

Review Response for: Characterization of Titan Dome, East Antarctica, and potential as an ice core target

1 General Comments

1.1 RC1: somewhat already published in Beem et al., 2017

We disagree with this characterization. The work in this manuscript is not a republishing of past work. This manuscript supplies new observations that support numerous hypothesis. The most important of which is that ice age at this location is too young to study the middle Pliocene transition. Also of interest, the observations support the hypothesis proposed in Beem et al., 2017 and those ramifications are addressed in the discussion section. We think it well within the typical practice to build upon past work, even our own, and discuss the new observations within the context of past work.

1.2 There are numerous references in the text to “ice catchments” but these are never named (e.g. Academy Glacier, Patuxent Ice Stream etc.).

The “ice catchment” nomenclature has been simplified which obviates the need for geographical place names.

1.3 Engagement with relevant recent literature should be improved. Papers by Winter et al. 2018 <https://doi.org/10.1029/2018GL077504>, Paxman et al. 2019 <https://doi.org/10.1029/2018GC008126> and Studinger et al 2020 <https://doi.org/10.5194/tc-14-3287-2020> may help to provide more context (e.g. for naming ice catchments etc.), and for placing Titan Dome in a wider geographical, glaciological and geophysical context - the authors may wish to consider using some of these papers for a more developed study area section, and may wish to integrate the Studinger et al. 2020 paper into their discussion on surface accumulation?

Thank you for these suggestions. Citations have been added.

2 Title/Abstract

2.1 RC2: change title to "Aerogeophysical Characterization..."

This is a fine suggestion and makes the title more informative. We have made the change.

2.2 RC2: a rather unusual opening sentence for an abstract? I'm not sure that the geographical information is that necessary in the abstract. It is detail that can be provided later.

Geographical context seems like a solid place to start but "along a meridian" might be superfluous at this moment and has been removed.

2.3 RC2: suggestions for additions or wording change

These were helpful and improvements were made to the abstract. Thank you.

3 Introduction

3.1 RC1: Line 22 and 234: the threshold of ice thickness in Fisher et al. 2013 is much higher than 2000 m for snow accumulation of 4.4 cm/yr i.e., and ice velocity lower;

In both instances, the 2000 m threshold was discussed in regard to previous work (i.e. Van Liefferinge and Pattyn 2013). We have changed the language of the sentences to make it clearer we were discussing the work of others that used this specific threshold.

3.2 RC1: add the minimum desired temporal resolution of ice of 10 kyr/m, it is very important the resolution at MPT to resolve 41 kyr cycle

We agree the temporal resolution is significant for any 'old ice' core. This detail is discussed later in the paper. It was not included here, as this paragraph described the motivating work of Van Liefferinge and Pattyn 2013. Since these other authors did not consider this threshold explicitly in determination their potential coring regions, it is not discussed here.

3.3 RC1: Titan Dome position is along around 160°E meridian

It is accurate for us to use -170 degE. We have made that change.

- 3.4 RC2:** The authors should consider adding to the introduction a short section describing the use and modelling of englacial layering for developing age stratigraphies for the ice sheet. There are a number of recent papers (e.g. <https://essd.copernicus.org/articles/11/1069/2019/> or <https://doi.org/10.1029/2019GL086663>) that would be relevant, in addition to the papers of Marie Cavitte that several of the authors of this manuscript were also involved with.

Thank you for these suggestions. Citations have been added.

4 Methodology

- 4.1 RC1:** both the age model use snow accumulation value, but the authors do not provide sufficient information on spatial variability in the analysed area and their source data (e.g. Fig.2 snow accumulation map on the base of Arthern or Wessen with background the snow accumulation derived by 4.7 kyr isochrone), the source of snow accumulation value are not everywhere clarified and made the manuscript difficult to follow; the snow accumulation of 4.4 cm/yr i.e. are not explained, and not clarify if represent the present snow accumulation or the mean value snow accumulation rate histories taking in account the reduction during glacial period and how is calculated.

We point the reviewer to lines 108-111 of the "discussion" article where we state: "The [Nye] model is solved for each vertical record independently by finding the accumulation rate that minimizes the root mean squared error between the age model and the traced isochrons." The accumulation rate is the result of the model.

In the results section, where 4.4 cm/yr is reported we changed the language to make it clearer that this is the output of the Nye model and necessarily assumes a steady state accumulation rate. Within that same paragraph we discuss how these magnitudes should be viewed, as rough approximations of present day accumulation given the simplicity of the Nye model, but that spatial patterns and magnitudes are qualitatively similar to available surface accumulation datasets.

