Author's response
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Note: Reviewers' comments are given in italic font whereas the authors' responses read in regular font.

1 Relevant changes made in the manuscript

Following the editor's guidelines, we have included the neccesary changes to make the paper of deeper practical use for the cryospheric community. The current work makes now a stronger case in the paper about what is new and why the method is a useful tool, i.e. the significance of the paper.

Here is a summary of the main changes of the latest manuscript version:

- Reframed introduction.
  - Clear identification of the current theoretical gap in available analytical solutions.
  - Statement on how the current work aims at filling this gap.
  - Suite of benchmark experiments.
- Re-written discussion and conclusion to avoid repetition.
- Brief description on Newton's law of cooling as requested by the reviewer.
- New additional Table 2 with a summary of physical parameters to justify the dimensionless explored range in the temperature solutions.
- Relaxed assumption on the strain rate regimes. Solutions are now applicable to any regime:
  - Shear margins.
  - Centerline of fast-flowing ice streams.
- Figures are now expressed in dimensional terms to ease interpretation.
- Table 1 has been rearranged in a different location to define variables before appearance.
- Expanded discussion on potential depth-averaged lateral advection and depth-independent strain-rates assumptions.
- Clarification on the importance of β parameter (see Discussion section).
- Updated GitHub repository (scripts to plot figures and necessary calculations).

2 Reviewer #5

The authors are deeply grateful to the reviewer for their constructive comments. The current work has strongly benefitted from them. We now provide our answers (regular font) to the main concerns risen by the reviewer (italic).

Larger comments

- *The introduction has a lot of good insight but is a bit disorganized – it begins with a list of the previous temperature modeling studies, goes into the need for validation of numerical models and model initialization, and then starts talking about optimizing for temperature. I think there needs to be more organization and clear takeaways for (1) where the current knowledge gap is, and (2) how this study fills that knowledge gap.*

We have improved the organization of the introduction by emphasizing the current knowledge gap in the literature and how the present work provides new insight. Additionally, the manuscript contains a clear statement on the limitations that the current study presents.

- *Why make the assumption that the lateral strain rate is a dominant component of the strain rate tensor – this seems to be an assumption that is really only valid in shear margins, since outside of fast-flowing glaciers it is very likely that vertical strain-rates are the dominant component, and in the centerline of fast-flowing glaciers it is likely that longitudinal strain rates are dominant. Given that the paper presents this model as very general to lots of ice sheet conditions, this seems to be an assumption that constrains the model. Further, since strain heating is largely treated as a free parameter in the model (through the Brinkmann number) there seems to be no reason to constrain this to just lateral strain rates.*

We thank the reviewer for noting that the solution can be in fact applied to a more general set of conditions. There is no need to further assume that the lateral strain rate is a dominant component and thus the results can be applied to any other conditions: slow-moving regions and centerline of fast-flowing glaciers or icestreams. We have relaxed this assumption so that the strain rate contribution is in fact general and applicable in any other conditions (Section 2 and Eq. 4).

- *There are a number of assumptions within the representation of heat sources and sinks that needs to be further discussed in the Discussion section – for example, the assumption that strain-rates are constant with depth when calculating the shear heating term (certainly not the case everywhere) and the use of a depth-averaged lateral advection term (given the uncertainties underlying lateral advection, I would assume that there is a possibility that there is depth variation in lateral advection). While I don't think this makes the model incorrect by any means, it does affect the results shown in the "stationary solutions" and "full solutions" sections and means that the model as presented here is not necessarily general for all possible ice sheet conditions.*

We have expanded the discussion, particularly on the assumption that strain-rates are constant with depth and the implication of a depth-averaged lateral advective term. As the reviewer points out, it only has a slight impact on the temperature solutions and it is worthy of discussion. We agree that there are certain conditions under which the analytical solutions herein presented are not fully applicable. Particularly, in regimes where vertical shear dominates and the strain heat dissipation is concentrated near the base, a vertically-averaged contribution appears to be inaccurate. However, as already noted by Rezvanbehbahani et al. (2019), this effect is well captured by an increase in the inflow of heat from the base (i.e., an increased geothermal or frictional heat term) under conditions where most of the vertical shear is concentrated in the basal layers (Fowler, 1992).

- *I was a bit confused about the importance of the beta parameter for the results – in Figure 2, it appears that beta has little effect on the estimated ice temperature profiles (at least for the stationary solutions), but then in lines 328-329, it is stated that beta does affect temperature through the ice column. And if beta doesn't affect temperature, then I wonder why spend so much space early on describing the surface boundary condition?*

The role of the surface insulating parameter β is important for the transitory solution. As shown in Figs. 3 and 5, β is essential to accurately describe the transient regime of the ice temperatures. However, Figure 2 shows instead solutions at equilibrium (i.e., stationary regime), where β is in fact less relevant compared to the remaining parameters. The subtlety thus relies on the fact that β determines the transitory behaviour of ice temperatures while leaving nearly unchanged the solutions at equilibrium. This has been clarified in the text.

Smaller comments

- *The last paragraph of the introduction is a bit repetitive*

We have entirely rewritten the last paragraph.

- *Some of the variables names need to be clearly defined up front (e.g. l105, it is worth stating outright that theta is ice temperature)*

Variables have been clearly defined in the updated manuscript version (current line 111).

