Authors response to review by Tobias Stål for:

"Improved subglacial boundary conditions near Dome C, Antarctica, through rigorous integration of multi-campaign radar data and an ensemble stochastic simulation approach"

Shackleton et al., The Cryosphere Discussions.

Thank you for your review of our work, we appreciate the useful suggestions and insightful comments you have made and have incorporated these into our revised manuscript. In the following response to reviewers comments, the reviewers text is shown in *blue italics* and our response or action taken is shown in black. Text added to the revised manuscript is indicated by quotation marks.

"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 following action was taken to address the comments made above, which include discussing the framework outlined by McCormack et al., 2026, expanding upon the description of simulation sensitivity to chosen roughness model, parameters, and anisotropy solutions, as well as adding description of the range of tests we ran during the development stage of our approach. The following text was added or modified:

a. *"Compared with single-grid, elevation-preserving products such as Bedmap3 (Pritchard et al., 2025) and Bedmachine v4 (Morlighem, 2026), texture-preserving bed gridding approaches can provide more realistic boundary conditions for model projections (McCormack et al., 2026). While requiring an ensemble-based framework which increases computational cost and methodological complexity, this approach generates spatially explicit uncertainty estimates to propagate into models and can be used to prioritise sites for future geophysical surveys. Sequential Gaussian Simulation (SGS) is one texture-preserving geostatistical method that simulates values between measurements based on the values and statistical properties of nearby data (Deutsch & Journel, 1997)."*
b. *"To more accurately model spatial variability accounting for azimuthal differences, we calculated directional variograms across grid azimuths centered at 0°, 45°, 90°, 135° (Fig. 3c) relative to Polar Stereographic projected axes. This range adequately captured azimuthal variability when compared to larger ranges examined during exploratory tests and should be sufficient for data aligned on a grid."*
c. *"We tested combinations of search radii from 30 – 100 km and maximum number of conditioning data between 60 and 400 during the developmental phase to find optimum parameters which balanced simulation stability and compute time."*
d. *"Methodological, model, and parameter choices may impact our simulation results, and during the development stage of this methodology we considered and experimentally explored the impact of these choices to ensure robustness of the results. Our simulation approach can be sensitive to the choice of variogram model used as well as and the model fitting step to determine the range and sill for each azimuth. While regional spatial patterns in simulated elevation were mostly unaffected by these in preliminary tests, model choice impacted local spatial patterns and model fit influences the scale of local variability in simulated grids. It is therefore crucial to conduct a thorough exploratory analysis of the input data to ensure that the model choice and parameters adequately represent the spatial statistics of observations before proceeding with simulations. We mostly implemented a programmatic approach to ensure objectivity in model and parameter selection and used the exploratory experiments to manually check parameter selections were appropriate. Also, modelling spatial variability using isotropic instead of anisotropic experimental variograms produced similar regional spatial patterns and range in simulated bed elevations, and we chose anisotropic to more closely follow the patterns observed in measurements (Fig. 3c). Representing anisotropy using 2 orthogonal directions rather than implementing a more complex approach using continuously varying directional ranges balances improved local heterogeneity and computational complexity."*

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.

This is an important point and we agree that it should be addressed in the manuscript to further clarify our work. The following text was added to the methodology:

"We assume that spatial patterns in subglacial water routing are dominantly controlled by topography and hydraulic pressure and not influenced by basal melting rate, and that the ratio between subglacial water pressure and ice overburden pressure, k, is equal to 1. In reality, these properties are expected to vary spatially and temporally in response to drainage configurations, subglacial geology, and groundwater interactions which are highly uncertain beneath Antarctica. During sensitivity testing, alternative pressure scenarios of $0.6 \leq k \leq 1.0$ at 0.1 intervals resulted in local shifts in drainage pathways and basin boundaries particularly in regions with low hydraulic gradients, while the broader patterns remained unchanged. Using our assumptions, we quantify how uncertainty in bed topography propagates into subglacial hydrological predictions under commonly adopted assumptions rather than reproduce the full complexity of the basal drainage system."

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.

Thank you for drawing our attention to this and for pointing us to the expert review by Lösing et al. 2026. After reading this I agree that some of the GHF models included in Figure 6 could have limited relevance for present-day applications. Since the inclusion of the legacy/experimental model outputs does not affect our analyses or impact the results/conclusions of our work, we will keep them in here, but certainly take this recommendation into account in future work.

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)."*

Thank you for these useful suggestions and insightful comments, especially regarding the conductivity contrast and potential for reversed polarity in topographic modifications of GHF. We agree that our estimate is illustrative and modify the manuscript to reflect this.

Added/modified the following text:

"Topographic focusing of geothermal heat can redistribute heat to local ridges or valleys, and the polarity of heat refraction patterns depends on the thermal conductivity contrast between ice and the underlying rock (Wilcocks et al., 2021; Reading et al., 2022). The thermal conductivity of ice at depths beyond 750 m is estimated between 2.0 – 2.8 W m^{-1} K^{-1} (Wilcocks et al., 2021), whereas igneous Antarctic bedrock ranges 1.5 – 4.0 W m^{-1} K^{-1} in samples limited to exposed outcrops mostly near the ice margin (Wilcocks et al., 2025). The geology and corresponding thermal conductivity are uncertain beneath Dome C, so as an illustrative example of how topography could impact geothermal heat distribution we calculated an adjustment factor to account for local topographic modifications of background GHF following a simplified geostatistical approach (Colgan et al., 2021), using their equation 2 with empirically determined averaging radius of 5 km and characteristic height needed to induce 100% change in local GHF of 950 m. However, we recognise that the magnitude and polarity of such modifications could be substantially different in reality depending on the conductivity of the bed."

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).

Replaced the previous statement with the suggested text and added references to reference list.

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.

This is a good point and the authors agree that a high degree of precision is not yet achievable due to the many unresolved Antarctic subglacial environment knowledge gaps. To more explicitly state that our work presents applications of the simulated bed to illustrate a potential pattern of topographic modification to GHF and attempts to constrain the range of possible meltwater routes rather than offer precise values/locations, the authors added the text previously described in the section on topographic focussing of GHF as well as added/modified the following text in the subglacial meltwater drainage section:

"We modelled basal hydraulic potential across the ensemble to constrain the range of possible subglacial water flow directions, meltwater routing, and drainage basins assuming uniform basal melting. The simulated bed ensemble constrains drainage uncertainty associated with bed elevation uncertainty, which we compare to drainage predictions using the Bedmap 3 interpolation (Fig. 5a,b)."

Minor points:

- *L220 and L284: "Sterographic" ->"Stereographic"*

Changed.

- *L421: "focussing" -> "focusing".*

Changed.

- *A scale bar in Fig. 7b would be useful.*

Added a scale bar in the upper right corner of Fig. 7b.

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.