# Referee Report for TC2022-97 (Thermodynamic evolution of a finite ice column: analytical solution and basal-melting timescales) by Moreno et al.

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

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'binge-purge' terminology used in their ms.** I appreciate that this name is not their invention, but such language is outdated, trivialises a serious condition and is potentially triggering to a reader (see e.g.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

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

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

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

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

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

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

One dimensional heat diffusion through a column.

— $z = L$ : $\theta = T_{air} + \beta \theta_z$

$\theta_t = k\theta_{zz}$ + initial condition:

$$\theta(z,0) = \theta_b + \frac{z}{L}(\theta_s - \theta_b)$$

— $z = 0$ : $\theta_z = G/k$

introduce scaled variables: $[z] = L$, $[t] = L^2/k$ (diffusive timescale)

(dimensionless variables w/ hats) $[\theta] = T_{air}$

Then : $\theta_t = k\theta_{zz} \mapsto \hat{\theta}_{\hat{t}} = \hat{\theta}_{\hat{z}\hat{z}}$

bottom bc : $\theta_z = G/k \mapsto \hat{\theta}_{\hat{z}} = \frac{G}{k} \cdot \frac{[z]}{[\theta]} = \frac{GL}{kT_{air}}$

top bc : $\theta = T_{air} + \beta\theta_z \mapsto \hat{\theta} = 1 + \frac{\beta}{L}$

IC : $\theta = \theta_b + z(\theta_s - \theta_b) \mapsto \hat{\theta} = \frac{\theta_b}{T_{air}} + \hat{z}\,\frac{\theta_s - \theta_b}{T_{air}}$

4 dimensionless parameters: $\frac{G}{K}\frac{L}{T_{air}}$ : dimensionless geothermal heat flux

$\frac{\beta}{L}$ : dimensionless insulation length (already identified by authors)

there could also be ... expressed as a temp gradient and temp difference.

$\frac{\theta_b}{T_{air}}$ : dimensionless initial surface te

$\frac{\theta_s - \theta_b}{T_{air}}$ : dimensionless initial temp diff