Review of
Thermal structure of the Amery Ice Shelf from borehole observations and simulations by Wang et al.

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General comments

The authors present steady-state temperature-depth profiles through the ice at several locations along two distinct flow-lines at the Amery Ice Shelf, East Antarctica, derived from several years of observation within boreholes using thermistor strings and fibre-optical temperature sensing. They further study the sensitivity of simulated steady-state temperature fields on basal mass balance forcing using the full-Stokes ice sheet model Elmer/Ice in a three-dimensional model domain. Too add some sort of time dependency, the authors also conduct simulations with one-dimensional 'temperature only' simulations, where the boundary conditions vary along the path and vertical strain is prescribed from the borehole observations.

The authors state that the internal temperatures represent a record of the past climate and the thermal conditions upstream, and I would add, that they also serve as a very valuable observable to validate and/or calibrate numerical ice flow models. Especially as ice sheet model assessment is still mainly based on ice sheet wide integral or two-dimensional quantities (Goelzer et al., 2020; Seroussi et al., 2020) ignoring the information that is also preserved in the ice column, e.g. layer depths or temperatures.

The authors find, that basal mass balance forcing derived from remote sensing data leads to the best agreement between the model and the data (given the limitations of the model). I think, this is not surprising as long as ocean models only 'inform' the ice flow model about the basal melt rate without taking the heat flux within the ice towards the ice-ocean interface into account. The same holds for basal melt parametrisations that additionally do not account for basal freeze on.

A huge amount of work, time, money and will is needed to measure the temperatures within the ice and this data should definitely be published. The authors make a decent effort at clearly presenting data, models and methods. The figures are well thought out and informative. An honest discussion about the limitations in the models and the model set-ups is presented, and I appreciate that.

Nevertheless, I struggled following the line of thoughts. I think the authors should state in a more explicit way, there research questions and how the models can be utilised to find answers to those questions. Especially, which model is able to answer a particular question. I got the impression, that one could come to very similar conclusions running only the '1-D temperature column simulations', but with different forcing fields for the basal mass balance.

Provided Gladstone et al. is published, I could support publication of this manuscript with what I'd call 'minor' revisions.

Specific comments

The boreholes exist since several years and a lot of related work has been published as cited in the introduction and section 2.1. Unfortunately, I have difficulty in identifying which temperature-depth profiles are published here for the first time.

All the material regarding the 3-D steady-state temperature simulations with Elmer/Ice heavily depends on Gladstone et al. (2021, in preparation). Although the authors try to summarise the set-up, decisions relevant to this study, and conclusions, this is far from conclusive. E.g.:

- The 'quality' of the simulated 3-D temperature field depends on the inversion quality, and thus information about the missfit between observed and simulated velocity field should be provided (e.g. map of relative velocity difference).
- To what extent do the "further inversions for β and E_η^2" (P. 9, L. 201) change the initial fields shown in Fig. 2?
- I understand, that β and E_η^2 in Fig. 2 are the result of the optimisation in Elmer/Ice to match the observed velocities. Nevertheless, those results appear disconnected from or irrelevant for this manuscript. Further discussion or guidance is needed.

At several places within the manuscript the authors discuss the ice stiffness factor, $B(T_\mathrm{h})$. Often this seems disconnected from the surrounding text and from my perspective, adds nothing important to the manuscript. I would highly recommend to drop this entirely. Although an Arrhenius law is usually used, "standard physical parameters" (Table A1) do not exist and vary within the literature (e.g. Hooke (1981) versus Paterson and Budd (1982); Paterson (1994)). Even within the same numerical ice-flow model (Elmer/Ice), the choice of rheology parameters A_0 and Q (Eq. A1) depends on the model set-up/application (c.f. Gillet-Chaulet et al., 2012; Brondex et al., 2019, Tab. 1). Given the simple algebraic relationship between the ice stiffness factor and temperature, everyone could calculate this according to the specific application.

Detailed, line-by-line comments, suggestions and technical corrections

P. 1, L. 23–26: Remove "Given the temperature dependence ... input to future modelling studies" — This is only presented in the Appendix Fig. A1.

P. 4, L. 85: Reference to Warner at al., 2012 is missing in the "References" section.

P. 4, L. 96: "... The temperature profiles ... showed similar profile patterns (~~Craven et al. 2004,~~ Craven et al. 2009)." — I can't find temperature-depth profiles within the ice in Craven et al. (2004).

P. 4, L. 107–: "... with appropriately varying boundary conditions" — That sounds too evaluative to me (non-native speaker). Somewhere in discussion section (4.2.1) the steady-state assumption based on using present-day boundary conditions, even though one particular ice column could travel several hundred years, is mentioned as a model limitation. Probably something like *... with boundary conditions varying with position.*

P. 5, L. 133–: "To eliminate the seasonal signals and derive "steady-state" vertical temperature profiles ... " — From my understanding some sort of temporal averaging was applied to all the datasets listed in Tab. 1. If so, please state this.

P. 5, L. 137: "…a multi-year average surface temperature field" — Please state the period. 1979–1998 as on P. 11, L. 254 and P. 13, L. 285+ 305? If the same dataset is used for all the experiments please state this somewhere.

P. 8, L. 190: Consider to join the Seroussi et al., 2020 with Greve et al., 2020 to cite the model (Greve) and the specific model application (ISMIP6: Seroussi).

