This is a very strong and promising manuscript that I would recommend for moderate revision. It presents a well-executed regional case study and an insightful assessment of radar-derived bed geometry and properties around Dome C. My only real concerns are related to the presentation of hydrology and GHF estimates, and should be easy to address.

The background is well written, and the processing steps are generally described in sufficient detail. The data repositories and links, and GitHub provided code repository are well organised. The figures are very clear and informative; however, the labels are a bit small sometimes. Figure 2 prompts useful reflection on continental-scale products and survey consistency. The overall presentation is careful and transparent. New KOPRI–UA data are presented; as far as I am aware, they are not yet incorporated into continental-scale maps.

The use of SGS is appropriate and well-motivated. The ensemble approach allows the authors to quantify uncertainty and preserve basal roughness and other derived properties. Since this SGS ensemble underpins all downstream analyses (roughness, MAD, drainage, GHF), a short sensitivity discussion would be helpful. For example, how sensitive are roughness metrics and MAD fields to the chosen model and its parameters, or to different anisotropy assumptions? Even a description of tests performed during development would increase confidence in the robustness of the simulations. The recent work by McCormack et al. (2026) could provide a useful framework that the authors could draw on. Here, elevation-preserving and texture-preserving synthetic beds are distinguished to show that methodological choice can substantially affect model behaviour and projections. This supports the authors' argument that smoother, single-product interpolations (such as Bedmap3 or BedMachine) are not interchangeable with ensemble, texture-preserving products. I suggest the authors explicitly frame their SGS-based Dome C ensemble in the sense of McCormack et al. and potentially also emphasise that ensemble-based beds and spatially explicit uncertainty maps are central not only for propagating uncertainty into models but also for designing further targeted surveys. This aligns well with the manuscript's MAD fields and the discussion of data-sparse regions.

The subglacial hydrological analysis has no major flaws. However, there are some assumptions applied. The drainage analysis assumes uniform basal melt and a water pressure ratio that effectively equates water pressure to full ice overburden, whereas real systems typically exhibit spatially variable effective pressure due to evolving drainage configurations, subglacial geology, and, likely, groundwater coupling. It would strengthen the manuscript if the authors either tested a small set of alternative scenarios (for example, different k values or spatial patterns) or, at a minimum, discussed more explicitly how these assumptions might affect predicted drainage pathways and the robustness of the conclusions in this case study.

Some of the GHF models listed around L416–L417 and plotted in Fig. 6 appear to be of limited practical relevance for present-day dynamics and might best be regarded as legacy or experimental products; discussed in Lösing et al. 2026.

On the geothermal heat flow (GHF) analysis, I appreciate the effort to quantify topographic focusing and to place the Dome C region into a broader GHF context. However, I am less convinced that the GHF results are as reliable or as useful as the rest of the manuscript. The reported topographic adjustments in Fig. 7, with median anomalies up to about ±30%, are difficult to reconcile with the amplitudes and wavelengths of the bed undulations described here. For topography wavelengths of order 10–20 km and amplitudes below roughly 2000 m, an unrealistic conductivity contrast is required, and a significant fraction of heat must have been refracted horizontally over long distances. This could almost raise the possibility of a unit mismatch, or at least an over-interpretation of the magnitude of the effect. I would expect most of the topographic effect to reshape the heat transfer on a smaller scale, a few kilometres. The effect has a certain bandwidth and should be negligible over, say, 10 km or so. The key control (in addition to bed geometry) is the contrast in thermal conductivity between the shallow crust and the base of the ice sheet. Ice conductivity is temperature dependent in a fairly narrow range, but it lies near the centre of the distribution of plausible rock properties. These issues have been discussed in Willcocks et al. (2021). Willcocks et al (2025) use the Petrochron database (Sanchez et al 2021) to examine thermal conductivity distributions. It is likely that the refraction is reversed in many regions, focusing heat on ridges if the thermal conductivity of the rocks is higher. Especially for e.g. granite and sandstone. I suggest that the authors consider these complications and present their GHF adjustments as illustrative rather than as actual estimates ready to be used in thermomechanical or ice-dynamics models. In its current form, the GHF section risks being overinterpreted compared with the strength of the underlying assumptions. The finer details of the upper-crustal geology strongly influence heat transfer (Stål et al., 2024).

