

Interactive comment on “Ice core evidence for a recent increase in snow accumulation in coastal Dronning Maud Land, East Antarctica” by M. Philippe et al.

Anonymous Referee #2

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The paper investigates the surface mass balance (SMB) at Derwael Ice Rise, Fimbulisen, Dronning Maud Land, using an ice core drilled at the top of the ice rise. The authors describe their dating methods in some detail and investigate trends in the SMB. They find a positive trend in SMB in the last century, which they state is expected by climate models. They also try to explain this trend by sea ice, temperature, and surface pressure data from a General Circulation Model (GCM). They report that the presented core is the first coastal core in East Antarctica that shows a steady increase in SMB since the beginning of the 20th century.

The paper is generally interesting and the ice core certainly yields valuable new data from a still quite poorly explored area. The English is mostly correct, even though

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sometimes the wording is a bit awkward and some formulations are not clear or ambiguous.

However, there are several important points that need to be reconsidered. I would recommend major revision.

General comments:

I have only basic knowledge in dating ice cores using flow models, so I cannot assess the critics of referee #1 considering this point. The authors do show both the uncorrected data and the correction with the different models, so the reader can assess what they have done. Also, their main conclusion (positive SMB trend in the last 100 years) would still be valid for any calculation of layer thinning that lies between the two methods they use.

However, I share Referee #1's doubts about the details of the dating, particularly the use of volcanic horizons, since the attribution of the ECM peaks in Figure 4 to the different eruptions is not convincing, except for Tambora. Also, the authors do not give details about the layer counting using stable isotopes, to which depth this was possible etc. Nobody expects a perfect dating of an ice core because this hardly ever exists. However, I think the authors should discuss the error possibilities of the dating a bit more and give a more realistic quantitative estimate of the error. Probably, within the error bounds, their main result would hold. But, see above, I cannot assess the details of the used models. The authors state that their findings (increase in SMB in a coastal East Antarctic core) are the first ones that support model predictions. This does not make them discuss how representative their results are. They compare their results with other firn/ice cores, but do not compare the temporal variations of the SMB derived from the core with temporal variations of measured and/or modelled air temperature, sea ice, or surface pressure data). Instead they look at composites for very positive and very negative years, which is, in principal, not a bad thing to do, but I would expect stronger signals here in order to be convincing. The arguments using the output from

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the Community Earth System Model are a bit weak. The discussion of the atmospheric dynamics involved is not clear and mixes up conditions at the coast and in the interior of Antarctica. Also, different time scales are mixed together and often it is not clear, which time period is meant when certain trends are reported.

Specific comments:

Title: what does “recent” mean?, and, to be correct, “snow accumulation” should be “surface mass balance”.

Abstract: It would be good to re-write the abstract after the main text has been revised.

P2:

I5: increasing ice discharge

I8: What does the Polvani paper have to do with warming- related increase in precip? There are other papers that involve data and modelling and do not find either warming or increase in precipitation in the considered period. Please, make sure that it is clear about which time period you are talking.

I23: “both authors concluded that the trends were insignificant”. This is not correct and not exact. Which trends? Altnau et al. found a statistically significant positive trend in SMB for the interior DML.

P3: L10ff: grammar: in your sentence, “which” refers to the project.

L12: a local flow regime

How high is the accumulation rate? It would be good to give this information already here.

P4:

L3: do you mean 30mm x 30mm?

L13: the boundary between annual layers

L21: better: were carried out

P5:

L5: snow burial: better: the compression of the snow under its own weight It would be interesting to see the density profile here, maybe you could add this in a figure. I also miss some information about the depth until which seasonal variations in the isotope ratios can be resolved.

P6:

L3: how reliable are the CESM data for the 19th century, especially sea ice?

L24: better : mainly derived from. . .

P7:

1ff: see above. The volcanic peaks in Figure 4 seem to be pretty ambiguous in most cases.

P8:

L15ff: This is a very short and simplified view. The sea ice argument is not convincing, especially the hatched area of anomalies is fairly small and should not have a large impact on precipitation amounts. A decrease in surface pressure of not much more than 1hPa is not very much, even in a composite, and in that case, lower surface pressure does not necessarily mean higher precipitation. I'll get back to that in the discussion part.

