

Response to reviewers on “High resolution boundary conditions of an old ice target near Dome C, Antarctica” by Duncan Young et al.

We thank the reviewers for their advice; the paper is now greatly improved, with a much greater focusing on the motivating old ice search. The discussion section has been added, and the results section expanded. Subglacial lakes are included in detail, as requested by a couple of the reviewers. However, englacial layer interpretation is outside the scope of this paper. A large section in the original paper focusing on cross over analysis has been migrated to an appendix.

Reviewer 1

Reviewer 1 wished this paper to be more focused on ice core scientists, which is appropriate given the target volume. We expanded the suggestion that we focus more on subglacial water; however, englacial layer mapping and modeling is outside the scope of this paper, with followup papers dealing with those subjects in progress. We also at the reviewer's recommendation expanded our discussion of the compilation of the datasets.

Specific comments:

Abstract: It is too concise with about 100 words. Please use space of the abstract efficiently. Please use much more words and space (2~3 times of present length) to explain what are really new in the manuscript in terms of science...

This has been expanded, and subglacial water has been added to the manuscript

P2L6: I did not find candidates A - E in cited papers. Please explain.

This paper now claims the A-E nomenclature on page 5, line 6. *“In the Dome C region, five candidate sites exist, which we term A, B, C, D, and E.”*

P2L9-10: Please provide citations for ICECAP and ICECAP2.

A citation is now provided for ICECAP. This paper is the first publication from ICECAP II

Section 2: Please provide a map showing entire Antarctica and Dome C region in it. In addition, please provide a map with site locations such as Totten Glacier, Byrd Glacier or George V Coast, VCD corridor...

A new Figure 1, showing East Antarctica is provided with the key sites referenced in the text

P2L20: "crude" -> "pioneering"? ...

crude is changed to pioneering

P2L21: Why "however"? Did somebody question presence of dome?

in this expanded version of the paper, we make a stronger point on the uncertainties in the older data.

Figure 1 Caption "OIA" appeared here without any explanation before. Background MODIS data has no contrast within it. Thus there is no meaning to show it here. "Antarctic Polar Stereographic" Please provide standard latitude.

We move up the introduction to the OIA survey, we replace MODIS, and we describe the standard latitude.

Figure 1 Figure Please indicate X and Y directions because they are often mentioned in the text. In addition, horizontal axis is used as Y by the authors.

This figure (now merged with figure 2) is greatly expanded. We indicate the directions in supplementary material.

Section 2.1: Please provide much more information of the radar system used for this work, such as peak power, pulse compression rate, antenna gain, beam (half power) width in E direction and H direction, effective thickness resolution in ice. Perhaps it may be found in the Peters paper or the other papers. However, it is not kind at all for the authors not to show concrete information to readers in this paper. What is "focusable data"? Please explain to readers kindly and properly.

We now describe relevant parameters in more detail in section 3.4. Peak power and the details of the beam pattern do not affect the conclusions of this paper.

P3L14: What is Vostok/Concordia/DDU corridor (VCD)? Please explain using a figure. What is DDU? Please think about readers who are not familiar to this region.

This is expanded in Figure 1

Figure 2: Please indicate area A - E once again in this figure. Otherwise, readability is not good. Please indicate flight lines of Figure 1 once again on this map to make better readability.

Candidates A-E are now indicated on all maps of the region of interest.

P4L4-5: “Sites B, C, and D are located on the steep and poorly sampled peaks on the northern side of the Concordia Subglacial Trench (CST)”; Does it mean bed or surface? B, C and D seems on the southern side of the CST in Figure 2. Am I wrong? Then why?

Rewritten for clarity: *“Sites B, C, and D are located on the steep and poorly sampled subglacial peaks on the northeastern side of the Concordia Subglacial Trench”*

The authors seem to use too much symbols such as DDU, CST, VCD and so on. It seems too much for readers. Later in the manuscript, I felt hard to remember their meanings. Where is CST on the map? It is hard for readers who are not specialist for this region.

