Author's response
TC-2022-97

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Contents

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 several changes to make the paper of deeper practical use for the cryospheric community.

In particular, from the potential paths for publication suggested by the editor, we have applied our analytical results to test numerical solvers (Table 3) as shown in the new Section 6 (see also Fig. 6 and 7). A suite of benchmark experiments is summarised in Table 2. The current manuscript version exhibits a strong balance between theoretical knowledge and a clear application for glaciology.

Regarding our choice of time-scaling, there are other equally valid scaling options, such as the advection time scale L/w_0 noted by the editor. However, this scaling depends on the particular advective regime w_0, thus implying different time scales for each w_0 value employed. We prefer to non-dimensionalise our model with the same time scaling for all scenarios so that we obtain a unique dimensionless time scale. The particular choice will have no influence on the results so long as the conversion among variables is consistent.

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

- Reframed motivation.
  - Clear identification of the current theoretical gap in available analytical solutions.
  - Test for numerical solvers: benchmark experiments.
- Re-written conclusion for a clear message of the work.
- Updates on the physical description of the system:
  - Focus on the downwards advective scenario $w_0 < 0$ (all figures have been accordingly updated).
  - Time scale analysis from eigenvalues as suggested by Reviewer 2 (Fig. 5 and Discussion section).
  - Rewriting of vague statements that were previously based on a small subset of the solutions.
  - EISMINT section removed as requested by the editor.
- Test for numerical solvers: whole new Section 6.
  - Additional suite of benchmark experiments to test numerical solvers (Table 2, Figures 6 and 7). This entails two main results:
    - Best discretisation choices.
    - Minimum resolution for reliable temperature profiles.
  - Discretisation over nonuniform grids as state-of-the-art ice sheet models employ (Table 3).
  - A number of different numerical schemes to test accuracy and reliability as a function of the convergence order (Table 3).

    - Resolution study (Fig. 7).
- New detailed appendices with disretisation schemes for numerical solvers (Appendix D).
- Updated GitHub repository (scripts to plot figures and necessary calculations).

2 Reviewer 1

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

- *I do agree that analytical results have value, e.g. for easily extracting characteristic timescales as functions of parameters, but this has not been done and instead the work presented here, inspecting the solutions by eye, could have been based on purely numerical results with no difference in the discussion. I think this speaks volumes to the lack of real depth in the analysis.*

  [...]

  Around l.255 it is stated that there are two different timescales visible in the results. One way to quantify this would be to relate these timescales to the different eigenvalues (which are the exponential decay rates). This is not done.

We have included an entirely new figure (Fig. 5) to extract the characteristic timescales as a function of relevant parameters. Figure 5a shows the decay time dependency to the particular eigenvalues and Fig. 5b further maps the decay times as a function of the two controlling parameters: β and Pe. Results and discussion sections have also been updated accordingly and we have determined what parameters affect the timescales of the system. Moreover, we have completely removed any inspection by eye.

- *The idea of upwards advection (from ice being created at the base?) is unreasonable, yet presented throughout as though it is one of the two equally plausible regimes.*

We have entirely removed the upwards advection regime from the manuscript. Even so, solutions are still applicable to both positive and negative values of w_0.

- *The strain-heating term dependent on u_x should, by the incompressibility of Stokes flow, also be expressible in terms of w_z – a quantity which is explicitly calculable from the vertical advection profiles $w(z)$ - yet is instead taken as constant throughout the paperand then is neglected completely in section 7, when w_z varies in depth.*

The strain-heating term is introduced as a vertically-integrated magnitude (following Meyer and Minchew, 2018) to keep the analytical tractability of the solution. An additional vertical dependency of the inhomogeneous term falls beyond the analytical tractability of the equation, since it cannot be expressed as a confluent hypergeometric equation.

The physical implications of such approximation can be divided into stationary and transitory. In the former, the eigenvalues of the problem (and consequently the relaxation times of the system) are independent of the lateral advection of heat. This would not be true for a numerical solver if an explicit advective horizontal term was to be introduced. Regarding the stationary solution, one could expect small variations in the temperature profile if the lateral advective term was

z-dependent, even though the surface and the basal temperatures would be still determined by the boundary conditions.

- *Finally, there are consistent spelling and grammatical errors throughout the manuscript, and the discussion section needs editing for clarity.*

We have thoroughly revised the manuscript to correct such errors and provide greater clarity in the discussion section.

3 Reviewer 2

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

- *Line 1 in the abstract: 'of paramount importance' is 'paramount'.*

This typo has been fixed.

- *Line 5 in the abstract: 'sophisticated' can be replaced by 'Robin-type'.*

We have included the suggestion.

- *Line 6 in the abstract: non-equilibrium temperature and non-equilibrium thermodynamics are very different, but I think the authors are referring to the former, here and throughout. I suggest clarifying the language.*

Indeed, we refer to non-equilibrium temperature. We have changed the manuscript accordingly.

