Author's response
TC-2022-97

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Contents

Note: Reviewers comments are given in blue font whereas the author response reads in black.

1 Relevant changes made in the manuscript

Here is a summary of the main changes of the current manuscript version.

- Removed all connection of our study with Heinrich Events timescales (MacAyeal, 1993a, b).
- Reframing of the manuscript.
  - New title.
  - Re-written introduction for a more general thermodynamics approach.
- A more sohpisticated physical description of the system:
  - Vertical advection. Including z-dependency $w(z)$.
  - Strain heating term.
  - Hoizontal advection (vertically-averaged).
  - Generalised basal inflow of heat:
    - Geothermal heat flux.
    - Potential basal frictional heat term.
- Structure of the solution: stationary/transient components. Independent study.
- Compairson with the EISMINT benchmarks to provide realistic values to our dimensionless study.
  - Additional Table 2 with dimensionless parameters, ranges and particular EISMINT values for a realistic ice sheet..
  - General power-law formulation of the z-dependency of the vertical advective term $w(z)$.
- New detailed appendices with solution derivations.
- GitHub repository (scripts to plot figures and necessary calculations).

2 Reviewer 1

I am concerned that the authors have not actually taken on board a number of the referee comments in a significant way. I still think it could be a stronger paper than in current form. Frequently, the authors have only amended the text to acknowledge limitations of their study even if the suggested improvements seemed quite minor. I am fine with the authors explaining that a suggestion was not helpful/beyond the scope/did not reveal anything new but I would like to hear that. Multiple times in the responses to referees, the authors state that a comment 'has been considered' with neither further explanation, nor updated text in the manuscript. For example, I suggested a plot showing the effect of L/L on the timescale, which seems to have been ignored.

Since all the referees highlighted the strong dependence on initial conditions as an issue, perhaps the authors could show some results with something other than a linear initial profile - e.g. piecewise linear or exponential. This would really help to clarify how the finite depth of the ice column plays a role in the time evolution, compared to by changing the initial temperature profile (which, as another referee highlights, is really the most uncertain part of the proposed application). Also, what is a 'lapse rate' as referred to several times when discussing the temperature profiles?

Both referee 2 and I asked about the 2km transition depth, and I think the authors could do better than their current heuristic explanation. They should ideally predict how the transition depth would vary with the parameters that they say it should depend on - it seems like from their argument the transition to infinite time should only require an analysis of heat fluxes in the steady state? Just because a value depends on parameters doesn't mean it cannot be quantitatively analysed.

Similarly, the depth of 3km to return to the infinite depth solution - is this found by eye? What is the threshold being applied, as it looks more like 2.5km for some surface temperatures? What parameters does this depend on? The suggestion by another referee to non-dimensionalise the problem would clarify these points significantly.

Paragraphs at lines 225 to 237: much of this a description of the non-monotonicity, in quite a verbose way, rather than a physical explanation. This could certainly be condensed, probably into a single paragraph. Consider what the important points are. If it possible to explain why some of the curves are monotonic and others are not, that would strengthen the argument.

The authors are deeply grateful to the reviewer for their constructive comments. The current work has strongly benefit from them.

With this new revised manuscript version, we have taken on board all comments in a significant way, as it can be seen in the relevante changes section. Particularly, following the suggested non-dimensionalization of the problem, we have addressed the effect of θ_L/L. Furthermore, we have defined four dimensionless numbers (see Table 2 in the revised manuscript): the Peclét number Pe, the effective geothermal heat flux γ, Brikman number Br and normalised surface insulation parameter β.

Moreover, all references to MacAyeal (1993) and periodicity have been removed, so that the current paper is completely general and does not rely on any of MacAyeal's assumptions. This further solves the issue of the dependency to the initial conditions, given that the solutions are expressed as the sum of a transient and a stationary component.

All discussion on non-monotonicy is thus avoided as we no longer focus on the time required to melt the column base from a particular initial state. Rather, we now describe a much more sophisticated physical system where vertical advection, strain heating and vertically-averaged horizontal advection are also considered.

Minor comments: Throughout the text, 'reads' is used instead of 'is' The term 'periodicity' persists in a few places - double-check and replace. When discussing the convergence towards the $L \xrightarrow{\infty}$ case, variables with tildes are not defined. The structure of the introduction does not reflect the change in title of the paper. Either find a title that is more of a common ground, or at least pretend the paper is about the more general thermodynamics of ice sheets by adding an introductory paragraph.

- The used of 'read' has been reconsidered when necessary.
- The term 'periodicity' has completely disappear as we do not link our analytical solution with MacAyeal (1993) or Heinrich Events.
- Likewise, the convergence study towards MacAyeal (1993) has been omitted.
- Both the title and the introduction have changed to reflect the more general thermodynamic approach.

3 Reviewer 2

This referee report follows a major revision of the paper by the authors. I believe that the changes made to this manuscript have improved it, but their remain serious issues with it and I outline these below.