Additional text (P3L4) now indicates the location on the Concordia Subglacial Trench “The coarse subglacial geography revealed by the Italian survey comprises of a deep subglacial trough (the Concordia Subglacial Trench) to the northeast of Dome C (see lower left of Fig. \ref{fig:context} a)”

P4L6: “basal ice in this region likely traverses the deep, wet CST and is unlikely to be stratigraphically intact.” t seems still a vague guess. Mountainous area has at least width of ~10 km. Can you suggest some direct/indirect evidence, for example, internal layers?

The language was softened by eliminating the last clause. Analysis of the internal layers is the subject of a followup paper.

P4L11-12: “The ice surface above Candidate A forms a topographic extension to the south of Dome C informally termed ‘Little Dome C’. The central part of Candidate A lies 40 km south from Concordia Station.” It seems south and north are very confusing in this manuscript and maps, like X and Y. Please make them very clear to readers.

We have added north arrows to the figures, and Figure 1 should help orient the reader.

P4L13: VCD/JKB2g/DVD01a is a kind of jargon for readers.

The line name is a necessary index into the dataset, which is in the process of being released at NSIDC.

P4L13-14 “Focusing of the radar data showed that the southern flank of the Candidate A massif ended in a steep cliff over which englacial layers dive.” First, I was confused in terms of directions. Second, “dive” seems inadequate because the authors used very high vertical exaggeration of ~20 times in Figure 3. If the authors use real scale, it should be very smooth, flat and continuous layers. “dive” is just an artifact effect that the authors produced by exaggeration.

Figure 3 is now redone at true scale, and the layers still noticeably dip over this scarp. Directions are clarified.

Figure 3: Did the authors apply the geometrical spreading effect in dB? Or, are these data just return power from targets? Please make this point clear for readers. Please indicate south and north.

The geometric loss correction is clarified, and distance from Concordia is explicitly shown.

Please indicate this segment of the flight line on the maps (Figures 1 and 2).

This line is now shown on the combined Fig 2.

This figure is given but not discussed in the manuscript.

This figure is discussed in two locations in the text: Section 2.2 and Section 2.3.2

Figure 3 I suggest that roughness and amplitude/frequency of it should be analyzed using such data here. It is much better data source compared to the crossover differences or RMS that the authors are discussing in this manuscript. For example, at a site of X=50 - 55 (km), difference between the first echo and the strongest echo is as large as 200 m or more. By choosing only the first echo for ice thickness analysis, the authors ignore the strongest echo which is most probable echo from the nadir. With this reason, the authors analytical produce is causing a bias of underestimation of ice thickness from the beginning. In addition, analysis of the both leading and trailing edges will give very good measures of the bed roughness.

We have to be careful here, for X=50-55 the difference is not really a vertical distance 200 m, it is a delay $\sim 2.5 \mu\text{sec}$, with a large cross track and smaller vertical component. The methodology suggested by this comment is not stable in the presence of complex geometry, with apparent ice thickness jumping hundreds of meters between traces as first the lower then the higher echo trade being the brighter echo, and would serve to exaggerate the along track roughness.

Exactly this effect is seen in the older Italian data, which we comment on in this paper.

Analysis of the leading and trailing edge does not quantify roughness without a complex scattering model for the bed which is outside of the scope of this paper. It is also sensitive to SNR considerations (ie, if a bed echo is close to the noise floor, the trailing edge will be truncated, and thus appear “smoother”). For a range compressed system like HiCARS, the leading edge has no physical meaning. The lead author has employed trailing edge analysis (Young et al., 2016, PTRS), however in very specific circumstances to evaluate specific hypothesis.

P5L7 What is Internal Measurement Unit? Please explain to readers.

This is now explained in Table 1.

Title of the section 3 Explanation of the OIA is given only later. Please explain to readers.

