Reply to the reviewer 3

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).

Summary

The manuscript presents recent high-resolution radar surveys around Dome Fuji, Antarctica with the motivation to inform future selection of oldest ice core drilling locations.

Thanks for the summary. As for the motivation, we gave in the manuscript "*For identifying suitable sites for drilling very old ice, gaining the knowledge of the subglacial topography and englacial layering is crucial. For this purpose, extensive surveys were done*." Thus, our motivation is gaining the knowledge, and not "to inform future selection of oldest ice core drilling locations". We hope to make this point clear.

They extract high-resolution bed topography and make a convincing case of the advantages of using a radar system with a highly directive beam pattern to study this region. The authors also combine recent and earlier surveys to generate new gridded ice thickness data covering the Dome Fuji region which is useful for oldest-ice drilling projects. However, it is unclear how the authors combine data from multiple systems and there could be missed opportunities to integrate the JARE data further with AWI or University of Kansas/University of Alabama data that was also mentioned in the manuscript.

Using systems listed in Table S1, we measured ice thickness. Basic principle of the radar measurement for all systems was common: time series data in terms of round-trip time of the radio wave was converted to ice thickness. Then, what we need to be very careful is calibration. That is, depending on system, there can be small differences in triggering timing or difference in pulse widths which can cause systematic errors. To remove effects from these, we have conducted statistical comparisons between sets of data at cross points in addition to down-hole radar target measurements. These were developed in section 3.3. This is our reply to the reviewer's view "it is unclear how the authors combine data from multiple systems".

Our reasons for not including the AWI data or the Japan-Norway-USA collaborative data were explained in detail in our reply on RC1. We copy here the reply on RC1 as italic letters. It is a bit long copy. If you already read our reply on RC1, please skip this italic letter part.

There are a few groups who have covered some part in the vicinity of the Dome Fuji area with their ice sounding radar surveys. In addition to the JARE's 30-years-long efforts of the ground-based radar surveys (the most extensive surveys were carried out from 2017 to 2019), there are airborne surveys conducted by the former Soviet Union in the previous century (as data were used in BEDMAP and Bedmap2), and the ground-based survey conducted by Japan-Norway-USA collaboration (as radar science/technology was presented by Rodriguez-Morales et al., 2020). We agree that merging all data to compile a subglacial map is one of many steps of progresses. However, each set of data needs to be fairly evaluated before merging each other. Otherwise, it will be difficult to guarantee quality control of the data compilation. One of our focuses of the present manuscript is to present quality of the JARE data to the community and build a firm basis to merge with the other parties' data in future works. Thus, the authorship reflects people who are suitable for, and responsible for our data compilation.

The reviewer is right in some sense that the manuscript gives "subset" of the entire data that are linked to Dome Fuji. However, quality check of the total set of data is beyond the scope of the present manuscript. We would have a sound step of science to make more reliable and robust ice thickness data in the end. Evaluation of the total huge sets of the JARE data by the responsible authors (without making mixture of various sets of data with various quality level and with various weight of responsibility from the beginning) is the necessary and very important step.

In addition, for each set of data that we did not merge in the present manuscript, we see various situations as follows.

(i) Airborne radar data obtained by the former Soviet Union
We understand that the data were used in the BEDMAP and the Bedmap2 compilation. To our knowledge, data is not available for public use yet. In addition, little is known as for the errors in ice thickness or, more importantly, accuracy for positioning of the airplanes. To discuss the airborne radar data obtained by the former Soviet Union is not

our choice that we should take. It is well beyond the scope of the present manuscript.

