Referee Report: “On the periodicity of free oscillations for a finite ice column” by Moreno et al.

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.

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.
- 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.
- 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.
- 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).
- 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.
- 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?
- Line 42: need to explain what the low order model is — an ice sheet model?
- Line 57: what does a ‘unique spatial element’ mean?
- 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.
- Line 61: what is a zero dimensional spatial model? One spatial dimension?
- Line 80: I disagree that this is the most general approach, what if there is e.g. precipitation at the surface which transfers heat?
- 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.
- 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_{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.
- 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.
- 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_I and theta_b, related to G and k etc?

- 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. I also found it quite surprising that the authors did not even mention the heat equation by name.
- 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?).
- 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.
- Line 113: what does the tolerance refer to here?
- Line 112-115: see above: this equation requires a numerical method, so the solution is not analytic!
- Line 122: what is \tilde{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.
- 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.
- 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).
- 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?)
- 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.
- 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!).
- Line 128-129: what are L1 and L2? I guess you are saying that if we have two columns of lengths L1 and L2, 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.
- 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_air, kappa etc).

- Line 134: so beta is the lengthscale over which the ice column feels the surface? This should be stated earlier.
- Is the value of \beta = 50m 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.
- 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}, kappa etc.
- 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.
- 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?
- 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.
- Figure 4: caption: this is picky, but really theta_I 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.
- Figure 4: I find it surprising that 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?
  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)?
- 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.
- 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).
- 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?
- Line 192: Again, the strength is not determined solely by L, beta will also be important (for example)
- Line 194: It would be helpful to make it clear that the new development is the consideration of a finite length domain.
- Line 195: again, I disagree that these solutions are analytic, for the reasons outlined above. ‘Analytic approach’ also mentioned on line 205.
- Line 198: again, I don’t think this is a general boundary condition considered. It’s a Robin type (as discussed above).

- 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).
- 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?
- Line 207: It was certainly not shown analytically that L = 2km is an upper bound! That was a numerical result and there is no proof that this value holds in general (numerically or otherwise).
- Line 215: what is the estimate of a “medium” size based on?
- 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?
- 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.
- 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?
- 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.

Technical Corrections

- Line 13: “these results ultimately manifest a”: results are not active, they do not manifest things. “These results suggest/show”?
- Line 35: need a reference for “appear to be dependent on environmental factors”
- 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 = 7000yrs corresponds to an e-folding decay length of 14m?
- Line 50: SIA has not yet been defined.
- Line 52: causes -> caused
- Line 53: Is this comment ‘thus’? I don’t think it necessarily immediately follows from the examples above…
- 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 \to \infty)
- Figure 1: I don’t see why it is necessary to show the x axis? The problem is one dimensional.
- Figure 1 caption: specify that non-equilibrium thermal states refers to equilibrium across the ice-air interface.
- Line 84: “as a result”: I don’t think this sentence is a direct consequence of the previous one.
- Line 90: “Thus”: this is not appropriate: the heat equation doesn’t follow from the description of the BC.

- Line 98: there is a backwards quote mark. Also, what is 'shifting the data’ an example of?
- Line 108: to my mind, that is an initial boundary value problem, rather than just a boundary problem.
- 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.
- Line 140: referred as -> referred to as
- Line 171: “excess” has a backwards quote mark at its beginning
- Line 222: which events does ‘such events’ refer to?