OIA is now explained in Section 2.4.

Table 3 This seems a strange table to me with several reasons. (i) Why commercial names of the instruments appear in the first column? Item of measurements should be given first such as ice thickness, distance between aircraft and the ice sheet, position, etc. (ii) F11 - F14 are not given well in this manuscript. It is hard to understand. Only the authors know well.

As suggested, we added the measured parameter to the table. We change the flights to dates; however, we feel some metadata must be provided to give readers informed access to the data.

(iii) Please give priority to instruments that you really used for discussions of this manuscript. Gravity and Geomagnetic are not discussed in this paper. Radar sounder and positioning should be shown with higher priority here.

MARFA was already at the top of the list; we move the laser up as well.

(iv) What is ICECAP2? What is different from ICECAP? Few readers know them.

We change this to the OIA instrument suite.

P6L4-5 “Flight lines were designed to avoid Concordia’s clean air sector to the south of the station, as well as to allow the aircraft to make VHF communications with the station before landing.” I suggest this should be removed because it is something that only very limited people should know.

This language was added to inform anyone who wishes to perform a follow up investigation.

P6L14 "Elevation difference" Does it mean elevation of the ice sheet surface?

we clarify to “Apparent surface elevation differences between survey lines”

Section 3.2 Please prepare a figure showing flight lines of F11 - F14 in a figure. Otherwise, by words alone, readers feel hard to imagine.

This is added in supplementary data

L6L16-17 Does it mean something for readers to know gravity in this paper here?

This line was deleted.

Section 4.1: I feel there are too much technical terms such as "Waypoint Inertial Explorer", "Precise Point Positioning (PPP)" or "the SPAN IMU". It seems too much for readers who are just interested in candidate locating of ice coring. Please provide explanations more here or in the appendix.

This section is consolidated within the Methods section, and was designed to appeal to those interested in high resolution aerogeophysical surveys.

P7L4-5: Internal estimates of uncertainty for these data have 2 cm height standard deviation. The authors seem to tell that 2 cm is for height. How about uncertainty for horizontal positioning?

An estimate for horizontal uncertainty is added.

L7L10-13 The data was then processed using the "1-D" focused SAR approach of Peters et al.(2007), where focusing of the along track Doppler phase variations within each range resolution cell was employed to improve the along track resolution to approximately 10-20 meters for scattering targets. Meaning is unclear to me. Do you mean that the processed data contain information over 10-20 meters along the flight line? The data was resampled to 4 Hz along track sampling (~22 m) for manual interpretation.

This section has been expanded and clarified.

P7L18 The authors did not apply firm correction. Why? The authors gave systematic error of ~15 m to the ice thickness data by ignoring the correction. How did the authors consider it when compiling with the other data? How can it be compatible with your effort to use the SAR processing?

We add the following: "We choose to not apply a firm correction to ice thicknesses; as shown in Peters et al., 2007, a firm correction is not required for our focusing, and will not affect the conclusions in this paper (firm correction is however critical for layer interpretation)"

P7L21 Please explain more about the "first return policy" for readers of non radar expert. As I wrote at Figure 3, this policy will give a bias of underestimation for ice thickness. The policy means that when faint echo appear from the mountains far from the nadir, the faint mountain is considered as thickness from the nadir. A policy of the strongest echo seems better to me. The strongest echoes are most probably return from the nadir. Figure 4 Apologizing to authors, I strongly feel that this figure 4 is not very important. Just 4 points show large differences mostly because of combined effects of the first return policy (causing a bias to the ice thickness) and the along track SAR processing effect. If the roughness is evaluated like I commented at Figure 3, it seems much more meaningful.

When there are steep slopes, ice thickness data are disturbed because of the footprint. In case of this paper, shape of the footprint is just asymmetric along the track (short) and across the track (long). It does not seem good indicator of bed roughness.

