

General comments

Young et al. presented compilation of ice thickness data in an area around Dome C, combining the data that Young et al. obtained through their ICECAP II project and several other sets of older unpublished data that British, German and Italian parties obtained. The aim of the authors are to provide updated information on boundary conditions (ice thickness and bed roughness) in Dome C area in East Antarctica, where drilling of very old ice cores may be planned in future. Ice thickness data were collected with flight line spacing of 1 km. In addition, crossover differences in ice thicknesses were evaluated. The main body of the data were obtained from a radar with phase analysis, called as SAR processing, removing along-track hyperbolae effects of the radio echoes. The authors provided some discussions on the crossover differences caused effectively by highly elliptical shape of the radar footprint. At the end of the paper, the authors provided a new bed topography map as compilation of their ICECAP II project data and the other older data. The authors also performed evaluation of bedrock roughness in terms of RMS over a horizontal distance of 800 m along the flight lines. The authors suggested that there are some promising sites in a region of "A" for future ice coring.

This manuscript provide new information of ice thickness in this area with technical issues related to processing. My viewpoints are as follows.

(i) Main subject of the manuscript is improvement of information on boundary conditions (ice thickness and bed roughness). There is a large achievement in terms of making maps.

(ii) There are discussions on bed roughness, in terms of the effective shape of the footprint (caused by the SAR processing along the track).

There seems to me some problems and large rooms of improvement. I will explain details of them in the specific comments.

(iii) The ice thickness data with the SAR processing and the authors' procedure of echo extraction (always the first bed return and not the strongest bed return) were compiled with the older data with no information (or evaluation) of data quality. In addition, no evaluation of crossover errors are given between the new ICECAP II data and the old unpublished data. This situation causes question in terms of quality of the new compilation.

(iv) The authors also performed evaluation of bed roughness in terms of RMS over a distance of 800 m. This is a positive point though the analysis still seems preliminary.

(v) The authors conclude the candidate area A has some promising sites for future ice coring within it. But this conclusion does not seem to be based on good scientific discussions.

In the context of sites for drilling very old ice cores, main readers of the manuscript will be ice core scientists. They will want to see items as follows.

(i) Relation between detailed updated bed topography and known distribution of the subglacial water, both lakes and distribution of the wet/dry conditions of the bed. This is something that the authors can show relatively easily using published data and their new sets of data.

(ii) Distribution of the internal layers is also something that the authors can show relatively easily using published data and their new sets of data. The authors could show how dated layers near EDC ice core are traced over the candidate area A and the other candidate area. In addition, the authors

could discuss information of layers near the base of the ice sheet over the candidate area A

(iii) Like earlier BEDMAP and BEDMAP2 papers, I expect that electronic version of DEM data and the other related data should be published as supplementary data of this manuscript. In addition, I expect all sets of ice thickness data used in this work should be published.

Besides, I point out that directions in figures and descriptions seem highly confusing condition in this manuscript at the moment.

Overall, as a technical report for the boundary conditions, this manuscript has large rooms to repair, to improve and to add more items. Scientifically, more discussions for boundary conditions and suitability for the oldest sites are needed. When the authors publish maps, digital materials of DEM and raw sets of ice thickness data should be published as supplementary information. Orientation of the maps and descriptions in the manuscript should be carefully checked. If necessary, they should be repaired.

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. It seems scientific new is almost simple improvement of the topography map.

The third sentence in the abstract is not easily understandable. In addition, the authors did not show distribution of lakes or water in the paper. Then, please do not mention in the abstract. Otherwise, please show distribution of lakes and subglacial water in the manuscript.

P2L6

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

P2L9 - 10

Please provide citations for ICECAP and ICECAP2.

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. Such maps will help readers. Readers are not necessary familiar to this Dome C region of Antarctica.

P2L20 "crude" -> "pioneering"? I suggest so. Of course, pioneering work is usually crude.

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

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.

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.

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.

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.

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.

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?

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.

P4L6

basal ice in this region likely traverses the deep, wet CST and is unlikely to be stratigraphically intact.

It 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?

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.

P4L13

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

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

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

This figure is given but not discussed in the manuscript.

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.

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

Title of the section 3

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

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.

(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.

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

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.

P6L14 "Elevation difference"

Does it mean elevation of the ice sheet surface? Then, please clarify it to readers.

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.

L6L16-17

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

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.

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?

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.

Here again, meaning is unclear to me. What is your original sampling frequency. In think you did not tell it to readers. Do you mean that the aircraft fly ~88 m in a second along the flight line? It is hard to understand.

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?

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 34°? Please clarify this point, too.

Figure 5

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

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.

P10L15-16

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.

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?

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.

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"?

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.

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

Figure 8

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