Referee report for "Analytic solutions for the advective-diffusive ice column in the presence of strain heating"

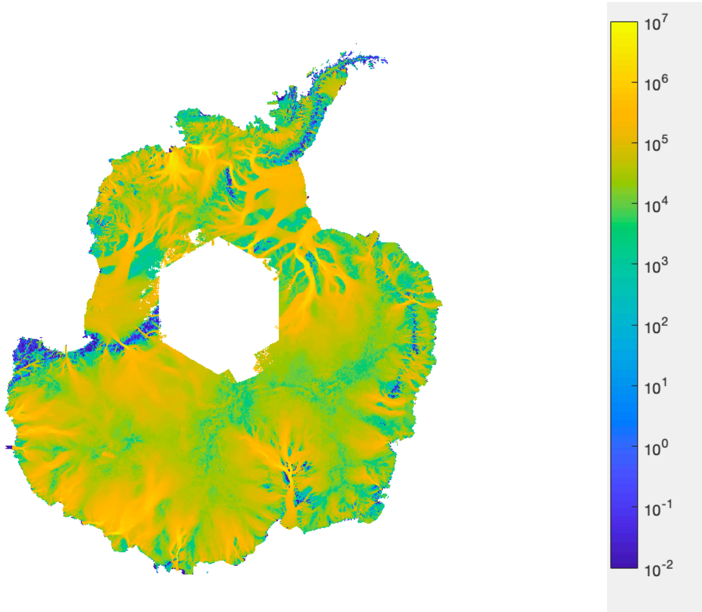
This paper has changed significantly since the last revision, which I welcome. After consulting with the editor, I am writing this review as if the paper is a major revision, rather than a new submission.

In this updated paper, the authors present a modified model, in which vertical advection, strain heating and horizontal advection are included. They non-dimensionalize the problem as suggested, identifying four dimensionless parameters which describe the behaviour: a dimensionless geomthermal heat flux, a dimensionless 'insulation coefficient', a vertical Peclet number, and a dimensionless source term. They solve this system analytically, and, firstly, present steady state configurations for a range of parameter combinations and, then, present some transient solutions from an arbitrary initial condition. They then present another analytic solution which corresponds to a vertical velocity profile with a more general power law dependence on depth; comparing this vertical velocity profile to that seen in numerical experiments gives a temperature profile which they claim can be used as an analytic control on thermodynamic ice sheet model behaviour.

However, despite the changes, I find that the current manuscript still has major flaws. These flaws are different to those in the previous manuscript, but they are major flaws nonetheless. It feels a bit unfair to be pointing out 'new' major flaws upon third revision, but I think that is perhaps inevitable given how much the paper has changed. The motivation (although different from before) is very tenuous, the analysis is very limited and includes mistakes (which affect the rest of the paper), their new section (the EISMINT experiments) doesn't, to my mind, offer what they claim, and they make many broad, sweeping statements based on very limited number of results in a small region of parameter. Below I expand on these in more detail.

