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.

- Pressure melting correction in our solutions.
- A more clear connection of our solutions with previous work (binge-purge oscillator; MacAyeal, 1993a, b).
- Additional table (Table 1) with all parameter values.
- Explicit definition of "thermomehcanical instabilities" referred to in the text.
- Extended discussion of main results and figures.
- Full physical interpretation of the β parameter.
- Quantification of the importance of β. Additional figures were included to ease interpretation.
- Non-dimensionalization of β referred to the ice column thickness L.
- Truncation errors are now included.
- Thorough description of (old) Fig. 4 (currently, Fig. 6).
- Physical justification for the initial temperature condition as noted in prior work (MacAyeal, 1993a).
- GitHub repository (scripts to plot figures and necessary calculations).

2 Reviewer 1

General comments:

This manuscript highlights the computational efficiency of using the method of separation of variables to solve the diffusion equation rather than direct numerical methods. While this is well-known, the application to the basal temperature evolution in an ice column had previously only been done for infinitely deep ice; this manuscript considers the case of a shallow ice sheet and performs some example calculations for different initial conditions and boundary conditions. Overall, I was somewhat disappointed that the authors did not go into more depth in analysing and describing their results, in particular exploring the wealth of curious trends shown in figure 4 - most of the paper is instead given over to a routine description of the method of separation of variables. In particular, given the stated threshold of 2km for the solution to approach the infinite depth limit, it would be nice to explore what factors set this threshold. Looking at figure 5 there seems to be a rather narrow band of depth values for which T is finite but larger than the MacAyeal solution. I think figure 4b also shows this rather sudden regime change.

We thank the reviewer for such a comment. The authors have accordingly elaborated in the manuscript to understand the physics behind this threshold (lines 202-210 of the new manuscript).

Briefly, we can understand the process as follows. Since we focus on the time required for the base to thaw, it is essential to consider the temperature gradient between the base and the top. The vertical temperature gradient must be supported by the geothermal heat flux. If the surface is too cold, the heat provided by G may not be sufficient to hold a temperature difference large enough (within the column) so that the base reaches the melting point. For a given choice of G, k and T_{air}, there exists a minimum ice thickness L_{min} that yields a temperature gradient that allows the base to thaw. For thinner columns, the base will remain below the melting point. This further translates into a sudden jump in the potential periodicity shown in Fig. 4b.

Specific comments:

If Equation (6) were given as $\cot\left(L\sqrt{\lambda}\right) = \beta\lambda$, there would be no need to treat $\beta = 0$ as a special case.

We thank the reviewer for such a comment. We have expressed Eq. 6 as $\cot\left(L\sqrt{\lambda}\right) = \beta\lambda$.

Figure 4 - the values of the parameters held fixed are not given.

Indeed, we have now included a table with the most relevant parameter values (Table 1).

Figure 4d - interesting that T is non-monotonic with L at $-14°$C. Why is this?

This is a quite complex behaviour since there are several factors that must be considered simultaneously. It is illustrative to look at the vertical profiles shown in Fig. 2. The fact that the temperature appears to be non-monotonic with L at $-14°$C is a consequence of two factors: the necessary energy budget to warm an ice column and the vertical temperature gradient. The former increases with L, whereas the latter decreases with L.

For slight variations of the thickness δL near $L = 1.5$ km (while fixing $T = -14°$C), the time required to thaw the base is larger regardless of its sign. In other words, it takes longer to reach the pressure melting point both for a thinner and a thicker column. This local minima is a balance between the total energy necessary to heat a column and the fact that a thinner one implies a larger vertical gradient for a fixed temperature difference between the base and the top. Namely, we could consider the effect of these factors explicitly. First, a thinner column requires a smaller amount of total energy to increase the temperature of the column. However, considering the second factor, a thinner column would yield a larger vertical temperature gradient (ultimately yielding a slowdown in the warming rate as the geothermal heat flux is fixed in the BC). The combination of both effects allows for local minima.

The manuscript has been changed to address and discuss this behaviour explicitly (lines 220-232/238-250).

Figure 4c - this figure shows the most interesting trends, but is barely discussed in the text. Perhaps using θ_L/L as the primary variable instead would clarify the impact of the temperature gradient on the basal evolution.

This comment agrees with the other two referees. A detailed discussion has been included in the manuscript addressing the most interesting trends (lines 219-225). Using θ_L/L has been considered as well to clarify the impact of the vertical temperature gradient on the basal evolution.

Line 162 - where T saturates to above 25kyr, are we in fact in a limit where T is infinite?

Yes, for certain boundary conditions the base would never thaw. The text has been updated for clarification (lines 223-224).

Convergence towards no dependence on the detailed surface boundary conditions as $L \to \infty$ could be moved to an appendix for better flow of the manuscript.

We have considered the possibility of moving this part to an appendix, but the comparison with previous work (MacAyeal, 1993a; b) fully relies on the limit $L \to \infty$. In fact, we recover the well-known 7000-yr-periodicity widely used in the literature. While moving it might lead to a better flow of the manuscript, we feel that an important point would be lacking in the main text. We have made modifications to the text however, to try to nonetheless improve the flow (Section 7).

Technical corrections:

Figure 4 colorbar caption could be oriented to match the axis label.

We thank the reviewer for such a comment. Figure 4 has been changed.

3 Reviewer 2

General comments:

In this manuscript, the authors consider how the length of a one-dimensional finite ice column modifies the timescales associated with heat transfer through the column, when compared to a semi-infinite column. They apply a Robin type boundary condition at the upper surface, which permits heat transfer across the ice surface to be accounted for. They apply Fourier analysis to the heat equation to derive a general form of the solution and present numerical evaluations of this solution. They focus in particular on the effect of the column length, surface temperature flux, and initial conditions on the temperature profile and the melting timescale (the time taken for the base to reach melting point). They conclude that considering a finite, rather than semi-infinite, column has important consequences for the temperature profile and melting timescale, suggesting that the previous results for the Heinrich event timescale based on one dimensional semi infinite ice columns could be quite wrong.