P8L6 "the critical angle of refraction" Do you mean Brewster's angle? If so, please add words. Then, more readers will understand. Do the antennas have beams wider than Brewster's angle of 34o? Please clarify this point, too.

This whole section has been condensed and moved to an appendix. Figure 4 has been removed.

Figure 5 To see this figure, I am afraid the authors' wording "Northings" and "Eastings" in many figures are wrong, confusing us.

Figure 5 is deleted. Northings and Eastings have been used to describe the x and y axis of the Antarctica Polar Stereographic projection, including by NSIDC, the Polar Geospatial Center, the Australian Antarctic Data Center, and SCAR in its formal definition of the Antarctica Polar Stereographic projection. We change this to Projected Northings and Projected Eastings, and add a north arrow to all plots.

P9L1 The underestimate is because the authors chose the first echo for determination of the ice thickness.

P9L3 The author' claim here is not clear to me, to see Figure 5, there is no clear tendency.

P10L15-1 I do not find any convincing tendency that authors are claiming here for Figure 6. Just four points like we saw in Figure 4 show deviation due to the effective asymmetric shape of the radar footprint.

This section has been edited back and put in the appendix.

Section 6 P11L7-9 If the authors compile the data with old unpublished data, please provide at least a list of comparison for data processing and radar specifications. In addition, a map showing the locations of measurements should be given. The authors' data probably have some bias. How are various sets of data compatible with each other, to be ready to compile together? What about crossover errors between sets of data?

Following on this suggestion, this section has been greatly expanded in Section 3.7.

Section 6 in general It is really hard for readers to understand what the authors are discussing in the figure. Please provide links between description and indications in figures. Otherwise, descriptions do not mean much. The authors sometimes mention subglacial lakes. However, without demonstration of lakes in figures, readers feel really hard. Please provide a figure showing distribution of lakes and subglacial water nicely. In addition, I felt confused by description of directions in this paper.

This section has been greatly expanded

Conclusion

2. Why is the candidate A promising? Did it pass all the conditions given in the introduction? "A large number of subglacial lakes" are not informed in this manuscript. What is "distinct basal ice"?

This section has been greatly expanded. The reference to basal ice has been deleted.

Publication of the data I suggest all the ice thickness data used in this paper should be published as supplementary data of this manuscript.

Publication of the DEM I suggest all the ice thickness map given in this paper such as Figure 7 should be published as supplementary data of this manuscript.

These data will be provided as supplementary data.

The paper will be much better if distribution of subglacial lakes and dry/wet distinction is mapped

We have greatly expanded our discussion of subglacial water

Figure 8 Again, background MODIS data means nothing for readers because it is just grey.

Figures 7 and 8 have been folded into a new Figure 5. MODIS has been removed from all figures.

Reviewer 2

Reviewer 2 addressed the lack of analysis/interpretation/and discussion in this paper, and the ad hoc structure of the paper. We addressed this with a large scale reorganization and expansion of discussion of the relevance for old ice.

Minor suggested global edits:

There should be no apostrophe in 1970s, 1990s etc.

Done

Is there any need for the acronym CST for Concordia Subglacial Trench? You don't use CST that often (unless an expanded Discussion will use it much more) so it just seems an unnecessary acronym.

Done

Introduce hyphens into "along-track," "across-track," "off-track," "range-compressed," "pulse-limited"... Ensure "crossover" consistently expressed as one word.

Done

Sections 1 and 2: P2, L1: Change "criteria" (plural) to "criterion" (singular).

Done

P2, L4: I suggest you don't need the aside about these features being called "blobs." You don't refer to blobs elsewhere in the paper.