P. 9, L. 201–: "We carry out further inversions …, with different upper and lower surface boundary conditions …" — I can't identify the use of different upper boundary conditions later in the text or figures. With and without constrained emergence velocity? In Fig. 6 only basal boundary conditions differ.

P. 13, L. 285: Any reason not to use skin temperature from the RACMO2.3p2 simulations?

P. 13, L. 287–288: "We assume that the grounded ice has zero basal mass balance, while the upper surface is in positive mass balance …" – Is a positive mass balance also assumed (enforced) at the surface or is the RACMO smb by its own always positive? I do remember that close to the grounding line at AIS the RACMO smb could be slightly negative.

P. 13, L. 288: "…the value is extracted from RACMO2.3p2 …" I assume that a multi-year mean from the RACMO smb has been used here. If so, please state the time period. 1979–2016?

P. 13, L. 289 and L. 300: "Melchior Van Wessem et al. 2018" → "Van Wessem et al. 2018"

P. 13, L. 292–294: "We thus impose a vertical downward velocity …" — Is "downward" a direct consequence of the positive smb from RACMO or is this enforced. See also P. 13, L. 287–288.

P. 13, L. 299: "…the surface mass balance is extracted from RACMO2.3p2 …" From the multi-year mean smb or from the RACMO time-series data?

P. 14, Fig. 4: This figure mainly duplicates the data presented in Table 3 and I don't think this is worth the space. This figure mainly duplicates the data presented in Table 3, and I don't think this is worth the space. I prefer the authors add the surface and basal temperatures in the figure and drop the table. Keeping just the table would also be acceptable but less illustrative.

P. 15, L. 21: "…heat capacity and conductivity, are constant everywhere and will not change with vertical strain process." — According to Tab 4, heat capacity and conductivity depend on the temperature and thus, will evolve over time. Please clarify.

P. 15, L. 338: "…the simulations can be evaluated and optimized by comparing the simulated column temperature profile at the borehole sites with borehole measurements." — I don't understand which quantity/process/…is optimized. I thought everything is set now by the choice of model, model parameter and initial and boundary conditions.

P. 16, L. 366: Missing space in "Fig.5a" and also in "530 m–624".

P. 16, L. 368: "…maintains a hydraulic connection with the ocean below."

P. 16, L. 369: Not sure, but "above which" → "where"?

P. 17, Tab. 5: Observed temperature gradients and …

P. 18, L. 409–411: “Differences between the simulations and the borehole measurements in the upper surface ... because the upper surface temperature in the simulations is fixed by the Antarctic surface temperature dataset (Comiso 2000).” — I think this only explains the temperature offset in the upper part of the ice column. The observations at AM05, AM04 and AM01 (Fig. 6) clearly show a temperature profile that is dominated by downward advection near the surface. This is not the case for BMB_CAL2 and only limited for BMB_CAL. I suggest to look into the differences in the vertical velocity (or horizontal flux divergence) at those locations for the different bmb fields. Which model run agrees better with the observed velocity field?

P. 18, L. 410: “in the upper surface” → “at the upper surface”?

P. 20, Fig. 7: Please extent the x-axis of the insets to align with x-axis of the temperature cross-section. I found it very difficult to relate the basal mass balance to the temperature.

P. 22, L. 457: “... substantially consistent temperature regime.” — I don’t understand what is meant here.

P. 22, L. 476–: I think this paragraph should go. See ’Specific comments’ section.

P. 22, L. 478–479: “... the depth-averaged flow rate factor is very similar to that of the depth-averaged temperature” — I don’t see any reason why this should not exactly correspond to the temperature field (Eq. A1).

P. 23, L. 490: “The 1-D ... higher ~~vertical~~ resolution in the vertical direction than that from the 3-D simulations.” — This sentence is redundant. The 1-D model has only one dimension (vertical) and the model resolutions are stated on P. 13, L. 282 and P. 18, Fig. 6 and again in Fig. 9.

P. 24, L. 512–513: “..., where basal melting is $7\,\mathrm{m\,a^{-1}}$ and downstream of a region where melt rate exceeds 15 m/a.” — Melt rates above 6 m/a are invisible in Fig. 3 due to colour saturation. Please modify Fig. 3 accordingly. Use consistent units ($\mathrm{m\,a^{-1}}$ versus m/a).

P. 26, L. 587: “... their impact on our steady-state temperature simulations is to impose a non-physical advection of ice through the upper surface.” — Please explain, why this would be non-physical? It is already mentioned that those could arise from advecting the geometric features. Wow to distinguish?

P. 27, L. 602: “LAGS” or “LAGs”?

P. 27, L. 606–609: This is a very long sentence. Consider to split in parts.

P. 28, L. 652–655: “Given the differing ... show marked differences between the four simulations.” — This is a very long sentence and I think something got lost.

P. 29, L. 664–677: Drop this paragraph. I think it is sufficient to mention the importance of the thermal structure on the rheology and thus on ice dynamics in the Introduction and/or Conclusion.

P. 30, L. 705–707: “The depth-averaged temperature-dependent ice stiffness factor, ... modelling studies of the AIS.” — Although the inferred basal resistance parameter and the viscosity enhancement factor could be informative for other model studies, I still don’t think that this holds for the stiffness factor. Models could use the temperature field directly.

References

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