Overall, I found this article interesting, but somewhat lacking. I appreciated the simplicity of the approach and use of analytic techniques. However, I felt that the analysis was a little thin, and the system was not explored in a great level of detail. In addition, I felt that the authors made a number of general statements that are based on the small range of parameters considered (outlined in the individual comments below). There are also a number of places where the language is difficult to follow; in these places, I have tried to infer the scientific content of the sentence, rather than judging the written English of the authors. It is possible that I have incorrectly inferred what the authors intend to communicate; in these places I would welcome the authors to respond critically. There are also many modelling assumptions which are not justified and steps jumped, while the method of separation of variables (which is fairly standard) is set out in great detail. While the analysis that the authors have conducted is fairly standard, that does not preclude it from representing an important advance. While I have an interest in them, I am by no means an expert in Heinrich events and Daansgaard Oeschger cycles and therefore I am not able to fully comment on the implications of this work in this context. However, I do think that the authors could do a better job at expressing the implications of the work more clearly and its place within the field. In addition, I believe that many of the main conclusions are heavily reliant on modelling assumptions which are not justified, most notably the initial temperature profile. Below I have set out my responses as scientific comments and technical comments (typos etc), however, there may be overlap between the two sets of comments.

The authors are deeply grateful for the elaborated and constructive comments of the reviewer. This work has strongly benefitted from them.

Scientific Comments:

Title: I do not think the title really gives an indication of what the paper is about. In particular, the authors do not consider periodicity in their model and it is not clear (at least to me) what free oscillations refers to. I think heat transfer should at least be mentioned.

Our model builds upon MacAyeal (1993a). In particular, we are exploring the theoretical periodicity of a binge-purge oscillator (MacAyeal, 1993a, b). It is true that the solution provided by Eqs. 5-7 describes the evolution of the temperature profile and does not show a periodic behaviour in itself; the periodic behaviour emerges once this solution is considered within MacAyeal's oscillator.

As stated in the introduction of this manuscript: "Free oscillations were first proposed in MacAyeal (1993a) as manifestations of the Laurentide Ice Sheet (LIS) purging excess ice volume. This interpretation rests on the assumption that a transition exists between two potential states of basal lubrication (Alley and Whillans, 1991; Hughes, 1992) and it is known as the binge-purge hypothesis. Namely, when the basal ice temperature is below the pressure melting point, the ice sheet is assumed to be stagnant and it simply thickens due to snow accumulation. As a result of the geothermal heat flow, the ice column is expected to warm and the base eventually yields melting. At this point, the ice sheet is no longer at rest and begins to slide over a lubricated sediment bed".

Line 4: "general boundary condition problem": this is a stretch. The authors consider a Robin boundary condition at the surface, which is more general than a Dirichlet or Neumann BC, but certainly not every BC can be expressed in this form.

This is a fair point, the manuscript has been changed to reflect it.

Line 7: "depends on several factor: the ice column thickness L. . . ". The time required depends on more than just these things (as the authors themselves show), e.g. the diffusivity. The phrasing makes it sounds as though the time depends only on L, the initial temp profile and the BC.

We agree. This sentence has been changed accordingly.

Line 14: This may be my own ignorance, but clarification on what the authors mean by "thermo-mechanical instabilities" would be appreciated (this phrase appears frequently throughout the ms, and I was unable to infer its meaning from the context in any place).

A clarification is now included for self consistency in the abstract. Besides, an additional paragraph has been expanded in the conclusion section.

Line 16: The solutions presented in this paper are not, to my mind, analytic solutions. While I agree that the form of the solution has been expressed analytically, the eigenvalues must be determined numerically (as the authors point out) and the infinite sum presumably is also evaluated numerically. This should be clarified and the many statements about deriving analytic solutions should be adjusted to reflect.

It is true that eigenvalues are given by a transcendental equation in our particular case and must be evaluated numerically. Nevertheless, the technique is analytical and the solution is presented as a convergent infinite series (considered an analytical expression). Truncation is only needed for visualisation purposes.

Following the definition, a closed-form expression uses a finite number of standard operations. The analytical expression is slightly less restrictive and allows for an infinite number provided that convergence is ensured. Our solutions fall within the latter, since they are expressed as a convergent infinite series. The eigenvalues may be evaluated numerically as it is the case of the Bessel functions where the zero-th of such function must be computed (e.g., Dirichlet laplacian in a circle), yet they are still considered analytical solutions.

Line 36-37: can you elaborate on the statement "since it would be prohibited by the ice sheet heat transfer physics". I guess you mean that if the ice sheet is sufficiently thick, the base is insulated from the surface?

Indeed, the reviewer is right, if the ice sheet is sufficiently thick, the base is insulated from the surface. This statement is further elaborated on in the sentences thereafter, but it was rephrased to avoid confusion (lines 39-46).

Line 42: need to explain what the low order model is — an ice sheet model?

Here we are referring to MacAyeal (1993b)'s model. It is in fact a 2D conceptual model of the Laurentide Ice Sheet built to verify the accuracy of the predicted periodicity of the binge-purge oscillator (MacAyeal, 1993a). Ice flow mechanics and mass balance are combined in a manner that yields null horizontal ice flow when the base is frozen, whereas deforming sediments allow for rapid sliding when the base is melted.

Our current work further expands the theoretical prediction of MacAyeal (1993a) by considering a more realistic domain (a finite ice column rather than a semi-inifinite domain), yet it builds upon the binge-purge oscillator hypothesis.

Line 57: what does a 'unique spatial element' mean?

Here we refer to the absence of spatial dimensions, quoting Robel et al. (2013): "single lumped spatial element". This has been further clarified in the text.

Line 57: "the surface temperature and the geothermal heat flux were found to determine the character of the ice flow": I don't think that's true: these two factors are important controls, but don't together determine the entire ice flow.

Robel et al. (2013) precisely wrote "In our simple model, we find that geothermal heat flux and surface temperature control vertical basal heat budget and determine the character of the ice flow, of which we found two potential modes of behaviour." Of course, these factors alone do not determine the entire flow, but they constrain it so that it is enough for our purposes.

Line 61: what is a zero dimensional spatial model? One spatial dimension?

