

Final response “Climatic signals from 76 shallow firn cores in Dronning Maud Land, East Antarctica” by S. Altnau et. al

To Anonymous Referee #2

The paper compiled surface mass balance (SMB) data and water isotope ($\delta^{18}\text{O}$) data from 76 shallow firn cores in Dronning Maud Land, East Antarctica. The authors made composite of these cores for several regions categorized geographically on the ice shelves, plateau regions and regions in between. Both spatial distributions and temporal variations were investigated with assessment of statistical significance. They found that temporal variations are contrasted between regions on the ice shelves and plateau regions. They discussed possible reasons for the contrast, in terms of trends of the Southern Annular Mode (SAM).

In the ongoing discussion about climate change, investigations for detection of possible changes in Antarctica is something that polar scientists must do. So far, compilation of the SMB data and water isotope ($\delta^{18}\text{O}$) data were done for each limited ice core(s) core or site group with limited number of sites in Dronning Maud Land. In contrast to earlier studies, this paper attempts to see comprehensively the entire regions in western part of Dronning Maud Land.

This paper addresses scientific questions well within the scope of TC. This paper presents novel compilation of the ground data in one of important regions in East Antarctica. Substantial conclusions are reached with description of statistical limitations. The scientific methods are valid and basically well outlined. The results are basically sufficient to support the interpretations and conclusions. The authors state in their conclusion that “This was the first comprehensive study of this data set from coastal, transitional and interior DML”, which I agree.

I suggest in this review that there are rooms for improvement for the description of their compilation and calculations. Otherwise, the paper may not necessarily allow their reproduction by fellow scientists (traceability of results). When I read the paper a few times, it was not easy to understand which site data are used or not for which compilation. For example, please see comments No. 9, 10, 11 and 14 below.

The authors gave proper credit to related work and clearly indicated their own new/original contribution. The title clearly reflects the contents of the paper. The abstract provides a concise and complete summary. The overall presentation is basically well structured and clear, except the rooms of improvements that I suggest in this review. English is not my native language but it seems to me that the language in this paper is good.

Overall, because of the significance and relatively good quality of this work, I suggest that the paper should appear in publication of TC after necessary improvements are made. I would like to encourage authors to improve this paper.

Specific points are commented below with numbers.

AC: We are grateful to Referee # 2 for the thorough review and constructive criticism.

1. This paper did not provide any comments to readers on how precipitation occur in this region, that is, clear sky precipitation, sporadic events of cyclonic activities, occurrence of blocking and redistribution of snow by wind and by sublimation/condensation. Please consider adding short statements for these for a better guide for readers.

AC: We added information about this in the discussion. (see changes in a marked-up manuscript version)

2. Page 5966, lines 4-5.

Please provide definition of shallow cores and medium-deep cores.

AC: We included the total depth/core length of the cores in Table A1.

Reference to Table A1: *...medium-deep cores (see Tab. A1).*

3. Page 5966, a paragraph near the page bottom.

Both Anschütz et al. (2009) and Fujita et al. (2011) investigated at sites outside of the Western DML, that is, outside of any figures in this paper. For a better understanding by readers, another figure for showing wider DML or wider Antarctica with indications of mentioned sites seems useful.

AC: We compare only the sites that are shown on the map. We do not like to add another figure since other reviewers even required to delete some figures and we do not think an additional figure is necessary here.

We gave some additional information in the text:

pp.5966, line 23: *... Dome Fuji (along ID2, Figure 1 and further east).*

4. Page 5966, line 24.

This was supported by a further study of ice core at Dome Fuji using volcanic time markers (Igarashi et al., 2011). Please consider addition of this paper in the statement.

Igarashi, M., Nakai, Y., Motizuki, Y., Takahashi, K., Motoyama, H., and Makishima, K.: Dating of the Dome Fuji shallow ice core based on a record of volcanic eruptions from AD 1260 to ad 2001, *Polar Science*, 5, 411-420, doi:10.1016/j.polar.2011.08.001, 2011.