- *Newton's law of cooling is mentioned twice as the model behind the surface boundary condition, so it is valuable to describe the law briefly and explain why it is applicable to this situation*

We have included a brief description of the law and the conditions under which it becomes applicable (see lines 120-125). It can be summarized as follows. Newton's law of cooling describes those boundary conditions where the heat flux across the interface is proportional to the temperature difference between the surface and the surrounding medium. It is applicable to a large variety of conditions such as a body cooling by forced convection (i.e., a fluid forced rapidly past the surface of a solid) or a a thin surface layer of a poor conductor. Moreover, Newton's law of cooling captures the two simpler boundary conditions as limit cases: (1) prescribed surface temperature and (2) no flux across an interface.

- *Lines 123-126 seem to be a restatement of the previous paragraphs*

We have deleted this additional paragraph to avoid repetition.

- *There are a lot of parameters in this paper, so it seems useful to me to eliminate parameters if they aren't strictly necessary – for example, why use $\mathcal{L}$ instead of just stating [0,L]?*

The interval $[0, L]$ was denoted by $\mathcal{L}$ solely for a more succinct notation. After non-dimensionalization, the interval becomes $[0, 1]$ and we thought that $\mathcal{L}$ (where tildes are dropped) would lighten the notation.

- *L145: S is technically a function of both the stress tensor and the strain rate tensor – one could argue that these are related but Equation 4 puts S in terms of strain rate, so it seems worth it to state the dependence on strain rate explicitly first.*

Indeed. We have stated this explicitly in the revised manuscript.

- *Equation 5: this equation uses xi before it is defined.*

We thank the reviewer for noting this. We have expressed the magnitude in terms of z.

- *Table 1: some of these characteristic ranges need some justification – for example, I believe that Br can be much larger than 2. Further, how do you estimate the ranges of the lateral advection parameter?*

We have included an additional table (Table 2) to summarize all the physical parameters employed to justify the dimensionless range in Table 1. Typical ice-sheet values are used as reference. To estimate the ranges of the lateral advection parameter, we explore realistic values of two phsyical magnitudes: horizontal ice velocity and longitudinal temperature gradients (along a flow line; see e.g. Dahl-Jensen, 1989).

- *Lambda isn't defined before it is used in line 178, I believe.*

We have rearranged the location of Table 1 so that the definitions appear before they are stated in the text.

- *l207-208: I didn't quite understand this statement.*

We mean that it is worth describing in detail the solution at thermal equilibrium for different combinations of the dimensionless parameter. We have updated the text for clarity.

- *l214: reduces dimensionality compared to what – other models, or the dimensional form of this model? 5 uncertain parameters still seem like a lot.*

Compared to the dimensional form of this model. Five dimensionless parameters fully determine the solution and it becomes a reasonable number given that we consider six physical processes for heat transfer: diffusion, advection (vertical and horizontal), basal inflow of heat (geothermal and frictional), strain dissipation and surface insulation. This leaves the model with one unique parameter for each physical process therein described, allowing for a straightforward interpretation of the results.

- *l218: "normalized geothermal heat flow also yields…" I had to go back and remember which parameter that corresponded to, since the Figures are only labeled by parameter. It would be useful to include the mathematical parameter symbol when you refer to them in the text. Also, isn't gamma a combination of both geothermal heat fowand basal friction?*

We have included the parameter symbols for clarity. Indeed, it refers to the combination of both geothermal heat flow and basal friction. The text has been modified accordingly.

- *There's a bit of repetition in the Discussion and conclusion sections, in which the results get recapped in both places.*

We have now re-organised the Discussion and Conclusion section to avoid repetition.

- *Figure 2: the colormaps make some of the distinctions between the lines hard to see, especially c and d. In theory, the reader can infer which lines are which but it'd be clearer to switch to a different colormap that distinguishes the lines better.*

We have improved the colour palettes to ease visualization. We thank the reviewer.

- *Is it possible to redo the x-axes for the temperature profiles (and for the timescale in Fig 4) in dimensional terms? It's hard to know the magnitudes of temperature variations/timescales we're talking about in the nondimensional terms*

Yes, we have now expressed it in terms of dimensional variables by inverting the transformation in Eq. 6. Dimensionless parameters have been consequently adjusted. We hope that this will ease the interpretation of temperature variations and timescales.

3 Reviewer #4

- *In this paper, the author derive solutions to the heat equation that are relevant to glaciers. The focus is on computing solutions and not solving a particular science question or using their tool to make a prediction. Although I see merit in their analysis and find that there are some interesting insights, the results are not a significant enough advance beyond what is already published to warrant publication in the Cryosphere.*

We strongly differ from the opinion of Reviewer #4. Together with a number of other reviewers of this paper, we consider that this work brings new insight to the description of time-dependent ice temperatures and further provides a set of benchmark experiments to test numerical solvers widely used in state-of-the-art ice-sheet models. To illustrate this, Reviewer #5 provides a clear statement: "*The analytical formulation of a transient ice temperature equation is certainly interesting and provides useful insight, in my opinion, in two ways – firstly, allowing for a simplified way of deriving physical insight into the physics of heat transfer in ice (as demonstrated by their analysis on equilibrium timescales) and secondly, by providing a way of benchmarking numerical solvers for heat transfer*".

From the early works of Robin (1955) and Lliboutry (1967) to the most recent advances such as Rezvanbehbahani et al. (2019), great effort has been made to expand our knowledge on how temperatures behave within the ice. Nevertheless, there is a clear gap in our understanding of the inevitable temporal evolution. The present study aims at filling this gap not only by providing an exact analytical solution of the time-dependent nature of ice temperatures, but also by providing a suite of benchmark experiments to test numerical models. The novelty of the current work is thus clear and Reviewer #4 has chosen to overlook it.