No, there is one single lumped spatial element so that we cannot talk about "spatial dimension". This comment referred specifically to Robel et al. (2013); this has been made more explicit in the text.

Line 80: I disagree that this is the most general approach, what if there is e.g. precipitation at the surface which transfers heat?

The reviewer is right. The text has been changed accordingly (lines 88-90).

Line 86: I think it would be useful to explain the physical interpretation of β. What is a typical value? This would help a lot with interpretation of the numerical solutions (see below). The authors say it "modulates the permissible deviation between ice and air temps", which I agree with, but I don't think adds anything beyond what the equation says directly.

The authors thank the reviewer for such a comment. Additional text has been added to link this to the physics of the ice column (lines 94-102/98-107).

Line 90: "the ice surface will consequently evolve in time towards T_{air}" - I don't think that's true? e.g. in the limit of a very short ice column, the heat flux at the surface will match that at the base (i.e. $-G/k$) and thus the surface temp will evolve towards $T = T_{\mathrm{air}} + \beta G/k$? More generally, it is possible to have a heat flux out of the domain while the system is in steady state.

We are grateful for this comment. The reviewer is right and we have therefore suppressed this sentence.

Line 92: This formulation assumes that the diffusivity is constant, which should be stated. Is that a reasonable assumption to make? I am not an expert, but it seems like the firn layer at the surface might have a vastly different diffusivity to the rest of the column.

We could include a deviation of the thermal diffusivity for a range of temperatures (e.g., Cuffey and Paterson, 2010; Greve and Blatter, 2009). However, for the purposes of this work, a constant diffusivity is a fair approximation. In the firn, the density approaches zero as we reach the top and so does the diffusivity. Nevertheless, we are not explicitly considering the firn layer above the ice. A statement on this simplification has been added in the text (lines 105-106/110-111).

Line 93: (Key): where does this initial profile come from? Why is it appropriate? It is simply introduced with no justification! The authors go on to show that the time to melting is sensitive to the particular linear profile chosen, so presumably it is also sensitive to the type of profile. Shouldn't the initial condition be set by the temperature profile immediately after an event in the binge purge cycle?

The authors should also make it clear that this initial profile is something they impose on the model. In addition, surely the initial profile is only compatible with the BC for specific values of θ_L and θ_b, related to G and k etc?

The linear profile is introduced for simplicity. It allows us to explicitly determine the impact of the initial basal/surface ice temperature independently.

Indeed, this profile is imposed as the initial condition of the model and then, a broad range of θ_b and θ_L values is explored. Ideally, the initial condition should be set by the temperature profile immediately after an event in the binge purge cycle, yet such a profile is not available. A linear profile assumes that the temperature in the ice reflects a linear lapse rate in the atmosphere as the ice thickness builds up over time. The manuscript has been updated to account for this comment (lines 113-114/108-109).

General comment on modelling: I am not sure why the authors did not non-dimensionalize their model. That seems like a straightforward way to reduce the number of parameters in the system, as well as gain insight into which processes are important in which locations. Reformulating the system in terms of dimensionless variables might help with the lack of generality of the numerical results which I point to later in my review.

We thank the reviewer for such a comment. Non-dimensionalization provides some clarity, but the results will not change. We believe the equations are simple enough that keeping dimensionality will help the reader.

I also found it quite surprising that the authors did not even mention the heat equation by name.

The heat equation is named in several places throughout the text.

Line 96: I would not call this method "separation of variables", which I understand to be more general, and others may also have this confusion. The authors might consider removing mention of separation of variables (does it add anything?).

The reviewer later includes some notes on the standard derivation in which these are also referred to as "separable solutions". Other references also referred to this as separation of variables (e.g., Arfken, G. "Separation of Variables" and "Separation of Variables–Ordinary Differential Equations." §2.6 and §8.3 in Mathematical Methods for Physicists, 3rd ed. Orlando, FL: Academic Press, pp. 111-117 and 448-451, 1985.)

Line 112: the authors mention using a numerical method, but provide no link to code required to solve equations or produce figures. Code and data should be held in (e.g.) an open repository.

We thank the reviewer for such a comment. A github repository has been created where all scripts can be found (`https://github.com/d-morenop/Supplementary_TC-2022-97`).

Line 113: what does the tolerance refer to here?

This is a root-finding algorithm. In our particular case, it is applied to the eigenvalue equation where we are solving for λ_n. This iterative algorithm continues until numerical convergence is ensured by setting a certain tolerance, as is standard procedure (`https://rdrr.io/rforge/pracma/man/brentdekker.html`). When implemented, the difference between two consecutive solutions must be lower than the tolerance value.

Line 112-115: see above: this equation requires a numerical method, so the solution is not analytic!

A closed-form expression uses a finite number of standard operations. The analytical expression is slightly less restrictive and allows for an infinite number provided that convergence is ensured. Our solutions fall within the latter, since they are expressed as a convergent infinite series. The eigenvalues may be evaluated numerically as it is the case of the Bessel functions where the zero-th of such function must be computed (e.g., Dirichlet laplacian in a circle), yet they are still considered analytical solutions.

Line 122: what is $\tilde{G}$?

Yes, this is a typo, it should be just G.

General note on appendices: Appendix A is entirely standard (e.g. `https://courses.maths.ox.ac.uk/pluginfile.php/22143/mod_resource/content/1/FS-PDE-Slides-Week-4.pdf`) and does not need to be included (a reference to a textbook would suffice). Appendices B and C could also be described as standard, but could also be quite easily incorporated into the main text.

We thank the reviewer for such a comment. A standard reference has been included. We prefer to keep Appendices B and C separate from the main text to make the main message clear and leave the mathematical derivation/subtleties aside for the interested reader. We have cited the standard reference, but prefer to keep Appendix A, so that the method is clear without further searching.

Figure 2: need to state the values of kappa, T_{air}, k, G (etc?) used to generate these figures. Also, there is no need for a legend in both figures, and the final legend entry is not necessary (imo). Labels (a) and (b) are missing from the panels.

We agree, the manuscript has been changed accordingly by modifying Figure 2 and including Table 1.