- 4.2 Line 85: the theoretical value of attenuation of -35°C is not reported in Beem et al. 2017, provide more information;**

Attenuation is reported in Macgregor et al. Beem et al. supports the contention of the average ice column temperature. We have rewritten the sentence to more clearly delineate what information can be found in the associated citation.

5 Results

5.1 **RC1: the source/process of isochronal layer ages are not explained and does not take in account the recent result of SPICEcore (e.g. Winski et al., 2019) everywhere;**

We have taken into account Winski et al., 2019. We used that data set in our analysis. It was inaccurately cited. We have fixed that oversight.

We find it beyond the scope of this paper to discuss ice core dating methodology.

5.2 **RC1: the comparison with the previous DEM does not take in account the source of the data (SPRI-NFS-TUD airborne radio-echo sounding) southern of 88°S. The uncertain and accuracy in elevation and position of the data southern of 88°S are very different from altimetry satellite data;**

Neither of the compared surface elevation DEMs (Bamber et al. and Helm et al.) used airborne echo sounding to determine surface elevation. Considering the uncertainties differences between airborne and space-borne observations is not necessary.

5.3 **RC1: the proposed geographical coordinates of the Titan Dome position are in an unusual format (88.1716° N, -99.5234° E) and longitude is wrong (line 148 and 281). The longitude value must be correct everywhere.**

There was an error in the coordinate. The correct coordinate reporting in the convention of using North and East coordinates, the values should have been -88.1716 degN and -170.4765 degE. These values have been corrected.

5.4 **RC1: Please explain how the spatial variability of snow is consistent with Nye Model, it is a circular reasoning;**

The reviewer appears to believe that the accumulation rate is supplied a priori as an input to the model. Instead, accumulation is the value that the model solves for upon fitting to dated internal horizons. Comparing the output of the model to independently collected observations is a means to validate the model results. We have made changes to the text, in both the methodology and results, to try to enable readers to more easily understand our work.

5.5 **RC2: An improved description of the datasets (e.g. ice thickness, bed topography) are required in section 4.1.**

An additional paragraph expanding the description of the bed/ice thickness has been added.

6 Discussion

6.1 RC1: the analysis of Candidate B site is reduced at 5 lines on discussion, remove the B site or analyzed in more detail

Although Candidate B is tangential to the new observations, we believe the little information we have concerning Candidate B is still of relevance to the scientific community's search for future drilling sites.

6.2 RC1: the suggest change in ice velocity from Beem et al. 2017 have impact on dome/ice divide position, the authors should analyze this point in relation with potential ice core site, column stratigraphy integrity and upstream correction, more than report the previous results;

To analyze column stratigraphy integrity and up stream correction would, of course, be of significant interest. However, to do this properly would require isochron tracking 3D ice sheet model experiments which far exceeds the scope of this paper. We hope that this publication would serve as possible motivation for this work to occur in the future.

What can be said based on our results in that the Titan Dome region shows contiguous and regional internal structure draw down which is consistent with the region experiencing faster flowing ice. We suggest the hypothesis that such faster flow could have impacted the ice divide geometry, moving it further interior. Such an ice sheet evolution history would be important to account for in any future ice core drilled in the location, and may disqualify such a location from consideration of a 1.5 million year old core.

6.3 RC1: Line 231-232 why the area between the two-candidate area were exclude? Explain the source of ice velocity $\approx 2\text{m/yr}$, surface elevation morphology and ice thickness are not different in the area;

The text here as been updated to make it clear that the region is removed by the authors Liefferinge and Pattyn 2013 and their work. This citation does not specifically report their ice velocity results. But the work of Bingham et al, 2007 (doi:10.1029/2006JF000556) finds enough difference in this area to calculate higher balance velocities for exactly the region between what we are calling candidate A and B. The additional citation is now included.

6.4 RC1:Line 232 please explain the 2000 m of ice thickness threshold;

New text has been added to make it clear that this threshold was used to determine the regions of cold bedded glacier defined by Liefferinge and Pattyn 2013. The Liefferinge and Pattyn 2013 work motivated the aerogeophysical survey that is reported in this paper.

6.5 RC1: Line 234 Titan Dome could be migrated in the past, the evidence of change in ice velocity has been proposed for the flank about 100 km far, not at dome site;

"Flanks" has been added to the text.

6.6 RC1: Line 236 the pattern is not clear evident in fig. 7a;

I understand that the spatial transition being discussed is a bit diffuse. But the linear feature in isochron draw down in figure 7a and 7d are difficult to explain by slow continental ice flow alone. We have added arrows to figure 7a and 7d to draw the eye to the region.

