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

To Anonymous Referee #4

AC: We thank all referees for their efforts and the constructive criticism.

Altnau et al. compile availabe records of 76 shallow firn cores from the western part of Dronning Maud Land to analyze the relationship between the temperature proxy, delta18O, and the surface mass balance. As can be expected from the complex terrain including ice shelves, mountain ranges, ice divides and the plateau considerable differences are found. It is an interesting analysis, well written. I think what I can add as another referee is the following:

The introduction is very detailed. I have the feeling from the title, abstract and introduction the point of your interest is changing climate and recent climate change.
What I am missing in the paper are a few sentences commenting on the massive mass changes in the DML area described by Boening et al. (GRL2012) and the following papers. The years after 2009 are not part of this work, I am aware of this, but you must have looked through lots of records and should be able to tell us whether or not the 2009 mass change event in DML has counterparts during the last 60 or 200 years.

AC: True, the years after 2009 are not part of our study. 2009 was a single year with very wet and warm conditions in East Antarctica, followed by the very dry and cold 2010. Since we were interested in climatic trends, we did not consider single years. It is also difficult to compare data from very different types of measurements.

Reanalysis data: The authors list tells me that you have expertise in the analysis of reanalysis data. There is a 50-60 year long record available. Of course, reanalysis data have lots weaknesses particularly in the polar regions as you mentioned. However, we generally see in the delta-18O a temperature signal. I do not expect a complete reanalysis work but you should be able to say more than Halley the only station with a longer record is too far south. Are the periods showing a positive 18O-trend in the ice core records reflected in the reanalysis data records as periods of positive temperature anomaly? I expect you know more what you tell us.

AC: We do say more than that Halley is too far south. We compare the $\delta^{18}O$ and the SMB to measured air temperature at Neumayer Station and find that the latter shows no trend, whereas $\delta^{18}O$ and SMB show opposite trends. We think that Neumayer is representative for this coastal part of DML and prefer measurements to reanalysis data. Also Klöwer et al. (2013) state that reanalysis data are not sufficient to access climate trends in Antarctica.

Klöwer, M., T. Jung, G. König-Langlo, T. Semmler, 2013. Aspects of weather parameters at Neumayer station, Antarctica, and their representation in reanalysis and climate model data. Meteor. Zeitschr., doi: 10.1127/0941-2948/2013/0505.

You argue with changes in seasonality. I do not like this argument. It is some form of "deus ex machina" everywhere right. From the Neumayer data E. Schlosser has

analyzed you should be able to make a clearer statement if accumulation or whatever may have changed recently if anything has changed.

AC: That is not a clear sentence and hard to understand.

This argument is often used but
it is a quite cheap argument and explaining nothing.

AC: The point is not whether Ref. #4 ”likes” this argument or not. We believe that to call it a “cheap argument that explains nothing” is a sign for lack of understanding. Seasonality is far away from being a “deus ex machina”. There are several studies, in which seasonality is considered in detail (e.g. Schlosser, 1999, Noone et al, 1999).

Little Ice Age Considering that you only present records not older than 200 years it is probably hard to make firm statements about the LIA. Furthermore, the bipolar seesaw may (?) also work on shorter and even decadal time scales then what can we expect to see in a 200 year record as evidence of the LIA.

AC: We deleted the remark about LIA in the discussion and conclusion section. We kept the reference Graf et al. 2002 in the description of Fig. 6, since he investigated longer cores.

No altitude effect on the Ekström ice shelf and the 600 m high ridges east and west of it. I believe that this is easy to understand. The 600 m is cloudy level and the Ekströmisen gets lots or most of its snow from clouds from this level. Don’t you think so?

AC: We do not think so. We assume you refer to the lifting condensation level when you say “cloudy level” and there is no reason it should be always at 600m since it depends on the individual dynamics of the frontal systems involved. Also, the cloud layers have different thicknesses and there could be multiple layers. We checked the radiosonde data from Neumayer and found no evidence for your statement. Even if it were true, that the LCL was always at 600m at Neumayer, the air mass would still be orographically lifted when it flowed over the ridges. Orographic precipitation is always a highly complex process and we don’t think what we observe is “easy to understand” as Ref. #4 states.