Done

P2, Section 1, final paragraph: The current wording is vague about whom the "European-led group" are, and includes some extraneous detail about the logistical delay to the survey – you have my sympathy on the latter, but it doesn't affect the findings of the paper. This paragraph just has to focus on the purpose of this paper, which I suggest is along the lines of: "In this paper we present the results of an aerogeophysical survey specifically targeting the candidate old-ice access sites that was conducted in January 2016. We show ... [now state what the paper fundamentally shows and adds to existing knowledge, e.g, a new map of the basal topography, and preferably one or two ways in which you use the data more than just presenting a map, i.e. what do the new data add to identifying an old-ice access site? As a further example of the new data's uses you could also state that the new data offer insight into the meaning of uncertainties in RES data analysis.] We focus this section, and follow up on the recommendation to reorder and expand the discussion and conclusions

Figures 1 and 2; wherein currently Figure 1 is first referenced within the text in the opening line to Section 2, essentially just to locate Dome C:

To aid overall readability of Section 2 and the inevitable flicking between text and figures, I think you could combine Figures 1 and 2, and add a new panel, so that the figures are more readily intercomparable by the reader and introduced more logically as the material is discussed in the text. Essentially, following the text, the first result I want to see is just the surface ice topography (as per Section 2.1, paragraph 1), then I want to see the pre-ICECAP-surveyed subglacial topography (as per Section 2.1, paragraph 2), then I want to see the Van Liefferinge model results (as per Section 2.2). So I'd suggest a new three-panel Figure 1 covers the bases in that order, i.e. panel (a) shows pre-ICECAP surface topo; (b) shows pre-ICECAP subglacial topo, and panel (c) shows Van Liefferinge results. It would be really useful if every panel had superimposed the 5x candidate ice-core sites from Van Liefferinge as well as Concordia. The figure would also need still to include an inset showing the general location of Dome C. It would also be valuable on at least one panel (subglacial topo perhaps) the locations of subglacial lakes from the latest inventory marked.

We converted Figure 2 into a new composite figure with bedmap, surface slope, old coverage and new coverage, all showing the van Liefferinge results.

W.r.t. the existing caption for Fig. 1, the acronym OIA is unexplained in the main text at the stage I first read the caption, and I suggest the sentence referring to the red line is reordered as: Red line shows radar profile

acquired in 2011 and shown in Figure [would now be 2, if you follow my suggestion to combine Figures 1 and 2].

Done

A minor point on the existing Fig. 2 – the blue contour line to the left, presumably denoting surface elevation 3200, could do with labelling within the figure.

Done

P2, L20: At end 1st sentence of this section, just point reader to relevant figure showing surface topo.

Done

P2, L20: Change “was” to “were” and explain acronym “INS.”

Done

P2, L22, 31 & 32: In these contexts, no need for “dome” to have capital “D.”

Done

P2, L25: No need for phrase about Dome C Lake District.

Deleted

P2, L32: At end sentence “...northward flow” point reader towards relevant figure (currently Fig 2, but as per above comment suggest this becomes Fig 1b).

Done

P3, L1–2: The sentence introducing the “broad channels” doesn’t make clear whether the broad channels are surface features, bed features, or possibly both, and it would be improved if some idea of the dimensions of the relevant features were included. Could the authors consider showing these features explicitly in my suggested new panel Fig. 1a?

The language has been expanded “however, revealed broad, shallow channels trending north-south within the subglacial plateau region.”

P3, L9–14: There’s some unnecessary information here about surveys which aren’t used for this paper. I also suggest, for structural reasons, that you introduce the 2011 data in Section 2.2 (see comment below). Thus I think you could just excise these lines.

These data are used in the final compilation, so we argue for retention of this language.

P3, L16: Remove “have”.

Done

P3, L17: sp. teleseismic

Done

P4, L3: change to “[none]....sites overlaps with the...”

Done

P4, L13: Here is where I think you could say, for the first time, that ICECAP/HiCARS2 profiled across the Candidate A site in 2011. I suggest you also find alternative wording to your use of “core” in the current sentence.

