Answer to reviewers comments

We thank both reviewers and the scientfic editor for their comments on our manuscript. In the following, we provide answers to the non-trivial comments.

As a first general comment: It is embarassing to see that we had a lot of small inconsistencies and typos in the manuscript. This manuscript was indeed somewhat rushed (the modeling study wasn't), and does not live up to the usual standards of a manuscript that I would expect myself.

Comments by Reviewer 1

> *Concerning the implementation of the heat transfer model I have a significant concern about the numerical modeling of heat transfer presented in the manuscript. Given the numerical setup, which is radially symmetric around the heating element, I would expect the numerical simulation to reflect this symmetry—that is, the temperature evolution at points equidistant from the heater (i.e., at $\pm d$) should be identical. However, this is clearly not the case in Figures 4, 5, B1, and C1, where the dashed lines show differing temperature evolutions for sensors located symmetrically around the heating source. This raises questions about the numerical implementation: Why does the model exhibit this asymmetry? Is it a result of mesh discretization, boundary condition settings, or another factor? Clarification is needed, as this issue could have implications for the interpretation of the temperature signal and, by extension, the derived water content.*

This is a misunderstanding which seems to stem from our incomplete explanation of what has been done. The model is fine and has been validated against analytic solutions of heat flow and the Stephan problem (shown in Appendix A). At the same distance and angle around the heater, the model solution is rotationally symmetric around the cooler axis. The chosen mesh resolution and other numerical artifacts have a negligible influence on the accuracy of the results, when compared to analytical solutions.

As stated on lines 100 and 111, the model results shown in the figures were extracted at the exact positions of the sensors in the field experiment. This fact was also the reason to perform a full 3D model of heat flow. Apparently our explanation of the modeling setup needs to be considerably improved to avoid this kind of misunderstanding.

> *Line 130 : The authors state that « The temperature evolution at sensors with at the same nominal distance from the cooling head differ considerably in our experiments, especially for small distances. Obviously, inhomogeneities in the polycrystalline ice can largely influence the spreading of the cooling wave. » Therefore, if the small distances sensors are more impacted by the material heterogeneities, why do you use those in order to fit the temperature evolution and therefore water content. Will it be more representative to use the sensor that are the furter of the cooling head as it will reduce the impact of the heterogeneity and lead to better water content estimation. For instance, in figure 5 the sensor at $\pm$ 10 cm present les variabilities that the one at $\pm$ 5 cm but the agreement with the model is worst.*

We totally agree with these statements. The logistical constraints dictated that the duration of the field experiment cannot be longer than half a day. Depending on water content, the cooling wave only moved out to a certain distance. For the determination of water content, we indeed should mainly evaluate the larger distances, since they, as noted by the reviewer, are more reproducible. The model runs have been already performed before. We will adjust the figures and discussion accordingly.

Following up on the concern regarding the numerical modeling (comment 2), I also find that the comparison between measurements and model output lacks sufficient statistical analysis to convincingly support the conclusions. Although the authors performed five measurements at each of the two sites, only a single representative result is used for comparison in each case. It is unclear why the remaining measurements are not utilized to provide a more statistically robust assessment of the water content. Could the authors include all datasets and present metrics such as the mean, standard deviation, or confidence intervals to better quantify the model–data agreement? Doing so would strengthen the validity of the inferred water content and improve the overall credibility of the method.

All measurements are shown in the Appendix. Some of them were not satisfactory due to limitations of the sensor setup, the positions of the sensors, or the cooling temperature. The first experiments at each site were tentative to understand how the optimal setup and cooling should be performed.

We agree with the reviewer that more information might be extracted from a more comprehensive comparison of the experimental cooling curves with model results. Although we already tried a lot of different approaches, it might be possible that new insights could be gained.

As a further insight, stemming from additional experiments performed a week ago, we should also evaluate total head flux from the cooling head towards the surroundings. Such an evaluation will be added, if the results prove to be useful.

In the paragraph spanning lines 21–26, the authors compare their calorimetric measurements of water content with radar-derived estimates. However, I would ask the authors to critically consider whether these two methods are probing the same type or scale of water content. Given the spatial resolution and physical principles involved, the calorimetric method likely captures interstitial water localized at grain boundaries (as discussed, for example, in De La Chapelle et al., 1997 [2]), while radar methods may be more sensitive to macroscopic features such as water-filled crevasses or pockets, depending on frequency and resolution. These two types of water may differ not only in their physical distribution but also in their respective impacts on ice rheology and, thus, glacier dynamics. I recommend that the authors elaborate on this point to clarify the physical interpretation of their comparison and to avoid overstating the agreement between the two methods.

