

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

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

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

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

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

Minor comments:

Throughout the text, ‘reads’ is used instead of ‘is’

The term ‘periodicity’ persists in a few places - double-check and replace.

When discussing the convergence towards the $L \rightarrow \infty$ case, variables with tildes are not defined.

The structure of the introduction does not reflect the change in title of the paper. Either find a title that is more of a common ground, or at least pretend the paper is about the more general thermodynamics of ice sheets by adding an introductory paragraph.