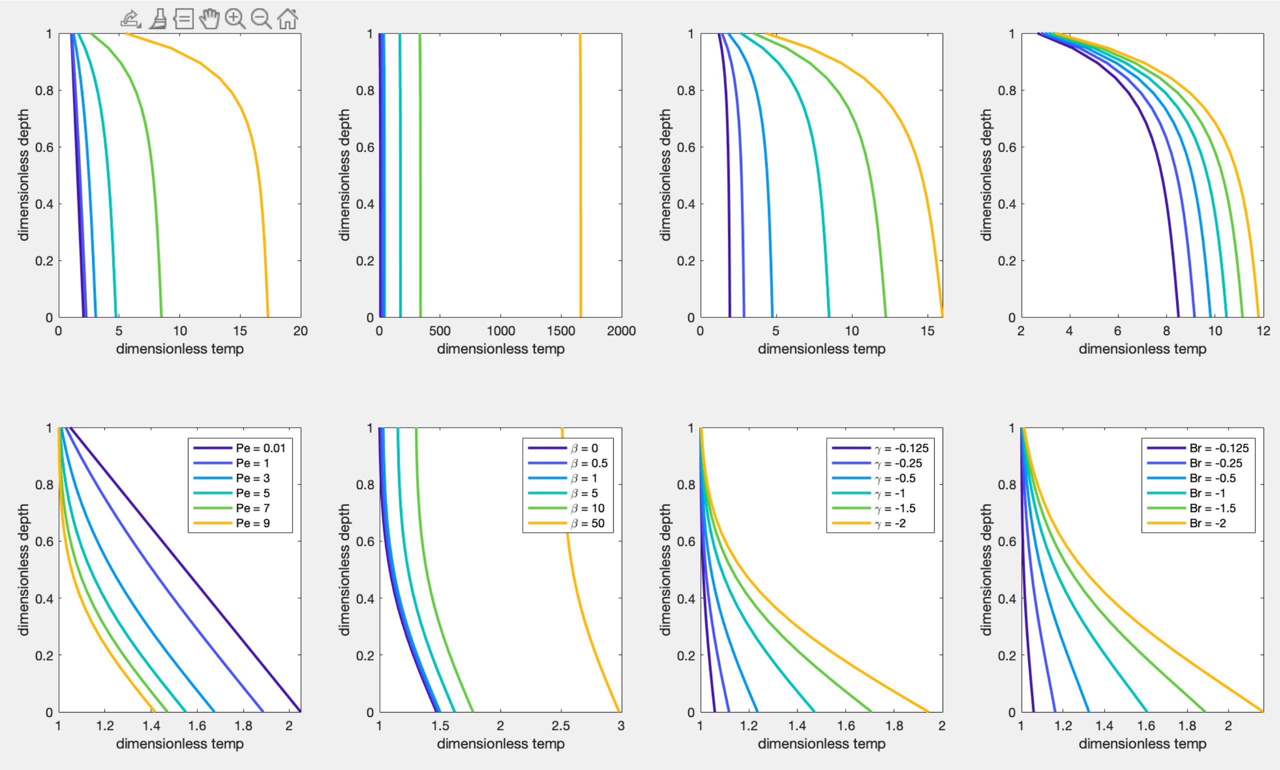
I find the motivation to be lacking. The authors claim that the paper builds upon the works of Robin and Lliboutry by including a time-dependent component, but I don't see what the benefit of the time dependent component is (more on this below). They say that this "allow[s] for a more accurate representation of the ice behaviour in response to changing external conditions", but then impose boundary conditions which are constant in time. They also claim that "transient solutions offer the potential to refine the interpretation of ice core data", but this seems to be a stretch to me (and is not elaborated on): the author's timescale on which solutions approach the steady state is kappa/L^2, which is on the order of minutes, suggesting that the ice column is always in quasi-equilibrium with the top boundary condition and the time-dependent state is not important.

While the authors have included horizontal advection in their model in a way, they do so via an awkward source term which is then completely ignored in their analysis (they focus only on a strain heating source term). The authors claim that the dimensionless horizontal advection term is in the range of 0-0.01, which I disagree with. To demonstrate this, I have quickly plotted Lambda = L^2 / (kappa*|T_air|) * V over the Antarctic ice sheet, where kappa = 36m^2/year is the thermal diffusivity, T_air = -20C is the air temperature, L is the ice thickness and V is the horizontal ice velocity, a proxy for the integral they consider. This plot shows that there are very few regions where the quantity Lambda is < 1; in fact, over most of the ice sheet it is very large, suggesting that horizontal advection is dominant. I had mentioned horizontal advection in my previous review and, while the authors need credit for trying to include it, they have not done so satisfactorily. The authors also mention the role of horizontal advection in their discussion, but it is so central to the problem that it cannot be ignored in a model that attempts to say anything useful about ice sheet temperatures.



Their equation (2), now updated to include a vertical advection term, is still missing a term from flow divergence. The first of equation (2) should read $\theta_t = \kappa\theta_{zz} - w\theta_z - w_z\theta + \Omega$. I am not sure how this would change the rest of the analysis but, given that they claim vertical advection is very important, this term could potentially be very important.

