Authors response to **review round 2** by Shuai Yan 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.

The authors have responded constructively to both reviewers, and I find the revised manuscript substantially improved. The manuscript is generally clear, the underlying radar-data integration and stochastic bed simulations represent a valuable contribution, and I do not think major additional analyses are required.

I have one remaining substantive concern, related to the geothermal heat flow (GHF) analysis. The authors now appropriately acknowledge that the magnitude and even the polarity of topographic redistribution of geothermal heat depend on the thermal-conductivity contrast between the ice and underlying bedrock, and that the geology and thermal conductivity beneath Dome C are poorly constrained. They therefore describe their GHF calculation as an illustrative example. I agree with this revised framing. However, this caution is not yet applied consistently throughout the manuscript. In several other parts of the manuscript, however, the resulting numerical adjustments are still presented more like physically constrained estimates. For example:

- The Abstract states that topographic focusing "predicts" local anomalies of up to ±30% and that several drill sites lie in a zone of approximately 12% reduced geothermal heating that is favourable for preservation of deep ice.

Modified the wording to the following, with relevant adjustments in red:

"Topographic focussing of geothermal heat could lead to local anomalies up to ±30% relative to the regional value, and several ice core drill sites are in a zone with potentially 12% locally-reduced geothermal heating which could be favourable for deep ice layer preservation."

- In Section 4.3, the manuscript states that elevated GHF of up to 30% "can be expected," interprets the 12–13% reduction at BELDC and MYIC as favourable for ice preservation, and suggests that borehole GHF estimates should be adjusted using the calculated topographic corrections.

Modified the wording to the following, with relevant adjustments in red:

"Using our adopted parameterization of GHF redistribution, we show that locally elevated GHF up to 30% more than the background value could occur within the troughs (e.g. Adventure, Concordia), yet along ridges (e.g. Concordia) local GHF could be 30% lower (Fig. 7a,b)."

"Our analysis shows that the EPICA core site could be situated in locally elevated GHF by 2% (Fig. 7c), whereas the new sites BELDC and MYIC are situated in a region with potentially lowered GHF by 12 -13% which could be favourable for deep ice layer preservation. Considering the impacts of uncertain bed geometry alone, variability over the ensemble is less than 0.08% for all ice core sites since they are generally close to radar data. Borehole GHF estimates should also be adjusted to account for the potential range of local topographic modifications before extrapolating over a larger region."

- The Conclusions state that topographic focusing "likely enhances basal melting" in troughs and could exert a control on the distribution of subglacial lakes.

Modified the wording to the following, with relevant adjustments in red:

"Topographic focussing of geothermal heat could enhance basal melting in many trough locations and exert a control over the distribution of subglacial lakes; our analysis shows most radar-inferred subglacial lakes occurring in a region where we predict locally elevated GHF."

I think these statements remain significantly stronger than is justified by the assumptions now acknowledged in the revised manuscript. If the conductivity structure of the bed is unknown and the polarity of thermal refraction can potentially reverse, then the specific values and signs shown in Fig. 7 should not be interpreted as robust local GHF estimates.

I do not think that a new thermal model is necessary at this stage. A simpler solution would be to consistently describe Fig. 7 and the associated numerical values as an **illustrative scenario under the adopted parameterization**. Statements in the Abstract, Discussion, and Conclusions should then be softened accordingly. For example, the results can demonstrate the *potential magnitude and spatial scale* of topographic redistribution, rather than establishing that GHF is actually reduced by 12–13% at particular drilling sites or enhanced by 30% in particular troughs.

The language used to describe our results has now been modified throughout the manuscript to better reflect that that we present an illustrative scenario for potential GHF redistribution.

Monir points:
(1) I think the manuscript should distinguish more clearly between two different sources of uncertainty. The very small ensemble variability reported at the core sites (<0.08%) appears to quantify uncertainty associated with the simulated bed geometry while holding the GHF parameterization fixed. It does not incorporate uncertainty in bedrock thermal conductivity, the assumed averaging radius, the characteristic height, or the polarity of thermal refraction. The present wording could therefore give the impression that the 12–13% adjustment is known very precisely, when the structural uncertainty in the GHF parameterization is likely much larger. Please state explicitly what the reported ensemble uncertainty represents and what it does not represent.

Added/modified the following text in section 4.3:

"Note that our confidence in adjustment factor is based only on uncertainty in bed geometry, and neglects uncertainty associated with bedrock thermal conductivity and polarity of thermal refraction, or the averaging radius and characteristic height parameters."

"Our analysis shows that the EPICA core site could be situated in locally elevated GHF by 2% (Fig. 7c), whereas the new sites BELDC and MYIC are situated in a region with potentially lowered GHF by 12 -13% which could be favourable for deep ice layer preservation. Considering the impacts of uncertain bed geometry alone, variability over the ensemble is less than 0.08% for all ice core sites since they are generally close to radar data."

(2) I also think the explanation of the 5 km averaging radius and 950 m characteristic height should be transferred from the response letter into the manuscript itself. In the response, the authors explain that, because the geology beneath Dome C is unknown, they adopt the globally averaged parameter values from Colgan et al. (2021). This is useful information and directly addresses my original question about the choice of these parameters. In the manuscript, however, they are currently described only as empirically determined values. Please state explicitly that they are adopted global-average values and explain briefly why they are used here.

Modified the wording to the following:

"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 the simplified geostatistical approach of Colgan et al., (2021). Since the geology is not known, we adopt their globally averaged empirically determined averaging radius of 5 km and characteristic height needed to induce 100% change in local GHF of 950 m, using their equation 2 to calculate potential magnitude and spatial patterns of heat redistribution. However, we recognise that the magnitude and polarity of such modifications could be substantially different in reality depending on the conductivity of the bed."

(3) My previous suggestion to add an Antarctica overview/inset map to Fig. 1 does not appear to have been addressed. I consider this optional at this stage, but I still think a small inset showing the location of Dome C within Antarctica would improve the accessibility of the figure for readers less familiar with the region.

(4) Please check the spelling of Willcocks et al. The newly added text appears to use "Wilcocks et al." in several places, whereas the reference list gives "Willcocks."

Modified throughout the manuscript.

(5) The manuscript cites Willcocks et al. (2025) in the revised GHF section. I believe it should be Willcocks et al. (2021).

Changed.

Overall, I am satisfied with the revisions to the core radar, stochastic-simulation, roughness, and hydrological components of the manuscript. My remaining concern is primarily one of

appropriately communicating the limitations of the illustrative GHF calculation rather than a need for substantial new analysis. I therefore recommend minor revision.

Authors response to **review round 2** by Tobias Stål for:

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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.

The revisions read well.

A few minor suggestions:
L70 ice core community are trying -> ice core community is trying

Changed.

L74 coring -> Coring

Changed.

L87 Dome C plateau -> Dome C Plateau

Changed.

L145 in-flight configured -> configured in flight

Changed.

McCormack et al. (2026, Philos. Trans. R. Soc. A) is cited but missing from the bibliography.

Added to the bibliography.

No further comments.