Reply to the reviewer 2

We thank the reviewer for careful review of our manuscript and thoughtful comments to improve it. In the following, we describe our responses (in blue) point-by-point to each of reviewer's comment (in black).

Review of "High-resolution subglacial topography around Dome Fuji, Antarctica, based on ground-based radar surveys conducted over 30 years" by M.J. Wolovick et al.

Summary

This MS describes a new grid of ice thickness and subglacial topography in the vicinity of Dome Fuji in East Antarctica. The underlying data and their strengths and limitations are summarized, the details of the gridding are discussed an evaluation of the output is performed against existing datasets.

The MS is mostly what it claims to be, which is refreshing, although no significant geophysical insight is gained into the Dome Fuji region beyond the subglacial topography that is presented. This limits the long-term value and reach of the MS, but the MS is thorough in its analysis of these data and in the clear application of necessary corrections (e.g., firn). The authors make a convincing argument that multi-element ground-based Yagi antennas are a reasonable alternative to SAR focusing. Separately, MS is well structured and visualized, but contains within several presentation decisions that raise concerns, outlined below.

We thank the reviewer for positive evaluation for multi-element ground-based Yagi antennae system used in this study. We will further discuss geographical and glaciological insights analyzed from ice thickness and subglacial topography data in the revised manuscript.

Comments

Data availability. It's not clear to me if the raw radargrams or lat/lon/thickness data are already available. If not, they ought to be. Otherwise, it implies proprietary data used herein are simply remaining so, which is not a great look in 2021. Along the same lines, the draft grid ought to be available publicly upon submission for review. This may not be required by TCD, but it is increasingly recognized as good practice and is required by some Copernicus journals. In my view, the authors should, at a minimum, point to a public repository with *both* the

JARE lat/lon/thickness data and the grid. Prior to publication.

As described in reply to the reviewer 1 comments, we have released both the gridded data and the point data obtained from 1992 to 2019 with bias corrections in ice thickness in data repository site of Arctic and Antarctic Data archive System (ADS), National Institute of Polar Research. Data title and DOI are attached in reply to the reviewer 1 comments.

What is NDF? It is never defined other than its location. I'd have assumed it meant "North Dome Fuji", but that doesn't make sense geographically based on its location. Further, it is inconsistently identified in the figures. Shows up in some, not others.

NDF is a name of our base camp on a 2017-2018 field campaign for the radar investigation. We considered "New Dome Fuji" although this exact location will not be a new site for the 3rd deep ice coring. We simply use this name to indicate location of our base camp. We will identify the location of NDF in the figures in the revised manuscript.

274-277: It's not clear to me why deep ice in subglacial troughs is subject to "complex ice flow" but that it is not the case for to subglacial ridges? See, e.g., Bell et al. (2011, Science, https://www.science.org/doi/10.1126/science.1200109) on the Gamburtsev Mountains.

According to radio echo images derived from ground-based and airborne radar measurements (Fujita et al., 1999; Karlsson et al., 2018; Rodriguez-Morales et al., 2020), no frozen-on ice features were observed in our study area in the vicinity of Dome Fuji (approximately 120 km x 100 km). In contrast, Bell et al. (2011) observed frozen-on ice features in the very wide region (approximately 720 km x 240 km) of Dome A. In the manuscript, we are discussing much narrower area around the dome summit than Bell et al. (2011) did for Dome A. Accordingly, horizontal flow velocity in our study area is less than 1 m a^{-1} (see Fig. S7 of Karlsson et al., 2018), suggesting that basal ice rheology is dominated primarily by a vertical normal stress, and horizontal shear stress is relatively small. Thus, we can focus our discussion on regions without frozen-on ice features and without major horizontal flow components. Under such dominance of the vertical normal stress, horizontal shear appears mainly on subglacial slopes than ridges or troughs. Basal troughs are often influenced by basal melt or connected to deeper troughs of more basal melt. Then, troughs tend to be fast pathways for ice flow. We therefore suggest that subglacial ridges in our study area are under simple ice flow condition, compared to slopes or troughs in terms of preservation of layered conditions. We plan to address this point in the revised manuscript.

Figure 3a: Given the contour lines shown, why not also use a discrete color bar? Little is gained from the continuous color bar, as features are not distinguishable between e.g., 2800 and 2825 m thickness at this scale.

We will modify the Figure 3a to a discreate color bar with a 50 m interval.

Figure 4: Was "H" defined prior to mention in this x-axis? I assume it denotes ice thickness, following convention, but it would be good to clarify if in fact it wasn't defined.

We will modify all the figures that H (ΔH) are changed to ice thickness (Differences in ice thickness).

15: Degrees/minutes/seconds are archaic. Please present station coordinates in decimal degrees instead.

We will address this point in the revised manuscript.

17: How close to the pressure-melting point?

The bottom ice drilled at the Dome Fuji reached to the melting point. We will change the description from "was close" to "reached" in the revised manuscript.

25: What is "it"?

It means "the bed". We will change the description to "the bed is estimated to be frozen" in the revised manuscript.

33: not yet identified

We will change the description from "not been retrieved" to "not yet identified" in the revised manuscript.

47: What is "solid" smoothing?

We used "solid" to indicate stronger smoothing effect with larger geographical parameters.

We will change the description to clear for readers in the revised manuscript.

75-77: The mean annual temperature and accumulation rate presented here and in Figure S2 do not appear to add much to the discussion in the MS.

Thanks for the suggestion. The mean annual air temperature and accumulation rate are fundamental information for glaciological environments of the ice sheet. In the revised manuscript, we will discuss these parameters with glaciological insights analyzed from ice thickness and subglacial topography data associated with identifying possible locations for old ice around Dome Fuji.

106: thicker ice to be detected

We will address this in the revised manuscript.

165, 166: bounce -> reflect

We will address this in the revised manuscript.

237-239: BedMachine Antarctica's supplement makes clear that streamline diffusion, not mass conservation, is used to interpolate data in the slow-flowing interior of Antarctica, including the Dome Fuji region.

We will address this point in the revised manuscript.