AC: We did not include the paper since Igarashi et al. (2011) dated the shallow firn core on a lower temporal resolution and they examined the accumulation on longer time scales. Additionally they found no clear trend for the considered time period.

5. Page 5966, lines 24-25.

Please confirm if the statement below is really correct.

“This is not confirmed by a study of Frezzotti et al. (2013) who provided a synthesis of Antarctic SMB during the last 800 years.”

These authors Frezzotti et al. (2013) stated in their conclusion “However, a clear increase in accumulation of more than 10% ($>300 \text{ kgm}^{-2} \text{ yr}^{-1}$) has occurred in high-SMB coastal regions and over the highest part of the East Antarctic ice divide since the 1960s.” It seems that the central part of DML is the highest part of the East Antarctic ice divide.

AC: We rewrote the sentence/paragraph:

Frezzotti (2013) provided a synthesis of Antarctic SMB during the last 800 years. They state that SMB over most of Antarctica do not exhibit an overall clear trend. However, they found a clear increase in SMB in coastal regions and over the highest part of the East Antarctic ice divide since the 1960s, which confirms the results of Fujita et al. (2011) but contradicts those of Divine et al. (2009) and Kaczmarek et al. (2004).

6. Page 5966, line 27.

Where in the East Antarctic Plateau, was it observed? Please specify a region. Otherwise, readers will not understand.

AC: We specified the traverse route in the text:

p. 5966, line 18:

...through DML from the Norwegian base Troll to the South Pole.

p. 5966, line 26:

...SMB changes in ice cores retrieved during the above-mentioned traverse from Troll to South Pole but found that almost all sites...

7. Table A1 in the context of Page 5967 line 5.

Can core depths/lengths be listed? It is informative if you can do it.

AC: The core depth/length is additionally listed in Table 1 now.

8. Page 5967 line 8.

What does “they” mean here, two cores or all cores?

AC: We refer to all cores in this sentence. We rephrased the sentence more clearly:

Spatially the dataset represents the entire western DML.

9. Page 5968 line 4 (Table 1).

Difference between Ekström Ice Shelf and Ekström Ice Shelf (R) was not clear to me. Did you give some explanation to readers somewhere in this paper?

We wrote that we included the six cores derived from Halvfarryggen and Søråsen in the Ekström Ice Shelf (R) composite record. Since the cores are strongly influenced by local conditions we stressed that this group has to be considered with care. In accordance with a comment from Reviewer 3 we added a sentence in parentheses that, for the sake of completeness, we do not want to omit these data.

Page 5968 line 6-9:

The second group includes these cores plus six more cores situated on Søråsen and Halvfarryggen (Fernandoy et al., 2010). However, as stated before, these cores are strongly influenced by local conditions, thus this group has to be considered with care.

(For the sake of completeness we did not omit them from our study.)

10. Table1.

For each group of site, please indicate name of the group such as ice shelf name, plateau or something like this. In general, my concern in this paper is that it is hard to understand which site data belong to which geographical category (such as my unknown Ekström Ice Shelf (R)) and how they were used. I think that an addition of such a table (or tables) as supplementary information is useful both for readers and for future researchers who will recompile the data using additional data of future.

AC: We gave additional information in the caption of Table 1.

“Ice Shelves” corresponds to all cores from Fimbul, Ekström and Riiser-Larsen Ice Shelves. “Ekström (R)” refers to all cores from Ekström Ice Shelf plus the adjacent ridges Søråsen and Halvfarryggen.

11. Page 5968 lines 3-20.

You described several groups as documentation here. Because of complication of many detailed information, I suggest that the authors should provide a supplementary table to explain how grouping and sub-grouping were done. In addition, I hope to see in Figure 1 and other figures that the authors use symbol markers so that readers can see intuitively relations between sites and the authors' grouping of sites.