The solutions are infinite sums, which must naturally be truncated somewhere. The authors should discuss their choice of truncation and justify that the terms ignored are not important (e.g. by considering their order of magnitude).

The reviewer is right. We actually kept more than 100 terms in the series, but never mentioned it in the text. From what we have seen, if $n = 13$ terms, the error reads:

$$\frac{||u_n - u_{n-1}||}{||u_{n-1}||} \leq 0.03\% \tag{1}$$

where u_n denotes the temperature solution truncated at order n. A detailed explanation was also included in lines 148-149/157-158.

Line 125: "the second time frame is chosen…": it would be nice to explain to the reader what they are looking for in the figure to show this (I think it is the blue line going through zero?)

Yes, this needs a clearer explanation and the manuscript has been changed accordingly (lines 148/154).

Important question: based on the previous comment, it seems that the authors are setting the freezing point (on which their timescale calculations are based) to zero? Is that correct? If so, why is there no pressure dependence in melting? Including pressure dependence would mean that larger columns don't need to get to as high a basal temperature in order to start melting, making the dependence of the timescale on L stronger, I think.

We thank the reviewer for this comment. Indeed, including a pressure dependence would make the dependence on L stronger, though changes will be generally small. Quantitatively, it follows that $\tilde{\theta} = \theta + \alpha P$, where $\alpha = 9.8 \cdot 10^{-8}$ K/Pa. Knowing that $P = \rho g L$ and our column spans the following thickness interval $L = [1.0, 3.5]$. For $\Delta L \simeq 3.0$ km, then $\Delta\theta \simeq 2.622$ $^{\text{o}}$C.

However, this dependency needs to be omitted in the comparison section with MacAyeal (1993a) since it was therein disregarded (if we want to check convergence to the 7000-yr periodicity as $L \to \infty$).

Line 128: "this rate is in fact proportional to…". I think you have to work a bit harder to get to this. I agree that each mode has the quoted rate, but it is not obvious what happens when only takes the sum over all the modes (the value quoted is dependent of the dummy variable n!).

The reviewer is right. We here meant the behaviour at leading order. We have elaborated on this more rigorously (lines 151-153/160-162).

Line 128-129: what are L_1 and L_2? I guess you are saying that if we have two columns of lengths L_1 and L_2, then the temp gradient at the base is larger in the longer one? If so, I don't think you can make this statement based on the rate argument made previously (see above comment). In any case, it is not obvious that the rate is increasing in L, particularly since λ_n are complicated functions of L.

We agree that it is not obvious, this is why it was explicitly worded. However, we do think that the reasoning is sufficient even if λ_n is a complicated function (lines 151-153/160-162).

Line 131-132: "the specific non-zero β does not alter this behaviour" - this requires justification, you have only shown the results for one non zero beta (and one value of T_{air}, κ, etc).

I actually proved (mathematically) that solutions are identical in the limit $L \to \infty$ for an arbitrary β (lines 249-261).

Line 134: so beta is the lengthscale over which the ice column feels the surface? This should be stated earlier.

Yes, the manuscript has been changed to make this more clear (lines 101-102/106-107).

Is the value of $\beta = 50$ m large or not? More generally, is the close agreement between the curves for $\beta = 0$ and $\beta = 50$ because the system is not sensitive to beta, or because of the particular value of beta chosen? Non-dimensionalizing the system would help with this.

This is a great point. To the authors' knowledge, we have not seen any β values in the literature since nobody has addressed the finite problem analytically. It is in fact sensitive to β if I give larger values, but 50 m is probably "small". We have included further discussion on the choice of β and its impact (lines 159-174; Figs. 4 and 5).

Line 142: "it is clear that the column thickness is the primary factor…". I don't think you can make as strong a statement as this based on the figure. It is clear that the system is sensitive to L, but it is not clear that this is the most important factor (is it even possible to rank importance of factors?), and this figure is only for one particular set of T_{air}, κ etc.

We agree, the text has been changed accordingly (185/199).

Line 148: "…solely depends on L, G, T_{air}". That can't be true! You even said yourself that the rate depends on kappa (line 127: "in fact proportional to $\kappa\lambda_n e^{-\kappa\lambda_n t}$". There are also potentially other constants in the prefactor.

The sentence has been modified to account for additional dependencies (lines 190-195/205-211).

Line 151: "dependency…on both the initial and boundary conditions". This is crucial: there is a very strong dependence on the initial conditions, which the authors have not justified. Why are they a sensible choice physically? Why is the timescale determined as the time from this (seemingly arbitrary) initial profile?

The referee suggested using a profile right after an event, but such a temperature profile is unknown. Yet it is interesting to justify why the linear profile is a sensible choice: it is in fact the simplest scenario where base and surface temperatures may differ and their impact can be independently quantified. The timescale was defined by MacAyeal's binge-purge oscillator (MacAyeal, 1993a). Since he assumed a semi-infinite domain, his initial temperature profile is constant along the column and reads -10 ºC (a linear case with identical base and surface temperature).

Line 153: "though solely for ice thicknesses below 2km": the figure only shows this for this particular set of beta, T_{air} etc. The following statement ("we therefore find L = 2km is a threshold value…") has not been shown in general. How does figure 4 change for different parameter values is not explained or pursued.

This is true. We had not proved it mathematically. The text now reflects this fact (lines 217-218).

Figure 4: caption: this is picky, but really θ_L and θ_b are parameters rather than boundary conditions. You should also state the values of all parameters used to create these figures. 'Initial basal temperature' in the final line should not be capitalized.

We agree that they are not boundary conditions but it should be noted in Figure 6 (old Fig. 4) we refer to them (panels 6c, 6d) as "Initial conditions". Boundary conditions refer solely to panels 6a and 6b. This has been modified accordingly (new Fig. 4).

Figure 4: I find it surprising that the timescale T is large for small θ_b for $L < 1.25$ or so. Is this because the geothermal heat flux is too strong? Is there a limit in which the base never reaches thawing, or does it always happen just on longer and longer timescales?

If the boundary conditions are such that the surface is cold enough (here, $T_{\mathrm{air}} = -25°\mathrm{C}$) and the column is thin enough the base will never melt. This means that the temperature at the base does not reach 0°C at infinite times but may rather cool down. The manuscript has been updated to avoid confusion.

