Review comments for the manuscript "**Improved subglacial boundary conditions near Dome C, Antarctica, through rigorous integration of multi-campaign radar data and an ensemble stochastic simulation approach**" by Shackleton et al., for *The Cryosphere*.

This manuscript presents an improved subglacial boundary-condition dataset for the Dome C region of East Antarctica, by rigorously integrating multi-campaign radar-derived ice-thickness measurements and applying an ensemble stochastic simulation approach. The study combines new airborne radar observations with existing data products, performs careful measurement-compatibility screening, and generates an ensemble of simulated ice-thickness and bed-elevation grids. The resulting products are then used to assess bed-elevation uncertainty, basal roughness, subglacial meltwater routing, and local topographic effects on geothermal heat flow.

The manuscript is especially valuable because it goes beyond producing a single deterministic bed product and constrains topographic uncertainty through an ensemble framework. This contribution is also very timely. The recent recovery of a continuous, very old ice core confirms the Dome C region as a promising target for old-ice studies. At the same time, the region contains many radar-inferred subglacial lakes and possible meltwater pathways, which could threaten the preservation of deep ice stratigraphy. In this context, the authors' effort to link improved bed topography, drainage uncertainty, geothermal heat-flow modification, and old-ice preservation is valuable and relevant.

My comments are listed below, and I hope they will help improve the manuscript. I would be happy to review the manuscript again, if needed, once these comments have been addressed.

Major comments:

(1) My main concern is about the interpretation of local topographic redistribution of GHF. I appreciate that the authors examine the apparent coexistence of well-preserved old ice and subglacial lakes in the Dome C region, which is an interesting and important question. The manuscript suggests that topographic variation can redistribute GHF, and uses it to discuss basal melting, subglacial lakes, and old-ice preservation. However, this interpretation appears to rely on a topography-only correction, whereas topography is not the only factor that can modify GHF locally. In particular, the analysis does not consider lateral variations in thermal conductivity associated with bedrock geology. This is important because thermal refraction can substantially modify, or even reverse, the expected topographic effect, depending on the conductivity contrast between ice and bedrock (Willcocks et al., 2021, doi.org/10.1017/jog.2021.38). I strongly recommend the authors discuss this limitation.

(2) The authors find that approximately 80% of radar-inferred subglacial lakes are located in areas of locally elevated GHF, and use this result to suggest that topographic redistribution of geothermal heat may help explain the distribution of subglacial lakes. However, I think this interpretation needs to be stated more cautiously. A locally elevated GHF relative to the regional background does not necessarily imply that basal melting will occur. If the heat flux is still insufficient to bring the bed to the pressure-melting point, no lake would be expected. Therefore, the authors should clarify whether the magnitude of the estimated GHF enhancement is sufficient to change the basal thermal state, or whether this result should be interpreted only as a possible contributing factor.

(3) The method used for the topographic GHF correction needs more explanation. In particular, the authors should clarify how the 5 km averaging radius and the 950 m characteristic height (line 425-426) were chosen or derived. Although a reference is provided, these parameters directly affect the magnitude and spatial pattern of the correction, so readers need more information to evaluate whether they are appropriate for the Dome C region.

(4) One strength of this paper is the incorporation of newly acquired radar data relative to previous datasets. The authors provide substantial technical detail about the radar instruments and data processing, but they do not clearly show where the new data are located relative to the previously available measurements. I recommend adding a map, or modifying Figure 1a, to distinguish the new survey lines from older datasets.

(5) I recommend that the authors increase the font size in the figures, particularly some of the axis labels and in-figure annotations.

Minor comments:

Line 15: I suggest softening the statement that subglacial topography "remains highly uncertain across Antarctica." A large volume of direct measurements (from radar sounding, etc.) of subglacial topography are available now across a large portion of Antarctica. The uncertainty is still substantial in data-sparse regions and between survey lines, but the current wording may overstate the level of uncertainty across the entire continent.

Line 124 – 129: What's the impact on data quality? And what's the resolution for this system?

Line 337: "for each of our ensemble simulated bed" should be "for each of our ensemble simulated beds".

Line 400: "The preservation of deep ice layers … are sensitive" should be "The preservation … is sensitive".

Line 416: "large-sale GHF models" should be "large-scale GHF models"

Lines 422: "increase it in valleys" should be "increases it in valleys".

Line 436: "regional standard deviations 0.1 ± 0.0%" should be "regional standard deviations of 0.1 ± 0.0%" or "regional standard deviations (0.1 ± 0.0%)".

Figure 1: I suggest adding an Antarctica overview map. Also, I recommend that the authors explicitly state what the histograms along the colorbars represent.

Figure 3 caption: "Polar Sterographic projection" should be "Polar Stereographic projection".

Figure 7: The lake sites and core sites are difficult to see. The symbols are too small, and the green and orange colors do not stand out clearly against the background.