AC: We provided information about the grouping in the caption of Table 1. We do not think a a supplementary table is necessary here.

The cores in Fig. 1 are colour-coded, each sub-group has its own colour. In Fig. 2 it would have been not clear to use circles for the plateau cores: we used crosses because the data points are lying so close together that it would be difficult to distinguish between single circles. On the other hand, crosses would not have been very clear on the map. Therefore we did not use the same symbol markers in all figures.

12. Page 5969 line 26 – Page 5970 line 3.

You stated “*Thus the positive phase of SAM is characterized by strong, mostly zonal westerlies with only low amplitudes of planetary waves. This means little exchange of moisture and energy between mid and high latitudes and consequently a cooling of Antarctica, with the exception of the Antarctic Peninsula, which projects farther north than the rest of the continent .*”

Is this your scientific claim or well-known meaning of SAM? Please clarify. If the latter is the case, please provide reference papers. I did not find such a view in Marshall (2003) paper.

AC: We provided a reference paper by Marshall (2013) in this paragraph to clarify this view.

13. Page 5972 lines 20-21.

You stated “*latitude and elevation effect are closely connected in Antarctica since generally the elevation increases with latitude.*”

This is not a useful rule. It is true only for very limited area in Antarctica. I suggest you to consider to remove this statement and related statements.

I felt that showing the data (SMB and water isotopes) in terms of latitude has little meaning. It is because earlier studies (for example Satow et al. 1999 below) showed examples showing elevation had very strong effects.

I agree that angle of insolation (that is, latitude) potentially have some effects to SMB and water isotopes. But I do not believe that such a faint effect can be visible in simple X-Y plot here. With simple X-Y plot here, we simply see elevation dependency of data through distortion of incompatibility between latitude and elevation. Nothing more can be seen.

Rather, I can see that you did not analyze the data in terms of continentality (distance from open ocean) or relative location in terms of wind-lee or windward side of ice divide. In Figure 3a, deviation of the data points from the regression line seem to mean such effects, which you did not examine.

Satow, K., Watanabe, O., Shoji, H., and Motoyama, H.: The relationship among accumulation rate, stable isotope ratio and surface temperature on the plateau of East Dronning Maud Land, Antarctica, *Polar Meteorol. Glaciol.*, 13, 43-52, 1999.

AC: We agree that this is not as straightforward as we claimed. We removed this statement and related statements:

p. 5972 line 19: remove the word “latitude”

We removed the paragraph line 20 -line 25. We also removed Figure 3b and the paragraph where we discuss the figure.

14. Figures 2, 3 and 4

I suggest that you use the common symbol markers for the same sites in these 3 figures, to improve readers better understanding.

AC: we explained our choice of symbol markers above.

15. Page 5976 lines 15–17.

You stated “*The Little Ice Age (LIA), a colder period widely seen in the Northern Hemisphere between 1650 and 1850 is not clearly present in DML. In a 1000 years chronology from Amundsenisen (Graf et al., 2002).*”

Around here, it is not clear whether the statements are based on data in this work (Figure 8), citation (Graf et al. 2002) or both. Please clarify. If readers need to see Figure 8, please specify which feature in the figure readers should see.

AC: We rewrote this sentence:

Our study covers only the second half of the LIA and the relatively cool period in the second half of the 19th century (seen in Fig. 8) cannot clearly be related to the LIA in the Northern Hemisphere.

17. Page 5977 lines 21–25.

I did not understand well your logic at these lines.

You term “atmospheric flow”. It seems to me, in any case, moisture transport occurs due to atmospheric flow from lower latitude, occurring due to cyclonic activities or occurrence of blocking, by which precipitation is induced both in inland and on ice shelves. It seems to me that a main difference between ice shelves and plateau sites is flat land or presence of large scale slope. By a term of “atmospheric flow”, do you mean that flow of moisture on topographically flat area? Please make me (and readers) understand.