This figure is very interesting but not really discussed. It raises many questions, e.g. the non-monotonic behaviour in (d) is also quite interesting, why is that? Also, there is a very sharp transition in (b): why is this transition so sharp (what sets this boundary)?

The question regarding the monotonic behaviour was also asked by Referee 1 and has been thoroughly included in the manuscript (lines 220-232/238-250). Briefly, the mechanism is the following:

This is a quite complex behaviour since there are several factors that must be considered simultaneously. It is illustrative to look at the vertical profiles shown in Fig. 2. The fact that the temperature appears to be non-monotonic with L at $-14°\mathrm{C}$ is a consequence of two factors: the necessary energy budget to warm an ice column and the vertical temperature gradient. The former increases with L, whereas the latter decreases with L.

For slight variations of the thickness δL near $L = 1.5$ km (while fixing $T = -14°\mathrm{C}$), the time required to thaw the base is larger regardless of its sign. In other words, it takes longer to reach the pressure melting point both for a thinner and a thicker column. This local minima is a balance between the total energy necessary to heat a column and the fact that a thinner one implies a larger vertical gradient for fixed temperature difference between the base and the top. Namely, we could consider the effect of these factors explicitly. First, a thinner column requires a smaller amount of total energy to increase the temperature of the column. However, considering the second factor, a thinner column would yield a larger vertical temperature gradient (ultimately yielding a slowdown in the warming rate as the geothermal heat flux is fixed in the BC). The combination of both effects allows for local minima.

Concerning the sharp transition, we could understand the result knowing that at some point, the geothermal heat flux is not enough to sustain the amount of heat being lost at the surface (while warming the base). If we cross that threshold, the base will no longer reach melting.

Line 180: this is absolutely not clear, and took me about 10 minutes to figure out that the right hand side of the square bracket goes to zero, so you're looking for roots of tan(theta)=0. This should be explained in more detail.

We agree that this may need more explanation, which has been provided in the revised manuscript (lines 257-258).

Line 185-186: you have shown that the results are asymptotically equivalent, rather than identical (they differ by some small amount for any finite L).

We completely agree. We were not fully mathematically rigorous here. This has been rephrased (lines 256-274).

Line 190: so MacAyeal considered periodic forcing in semi infinite domain, and this work looks at a finite length domain with non-periodic forcing. Can the two changes (periodic vs non periodic and finite vs semi infinite) be decoupled?

MacAyeal actually dismissed external (atmospheric) periodic forcing as the source of the oscillation. Here we are not saying otherwise, we are saying that in the binge-purge oscillator a non-period forcing with a more realistic (finite thickness) description of the system would yield a broad range of periodicities for the same set of parameters that MacAyeal used.

Line 192: Again, the strength is not determined solely by L, beta will also be important (for example).

Yes, this has been rephrased for clarity (lines 262-280).

Line 194: It would be helpful to make it clear that the new development is the consideration of a finite length domain.

Yes, we have made this more explicit in the revised manuscript (lines 273-278).

Line 195: again, I disagree that these solutions are analytic, for the reasons outlined above. 'Analytic approach' also mentioned on line 205.

We discussed this in the comments above, and prefer to stick with the terminology as used.

This is our answer to the same comment in Line 112-115: "A closed-form expression uses a finite number of standard operations. The analytical expression is slightly less restrictive and allows for an infinite number provided that convergence is ensured. Our solutions fall within the latter, since they are expressed as a convergent infinite series. The eigenvalues may be evaluated numerically as it is the case of the Bessel functions where the zero-th of such function must be computed (e.g., Dirichlet laplacian in a circle), yet they are still considered analytical solutions".

Line 198: again, I don't think this is a general boundary condition considered. It's a Robin type (as discussed above).

We agree, it is rather a more general approach. The text has been changed (lines 272/291).

Line 202: You actually showed something stronger: you showed that the semi-infinite domain is an oversimplification provided that the column length is not large (within the context of the assumptions made).

The authors thank the referee for bringing a positive statement that was overlooked while writing the manuscript. This has been expressed clearly in the conclusions.

Figure 5: this figure should appear earlier in the text. It also has two black solid lines. Again, fixed parameter values should be stated. $\tilde{G}$ appears again here. The behaviour of the blue and black solid lines in particular suggests that there is a critical value of the ice thickness below which the potential periodicity goes to infinity (as hinted at by figure 4). Is this the case?

This figure is just a particular case of our problem in which we set those values of MacAyeal (1993). This is now precisely stated in the caption. And yes, it approaches infinity below a critical ice thickness value.

Line 207: It was certainly not shown analytically that $L = 2$ km is an upper bound! That was a numerical result and there is no proof that this value holds in general (numerically or otherwise).

It's true that the particular threshold value is in fact parameter dependent, though we also expect its existence for a distinct choice. The manuscript has been updated to reflect this (lines 281-284/300-302).

Line 215: what is the estimate of a "medium" size based on?

An explicit quantification of the term has been given (i.e., 1-4 km).

Line 216: "the explicit consideration of distinct initial temperature profile manifests a high sensitivity…to its initial state". Indeed…see earlier comments on the initial condition. Why is the initial conditions correct, or even sensible? And why is the timescale the time from this initial condition to thawing?

The timescale was defined by MacAyeal (1993a). As stated before, we built upon his binge-purge oscillator, in which the initial temperature followed an atmospheric lapse rate (since his domain was semi-infinite, setting a particular surface temperature was not possible). Our approach is more general: we keep the linear dependence of a lapse rate, though we can explicitly set a particular initial surface temperature.

Line 222-223: “the periodicity of such events cannot be imposed by the frequency of an external forcing”. How do the authors come to this conclusion? There is no periodicity in their model.

We build upon MacAyeal (1993a), who determined the e-fold decay. The periodicity in that study results from his conceptual model and is numerically demonstrated in MacAyeal (1993b). We here extend the implications for the timescale required for the base to reach thawing to a finite column. In that sense, the rest of the model would remain identical.