- *Line 8 in the abstract: the surface insolation number? As I note later, it is typically called the Biot number.*

The Biot number (Bi) characterizes the relative importance of conduction within a solid compared to convection at its surface. Therefore, it requires additional information of the convective heat transfer coefficient h of the air. The Biot number provides information of whether the solid has a uniform temperature ($\mathrm{Bi} \ll 1$) if convection is dominant. Empirical measurements show the strong vertical dependency of ice temperature profiles, thus implying that glacial ice is mostly in the conductive Biot regime ($\mathrm{Bi} \gg 1$).

We rather aim at capturing the fact that the thermal conductivity of glacial ice $k(\rho)$ is reduced towards the surface since density drops as we approach the ice-air interface (knowing that ice forms through snow densification). Following Carlsaw and Jaeger (1989), we simply apply a general "Newton's Law", where the heat flux across a surface is proportional to the temperature difference between the surface and the surrounding medium. The traditional approach, i.e. imposing a particular ice surface temperature given by the air temperature, is then a limit case of "Newton's law" (where $\beta = 0$).

- *Line 9 in the abstract: there is a typo in 'Brinkman' in many places throughout the text as 'Brikman'.*

We have changed the manuscript accordingly.

- *equation (7): I would put Pe ξ into the equation for w so that the system is closed and all of the parameters are clear.*

We agree. The manuscript has been updated to reflect this.

- *In figure 2, why does changing β have little effect?*

Since we focus on the downwards advective regime, the temperature profile is strongly determined by the downwards transport of colder ice from the surface. Since the upwards transport of heat due to difusion is significantly weaker than the advective counterpart, the upper half temperature of the ice column is mostly dictated by w_0. The particular insulating conditions are critical only if the heat inflow from the base (geothermal) can reach the uppermost layers of the ice column, thus yielding a temperature difference between the ice surface and the adjacent atmosphere. We have clarified the manuscript to reflect this mechanism.

- *There are positive and negative vertical velocities presented, what is the physical mechanism of positive vertical velocity?*

Upwards advective conditions are indeed quite rare in real ice sheets and glaciers. As suggested by the editor and other reviewers, we have focused our present work on the downward advective solution $w_0 < 0$, though the solution are still applicable to both scenarios.

- *Brainstorming a few ideas: this analytical solution is likely relatively fast to compute, could this be a good initialization for an ice sheet model? I know there is an interest in Heinrich events from the coauthors, could it be a better analytical model to use in a thermodynamically coupled ice sheet model?*

We really appreciate the brainstorming of new ideas. Indeed, the analytical solution is quite fast to compute. It would be definitely a good approximation to initialize ice sheet models as it captures the transitory component of the solution during the relaxation time. This will surely add value to the motivation of the present work.

Regarding Heinrich Events (HE), this model provides a more comprehensive description of the system. Yet the required analytical tractability of the solution implies simplified physics that reviewers pointed out in prior iterations. As a results, we decided to drop the application to HE and leave the present study to its broader theoretical implication and convenience to test numerical solvers.

4 Reviewer 3

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

- *The motivation (although different from before) is very tenuous, the analysis is very limited and includes mistakes (which affect the rest of the paper), their new section (the EISMINT experiments) doesn't, to my mind, offer what they claim, and they make many broad, sweeping statements based on very limited number of results in a small region of parameter*

Regarding the motivation, we have reframed its structure to reflect the two main points that the work provides:

1. Clear identification of the current theoretical gap in available analytical solutions:
   - Absence of a strain heating term due to internal ice deformation.
   - Analytical description of a vertically-integrated exchange of heat due to lateral advection (term previously employed for stationary solutions in Meyer and Minchew, 2018).
   - Lack of time dependency in previous studies. Particularly convenient for initialization of ice sheet models.
2. Test for numerical solvers: benchmark experiments.
   - Additional suite of benchmark experiments to test numerical solvers (Table 2, Figures 6 and 7).
     - Unevenly-spaced grids (both polynomial and exponential) to obtain higher resolutions near the base whilst minimising the total number of grid points, thus reducing computational costs (Table 3).
     - A number of different numerical schemes to test accuracy and reliability as a function of the convergence order (Fig. 6).
   - Resolution study (Fig. 7): minimum resolution for reliable temperature profiles.
3. The EISMINT section has been completely removed.

- *[...] the author's timescale on which solutions approach the steady state is κ/L^2, which is on the order of minutes, suggesting that the ice column is always in quasi-equilibrium.*

To give an estimation, for a typical vertical ice extent $L \sim 10^3$ m and $\kappa \sim 10^{-6}$ m^2/s, the timescale of the transitory solution is of the order $t = L^2/\kappa \sim 10^4$ years (note that the thermal difussivity has been expressed in m^2/yr), and not minutes (see also Fig. 5a and 5b). We have included an entirely new figure (Fig. 5) to extract the characteristic timescales as a function of relevant parameters. Figure 5a shows the decay time dependency to the particular eigenvalues and Fig. 5b further maps the decay times as a function of the two controlling parameter: β and Pe. Results and discussion sections have also been updated accordingly and we have determined what parameters affect the timescales of the system.