(ii) Airborne radar data obtained by Alfred Wegener Institute (AWI) in 2014/15 and 2016/17 seasons and the other seasons

With views and reasons stated above, we do not merge these AWI data at this moment of on the way of sound steps. In addition, we can observe that significant amount of the along-track hyperbolae features is apparent in the AWI data (e.g., see Figure 7 in Rodriguez-Morales et al., 2020 and Figure 5a in Karlsson et al., 2018). When we use data with more amount of the along-track hyperbolae features, the features will modify the topographic map so that area of the mountains/hills look erroneously wider or so that deep and narrow troughs are masked. We believe that we can observe this error (masking troughs) due to the hyperbolae features within the AWI data at Figure 8 in Rodriguez-Morales et al. (2020). This figure gives an example of comparison between data from the high-performance, multichannel, ultra-wideband radar system from the CReSIS, University of Kansas and the AWI airborne radar system along the same survey line. For the purpose to find suitable locations for the very old ice drilling in mountainous area, it requires a kind of pin-pointing assessment. The tendencies of errors will be problematic, which can bring us misunderstanding as for topography of the mountainous areas. Therefore, we are very careful to make an ice thickness map by merging different quality data. Present paper merging the historical JARE data will be very important basis for future merging between data obtained from independent parties.

(iii) Ground-based radar data obtained by Japan-Norway-USA collaboration in 2018
The data are under a task of SAR processing. The data will be compiled in future for updated topographic map, possibly together with new data that we are currently obtaining in 2021/2022 Austral summer season at Dome Fuji.

In summary, we are making the necessary and significant step to make the most reliable ice thickness map in the end.

The new high-resolution survey and gridded product provide new and useful details on bed topography. However, the authors miss opportunities to provide further radar analysis and interpretation of the subglacial environment which would better narrow down locations for potential oldest-ice drilling.

Thanks for the comment. In revision, we will discuss geographical and glaciological parameters such as bed roughness, bed slope, driving stress and hydraulic head calculated from our new ice thickness and bed elevation data. As for the bed reflectivity, Fujita et al. (2012, TC) already discussed some for the present area using the radar data obtained before 2008.

Major Issues:

Radar Processing: To create the gridded ice thickness data, the authors combine data from multiple systems, which could suffer many potential issues. The manuscript does not provide enough evidence for how potentially data-combination issues were measured, dismissed, or corrected for, which is needed for the reader to evaluate the findings clearly. This should be added to the relevant sections on the radar processing steps involved in developing the gridded product before discussing the uncertainties in ice thickness which is separate for potential issues involved in combining data from different radar systems.

Thanks for the comment. In many compilations such as BEDMAP, Bedmap2, or other compilations, they did not adjust differences in radar calibration from multiple radar systems. They basically combined available data to generate gridded map. From this point of view, it seems that the comment/criticism by the reviewer above is in principle directed to most of earlier compilations for ice thicknesses. In case of the present manuscript, our handling of the data from multiple settings of radars is given at the bottom of the 1st page in this reply. The data were fairly calibrated, and data from multiple sources were adjusted with each other based on statistics.

The authors also discuss data from AWI and the University of Kansas/University of Alabama, which seems could be added to the gridded ice thickness. Or if not added, this should be convincingly explained why not.

Our reasons for not including the AWI data and the Japan-Norway-USA collaborative data were explained in detail above.

Radar Analysis: Regarding the analysis and interpretation of the subglacial environment, the radar analysis does not go further than plotting ice thickness and extracting bed topography. Hence, the analysis does not provide sufficient evidence to demonstrate that there is an improvement in knowledge of the subglacial environment needed for selecting ice core drilling locations.

We argue against the comment above. We would like to give comments as follows. We do not agree that the reviewer evaluated the manuscript as “the analysis does not provide sufficient evidence to demonstrate that there is an improvement in knowledge of the subglacial environment needed for selecting ice core drilling locations.” The manuscript has main points as follows.

(i) To better understand the detailed bedrock topography for finding potential sites that contain ice that extends to > 1 Ma, we conducted ground-based radar measurements with a high spatial resolution across the Dome Fuji region, East Antarctica, in the 2017–2018 and 2018–2019 austral summer seasons.

(ii) We constructed an ice thickness map from the improved radar data and previous data collected since the 1990s.

(iii) The new ice thickness map sets substantial constraints for identifying possible locations for oldest ice drilling areas.