We agree with this comment. There is little information on the in-situ distribution of water within glaciers, especially at depth. With this experimental study we tried to provide at least a few data points. Although by necessity the conditions in an artificial cave are always "drained", i.e. bordering air with only atmospheric pressure.

We think that a in-depths comparison with water contents inferred from other methods is needed. We will re-write the discussion as to make clearer possible distinctions between different methods and settings.

> *Although many of the issues listed below might typically be considered minor, their frequency and impact on readability warrant more serious attention. The manuscript appears to lack a thorough final proofreading, and numerous typographical errors and formatting problems detract from the clarity of the work. Examples include:*

This is truly embarrassing, since I usually am very picky about details. Obviously, we will fix all these minor comments. Apologies to the reviewers for submitting a manuscript that was not carefully crafted.

> *Figure 3: The caption refers to subfigures (a) and (b), but these labels are not present in the actual figure. Additionally, the color scale ticks are too small to be legible.*

Will be changed.

> *Line 104: The expression "(re4)" is unclear and appears to be either a typo or an undefined term.*

We will add a table with the experiments, boundary conditions and designations. The designation "re4" was introduced on line 105 and refers to a specific experiment.

> *Figures in general: Font sizes are consistently too small across all figures, and in some cases, text overlaps with graphical elements—for example, in Figure 5, the labels "-10" and "10" overlap with the plotted curves.*

This will be fixed in a next version. Obviously, figure labels depend on published figure size, but ours should be larger.

> *Figure 4: green dash lines look continous (left figure) and is not visible (right figure)*

Likely this is the color that I perceive as orange. The problem will be fixed in a next version.

> *The authors state that "Soluble and insoluble impurities might complicate the picture (Harrison and Raymond, 1976), but will be ignored in this study." However, based on the visual appearance of the ice in Argentière Glacier (e.g., Figure 2), this assumption seems rather strong. Could the authors elaborate briefly on the expected effects of such impurities and justify their exclusion more clearly?*

The ice at Argentière is very dirty, but mostly due to sand and clay. There are little soluble impurities to be expected from ground granite. We rather refer to impurities and ions stemming mainly from atmospheric transport, such as SO4 and other salts. These usually collect in triple-junctions (e.g. Mader, 1992) and might change locally both the freezing temperature and the effective thermal properties. Since we have no information on ion content within the ice, we ignore any such effect in our modeling.

> *I recommend citing Schohn et al., 2025 [1] to strengthen the argument on the influence of water content on ice rheology. This would help contextualize the results within the most recent developments in the field.*

Like the reviewer, I was excited to read the Schohn et al paper and their findings, which are fundamentally different from standard theory. This paper was not published when the current manuscript was submitted. We will certainly discuss their findings in the context of the manuscript.

> *The manuscript would benefit from a clearer presentation of the physical parameters used in the simulations—specifically, the thermal diffusivity of ice (κ), its heat capacity (C), and the latent heat of freezing (L), mesh size. Including these in a dedicated table would enhance transparency and reproducibility.*

This is a good suggestion that we will follow up upon.

> *For clarity and completeness, I would also suggest adding a summary table listing all experimental measurements, including imposed temperature values, the positions of the sensors, and duration.*

Again, good suggestion that will be added.

> *Line 125: "which would agree" — Why do you think that the values you measure in Argentière Glacier should "agree" with those from other glaciers, given that you previously state that water content "depends on the history of ice formation, the heat supply from dissipative deformation, and on the drainage pathways between ice crystals"? Do you expect these factors to be similar across all glaciers?*

This was not expressed carefully. We think that water contents likely vary considerably between different parts of a glacier, and also between glaciers. This has to be re-formulated. Nevertheless, it is surprising that the inferred water content under such different settings is of similar magnitude.