There are other equally valid scaling options, as the advection time scale L/w_0. However, this scaling depends on the particular advective regime w_0, thus implying different time scales for each w_0 value. We prefer to non-dimensionalise our model with the same time scaling for all scenarios.

- *While the authors have included horizontal advection in their model in a way, they do so via an awkward source term which is then completely ignored in their analysis (they focus only on a strain heating source term). The authors claim that the dimensionless horizontal advection term is in the range of 0-0.01, which I disagree with. To demonstrate this, I have quickly plotted $\Lambda = L^2/(\kappa|T_{\text{air}}|)V$ over the Antarctic ice sheet, where $\kappa = 36m^2/year$ is the thermal diffusivity, $T_{\text{air}} = -20^{o}C$ is the air temperature, L is the ice thickness and V is the horizontal ice velocity, a proxy for the integral they consider.*

Horizontal advection is introduced following Meyer and Minchew (2018) as a vertically-integrated contribution Λ. This term is now analysed in Fig. 2e, Fig. 3b and Fig. 4c, as well as the Discussion section. We agree that our previous estimation of the nondimensionless range of values was mistaken since the spanned range was too narrow. We thank the reviewer for including an additional figure and we have accordingly updated the manuscript throughout and further updated Table 1.

The physical interpretation of Λ is a source/sink of heat due to horizontal advection and its value provides information about the relative strength compared to difussion. The sign of the horizontal temperature gradients along the ice flow will determine whether the contribution is a source or a sink. These gradients are bounded and often supplied by an additional heat source as the boundary conditions or the basal frictional heat. As noted by Robel et al. (2013), for rapidly–moving ice streams, strong advection can preserve the shape of the vertical temperature distribution, with the same basal and surface temperatures as before. Therefore, in strong horizontal advective regimes, temperature gradients along the flow become small and thus the overall heat exchange is bounded (e.g., Dahl-Jensen, 1989). To keep the analytical tractability, it is not possible to explicitly solve for the horizontal dimensions. Hence, the heat exchange in such dimensions must be parametrised by a vertically-integrated quantity that implicitly considers realistic horizontal temperature gradients (as Meyer and Minchew, 2018). This definition does not explicitly capture those gradients, so Λ values cannot be solely estimated from velocities, but also limited by a reduced horizontal gradient along the flow. For this reason, the non-dimensional values of Λ cannot span 9 orders of magnitude, but rather a more conservative range of ~ 1 order of magnitude. Such values yield temperature profiles that already capture the shape of a rapidly-streaming ice sheet flow line (e.g., Chapters § 9.6 and 9.8, Cuffey and Paterson, 2010).

- *Their equation (2), now updated to include a vertical advection term, is still missing a term from flow divergence. The first of equation (2) should read $\theta_t = \kappa\theta_{zz} - w\theta_z - w_z\theta + \Omega$. I am not sure how this would change the rest of the analysis but, given that they claim vertical advection is very important, this term could potentially be very important.*

We disagree that Eq. (2) has a missing divergence term (e.g., Cuffey and Paterson 9.7.1.). As the editor said: "The reason is that in the areas where horizontal advection is not important, thermal transport is usually dominated by vertical advection and the Peclet number is larger than one (See for example Table 3 in Nereson and Waddington 2017)".

Moreover, as noted by Reviewer 1, we can apply the "incompressibility of Stokes flow" to our problem, thus implying a divergence-free velocity field.

- *The solutions shown in figure 2 are not actually solutions to the problem (9): the solutions shown have the wrong boundary condition at the upper surface.*

This is indeed our mistake. As a preliminary test of our analytical solutions, we set $\beta v_\xi + v = 0$ to retrieve Clarke et al. (1989) stationary solutions. Unfortunately, we included those figures rather than our actual solution for the $\beta v_\xi + v = 1$ surface boundary condition. We have now updated our figures to account for the latter top boundary condition.

- *There are also sign errors in equation (9), Omega has the wrong sign (based on its definition in equation (2)) and so does gamma. The Brinkmann number is also referred to frequently as the Pe (see e.g. the caption of figure 2).*

We agree. The manuscript has been updated accordingly.

- *In addition to this, there are more general statements, including most of those presented in the abstract, which are based on a small subset of simulations in a small region of parameter space. Numerical solutions, such as those presented in figure 2 are useful to understand how varying parameters affects the behaviour, but general statements about the behaviour over the whole of parameter space cannot be made.*

We have completely rewritten the Discussion section to avoid vague statements on small subsets of the solutions. Moreover, the abstract is now updated to reflect those changes.

- *Finally, I find the EISMINT section very confusing. As far as I can tell, the authors fit a the velocity profile from the output of a numerical ice sheet model to a power law profile (this is not shown anywhere). They then use this exponent to determine the temperature profile, using dimensionless parameters which are also computed from the ice sheet model.*

The EISMINT section has been completely removed for clarity. Instead, there is a new section where benchmark experiments for numerical solvers are described in detail (Section 6). Additionally, we have included a number of different numerical schemes (Table 3) to test accuracy and reliability as a function of the convergence order (Figures 6 and 7).