Done

Figure 3: Just a minor point – why have distance going from right to left? Intuitively it would just seem preferable to have this axis reversed, if only to adjoin the description of the englacial layers diving off a cliff as mentioned in the main text. Admittedly this is not a major issue.

Figure 3 is flipped as requested

In the caption to Figure 3: typo: “along” rather than “alone”. The caption should also mention that the profile location is shown on Figure [1c...?].

Done

Sections 3 and 4 I suggest that both of these sections essentially outline the “Methods” or “Methodology” and could be titled as such in a single section. I think Section 3 misses an opening sentence or two to remind and re-orient the reader that you are now going to focus on data collected in 2016.

We expand the end of section 2 (The Dome C region) to provide context, and combine Section 3 and 4 into a Methods section.

Table 1, Row 2: sp. Scalar

Done

You introduce/describe in this section and list in the table some instruments whose data are not apparently used at all within the paper.

the description of the potential fields instruments is reduced.

P6, L4: reverse order: “constrain better”

Done

P6, L8–9: Unnecessary and could just be cut.

Removed

P6, L12: “...helped to refine...”

Done

P6, L13 and throughout Section 4.2: “...data were...”

Done

P7, L9: “...[hyperbolae]....characterize...” i.e. not characterizes with an “s”

Done

P7, L17: Move comma: “...column and, using...”

Done

P7, L18: Reverse “not” and “to”.... We choose not to apply...”

Done

Reviewer 3

Reviewer 3 felt that this paper needs a discussion section, and that we must own our preference for Candidate A. We have substantially enhanced the discussion in terms of the hydraulic and glaciological context for Candidate A.

Minor Comments:

P 1, L 3-4: “We find under the primary candidate region elevated rough topography, near a number of subglacial lakes, but also regions of smoother bed.” This wording is awkward and requires several readings to understand. One possible rephrasing is, “We find that the primary candidate region contains elevated rough topography interspersed with scattered subglacial lakes and some regions of smoother bed.”

Adopted

P 1 L 15-18: requirements for an intact ice column

Requirements 1 and 5 (low geothermal flux and low ice thickness) are really part of the same requirement: that the ice must be cold-based. Cold-based conditions require that the geothermal (and frictional) heat flux be low relative to the conductive heat flux, which is inversely proportional to ice thickness. The stated threshold of 2500 m is really a function of the geothermal flux. In addition, the cold-based requirement is in conflict with the low accumulation requirement, as lower accumulation rates tend to produce a warmer ice column and higher accumulation rates produce a colder ice column. A sentence or two outlining the physics behind these requirements would be useful here.

We add: “Items 1 and 2 interact, as low accumulation limits the advection of cold, requiring low geothermal heat flow to offset melting. Items 3, 4, and 5 lead to the somewhat contradictory requirement of a flat subglacial mountain.”

Section 2.2:

Mention that Candidate A is favorable because it is the largest candidate. The other candidates are only a few grid cells large, and are therefore unreliable. The thermomechanical model used to define the candidates is a continuum model, and therefore cannot be expected to accurately describe features on the grid cell scale. Candidate A is the only candidate that is much larger than the grid size, and is therefore the only candidate that can be considered a robust prediction of the model. This is actually the most powerful argument in favor of Candidate A.

We add in Section 2.2: “The size of Candidate A compared to the other local candidates also makes it more likely that the Van Lieffering & Pattyn (2013) model captured basal temperatures correctly.”

P 4 L 6: “...basal ice likely traverses the...”

The basal ice traversed the trough in the past, replace with “has likely traversed”.

Done

P 4 L15 – P 5 L2: “...while in the bottom 500 m, a region of more diffuse englacial scattering is present. This distinct zone of basal ice is also apparent in McCoRDS radar data that operates at a higher frequency.” This is a good place to reference Bell et al., 2011. The diffuse englacial scattering is similar to what they described as “valley wall” accretion ice near Dome A.

Added

P 7 L21: “We...maintain a strict first return policy.”