Comments by Reviewer 2

> *line 42: The analytical model assumption should be presented in more detail. I guess it is based on an infinite medium, with spherical symmetries? Is the water content included in the analytical model? I am not sure that the analytical solution should be presented here, it would better fit in the method section? The erf function should be defined (at least mention that it is the Gauss error function). Motivate the interest of presenting this analytical solution with what is done after.*

We think that a “Theory“ section makes sense in this manuscript, and the analytical solution is just theory. All of this is standard heat conduction and Stefan problem mathematics, and is presented for completeness, and especially since they provide both the motivation of the experimental method, and an effective test of model accuracy. If we discard the “Theory” section, then we would move it to the “Methods”, but we feel this would be rather confusing.

For me, “erf” is one of those functions that need no explanation, such as exp, sin, cos etc. We will add the proposed remark what it designates.

> *Figure 1: this figure could be improved by adding a few dimensions. Also, it’s not clear why there are two heads drawn?*

We will add a ruler to indicate the dimensions. The cooling head is a marvel of engineering, and it is shown from both sides. We will add labels and a better caption to this effect.

> *section 3.3: What is really solved by the model should be presented. Which parametrization is used for exemple for the parameters dependency to temperature and water contents? What is the geometry of the domain (half a cylinder?). If it is half a cylinder (as shown on Fig. 3), what are the boundary conditions applied on the plane? What are the dimensions of the model domain? What is the size of the element where the cooling is applied. Is the cooling temperature enforced at only one node or over a small volume? It is mentioned that the cooling temperature is either constant or driven by the measurement, but the difference between the two approaches is never mentioned in the results section.*

We agree that physical parameters and their temperature dependence should be better described, and will add this in a future version. The model geometry is shown in Figure 3 with the element sizes indicated by thin lines. Due to symmetry, we model just one half of the physical domain (i.e. a half-space). Also here, we should add a scale for the dimensions.

The element sizes around the cooling head are ca. 5 mm in size. The temperature is enforced on all element sides where in reality the cooling head is located.

In the “Results” section, we just show the results from the model runs with measured forcing. The constant forcing temperature is used to validate the model with the analytical solution.

> *Figure 3: There is no "Top" panel in this figure; after after, there is no (a) or (b) indicated in the two panels. The colorscale indicates a minimum temperature of -2 whereas the caption mentions -10 for the blue colors.*

Apologies. This will be improved in the next version. The color scale shows only -2 degrees, but -10 was used as a forcing at the cooling head. The caption will be altered accordingly. Also, we will also try a logarithmic color scale which would likely better show the differences.

> *line 107: why not give the exact measured position of each temperature sensor?*

We give the nominal distances, since drilling with a guiding device used those, but the temperature sensor heads had different distances in all three directions. In glacier ice the drill gets deviated in random directions, and it is almost impossible to mechanically drill a straight hole of 8mm diameter. We could add those positions in a table, although we think it would not add a lot of value for the manuscript. Maybe a table in an appendix would be appropriate.

> *Figure 4 (and other similar figures): I would suggest to have one color for each sensor instead of having a color function of the distance to the cooling head. The given distance should be the measured distance, not the expected one, as it has been measured on the field. On the right panel, what represents the top orange curve? It is not possible to see on the two panels the dashed orange curve of the model. Why? As you are comparing two features on these curves (arrival time of the cold wave and shape of the curves), I would suggest to put a marker for the arrival time of the cold wave to really emphasize the difference between the model and the experiment. There is no label A and B on the panels.*

These are all very good suggestions which will greatly improve the figures. They would likely also fix the misunderstanding of reviewer 1 concerning the different curves.

> *line 132: what is the effect of a large crystal in comparison to small crystal? May be you should explain where the water is stored in temperate ice such that the reader can understand your point?*

According to literature, the water is stored in triple junctions between ice crystals. Consequently, the large crystals encountered in the field lead to very inhomogeneous conditions, whereas small crystals (Millimeter-size) would lead to a more homogeneous appearance at the scale of our experiment. We will change the text accordingly.

> *Figure A1: what is the difference between the two panels? What is the model and what is the analytical solution?*

The caption is really lacking any information, apologies for that. Both figures show model and analytical results, left for w=0%, right for w=2%.

> *Figures B2 and C2: give the differences between the different experiments, else the comparison is useless.*

Agreed. We will add a table with all experiments and their parameters. This then allows to link this figure to the relevant quantities.

> *Figures B1 and C1: mention in the caption that the panels for a water content of 1 and 2% are identical to the figures in the main text.*

These are good suggestion and will be added.