In light of the above, I suggest:

L 422:
Topographic focussing of geothermal heat lowers the amount of heat received along ridges and increase it in valleys relative to the background value (van der Veen et al., 2007). -> e.g: Topographic focusing of geothermal heat depends on the thermal conductivity contrast between ice and rocks and can redistribute heat to local ridges or valleys (Willcocks et al., 2021; Reading et al., 2022).

A broader point is that subglacial hydrology, including channelised drainage and distributed systems and groundwater, has the potential to redistribute heat by advection at rates far exceeding the topographic focusing effects emphasised here, and to include latent heat associated with melting and refreezing.

I want to stress that I appreciate what the authors are trying to communicate in this section: that local variability in basal conditions is likely large and that topography will contribute to this variability. My main concern is that, given current uncertainties in conductivity structure, hydrology, and especially the unknown shallow geology, the specific numerical values presented for GHF adjustments may give a stronger impression of precision than is warranted. A clearer framing of this analysis as exploratory, together with a cautious recommendation for how (or whether) these adjustments should be used in downstream modelling, would make this part of the manuscript more balanced.

Minor points:

- L220 and L284: “Sterographic” ->“Stereographic”
- L421: “focussing” -> “focusing”.
- A scale bar in Fig. 7b would be useful.

In summary, I find this to be a very promising and generally well-executed manuscript. The radar data integration, SGS ensemble, roughness analysis, and hydrological assessment are all valuable contributions. With a clearer and more cautious framing of the GHF results, a brief sensitivity discussion of the SGS ensemble, the manuscript will make a strong contribution to our understanding of subglacial boundary conditions near Dome C. I recommend moderate revision; however, hopefully easy to implement.

Below are some suggested references:

Beardsmore, G. R., and J. P. Cull. *Crustal Heat Flow*. In *Crustal Heat Flow*. Press, Cambridge University, 2001. https://doi.org/10.1017/cbo9780511606021.

Lösing, Mareen, William Colgan, Tobias Stål, et al. “Community Heat Flow Recommendations: Suitable Basal Boundary Conditions for Greenland and Antarctica in ISMIP7.” *GEUS Bulletin* 62 (March 2026). https://doi.org/10.34194/r0w9rf81.

McCormack, Felicity S., Tobias Stål, Niya Shao, et al. “Synthetic Bed Topographies for Antarctica and Their Utility in Ice Sheet Modelling.” *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences* 384, no. 2319 (2026): 20240537. https://doi.org/10.1098/rsta.2024.0537.

Reading, Anya M., Tobias Stål, Jacqueline A. Halpin, et al. “Antarctic Geothermal Heat Flow and Its Implications for Tectonics and Ice Sheets.” *Nature Reviews Earth and Environment*, no. 12 (2022): 814–31. https://doi.org/10.1038/s43017-022-00348-y.

Stål, Tobias, Jacqueline A. Halpin, John W. Goodge, and Anya M. Reading. “Geology Matters for Antarctic Geothermal Heat.” *Geophysical Research Letters* 51, no. 13 (2024): e2024GL110098. https://doi.org/10.1029/2024GL110098.

Willcocks, Simon, Derrick Hasterok, and Samuel Jennings. “Thermal Refraction: Implications for Subglacial Heat Flux.” *Journal of Glaciology* 67, no. 265 (2021): 875–84. https://doi.org/10.1017/jog.2021.38.

Willcocks, Simon, Derrick Hasterok, Jacqueline A. Halpin, Jessica Walsh, and Samuel Jennings. “Compositional Controls on the Thermal Conductivity of Igneous Rocks and a Model for the Conductivity of Antarctic Crust.” *Tectonophysics* 911 (August 2025): 230802. https://doi.org/10.1016/j.tecto.2025.230802.