Philippe et al. TCD

The authors put a lot of work into the revised version and the manuscript has been greatly improved. However, some points remain, which I will address in the following.

First a remark: the font size of the manuscript pdf file is a bit of an imposition, it was completely unnecessary to keep the formatting info on the right side. Especially with all the marked changes it was very hard to read. I suggest to avoid that in the future.

Comments to reply to specific comments (line numbers refer to the original manuscript:

P2, l23: you still do not mention that Altnau et al. found a negative trend for SMB at the coast at this point. Why?

P6, L3: your reply should be discussed in the text.

P10, L5: I still think that an SMB of 0.3 is difficult to use as a threshold since many sites at the coast have SMBs just around 0.3. so slightly above or below 0.3 would not mean a systematic difference here. (I am not sure why Massimo Frezzotti chose it in the first place.)

P12, L20: Please, discuss this in the text, too. The high SMB in 2009 and 2011 found by Lenaerts was mainly due to the atmosheric circulation patterns durings those years. Those patterns have a much stronger influence on SMB than a couple of days longer or shorter sea ice coverage. (and keep in mind that sea ice extent refers to 15% sea ice concentration, so plenty of open water available for evaporation.)

From now on, the line numbers refer to the revised version:

P2

Ll9: what does it mean that this is the only record that supports model results?? Are all the other cores not representative and the DIR core is the only representative one? This is not self-evident.

L20: this is not consistent evidence: e.g. Fudge et al. found that SMB and temperature are not always positively correlated.

Fudge, T. J., B. R. Markle, K. M. Cuffey, C. Buizert, K. C. Taylor, E. J. Steig, E. D. Waddington, H. Conway, and M. Koutnik (2016), Variable relationship between accumulation and temperature in West Antarctica for the past 31,000 years, Geophys. Res. Lett., 43, 3795–3803, doi:10.1002/2016GL068356.

P3

L 18: see above, negative trend in SMB in coastal cores in Altnau et al.

L20: I would not call this “very few”, there are quite a few investigations of DML cores from German, Scandinavian and Indian expeditions.

L21: higher than the interior (a comparative needs something to compare to)

P4

L15: ice rises are too small to "block" atmospheric circulation. (this would mean that the air flows AROUND the ice rise rather than over it. Blocking is a clearly defined term in meteorology.

L18-22: good!

P9

L3: this is not correct, there is ERA20C (ECMWF) meanwhile, which covers the entire 20th century.

P12

L7: delete "is"

P17

L4. Better: source region of atmospheric moisture for DIR.

Fig. 8 should be described as part of the results section.

L2-8: sea ice is only one factor. The same factor that causes high accumulation might influence the sea ice extent without changes in sea ice being the reason for the accumulation deviations. I still do not find this paragraph very convincing. E.g. high sea ice extent related to lower air temperatures might be caused by a generally more zonal atmospheric circulation pattern, which at the same time could be the reason for low accumulation due to lack of meridional moisture transport (as Lenaerts et al. showed for 2009 and 2011. These things should be discussed in the text.

L18: see above. Does it not make you think that no other coastal core in DML shows an increase in SMB?

L27: see above, please explain the physical reason for the choice of the threshold.

L31: if we compared

P20

L1-7: see above, maybe quote Fudge et al. here, too.

L8ff: this is still not clear. Atmospheric rivers don't occur for the whole year, just for certain events. Precipitation at the coast is usually event-type, but the events occur during the whole year, whereas in the interior those events happen not very often, but are related to amplified Rossby waves.

L25: 2009 and 2011

P21

L8: wind is certainly a very important factor, but e.g. the interannual variability in the years 2009-2011 was definitely not mainly caused by the wind. Be careful with general statements like this. Of course, in years with fairly “average” flow patterns, the wind is the main factor, that is correct.

L21: why should there be a decrease at the coast then?

L24ff: see comment on P1

Parts of the discussion actually belong into the results section.

p23

l16ff: see above