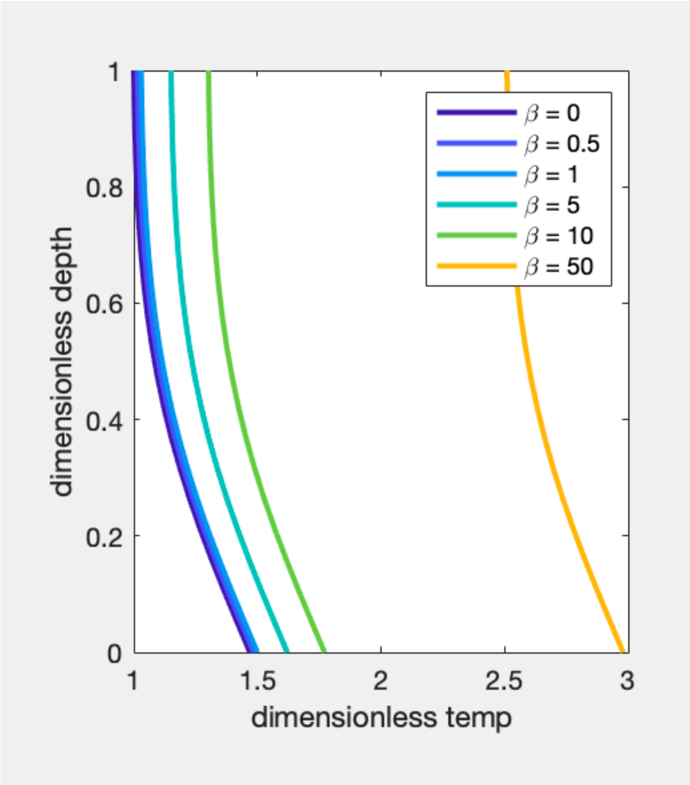
The solutions shown in figure 2 are not actually solutions to the problem (9): the solutions shown have the wrong boundary condition at the upper surface. You can see this from panel (f), for example: for beta = 0, the temperature should be 1 at the upper surface (just from reading off the boundary condition), whereas these plots show 0 temperature there. I suspect the authors have solved with a boundary condition $\beta v_\xi + v = 0$ at the upper surface. Whilst this doesn't seem to change the qualitative behaviour of the solutions (I coded this problem up myself and solved it numerically, see figure below.), it does call into question their analytic solution: is this only valid for the boundary condition $\beta v_\xi + v = 0$? In addition, the rest of their discussion in this section is based on these solutions, which are wrong. There are also sign errors in equation (9), Omega has the wrong sign (based on its definition in equation (2)) and so does gamma. The Brinkmann number is also referred to frequently as the Pe (see e.g. the caption of figure 2). It's also very confusing to introduce *parameters* and then not change their names when they are non-dimensionalized.



The time-dependent aspect of the model is not, too my mind, physically relevant: there is no situation where a 1km thick block of ice simply appears with a uniform temperature and then relaxes to an equilibrium. The authors also claim that "the time required to reach the stationary state is considerably shorter for w0 < 0", which is based on a single solution. Clearly, if you started at the w0 > 0 steady state, the opposite would be true, and so this statement cannot hold in practice. There may well be some physical reason why solutions with w0 < 0 converge faster (e.g. the interaction between the sign of w0 and the particular asymmetric boundary conditions), but this is not probed at all. The authors also present the time evolution of the energy in the system and say that "we can study how the total energy balance of the ice column depends on the four dimensionless numbers that determine the stationary solutions", but then don't study it at all…

In addition to this, there are more general statements, including most of those presented in the abstract, which are based on a small subset of simulations in a small region of parameter space. Numerical solutions, such as those presented in figure 2 are useful to understand how varying parameters affects the behaviour, but general statements about the behaviour over the whole of parameter space cannot be made.

- They say that "The Peclét number produces the largest changes in the equilibrium solutions", which doesn't even appear to be the case in figure 2 (for example, the variations in gamma are equally large).
- They say that "..under downwards advective conditions, the thermal basal equilibrium is found irrespective of the specific top boundary condition" (i.e. the solution is independent of beta): this is only true in figure 2f because the range of beta is so small. For larger beta, the solution does vary with beta (see e.g. figure below, which is as in figure 2f but for a larger range of beta). The authors claim that beta is in the range 0-0.125, which no justification, but, even if that were true, for other values of the parameters (e.g. weaker geothermal heat flux, weaker downwards advection), dependence on beta would be seen I suspect.



Finally, I find the EISMINT section very confusing. As far as I can tell, the authors fit a the velocity profile from the output of a numerical ice sheet model to a power law profile (this is not shown anywhere). They then use this exponent to determine the temperature profile, using dimensionless parameters which are also computed from the ice sheet model. They claim (but don't ever show) that this profile matches the output from the numerical ice sheet model. They also claim that this result gives an independent control against which ice sheet models can be verified. I disagree with this, since both the velocity profile and model parameters have to be determined from the ice sheet model itself. Even if so, I'm not really sure what benefit the analytic solution gives (by the way, the incomplete gamma function has to be evaluated numerically, so this isn't truly an analytic solution, and I think I commented on similar in previous revisions) over just solving the equation numerically, which authors have don previously (e.g. https://tc.copernicus.org/articles/16/1221/2022/).