Please note (both author and editor): should this paper be revised and resubmitted, I would be happy to provide another review but only in the case that the **authors completely remove the 'binge-purge' terminology used in their ms**. I appreciate that this name is not their invention, but such language is outdated, trivialises a serious condition and is potentially triggering to a reader (see e.g. https:// www.hopkinsmedicine.org/health/conditions-and-diseases/eating-disorders/bulimia-nervosa for an overview). I also strongly encourage anyone external to this review process who might see this review, to refrain from using such terminology. I do not hold the authors use of this terminology against them in the present review.

As I see it, the authors essentially take the study of MacAyeal and consider how the timescale derived therein would change should I different upper boundary condition be applied. They consider the study of MacAyeal to be infallible, a fact that is evident in both their replies and in their ms (in particular, they dedicate an entire section of the ms to describing how the MacAyeal result can be recovered from their model.) They assume that, because MacAyeal suggested that the timescale of HE events is related to the thawing timescale, then this can safely be assumed. My main issue with the ms (and as raised by reviewer 3 in the previous round) is that the MacAyeal model describes largely the wrong physics for ice temperatures: ice sheet temperatures are set almost entirely by horizontal advection of heat (see e.g. https://doi.org/10.1002/jgrf.20054), rather than diffusion of heat; however, the authors assume that temperatures are entirely set by diffusion. To me, this means that no inference can be drawn between the model in the ms and the timescales of HE (even if this comparison was made in previously published work). Since this is the central premise of the paper (there is even a section 'a new period for the binge/purge oscillator') I am not sure if this is surmountable, but at the least I think the paper needs to be entirely reframed with the connection to the HE timescale removed.

I find the authors response to my comment on non-dimensionalization wholly unsatisfying. In their system, there are 7 parameters: κ, T_{air}, L, G/k, β, θ_L, θ_b, which represents a massive parameter space. Non- dimensionalizing the system (see below for an example on how this would look), reduces the dependence of the 'thawing timescale' to only four dimensionless parameters. This would also allow an 'apples with apples' comparison in many of the figures: in figure 2, for example, configurations with different values of L should be evaluated at different times to properly understand the effect of changing L (in particular, the solution with $L = 2.5$ km should be evaluated $1.5^2 = 2.25$ times later than that with $L = 1$ km because the diffusive timescale is L^2/κ). The authors say that 'the simple enough that keeping dimensionality will help the reader.': I think the opposite is true — it is a complicated system with 7 parameters; reducing the dimensionality will help the reader understand which parameters truly matter.

Regarding the initial condition: the authors responded by saying that 'the linear profile is introduced for simplicity'. That is OK, but, if so, drawing analogy with actual observed timescales is even more tenuous. In addition, they say that it 'allows us to explicitly determine the impact of the initial basal/surface ice temperature independently'. In fact, the authors shouldn't be considering the initial basal temp and surface temp independently; as the non-dimensionalization shows, these quantities are coupled, e..g changing the surface temperature without varying the basal temperature changes the temperature gradient in the column. This is another advantage of using the dimensionless approach.

I would also stress reviewer three's comment that the timescale determined is most sensitive to the basal temperature. I will not repeat this point here, but I do not think this comment has been satisfactorily addressed in the updated ms.

The authors mention that they have placed the code in a repository (by the way, the repository mentioned in the updated ms is different to that mentioned in the referee responses) but this repository is not open.

I do not provide another line by line breakdown of the paper as I feel their are more fundamental problems to be overcome before another such critique is useful.

We are deeply grateful to the reviewer for their constructive comments and the provided example as part of the report. The current manuscript version has strongly benefit from them.

As the reviewer requested to further proceed with another review, **we have completely removed the 'binge-purge' terminology used in our ms**.

We have entirely reframed the paper and omitted the previous central premise of the manuscript, thus removing any connection with Heinrich Event timescales. As a result, the paper has taken a much more general approach and does not rely on the any of the assumptions found in MacAyeal (1993). We now describe a more realistic physical system with several processes that were previously missing: vertical advection, strain heating and vertically-averaged horizontal advection among others (see Section 1 of this document for a summary of the relevant changes made in the manuscript).

Regarding non-dimensionalisation, we have rigorously followed Reviewer's 2 suggestion. We are deeply grateful for the example provided in their report. As shown in Eq. 6 and Table 2 of the revised manuscript, we only have now 4 dimensionless parameters that determine the analytical solutions: the Peclét number Pe, the effective geothermal heat flux γ, Brikman number Br and normalised surface insulation parameter β.

Concerning the initial condition, we have eluded previously raised issues by reframing the current manuscript in two main points. First, we no longer focus on determining the thawing timescale as any connection with MacAyeal (1993) has been removed. Moreover, as noted by Reviewer 2, our dimensionless approach further surmounts this problem as changes in the basal/surface temperatures perturb the temperature gradient in the ice column.