# Authors final response to Reviewer 2
# TC-2022-97

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

**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 will strongly benefit 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). Note 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 will be 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 will be changed accordingly.

Line 14: This may be my own ignorance, but clarification on what the authors mean by "thermomechanical 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 will be included for self consistency in the abstract. Besides, an additional paragraph will be 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. This statement is further elaborated on in the sentences thereafter, but it will be rephrased to avoid confusion.

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 will be 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 will be 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 will be changed accordingly.

Line 86: I think it would be useful to explain the physical interpretation of $\beta$. 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 will be added to link this to the physics of the ice column.

Line 90: "the ice surface will consequently evolve in time towards $T_{\text{air}}$" - 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 $T = T_{\text{air}} + \beta G/k$? 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.

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, Greve...). However, for the purposes of this work, this is well known and 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 will be added in the text.

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 $\theta_L$ and $\theta_b$, 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 $\theta_b$ and $\theta_L$ 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 will be updated to account for this comment.

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?).

As the reviewer later includes some notes in the standard derivation (e.g., `https://courses.maths.ox.ac.uk/pluginfile.php/22143/mod_resource/content/1/FS-PDE-Slides-Week-4.pdf`), 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 will be created where all scripts will be uploaded.

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 $\lambda_n$. 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`).

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 will be 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 will cite 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, $T_{\text{air}}$, $k$, $G$ (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 will be changed accordingly.

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 60 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 $n$.

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 will be changed accordingly.

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 $\alpha = 9.8^{-8}$ K/Pa. Knowing that $P = \rho g L$ and $L = [1.0, 3.5]$. If $\Delta L \simeq 3.0$ km, then $\Delta\theta \simeq 2.622$ ºC.

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 will elaborate on this more rigorously.

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 $\lambda_n$ 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 $\lambda_n$ is a complicated function. REVISE THIS!!!!!!!

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

I actually proved (mathematically) that solutions are identical in the limit $L \to \infty$ for an arbitrary $\beta$. I kept everything else general so it should work in my opinion. I'll go over the derivation again though.

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

Yes, the manuscript will be changed to make this more clear.

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 $\beta$ values in the literature since nobody has addressed the finite problem analytically. It is in fact sensitive to $\beta$ if I give larger values, but 50 m is probably "small". We will add some discussion of the choice of $\beta$ and its impact.

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 $T_{\text{air}}$, $\kappa$ etc.

We agree, the text will be changed accordingly.

Line 148: "…solely depends on $L$, $G$, $T_{\text{air}}$". 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 will be modified to account for additional dependencies.

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, $T_{\text{air}}$ 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. I haven't proved it mathematically. Maybe we need a more detailed discussion of Fig 4 to avoid this comment?

Figure 4: caption: this is picky, but really $\theta_L$ and $\theta_b$ 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 4 we refer to them (panels c, d) as "Initial conditions". Boundary conditions refer solely to panels a and b. This will be modified accordingly.

Figure 4: I find it surprising that the timescale T is large for small $\theta_b$ 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, $T_{\text{air}} = -25°\text{C}$) 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 will be 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 will be thoroughly included in the manuscript. 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 $-14C$ 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 $\delta L$ near $L = 1.5$ km (while fixing $T = -14C$), 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 will be provided in the revised manuscript.

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 will be rephrased.

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 will be rephrased for clarity.

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

Yes, we will make this more explicit in the revised manuscript.

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.

[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're 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 will be changed.

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 will be 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 tha the particular threshold value is in fact parameter dependent, though we also expect its existence for a distinct choice. The manuscript will be updated to reflect this.

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

An explicit quantification of the will be 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 will be included and its relevance related to the binge-purge oscillator.

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 will be 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 will rephrase this as suggested.

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

Thank you, we will include 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 will be modified to avoid confusion.

Line 50: SIA has not yet been defined.

SIA stands for Shallow Shelf Approximation. The text will explicitly include it.

Line 52: causes -> caused

This typo will be corrected.

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

This will be 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 will be 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 will be 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 will be changed.

Line 90: "Thus": this is not appropriate: the heat equation doesn't follow from the description of the BC.

We will change 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 will be clarified.

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

Yes, we will make 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 will modify the text as suggested.

Line 140: referred as -> referred to as

This will be changed.

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

This will be changed.

Line 222: which events does 'such events' refer to?

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