In an area of rough basal topography, there is a good chance that the first return may come from off-nadir bed returns. In fact, this is almost certainly what happened, given the results of Section 5. It might be good to include a sentence here mentioning that this first return policy likely resulted in picking off-nadir returns as the bed, and that you explore this in greater detail in the next section.

We add: “The first return represents a stable interface to interpret in radar, but has a high likelihood of selecting off nadir echoes in steep topography.”

Section 5: A large amount of the material in this section would be more suitable for the results section.

We move the cross over analysis to the appendix.

P 10, L5-12: Why not compute H for this dataset (or for the subset of this dataset within Candidate A)? You could determine how RMS roughness varies as a function of window size, and perhaps use this information to

say something about the processes responsible for shaping the landscape. This goes to my major comment above.

P 10, L13: “Figure 6 shows the relationship between RMS deviation at 1600 m length scale...” The axes label of Figure 6 says 800 m length scale.

Corrected to 800 m

P 10, L15-16: “A stronger relationship is seen for the focused data than for the pik1 data, primarily due to the larger crossover differences seen in the focused data.”

First comment: the sentence would be clearer if you said “...seen for the focused data than for the unfocused data...” rather than using code (“pik1”).

Second comment: The second half of this sentence would be more compelling if you said “primarily due to the geometric arguments given earlier”. The crossover differences are larger for the focused data than for the unfocused data because the unfocused data includes off-nadir returns in both the along-track and across-track directions, but the focused data only has off-nadir returns in the across-track direction.

Added

Figures

The map figures need to have some indication of latitude and longitude.

The figures are placed in the context of a new Figure 1 with latitude and longitude, and added north arrow to all maps.

Figure 1:

Specify in the caption that the “candidates” refer to the cold-based regions. I was looking for specific dots on the map.

This has been clarified in the figures

Figure 2:

The 10 m contours are extremely difficult to see in a printout. The inset map of Antarctica would be better suited for figure 1. Overlay the boundary of Candidate A.

This has been clarified in the figures.

Figure 3: Add more x-axis labels (say, every 10 km). Put the units (dB) on the colorscale. Indicate the boundaries of Candidate A. Add a note to the top left or bottom left corner of the image indicating the direction to Dome C.

It might also be helpful to show the echogram going all the way to Dome C. This will allow the x-axis scale to begin at zero, and (more importantly) it will allow the reader to assess how the continuity of the internal layers in Candidate A compares with the continuity of the internal layers at Dome C. If the echogram is expanded this way, you should also add a vertical line indicating the location of the Dome C ice core (or the closest approach to the core), with a tick indicating the lowest depth from which stratigraphically intact ice was recovered.

A second profile has been added showing the full area from Candidate A to Concordia.

Figure 4: It might be helpful to show another set of histograms where the range has been truncated at ± 100 m, so that the scale is not distorted by a few large outliers.

Figure 4 has been deleted

Figure 5: It is hard to see both the crossovers and the bed elevation, as both have similar color scales. It might be better to have the bed elevation in black and white. Alternately, it might be good to have two separate panels, one showing the bed elevation and one showing the crossovers. Consider merging figure 7 with this figure in that case.

Figure 5 has been deleted

Figure 6: It is hard to tell the two colors apart, and most of the figure space is blank white space. Consider splitting foc1 and pik1 into separate subplots. Also, consider using log-log axes to more efficiently use all of

the space. In addition, the caption says that the RMS window was 1600 m, while the axis label says it was 800 m.

This figure has been redrawn to be more clear.

Figure 7: See my note above about potentially merging this figure with figure 5.

Figure 7 has been merged into a more discussion focused Figure 5.

Figure 8: Again, was the length scale 800 m or 1600 m? The MOA background adds nothing to the figure, as it is a uniform gray. Consider using Bedmap2 as the background, with the same grayscale as the new bed elevations.

Figure 8 has been merged into Figure 5.