L26: define "current" , please.

P9:

L2: How do you define "climate-related"? What else could it be on this time scale? Could it be that the first in-situ validation of increased precipitation in coastal Antarctica is due to the fact that the drilling location is influenced rather locally? Did you compare

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it with temperature proxies? I am not saying it is wrong or right what you state, but you should discuss this.

L8: strange usage of “refer to”. Maybe better “represents” or similar.

L13ff. Decreasing trend: I assume you mean “negative trend”. Decreasing would mean getting stronger negative with time.

Please, make sure that it is clear, which time period is considered in your respective comparisons.

L10: Stenni et al: 1992-1996: too short a period to consider any trend calculation

P10:

L5. What is the reason for the choice of the threshold? Many coastal stations have SMBs around 0.3. This seems a bit arbitrary.

L9: this is covered by only two high accumulation sites..

L14: dating accuracy

P11:

L4ff: the positive trend in SMB. . . the result of various forcings

L7: the air does not “hold vapor”, a higher temperature means a higher saturation vapor pressure.

L7ff: Paragraph 4.3 is very important, but, unfortunately, it contains quite a few misconceptions (in spite of the fact that one of the co-authors is a meteorologist and expert for polar/Antarctic meteorology) and thus should be re-written:

First of all, there is quite a bit of confusion of coastal and continental conditions. Several papers are quoted, of which some deal with the interior and others with the coastal areas of Antarctica, which, however, have very different precipitation regimes. Amplified Rossby waves are particularly important for precipitation in the interior of the continent,

NOT for the coast. The coastal areas are always under the influence of synoptic activity in the circumpolar trough. The individual events quoted in line 18 can bring up to 50% of the total accumulation in the interior, not at the coast. And also this means the sum of all events, not one single event. 2009 and 2011 were years with such events in the interior, which of course, also bring high precipitation to some coastal areas, but are not necessarily associated with lower surface pressure, on the contrary, the pressure in the coastal areas of Antarctica is usually lower in years like 2010, where a zonal flow was predominant and the interior of the continent got less precipitation than on average.

L25ff: SAM: what was the temporal resolution of your comparison of SAM, SOI and your data? Annual means, monthly values? You should not expect any signal in the annual mean since the SAM index has high intra-annual variations.

P12:

L 4ff: you discuss topographic influences here, but never question that the result for the ice rise might be more locally influenced than climate-related (whatever that means). The topography of an ice rise influences the synoptically caused winds much more than the surrounding ice shelf or the plateau since the ice rise represents a disturbance in the main flow. This is especially surprising since the authors include the Lenaerts et al. J. Glac.2014 paper, which investigates the climate and mass balance on ice rises, in the reference list, but never discuss it in the text.

L19: what do you mean by “these two highly variable accumulation events”?

L20: what is the physical explanation for DML being most susceptible to an increase in snowfall in a warmer climate? So far, a positive trend in Antarctic sea ice has been observed, which according to your findings, should decrease precipitation. (not sure about the regional trends, though, I am no sea ice expert.)

L24ff: see general comment. What is the temporal resolution of the investigation of the

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relationship between SAM, SOI and SMB?

L26ff: Low pressure: see above. Usually the pressure in the circumpolar trough is lower (on average) in years with more zonal flow and less meridional heat and moisture exchange (positive SAM index) than in years with amplified Rossby waves.

P13:

L4: positive trend

L12ff: I do agree that the ice rise is a suitable potential drilling site for a longer core. However, you should investigate the representativeness of your results a bit closer and keep this in mind when interpreting a deeper core

References:

The reference list contains quite a few publications that are not quoted in the text. Please, check.

P16: L15: new paragraph: Hofstede. . .

P20: l25; new paragraph: Schlosser. . .

P26: the caption of Figure 26 should be rephrased: “Diff. in mean annual SMB between ~1960-present and ~1816 –present (a,b)” (c,d accordingly)

P31: Figure 6: a) b) labels missing

The legend is a bit confusing, since the dotted lines claim to be a mean SMB, only the caption explains that it is mean plus/minus STD. Maybe a single line with some shading for the range of the STD would be show this more clearly. For 1992 to 2012, one would expect that the averages are not very different, given the closeness of the green and the black line. ??

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