Line 227: “this double fold nature. . . .is considered”: I don’t understand what the authors mean by thermomechanical instabilities here. Such things have not been discussed in the paper. Or is this a result from elsewhere? If so, what is the relevance?

A clarification was included and its relevance related to the binge-purge oscillator (lines 302-303/322-323).

While I appreciate that the authors intend to improve upon the work of MacAyeal (1993) by considering more general conditions, it would also be nice to discuss the many simplifications made on a real ice ice sheet, of which there are many: heat transfer is three dimensional in practice, ice columns do not all have uniform length etc.

This is in fact a caveat of both MacAyeal (1993a, b) and our current work. The problem is approached by using analytical techniques and so the complexity of the system is critical if a solution is to be found. A better discussion of the simplifications has been included in the manuscript, which was also requested by other reviewers. Nevertheless, the simplicity of our work provides new insight from a theoretical perspective.

Let us note that we are not claiming this is the mechanism underlying HEs. What we are saying is that, if it were so, the 7 kyr periodicity would be tightly linked to the size of the ice sheet, something that has been ignored even by all 3D ice-sheet models. Our results could be interesting for those too.

Technical Corrections:

Line 13: “these results ultimately manifest a”: results are not active, they do not manifest things. “These results suggest/show”?

Thank you, we have rephrased this as suggested.

Line 35: need a reference for “appear to be dependent on environmental factors”

Thank you, we have included a reference to MacAyeal (1993a).

Line 38-39: the sentence “In fact. . . , for a motionless ice column” is very confusing to anyone who has not read the MacAyeal paper in question. Could the authors clarify? Do they mean that MacAyeal showed that a periodicity of $T = 7000$ yrs corresponds to an e-folding decay length of 14m?

Yes, MacAyeal showed that such a surface signal would have a 314 m e-fold decay. As a result, such periodicity can not be imposed by an external forcing. The text has been modified to avoid confusion (lines 40-41/42-43).

Line 50: SIA has not yet been defined.

SIA stands for Shallow Shelf Approximation. The text now explicitly includes it.

Line 52: causes -> caused

This typo has been corrected.

Line 53: Is this comment ‘thus’? I don’t think it necessarily immediately follows from the examples above. . .

This has been changed.

Line 72-75: this description of the paper’s structure doesn’t actually match what appears in the following (e.g. section 7 has the limit L $\rightarrow \infty$)

This has been changed to improve consistency.

Figure 1: I don’t see why it is necessary to show the x axis? The problem is one dimensional.

This is just a conceptual view of the system to facilitate visualisation of the processes involved. It has been explicitly stated in the caption that the problem is one dimensional to avoid potential confusion.

Figure 1 caption: specify that non-equilibrium thermal states refers to equilibrium across the ice-air interface.

We will add this.

Line 84: “as a result”: I don’t think this sentence is a direct consequence of the previous one.

This has been changed.

Line 90: “Thus”: this is not appropriate: the heat equation doesn’t follow from the description of the BC.

We have changed the text accordingly.

Line 98: there is a backwards quote mark. Also, what is ’shifting the data’ an example of?

First is a typo. Shifting the data is an example of a change of variable that leaves our initial boundary problem with homogeneous boundary conditions so that the corresponding eigenvalue problem can be solved. This has been clarified (lines 119/120).

Line 108: to my mind, that is an initial boundary value problem, rather than just a boundary problem.

Yes, we have made (lines 122/117) this more clear in the revised text.

Line 127: I would avoid making statements like “the implications of a finite domain are clear”. . . It was not clear to me at this point! Your job is to explain the implications.

We thank the reviewer for such a comment. We have modified the text as suggested (lines 150/159).

Line 140: referred as -> referred to as

This has been changed.

Line 171: “excess” has a backwards quote mark at its beginning

This has been changed.

Line 222: which events does ‘such events’ refer to?

We refer to Heinrich Events (HE) as previously stated throughout the manuscript. We will modify this sentence to avoid confusion.

4 Reviewer 3

Recommendation: This manuscript, in anything like its current form, does not seem to contain a publishable idea. The most generous interpretation is that other researchers, over decades of analysis of temperature conduction in a solid rod, have failed to notice an intrinsic timescale which might relate to ice sheet binge-purge cycles. If that is so, something this reader thinks is not true, then the way the article is written must be completely redone. Critically, issues of incoherent definition ("potential periodicity" is here meaningless) and essentially-disregarded parameter dependence (the assumed initial basal temperature and geothermal rates are in fact dominant) must be somehow overcome. (It would be a different paper if so.) In any case, the many time scales potentially associated to full, physically-clear binge-purge mechanisms must be carefully considered if the claimed special time scale here is to be taken seriously.

It is clear from this review that our intended message has not come across clearly in the text, and this is something we will aim to improve in the revised manuscript. In short, we believe that our intended message is actually quite consistent with what the reviewer expects and the rather negative comments largely stem from misunderstanding of that message. We hope that addressing these comments will clarify any misconceptions and help us improve the manuscript. First, we would like to respond to the comments from this first paragraph briefly:

- *The apparent fact that the paper does not contain publishable ideas.* This statement is in disagreement with the other two referees. Referee 2 even considered the paper as “interesting” and “appreciated the simplicity of the approach and use of analytic techniques”. We believe that this statement results from a thorough misinterpretation of our intended message. We have made a strong effort to clarify the sources of this misunderstanding and hope that our message is much more clear in the revised text.
- *Incoherent definitions (e.g., “potential periodicity”).* Our paper is framed within the binge-purge oscillator framework, even though it reaches far beyond it for its generality and simplicity. This term merely relates to the “binge” phase timescale that would yield a binge-purge oscillator, as defined in MacAyeal (1993a) model. It is clear that this phraseology gives the reader a false impression of our aims, and we will maintain the more accurate phrase “time to reach the pressure melting point” whenever possible.
- *Disregarded parameter dependency.* Parameter dependency is not disregarded - in fact, one of our main aims is to show precisely that there is no special timescale intrinsic to the system, but rather that it generally depends quite strongly on the boundary and initial conditions. The precise 6944-year value arises only when employing the exact parameter values as MacAyeal (1993) and we use this to show that our system behaves consistently when considering the same (over)simplification. Otherwise, as Fig. 4 shows, we have exposed a broad range of response timescales that depend very much on the choice of several parameter values.

