The Cryosphere Discussions

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Comment on tc-2021-357

William Colgan (Referee)

Referee comment on "Evaluation of six geothermal heat flux maps for the Antarctic Lambert–Amery glacial system" by Haoran Kang et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-357-RC1, 2022

This study explores geothermal heat flow in the Lambert-Amery sector of East Antarctica. A complex system of coupled models (shallow ice, full stokes, and subglacial hydrology) are initiated by six available geothermal heat flow maps to estimate basal temperature distribution across the sector. The basal temperatures are generally realistic and conform to expectation, with their differences being informative. Basal heat conduction is also presented and discussed, although the value of this field is less clear to me at the moment. Below, I provide some comments from this article.

Surface Accumulation – The surface accumulation field is used in the balance flux model, and presumably ultimately influences vertical velocity profile. I do not see any description, or citation, documenting the source of the surface accumulation field. It would be helpful to have better description of the accumulation field, including how possible biases in accumulation (or "recent" versus "steady" temporal variations) may manifest in the parameterization of vertical velocity field from the balance flux model, and ultimately in the simulated basal thermal state.

Topographic Effect – We discuss the topographic effect on geothermal heat flow in https://doi.org/10.1029/2020JF005598. Specifically, we highlight how subglacial topography in the subglacial Gamburtsev Mountains, East Antarctica, can strongly influence geothermal heat flow at kilometer scale. Whereby subglacial ridges can receive 50% less heat flow and subglacial valley can receive 50% more heat flow, in comparison to the regional average. I suspect that explicitly acknowledging the influence of subglacial topography on geothermal heat flow, by applying such a topographic correction field to the GHF input field used by the 2D SIA model, might further improve the realism of simulated hydrology.

Temperate Ice – I understand how the basal state is parameterized as either melting (Dirichlet; Eq 6) or freeing (Neumann; Eq 5), but the reader would benefit from knowing whether a thicker temperate basal ice layer is permitted. Temperate basal ice layers can form at the convergence of outlet glacier flow, even with tremendous downstream advection relatively cold inland ice (https://doi.org/10.3189/172756502781831322). The presence or absence of a temperate basal ice layer clearly influences the vertical temperature profile, which here seems critical to presented basal heat conduction (i.e. whether temperature gradient simply pinned to the pressure-melting point at the bottom, or the temperature gradient effectively becomes the Clausius–Clapeyron gradient). Allowing ice to become temperate general requires assumptions about liquid pore water content, which I do not see stated here.

Heat Conduction – I am confused by Figure 8. I would expect that, in the absence of basal hydrologic processes, basal heat conduction is effectively equivalent to geothermal heat flux. Yet inland areas, where basal hydrology is not active, have a very different heat conduction from the forcing geothermal heat flux. The sign of basal heat conduction is also negative, in comparison to the positive sign/direction of geothermal heat flux. Finally, should there not also be "opposing signed" pockets where the basal heat conduction is opposite over subglacial areas where basal water is refreezing (i.e. Vostok in https://doi.org/10.5194/tc-14-4021-2020)? Right now, the axis stops at zero. For these reasons, I find Figure 8 (and associated discussion) difficult to follow.

Subglacial Lakes – Here, subglacial lakes are being used as an indicator of basal ice at the pressure-melting point. The subglacial lake literature, however, now has suggestions that melting temperatures can be depressed significantly lower than pressure-melting point by salinity (https://doi.org/10.1126/sciadv.aar4353) and that radar-derived subglacial water indications can be false positives (https://doi.org/10.5194/tc-14-4495-2020). I think these caveats should be mentioned. The L511-515 discussion should also use the terminology "false negative" to describe the "one-sided" aspect constraint.

Consensus Map – Given the time and interest that the authors have clearly expended with this study, it would seem that they are in a very good position to produce a consensus geothermal heat flow map of the Lambert-Amery sector. My final thought would be asking why the authors simply stop with saying the Li and Martos geothermal heat flow maps are most suitable for this region, and do not provide an accompanying data product of a geothermal heat flow map that is self-consistent, or optimized, with an ice flow model (i.e. https://doi.org/10.20575/00000006)?