**Review of "Climatic signals from 76 shallow firn cores in Dronning Maud Land, East Antarctica" by S. Altnau, E. Schlosser, E. Isaksson, and D. Divine**

The paper compiled surface mass balance (SMB) data and water isotope ($\delta^{18}O$) 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 ($\delta^{18}O$) 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.

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.

2. Page 5966, lines 4-5.
Please provide definition of shallow cores and medium-deep cores.

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.

4. Page 5966, line 24.
This was supported by a further study of ice core at Dome Fuji using volcanic time markers (Igaras hi 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.

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 $kgm^{-2}$ $yr^{-1}$) 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.

6. Page 5966, line 27.
Where in the East Antarctic Plateau, was it observed? Please specify a region. Otherwise, readers will not understand.

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.

8. Page 5967 line 8.
What does "they" mean here, two cores or all cores?

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?

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.

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.

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 then 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.

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.

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.

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)*."

16. 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.

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.

18. Page 5977 line 26 – Page 5978 line 4.
Generally accepted views? Then, citation?

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.

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.

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.

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.