Summary of the manuscript: The Introduction ties binge-purge (Heinrich event) cycles to ice temperature (which is fine) and concludes by asserting that 7ka periodicity is widely used in the literature.

Section 2 sets up an initial-boundary value problem for a motionless ice column of finite length, with geothermal (Neumann) basal and Robin surface boundary conditions, and linear-in-height initial temperature. Sections 3 and 4 sketch, with details in the Appendices, a Fourier series solution of the problem, in which (generally) the eigenvalues solve a transcendental equation requiring numerical solution. Section 5 visualizes the temperature profiles and their time-dependence, with an emphasis on how they depend on the ice thickness L and on beta, an insulation coefficient in the surface Robin condition. Section 6 starts by defining a certain solution time as "potential periodicity"—there is no given justification for connecting *this* solution time to periodicity!—and then illustrates and discusses dependence of this time on parameters. Section 7 then focuses on the dependence of the time on L, as L becomes large, revealing a time 6944a in the limit. (This value, conveniently near 7ka, entirely depends on the assumed conditions at the base, namely the initial basal temperature θ_b and the geothermal rate G/k.) Finally the Conclusion again emphasizes the role of L. Appendices then give details of the standard Fourier series analysis.

The connection to periodicity is justified by the definition in MacAyeal (1993a) as the time required to thaw the base (Sections 4.2 and 4.3, MacAyeal, 1993a). For a binge-purge oscillator, as expected, the 6944-yr limit depends on the conditions at the base (since it is an initial boundary problem) and MacAyeal (1993a) identical values are employed in that section for a one-to-one comparison. Our results now show that for a finite domain there is an additional dependence on the ice sheet thickness (Fig. 7) that did not appear in MacAyeal's original papers since the heat solution assumed a semi-infinite domain. As mentioned above, we have replace this term by the phrase "time to reach the pressure melting point" to avoid confusion.

Major concerns:

Understanding the consequences of conservation of energy in ice sheets is a nontrivial matter, thus it is included as a 3D partial differential equation into most modern ice sheet modeling efforts, and it is important because internal energy (e.g. temperature) is tied to the long time-scales at which ice sheets change. Because ice sheets are thin, variations in the vertical are generally larger than in the horizontal, but nonetheless the problem is advection-dominated. In ice columns near the divide the strongest direction of ice advection is typically vertical, but over large areas of ice sheets this direction is horizontal so that column-wise temperature distributions are commonly far from what any isolated vertical-column model might generate. Furthermore the bases of ice sheets are usually near or at the pressure-melting point. The thermo-mechanical condition of near-basal ice can dominate overall ice sheet dynamics because the presence of pressurized liquid water facilitates ice deformation and basal sliding. The near-basal thermal regime is dominated by geothermal flux, dissipation heat from sliding, and at times the transport of liquid water from elsewhere (e.g. ice surface or through subglacial hydrology). Because of the strong role of liquid water, it follows that conservation of energy is a two-phase problem, thus not one which can be well-modeled by temperature alone.

We agree with the conservation of energy reasoning. And it is clear that a more realistic and sophisticated description of the thermomechanical processes at the base of an ice sheet is possible and is employed by 3D ice sheet models. However, conceptual studies also have great value in helping to understand the importance of different processes, and mathematical simplicity allows for analytical solutions that facilitate the analysis. The main aim of our paper is to reevaluate an important foundational piece of literature in the binge-purge hypothesis (MacAyeal, 1993a, b) and advance our understanding of how the thickness of an ice sheet influences its thermal evolution. The context of the problem here relates to a region where the ice is initially frozen to the bed. Thus horizontal advection can be expected to be low, and there should be no liquid water at the base. The question addressed is how long would it take such a column of ice to reach the pressure melting point. The subsequent evolution of the ice sheet would indeed be more complex, but is outside the scope of this simple scenario. It can be argued, as indeed MacAyeal (1993a, b) did, that this initial time to reach the pressure melting point is related to the binge timescale of the binge-purge mechanism. We also note that the solutions calculated here are furthermore not restricted to a particular problem and can be used in any physical system that satisfies the initial boundary problem.

The current manuscript considers none of these realities, nor does it provide this reader any insights about ice sheet thermodynamics. Instead it examines a conduction-only isolated column model. Within this narrow, unpromising model it proceeds to ignore the dominant parameter dependencies and instead extract a special 7ka time scale, a time scale for temperature change at the base, by surreptitiously fixing some dominant, but unexamined, values. Then it confusingly discusses dependence on less-dominant parameters, especially ice thickness L and surface conduction beta, simultaneously arguing that L is important and irrelevant.

As mentioned above, the aim of the paper is not to provide a full description of all processes concerning the energy conservation within an ice sheet. It is well known that there exists no analytical solution to describe such a system. Nonetheless, parameter dependencies are not ignored in our description. They are considered in Table 1 and Fig. 6, where a broad range of values are employed to compute the time required for the base to thaw (i.e., periodicity, as defined by MacAyeal, 1993a). Moreover, ice thickness is never simultaneously argued as important and irrelevant. A careful look at the paper reveals the subtleties of such degrees of freedom (even in this idealised system).

Thus the manuscript first fails to consider the actual thermodynamics of ice sheets, and then it makes unreasonable claims for the relevance of its very-simplified model. An extremely well-trod mathematical analysis, namely Fourier series applied to conduction in an interval, a problem already addressed by Fourier and Kelvin, is offered as new and insightful, which it is not. The modeled time evolution of a column's basal temperature profile simply does depend strongly on the column thickness L, despite the "strongly dependent" claim in the abstract (line 5). The particular 7ka time scale revealed herein, and unconvincingly tied to binge-purge oscillations and Heinrich events, actually does have strong dependence on particular basal parameters in the model, namely the assumed geothermal flux rate and initial basal temperature. However, this special time scale would in any case be destroyed by any (here missing) advection mechanism including sliding, critical to any serious discussion of binge-purge.

