to models assuming stagnant liquid water (about an order of magnitude after 9 hours for an aperture size of 10 cm). In contrast to scenarios assuming conduction in stagnant liquid water, for which the temperature signal from the atmosphere is fully attenuated beyond a certain distance known as the diffusion length, the presence of free convection extends over greater distances. This thermal penetration also exerts an influence on the surrounding rock. Despite simplifying assumptions in the model and many uncertainties about the cleft geometry and the measured water flow rate, melting rate predicted by a model including free convection fit the order of magnitude measured in Monlesi cave (Fig. 12).

The significance of free convection should also be estimated in similar thermal configurations with different geometries such as cylindrical conduits or 3D cavities. Furthermore, the effect of free convection is not limited to hourly or daily oscillations and can be studied over much longer timescales, including centennial to millennial fluctuations. Currently, the computational costs are the main barriers for including free convection in long-term simulations. The full coupling of the momentum and energy equations requires the time steps being much smaller compared to simple conduction-based models. Further investigations are thus ongoing to reformulate the governing equations and simplifying them for simulations over longer time-scales. Moreover, refreezing processes have yet to be considered to fully represent the long-term evolution of such a system.

Eventually, the effect of water free convection on ice melting rate is not limited to permafrost regions. For instance, the melting of icebergs can also be impacted by water free convection (Couston et al., 2021; Hester et al., 2021) increasing production of freshwater in oceans with potential impacts on the climate at global scale."

L374: "Forced convection"

Would it be forced convection if the water comes from outside the domain? I would have said that forced convection happens when you stir in one given place, not when you bring water from somewhere else, I would call it advection. I am happy to be proven wrong though.

We feel that forced convection is correct in this context. In the field of heat transfer, "forced convection" (the flow is due to an external force) is opposed to free convection (the flow is due to buoyancy). The term "forced convection" applies equally to opened systems (as heat exchangers) or closed systems (as stirred-tank reactors). See for instance Bergman et al., (2017).

L403: Figure A1. Effect on the melting temperature range $\boldsymbol{\Delta T}$ of the melting?

Corrected.

Reference

Bergman, T. L., Lavine, A. S., Incropera, F. P., and DeWitt, D. P.: Fundamentals of Heat and Mass Transfer, Wiley, 2017.

Couston, L.-A., Hester, E., Favier, B., Taylor, J. R., Holland, P. R., and Jenkins, A.: Topography generation by melting and freezing in a turbulent shear flow, J. Fluid Mech., 911, A44, 2021.

Hester, E. W., McConnochie, C. D., Cenedese, C., Couston, L.-A., and Vasil, G.: Aspect ratio affects iceberg melting, Phys. Rev. Fluids, 6, 23802, 2021.

Anonymous referee #2

We thank the referee for pointing out the passages of the manuscript which require clarification. We hope that the clarity of the revised version has been improved. In the following, the referee comments are in bold characters, the modifications made in the manuscript are in italic.

The authors have sufficiently addressed many of my comments on the original manuscript. However, there are still a few issues that have not been addressed in a satisfactory manner. I feel that these issues need to be addressed before the manuscript is considered for publication in the journal. Please see my specific comments for the remaining issues. The line numbers indicate those in the original manuscript.

SPECIFIC COMMENTS

Line 91. This was the comment concerning the volume change during ice-to-liquid transition. Eq. 2 in the revised manuscript still assumes no volume change in water, implying that the change in volume associated with ice-to-liquid transition is neglected in the analysis. The authors presented an explanation for how this may be interpreted in their response, but it is not explained in the text. Omission of volume change is a major assumption in the system equation, and it needs to be explicitly justified in the text. Please add a convincing explanation to the texts.

Regarding volume changes, there are two distinct assumptions (independent from each other):

1) We neglect the change of volume induced by ice melting (i.e., the difference of density between ice and liquid water). We show in section 2.3 that the validity of this assumption depends on condition (14), and that this condition is satisfied for the problem that we investigate. We also provide a reference (Heitz and Westwater, 1970) showing that this assumption has no significant effect on the results, in a configuration similar to ours. We believe that the validity of this assumption has been fully justified in the text.
2) For the liquid phase, we apply the standard Boussinesq approximation, which consists in assuming constant liquid density in all the terms of the governing equations (including the mass conservation equation (2)), except in the buoyancy term of the momentum balance. Although this approximation may appear to lack rigor, it is very classical and widely used for modeling of free convection. It has been first stated by the French physicist Joseph Boussinesq at the end of the 19th century, and its self-consistency has been mathematically demonstrated later (see for instance the reference Spiegel and Veronis (1960) added in the revised version).

In order to clarify these points, we reorganized the passage of section 2.3 that deals with volume variations:

"The standard Boussinesq approximation is assumed in the liquid phase (Spiegel and Veronis, 1960) (Bejan, 2013). This approximation, widely used for free convection modeling, consists in assuming constant liquid density in the governing equations Eqs.(2-4), except in the buoyancy term of the momentum balance equations Eqs.(3-4). The thermal expansion coefficient β at the origin of buoyancy is estimated from the relation

$$\beta = -\frac{1}{\rho_l}\frac{d\rho_l}{dT} \qquad (11)$$

where ρ_l is the temperature-dependent liquid water density displayed in Fig.2 (the order of magnitude of β in the unstable temperature range from 0 to 4°C is approximately $-3 \times 10^{-5}\ K^{-1}$). In contrast, *the constant liquid density ρ_0 estimated at the reference temperature T_0 is considered in the inertia terms of Eqs.(3-4) ($\rho_0 = 999.84$ kg/m^3 at $T_0 = 0$°C). The Boussinesq approximation is valid if the maximum fluid density variation $\Delta\rho_l$ is much lower than the liquid density ρ_l, a condition usually satisfied in liquids ($\Delta\rho_l/\rho_l \sim 10^{-3}$ in our case).*