6.7 Line 255 candidate B site is forward to Dome A from South Pole, along the 80° meridian East;

Given its size and location, candidate B intersects both 45 degE and 80 degE. We have left the value as is.

6.8 Line 264 The subglacial catchment basin analysed by Jordan et al. 2018, include completely the candidate A and B area, rephrase.

We have made it clearer that we are referring to only the region of higher geothermal flux identified by Jordan et al. While the full data set (PolarGAP) used by Jordan et al does encompass numerous catchments including the ones that flow from Titan Dome. The region that they identified as warmer is in a different catchment than that which hosts the majority of our SPC survey.

7 Figure 1

7.1 RC1: Fig. 1 the summit position proposed is not visible, the PPT line of legend is too light;

Figure 1 has been remade.

- 7.2 RC2:** A zoom in of Titan Dome is required. define PS on the axis labels - it is unclear why Filchner Ronne Ice Shelf is annotated, but Ross Ice Shelf is not. Labelling of major outlet glaciers and/or glacio- logical catchments would be useful. - typo: “temperature” - authors should consider improving the colour scale for the basal temperature - the plotting of the polargap survey lines are (a) difficult to make out; and (b) incomplete – see figure 1 of <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018GC008126> - there is no need to have ‘candidate’ written on the figure. A and B will be sufficient.

Figure 1 has been remade.

8 Figure 3

- 8.1 RC1: Fig. 3:** add elevation contour line with value and proposed dome summit, add new panel with snow accumulation map with accumulation derived by 4.7 kyr isochrone, a new panel with ice thickness difference;

Age model results including accumulation can already be found in figure 6. Ice thickness differences can be found more in the remade figure 3 and in figure 4.

- 8.2 RC2:** the new data presented in this figure are generally rather lost in the background. What about plotting the new along track data without the underlying DEMs, and then having a set of line data vs. DEM difference figures (e.g. from Bedmap2, Bamber DEM etc.) - Contours would be useful where DEM grids are shown. - later in the manuscript there is numerous references to a new subglacial mountain. Please annotate this in 3b.

Figure 3 has been reorganized to included difference maps, which both reviewers suggested. The subglacial mountain has been removed as a point of reference, so it is not necessary to include in a figure.

9 Figure 5

9.1 RC1: Fig.5 Add proposed summit position and the value of hydraulic potential contour;

The summit position has been added to figure 3 for clearer determination by the reader. Although I am usually a proponent of direct labeling on figures. In this instance, the contour labels clutters the image. As the values of hydropotential are less significant than the gradients, the contours without labels communicate the important information. The contour values are included within the figure caption for any reader who is interested in their magnitude.

9.2 RC2: The way that the reflectivity data have been plotted in this figure makes it indecipherable, at least to me. Authors should look to improve the display of data in this figure. - Subglacial Troughs: these need described in section 4.1 of the paper, and they also need to be represented better in this figure. The blue polygon looks nothing like two troughs to me.

The blue polygon (and the discussion of the troughs) has been removed which reduces visual clutter. The colored lines representing basal reflectivity magnitude have been made wider for easier visual discrimination. The colormap has also been adjusted.

10 figure 6

10.1 RC1: add summit position and elevation contour value; For Dansgaard-Johnson model is not correct the label of panel as Basal ice age because the modelled age is at $z=0.2H$ and $0.5H$;

The summit position is now in figures 1, 3, and 5. The elevation contours of the dome are not the important details of this figure. The elevation contours are used for general geographic context. As a way to reduce visual clutter (which RC2 has asked to be addressed), the contour values have been omitted. An interested reader can determine contour values from information in the figure caption.

The reviewer seems to be associating z with the vertical coordinate, which is a fair assumption given convention. However, in this case, to be consistent with the nomenclature in the citation for the model, z is the transitional height not the vertical coordinate. This detail has been added to both the methodology section and figure caption to prevent such confusion.

10.2 RC2: this figure is afflicted with the same problems of figure 5. It is very difficult to make out the detail given the colour scheme and the way in which the data have been plotted.

The gray colorbar has been switched for a brighter map and a gray background was selected to increase contrast. Perhaps the black to white colorbars of figure 5 and panels 6c and 6f were causing the main difficulty in detail discrimination. We hope these changes makes the figures easier for readers to digest.

11 figure 7

11.1 RC1: fig.7 does not show clearly the difference in submergence velocity, improve the color scale;

A gray background and updated colormaps have been used to try to increase the visibility of the figure's detail.

11.2 RC1: The first panel fractional depth is not described;

The reviewer is correct. The figure caption has been updated.