Reply to the reviewer 1

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 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 Shun Tsutaki and coauthors.

Summary:

This a report on a subset of ice thickness data that has been collected over Dome Fuji, which has seen increased activity over the last few years as part of the Oldest Ice Challenge. This is a unique dataset, and I think the authors miss some opportunities to make it more relevant to the community and to the search for old ice.

There is a significant focus on uncertainty analysis, but little quantitative justification for the significance of the uncertainties in the context of the old ice search. I recommend revisions that more fully utilize the available data.

Thanks for this part of the review comment. We used significant parts of the paper to evaluate uncertainties, which we believe a very important step for an ice thickness compilation paper. Based on these analyses and discussions, we generated an accurate high-spatial-resolution (up to 0.5 km between survey lines) ice thickness map. Accordingly, this map revealed a complex landscape composed of networks of subglacial valleys and highlands, which sets substantial constraints for identifying possible locations for new drilling.

We understand that the reviewer had a view "but little quantitative justification for the significance of the uncertainties in the context of the old ice search". We felt this criticism as a bit enigmatic, because we made uncertainty analysis a lot within 9 sets of data by JARE and comparison with a few independent compilations. Little appearance of the along-track hyperbolae features is apparent in figures given in our manuscript as well as Figure 7 in Rodriguez-Morales et al. (2020). In this context, if anomalous topographic features (sudden bumps or troughs) are absent within the survey lines (typically 0.5 km in this study) the map will be useful without much risk for misleading for potential drilling sites.

Major issues:

Data: I think at a minimum, given the main point of the paper is the quality of the new grid derived from point data, to validate those claims the point data really should be released as part of the paper (if not here, where?). This will make this paper a lot more valuable for both future data intercomparison papers, but also research into interpolation methods, and comparison studies between old ice sites.

As we stated in the manuscript, historically we have provided the latest (at each time) sets of raw data to major ice thickness compilations (Bedmap2, BedMachine Antarctica and the AWI compilation) without any conditions (for example, requesting authorship). We keep this principle.

However, we would like to give comments as follows. We do not fully agree that the reviewer evaluated the manuscript as "at a minimum, given the main point of the paper is the quality of the new grid derived from point data". The manuscript has much wider context including the 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 argue that focusing on relations between point data and the compilation is not "the main point of the paper at the minimum".

As for data availability, the reviewer 2 gave us persuasive suggestions with which we can fully agree. Points are as follows. *The raw lat/lon/thickness data ought to be available. Proprietary data used herein should not be remaining, in our academic trend in 2021. In addition, the draft grid ought to be available publicly upon submission for review. This is not required by TCD, but it is increasingly recognized as good practice and is required by some Copernicus journals.*

Based on suggestions from the reviewers, we released the point data obtained from 1992 to 2019 with bias corrections in ice thickness as well as the gridded data in data repository site of Arctic and Antarctic Data archive System (ADS), National Institute of Polar Research. DOI of each data is as follows:

Data: Ice thickness around Dome Fuji, Antarctica, based on JARE ground-based radar surveys: a 500m resolution gridded data
DOI: https://doi.org/10.17592/001.2021110901

Data: Ice thickness around Dome Fuji, Antarctica, based on JARE ground-based radar surveys: JARE33 data
DOI: https://doi.org/10.17592/001.2021110902

Data: Ice thickness around Dome Fuji, Antarctica, based on JARE ground-based radar surveys: JARE37 data
DOI: https://doi.org/10.17592/001.2021110903

Data: Ice thickness around Dome Fuji, Antarctica, based on JARE ground-based radar surveys: JARE40 data
DOI: https://doi.org/10.17592/001.2021110904

Data: Ice thickness around Dome Fuji, Antarctica, based on JARE ground-based radar surveys: JARE49 POL 179 MHz data
DOI: https://doi.org/10.17592/001.2021110905