We believe that we sufficiently demonstrated improvements in knowledge. Our work is not simply the radar analysis plotting ice thickness and extracting bed topography. The paper revisits a well-studied survey site. We clarified geographical features in the interested area demonstrating improvements of radar technique. In addition, recognizing the improvement in data quality and, in particular, the comparison to existing topographic products is novel. Moreover, our message in Figures 6 and 7 is clear: that widely available bed topography products can be misleading and should be validated against in-field surveys where possible. We hope the reviewer to look at these points as significant improvements in knowledge.

There is no updated analysis of the basal thermal state or the bed roughness. I suggest more analysis on quantifying the subglacial environment such as extracting bed power/radar reflectivity/roughness to demonstrate the improvements of this new radar dataset in constraining the subglacial conditions.

Thanks for the comment. In revision, we will discuss geographical and glaciological parameters such as bed roughness, bed slope, driving stress and hydraulic head calculated from our new ice thickness and bed elevation data. As for the bed reflectivity and melt/frozen delineations (basal thermal state), Fujita et al. (2012, TC) already discussed some for the present area using the radar data obtained before 2008. Basal thermal state is beyond scope of this manuscript: it should be discussed in a separate paper(s).

Minor Issues:

NDF is not defined anywhere

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.

Figure 2: The values for the color range is not specified. It would be best to add colorbars or at least state the range in power that is plotted in these radargrams. Are the color ranges the same for a and b? Are the colors saturated?

In the context of this manuscript, gray scale bar for the z-scope figure does not mean much scientifically because we made no discussion on power of echoes. Thus, in this manuscript, gray scale is arbitrary. We will clarify it in the revised manuscript.

Figure 3: Plot/label NDF for consistency and comparison to figure 1.

Thanks for the comment. We will address it in the revised manuscript.

Figure 4: I suggest writing frequency on all the furthest left y-axes for clarity (instead of just the middle row). Same for delta H for the columns.

Thanks for the comment. We will address it in the revised manuscript.

Figure 6: again colorbar for the background radargram power.

Our reply is the same as one for Figure 2.

Figure 7: it would be helpful to have NDF labeled

Thanks for the comment. We will address it in the revised manuscript.

In section 4.3, the authors suggest that even higher spatial resolution is needed to resolve the best candidate points for drilling. The authors should suggest what radar system design might be needed to achieve this spatial resolution.

Thanks for the comment.
When we use data with more amount of the along-track hyperbolae features, the features will modify the topographic map so that area of the mountains/hills look erroneously wider or so that deep and narrow troughs are masked. Please look at z-scope data at Figure 8 in Rodriguez-Morales et al. (2020). The figures in it give examples of data from the high-performance, multichannel, ultra-wideband radar system from the CReSIS, University of Kansas and the JARE ground-based radar system with high gain and high directivity antennas. They both demonstrated very weak effects from the along-track hyperbolae features. For the purpose to find suitable locations for the very old ice drilling in mountainous area, it requires a kind of pin-pointing assessment. Practically, these radars are candidates to be used in further surveys. For examining ice coring candidate sites, we need to avoid locations with irregular bed topography. These radars are good enough to detect irregularity of basal topography because the data have little features of the along-track hyperbolae effects.

Discussion around Line 275 should be tied back to the radargram figure along with more concrete discussion. For example, I would like to see a list or examples of what portions of the radar survey shows undisturbed layers just above the bed.

The reviewer's comment contains common context with comment by RC2 for lines 274-277. We first copy here our response on RC2 with italic letters.

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.*

These are basis of our discussions for lines 274-277. The reviewer stated he/she would like us to develop z-scope examples of radar layering near the base to give examples for the idea in this paper. However, such development of z-scope for very deep layers are beyond the scope of this manuscript. Rather, this is indeed an important subject for examining presence of very old ice near the base: this subject will be developed in future papers, focusing on features of z-scope images. We do not hope to develop crude examples of internal layering data at this present timing of the paper focusing on "high-resolution subglacial topography".