AC: “Atmospheric flow conditions” means the general pattern of the atmospheric circulation, e.g. more zonal/meridional flow, location and movement of cyclones etc. We thought about giving a more detailed explanation of the meteorological conditions we discuss. However, although we fully support the requirement that a paper should be self-contained and a scientist, who works in the corresponding field, should be able to understand it without reading 5 other papers, we believe that ice core studies are highly interdisciplinary and we assume that the readers, who work with ice cores and climate, have (should have) some basic knowledge in meteorology. (We don’t explain e.g. snow metamorphosis in each ice core paper either.) Thus we concluded that more detailed explanations would destroy the structure of the paper and deter from our main points. We re-wrote the discussion and conclusion section, but refrained from explaining basic meteorological terms.

18. Page 5977 line 26 – Page 5978 line 4.

Generally accepted views? Then, citation?

AC: We refer to Marshall (2013) where he described the effects of the positive/negative phase of the SAM index to the energy and moisture exchange between high and midlatitudes.

19. Page 5978 lines 20 – 23.

You showed the effects of increasing altitude in this paper. But you did not show any effects of decreasing incidence angle of solar radiation (meaning decreasing temperature) or increasing continentality (meaning less moisture available). The statement is much more than you really showed with data. The statement should be given differently.

AC: We deleted the part about the latitude dependence.

20. Page 5979 line 29 – Page 5980 line 4.

Your statement is that cyclonic activities do not necessary increase precipitation on ice shelves. Is there no possibility that present sampling (statistical handling of sites) are still insufficient with some probability? If we try to access the SMB data and water isotope data with better statistical sampling what we do? Perhaps such information is useful for readers.

AC: The sampling issue in the case of Antarctica is a somewhat trivial to answer: there is so few data (also on SMB) that hardly one can have a risk of oversampling doing any field work. The more the better. With the available data we cannot state whether this is a sampling problem or a lack of correlation (causal link) in the nature itself. Better sampling (more data) of course would help to decrease the number of working hypotheses.

21. Discussion and conclusion in general.

Discussions are often mixtures of the data, citation and speculation. Because of this condition of the mixture, it is sometimes hard for me to understand basis of each statement. The item just above (Page 5978 lines 20 – 23) is one of such examples. Please be careful to tell to readers basis of each statement. Please clarify the statement is based on data, citation or speculation.

AC: We re-wrote the whole discussion and conclusion.

22. Recent significant papers.

I suggest that the authors consider to mention some of recent significant papers on water isotopes in the paper. They are Steen-Larsen et al. (2014) and Hoshina et al. (2014) as follows. The former showed that there is strong exchanges of water isotopes between snow and air at NEEM. It should surely occur in Antarctica. The latter showed that there is a strong post-depositional alternation of water isotopes. Both papers mean that exchanges of moisture between air and snow play important role to determine water isotope fixed as ice core data.

Steen-Larsen, H. C., Masson-Delmotte, V., Hirabayashi, M., Winkler, R., Satow, K., Prie, F., Bayou, N., Brun, E., Cuffey, K. M., Dahl-Jensen, D., Dumont, M., Guillevic, M., Kipfstuhl, S., Landais, A., Popp, T., Risi, C., Steffen, K., Stenni, B., and Sveinbjornsdottir, A. E.: What controls the isotopic composition of greenland surface snow?, *Clim. Past.*, 10, 377-392, 10.5194/cp-10-377-2014, 2014.

Hoshina, Y., Fujita, K., Nakazawa, F., Iizuka, Y., Miyake, T., Hirabayashi, M., Kuramoto, T., Fujita, S., and Motoyama, H.: Effect of accumulation rate on water stable isotopes of near-surface snow in inland antarctica, *Journal of Geophysical Research-Atmospheres*, 119, 274-283, 10.1002/2013jd020771, 2014.

AC: We fully agree that post-depositional processes are important. We re-wrote the discussion and conclusion section and included information on post-depositional processes (including the suggested references) (see changes in a marked-up manuscript version)