Data: Ice thickness around Dome Fuji, Antarctica, based on JARE ground-based radar surveys: JARE49 VHF 60 MHz data
DOI: https://doi.org/10.17592/001.2021110906

Data: Ice thickness around Dome Fuji, Antarctica, based on JARE ground-based radar surveys: JARE54 data
DOI: https://doi.org/10.17592/001.2021110907

Data: Ice thickness around Dome Fuji, Antarctica, based on JARE ground-based radar surveys: JARE59 POL 179 MHz data
DOI: https://doi.org/10.17592/001.2021110908

Data: Ice thickness around Dome Fuji, Antarctica, based on JARE ground-based radar surveys: JARE59 VHF 179 MHz data
DOI: https://doi.org/10.17592/001.2021110909

Data: Ice thickness around Dome Fuji, Antarctica, based on JARE ground-based radar surveys: JARE60 data
DOI: https://doi.org/10.17592/001.2021110910

We agree that our new data will activate intercomparison studies in ice thickness around the Dome Fuji region (with AWI and so on). It will also activate research into interpolation methods, and comparison studies between old ice sites.

Data integration: This does seem to be a missed opportunity to integrated both the Kansas-Alabama radar data from JARE59 and the AWI data, would make the conclusions stronger. The Rodriguez-Morales et al., 2020 paper cited for the Kansas-Alabama radar is a radar engineering paper, and does not deal with interpretation or presentation of the ice thickness data in the context of old ice at all.

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 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 and 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.

Beam patterns: Given the focus on the Yagi improvements over the years, having a figure plotting the beam patterns for the different systems would be beneficial, including any side lobes.

We already gave half-power beam width for both E- and H-planes in Table S1. First, the half-power beam width gives information of anisotropic beam pattern well. Second, focusing on detail of beam pattern will cause misunderstanding to some of readers; they may think that the authors are assessing such very fine detail of the antenna beam pattern, which is not true. We understand that the reviewer is interested in possible effects from side lobes. In some cases, there are side lobes which is about -15 dB of the main lobe and with wide angles (~40 degrees) from the nadir. Such side lobes in wide angles will return extremely little to the radar because of oblique angles to reflection horizons, signal loss as compared to main lobe (by -30 dB) and longer paths for wave propagation (thus more loss due to attenuation). We hope that we will not go into detail on this topic. We consider a risk that readers misunderstand scope of this manuscript.

Given the anisotropy in the beam patterns, caution should be used using crossovers to account for intersystem bias - over rough terrain, bias for an anisotropic sensor may be a function of the intersection angle (see the Appendix on the Young et al., 2017 Dome C paper). It seems expanding Figure 2 to include JARE 33 and JARE 37 would be instructive.

With the same reason described above reply, we do not hope to go into this detail in the present manuscript. To satisfy suggestions of the reviewer, we will need a separate paper, a technical paper focusing of antenna radiation pattern and roughness of terrain. It requires significant number of analyses and big discussions. This is beyond the scope of the present manuscript.

Comparison with other ice thickness products (section 4.2): It is unclear what the goal of section 4.2 is, in particular Figure 6. Comparing an interpolated DEM which includes recent radar along the line of comparison, with other that don't, does not seem to be a fair comparison.

It would be better to the different interpolations along lines that are not aligned with included radar profiles, or better yet, along a radar profile that was not included in any of the DEMs, including the JARE DEM. For each of the DEM's compared with, maps of the datasets that were used in their generation should be included in the supplementary material, with the line of comparison plotted.

We inform that our way of description caused a misunderstanding by the reviewer. We must correct our way of expressions. Our intention to make the section 4.2 and Figure 6 was to demonstrate as to how compiled, smoothed and gridded product look different along a survey line where ALL the compilations (Bedmap2, BedMachine Antarctica and AWI compilation) used common basic data provided by JARE33 and JARE37 who explored the region. Because we displayed a recent data of JARE59 in Figure 6, it gave an impression as if only present authors have had an opportunity to use the recent data in compilation. It was a fair comparison. We will revise the point clear to readers to avoid any misunderstanding.