Fourier analysis is not presented as new, but rather as a standard approach (appendices were only included for clarity with readers not familiar with it). Yet, to the authors' knowledge, this method has not been applied by the glaciological community to address the current problem. In particular, MacAyeal (1993a) did not use it but instead resorted to considering an infinite domain to simplify calculations, arguing this could be justified. We here show that considering a finite domain leads to a dependency on ice thickness that is not present in MacAyeal's solution. In addition, we demonstrate that the time scale also depends on the initial and boundary condition of this problem as expected (but ignored in the original work). As posed by MacAyeal (1993a), no horizontal advection is considered in this problem, though vertical advection is neglected by estimating the e-fold decay of a sinusoidal signal at the surface with a constant vertical velocity comparable to the accumulation rate at the summit of the GIS (e.g., Alley et al., 1993).

A key sentence (lines 138-140) is that "We further calculate the time required for the column base to reach the melting point ..., hereinafter referred as potential periodicity". There is no offered justification for why this solution time is a "periodicity" for anything! Indeed binge-purge is a periodic mechanism, one of great interest and importance, but there is not even an attempt to explain why this time is related to the desired periodicity.

There is clear justification in Sections 4.1, 4.2 and 4.3 of MacAyeal (1993a): "once the basal temperature reaches the melting point, the ice sheet begins to move". This is in fact the end of the growth phase (binge) of the cycle. We had assumed that the ideas presented in such a paper are known and fully understood by the reader, but we have explicitly addressed this to make our link to that work more clear (e.g., lines 24-31) and further avoid the "potential periodicity" term. The 6944-yr periodicity is then elaborated in Section 5 of the same paper.

This "potential periodicity" time is completely dependent on a parameter which is completely arbitrary, namely $\theta_b = -10°$ C as the starting point at time 0. It also depends strongly on the geothermal flux rate, which is known to vary substantially over a continent. (Geothermal flux rates are available for modern North America and thus could be used to explore this parameter dependence.) As shown in Figure 4(d), stably across a broad range of ice thicknesses L, variation of θ_b from -15C to -5C implies "potential periodicity" which ranges from about 4ka to about 20ka. Lines 161-162 actually mention this but the rest of the manuscript drops it: "the potential periodicity appears to be rather sensitive to the initial basal temperature, rapidly saturating to values above 25 kyr for $\theta_b < -11$ C". Attempting to interpret time scales as depending on L seems to deliberately ignore that they depend much more strongly on an uninspected parameters θ_b and G/k. Possibly θ_b should be regarded here as a proxy for the coldness of the cold part of the atmospheric-driver temperature cycles, but (as far as I can tell) even this is not argued.

Indeed, the timescale to reach the pressure melting point depends on θ_b (already noted in MacAyeal, 1993a). Nowhere in the paper is the contrary stated and, the fact there exist additional dependencies (e.g., L, β, G, $k\ldots$), does not say otherwise. We first explore a broad range of θ_b values (Fig. 4d), so we do not fully understand why the referee stated that this parameter had been ignored (see Table 1). Then, to perform a one-to-one comparison with MacAyeal's 6944-yr estimation, we employed an identical value of $\theta_b = -10°$C as we did in our Section 7. Additionally, geothermal heat flux values span those available for North America, thus exploring a realistic range. Likewise, θ_L is not ignored and presented in Fig. 4c.

Finally I want to describe two important figures, so as to illustrate the inappropriateness of the manuscript's analysis. Figure 4: What the parts of this Figure actually show, though this is ignored, is that the strongest dependence of the "potential periodicity" time is on the geothermal flux rate and the initial basal temperature. The discussion of dependence on air temperature and ice thickness is mostly a distraction.

As expected, there exists a dependence on both the geothermal heat flux and the initial basal temperature. Nevertheless, Fig 4b is enough to understand that the air temperature and ice thickness are of paramount importance. It is quite interesting to notice the sharp dependency with thickness for air temperatures below -20^{o}C. Even more, all panels in Fig. 4 share the x-axis where the strong L-dependency is clearly shown. These dependencies have been further elaborated in the text for completeness.

Figure 5: Here is my attempt to say what is shown in this Figure; note that Figure 2b in particular supports my interpretation. A geothermal rate and ice conductivity are fixed, giving a fixed value G/k. An initial basal temperature (θ_b) is fixed, most likely as -10C consistently with Figures 2a and 3, though its value is unstated. Then the time for the base to warm to 0C (the misnamed "potential periodicity") is shown as a function of ice thickness L. Different surface boundary condition treatments give several curves, but for $L > 2.5$ km they all coincide at a time about 7ka. I observe that the explanation for this value of 7ka is actually quite clear! Namely, as long as the top of the ice is far away, the chosen values of the initial basal temperature and the geothermal flux rate will determine the time taken for the base of the ice to warm up to 0C; this is a balance of upward conduction with the delivered heat in the time interval. Thus the special value 7ka is actually (and strongly, and entirely as L goes to infinity) a function of θ_b, G, and k, which were all fixed at certain values for no stated reason. This dependence should be examined, but instead the paper looks elsewhere, at L and beta, and then it spins the results as related to Heinrich events.

In the manuscript, our intention was essentially to give the conclusion of the reviewer here. The point is that the widely cited value of 7 ka is not special at all. Indeed, as L goes to infinity the time required to reach melting is a function of θ_b, G and k. In this section, the values used here were chosen to replicate those of MacAyeal (1993a) (see Table 1) and in that way show that our solution converges to his in such a limit. Meanwhile, the dependence of the timescale on various parameters has already been examined in our more realistic analysis with a finite L: different values of geothermal heat flux and the initial temperature profile of the column are tested (that is precisely the message of Fig. 6).

In the revised manuscript, we have made an extra effort to clarify the new results we present, namely that more realistic treatment of the problem demonstrates a strong relationship of timescales to boundary and initial conditions, and therefore no special timescale of 7 ka should be expected to exist. We maintain the section demonstrating how the 7ka timescale can be obtained under the assumptions made by MacAyeal (1993a) and emphasise that there is no reason a priori to expect those assumptions to hold universally. We hope that with these changes, it will be clear to the reviewer that the message of our paper is quite consistent with their expectations.