The density of water is greater than that of ice by approximately 10%. This induces a reduction of volume upon melting which is neglected in our model. This is equivalent to assuming that an external water flow replenishes the top layer domain with water at the ground surface temperature T_s. This would result in the additional vertical velocity in the liquid phase (Heitz and Westwater, 1970):

$$v_l = \frac{(\rho_l - \rho_s)}{\rho_l}\frac{dH}{dt} \qquad (12)$$

This velocity would be that of the liquid in the absence of free convection, or would be added to the free convection velocity field in the other case. This contraction-induced flow can be neglected if the heat advected in that way is negligible compared to the heat absorbed by the motion of the melting front:

$$\rho_l\, v_l\, c_{pl}(T_s - T_m) \ll L_m \rho_s \frac{dH}{dt} \qquad (13)$$

Eqs.(12-13) yield the condition of validity:

$$\frac{(\rho_l - \rho_s)}{\rho_s}\frac{c_{pl}(T_s - T_m)}{L_m} \ll 1 \qquad (14)$$

with the physical properties of table 1 and $T_s - T_m$ =15°C, the LHS of Eq.(14) is approximately equal to 0.02, much lower than 1. The volume change induced by melting can thus be safely neglected. Heitz and Westwater (1970) presented a comparison of mathematical solutions with equal and unequal phase densities. In a configuration close to ours, they show that considering equal densities for ice and liquid water resulted in a negligible loss of accuracy."

Line 122. This was the comment concerning the temperature range, in which ice and liquid water co-exists. The original manuscript had the range of -0.5 to +0.5 C, which did not make physical sense because there is no known mechanism of keeping ice above 0 C. The authors responded by stating that the variable is a numerical parameter with no physical sense. However, this statement is incorrect because fluid density is a function of the actual temperature (Fig. 2 in the response). Density of liquid water at -0.5 C is different from that at +0.5 C. Such as small difference in density is usually not a problem, but in this particular case, it may have a noticeable effect because the system is driven by a small density gradient. Please investigate this further and present a convincing justification in the texts.

Strictly speaking, the phase change transition over the melting range $[T_m - \Delta T, T_m + \Delta T]$ is valid for binary systems. However, when $\Delta T \to 0$, this kind of model asymptotically converges to the case of a pure substance with a phase change temperature at T_m. This is why models based on a melting range are widely used for pure substances (Michałek and Kowalewski, 2003; Zeneli et al., 2019; Bourdillon et al., 2015; Arosemena, 2018). When pure substances are considered, the choice of ΔT results from a compromise. Decreasing ΔT increases the model accuracy (the model gets closer to the ideal case $\Delta T = 0$), but requires more computational resources. Practically, we know that ΔT is small enough (i.e., the model is accurate enough) by checking that varying ΔT does not significantly change the results. This is the purpose of appendix A, where it is shown that the model is a good approximation of a pure substance if $\Delta T \leq 0.7$°C (decreasing ΔT below 0.7°C does not significantly modifies the results). This test implicitly validates that the change of density in the range $[T_m - \Delta T, T_m + \Delta T]$ has no significant impact on the results in this case. We tried to clarify this point in the new version:

"To avoid the difficult task consisting in tracking the moving boundary between ice and liquid water, we adopted a strategy that allows to define the same set of dependent variables and governing equations in the entire water domain. To this end, we do the approximation of smooth phase transition between solid and liquid phases. We assume that ice melting begins at temperature $T_{m1} = T_m - \Delta T$ *and ends at* $T_{m2} = T_m + \Delta T$ *(water is in solid state for* $T < T_{m1}$*, in liquid state for* $T < T_{m2}$*, and both phases coexist for* $T_{m1} \leq T \leq T_{m2}$*). It is important to note that in this study,* ΔT *is a numerical parameter with no physical meaning. Ideally, the behavior of a pure substance melting at temperature* T_m *is recovered for* $\Delta T \rightarrow 0$*. Decreasing* ΔT *thus improves the model accuracy, but requires more computational resources (see* (Michałek and Kowalewski, 2003; Zeneli et al., 2019; Bourdillon et al., 2015; Arosemena, 2018) *for more details). Practically, the setting of* ΔT *results from a sensitivity analysis. Its value must be decreased until it does not change the results. This is the purpose of appendix A, where it is shown that the model is a good approximation of a pure substance for* $\Delta T \leq 0.7$*°C."*

Line 260. This was about the method of flow monitoring and its uncertainty. The authors responded by indicating the uncertainty in flow measurement itself. However, from what I can infer from the texts, flow was measured at the main water inlet, which collects the total flow from multiple clefts and chimneys. Therefore, the flow measurement may not represent the actual flow form the particular cleft, to which model results are compared against. Please add more specific explanation about the measurement (or estimate) of flow from the particular cleft and its uncertainty.

The water flow was monitored at the main inlet which originates **from one single cleft**. The defined computational domain (i.e. H_{dom}=3 m, Ap=10 cm, L=1m) approaches the known cleft geometry to the best of our knowledge.

Whether the actual cleft is itself fed by a network of smaller secondary fractures hidden in the host rock is beyond our knowledge and there is no way to address this issue empirically. This point was already discussed in l. 327-331. But overall, our measurements represent indeed the actual flow drained by the visible cleft. Measurement uncertainties (±10%) were already added to the previous revision.

Assuming there is a conceptual misunderstanding, we edited l. 309 to make this point even more clear: *"The main cleft was instrumented to measure discharge …"*

Reference

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