The different interpolations along this example of line give interpolated results with apparently different features. Demonstrating the Figure 6 is beneficial to readers to better understand visually how such differences are occurring between widely known compilation maps. We will add a figure including interpolated ice thickness datasets of Bedmap2, BedMachine Antarctica and AWI in the supplementary material.

Importance of the uncertainty analysis: A lot of effort is spent on ways to quantify the uncertainty in ice thickness both of the profile data and of the interpolated grid; however, it is not made clear quantitatively what science reduced uncertainty allows. How does improved confidence in ice thickness allow for a better assessment of locations of old ice? What are the horizontal and vertical resolution requirements for constraining these targets?

Frankly, we did not fully understand what the reviewer intended with this comment “however, it is not made clear quantitatively what science reduced uncertainty allows”. The location of drilling old ice will be determined by considering very many conditions such as (i) frozen bed, (ii) little or virtually no horizontal ice flow, (iii) bed topography without roughness complexity, (iv) sufficient thickness of climatic events resolvable based on ice core analysis techniques, (v) preservation of flat and horizontal layering without disturbance by ice flow at very deep depths, (vi) ice temperature at the bottom well below pressure melting point to prevent diffusion of both ice molecules and gas, (vii) possible migration of dome position in glacial/interglacial cycles, and more. To consider all these points, we need to fully utilize both bed topography data, internal layer data, distinction of wet/dry condition at the base, and 1D, 2D and 3D ice sheet modellings. New and more reliable ice thickness data gives a kind of substantial constraints or boundary conditions for the assessment. The reviewer suggested us to discuss something more quantitatively. However, we wonder how one can provide some quantitative aspect for multiple conditions from (i) to (vii). It is obvious that we cannot specify any promising site of ice coring by very smoothed kriging interpolated map because there is mountainous topography. We increased data dramatically. In addition, we removed effects from the along-track hyperbolic features. Moreover, we assessed uncertainties between sets of data based on careful analyses. The question given to us "How does improved confidence in ice thickness allow for a better assessment of locations of old ice?" is not one that someone can answer as some quantity properly. In our case, we are clarifying mountainous topography assessing peaks, ridges, slopes, and troughs.

Analysis beyond ice thickness: The authors don't really go beyond ice thickness in any quantitative fashion. Bed reflectivity for water distribution with such heterogenous data could be a stretch, but some additional parameters, like bed interface roughness, bed rock slope, and ice driving stress could easily be calculated from these data and be informative of regions for follow up.

Thanks for the suggestion. 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.

Minor issues:

"Conventional" and "modern" radar should explicitly define in the introduction - I think you mean by "conventional' is "real aperture" or "incoherent" radar.

Thanks for the suggestion. We will address this point in the revised manuscript.

Line 238: Bedmachine Antarctica does not use mass conservation in slow moving regions, but instead a streamline diffusion method (Morlighem et al., 2019).

Thanks for the suggestion. We will address this point in the revised manuscript.

Figure 2: what is the cause of the change of gain in the NDF end of the JARE59 radargram? The authors should highlight the key 2500 m depth on these radargrams. It is notable that in general the bed roughness and the brightness of the scattered bed return appear much brighter above that line, consistent with a frozen, immobile bed with very little englacial attenuation.

Thanks for the question. We did not state in figure caption that we merged two images that were obtained in different date and in different radar conditions. We will address this point in the revised manuscript.

Figure 7: Using the final 2500 m ice thickness contour on these difference maps would help orient the reader as to where major differences are.

We will add contours indicating ice thickness of 2500 m from our gridded data.

Figure 8: Hill shading the zoomed in region may help in visualizing the roughness better.

Thanks for the suggestion. We will modify the map so that we can see slopes as dense (shade-like) contour lines.

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.

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”.