

# ***Interactive comment on “Interactions between Antarctic sea ice and large-scale atmospheric modes in CMIP5 models” by Serena Schroeter et al.***

## **Anonymous Referee #2**

Received and published: 22 December 2016

### General comments:

The manuscript explores the relationship between sea ice variability and large-scale atmospheric variability for the seasons of sea ice advance and sea ice retreat for five different sectors of the Southern Ocean. This is performed both for reanalysis data and model output from 16 CMIP5 models. The study provides very valuable insights into how large-scale atmospheric variability modes interact with Antarctic sea ice variability. As stated by the authors, oceanic drivers and atmospheric drivers not related to the major modes of variability are not investigated in this study. The study is clearly structured, well written and suited for this journal. I consider the manuscript almost ready for publication but want the authors to address the minor issues raised in the specific

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comments.

Specific comments:

1) p.1, ll.12+:

In the abstract and the conclusion section the authors state that their paper investigates the relationship between sea ice variability and atmospheric variability. Especially in the results section however, the authors do not mention variability, but e.g. talk about "the relationship between sea ice and atmospheric conditions during the seasons of ice advance and retreat" (p.6, ll.20+). This is confusing. I am finally not sure, whether the paper really investigates the atmosphere-ice interactions in terms of variability. I encourage the authors to consistently check whether they say what they intend to say.

2) p.1, l.15:

This study does not show the ocean to be a dominant driver of sea ice retreat. The statement is hypothetical and need to be changed or removed. I like the phrasing in the final sentence of the abstract.

3) p.4, l.5:

Is there a reason why September is not considered?

4) p.4, ll.29+:

The authors mention the use of monthly reanalysis data, but they never specify the time resolution of the CMIP5 model output used. I assume this is also monthly. Please specify this here. Further the authors use reanalysis data from 1979 to 2014 but historical model output only until 2005. Why don't the authors prolong the historical simulations until 2014? At least I would like to know whether the results remain qualitatively the same when prolonging the simulations by the last 10 years, i.e. with RCP4.5.

5) p.5, ll.6+:

It is not clear to me how the authors detrend the reanalysis data and the piControl simulations. Did they use linear detrending for both? If so, is this appropriate for the reanalysis data? The authors should explain more specifically the methods they use.

6) p.5, ll.10-11:

Related to 4) I wonder whether monthly data is sufficient to detect autocorrelation in the SLP and SIE data.

7) The authors mention the similarity of their approach to that of Raphael and Hobbs (2014) in the method section and the similarity of their results to those from Raphael and Hobbs (2014) in the results section. I roughly know the study by Raphael and Hobbs (2014). However, from the present study it is not clear to me which scientific insights go beyond those from Raphael and Hobbs (2014). This needs to be pointed out more clearly. I appreciate that the authors try this distinction especially on p.4, ll.1-15, but I feel that at least its role as a predecessor study is not sufficiently accounted for.

8) p.6, ll.2-4:

I am not convinced that ensemble averaging for the historical model output is a good solution when correlating to the reanalysis. The reanalysis (and also reality) is a single realization and thus cannot be expected to be related to the ensemble average of a model.

9) p.6, section 4.1:

I have some difficulties with the description of the results presented in Fig.1.

p.7, l.14: Please mention that the correlation pattern during retreat (Fig. 1d) is much weaker than during advance (Fig.1c).

p.7, ll.23+: I do not see a pattern similarity between Fig.1b and Fig.1f, even not of inverse sign. Please check again whether the interpretation is really supported by the

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results shown in Fig.1.

p.8, l.2: Why not a new paragraph for East Antarctica here?

10) p.8, l.31-32:

Are the numbers 12 for East Antarctica and 4 for King Hakon VII correct? According to Fig. 2d for King Hakon, there are more than 4 models situated above 0.5 for the advance season.

11) p.9, l.33:

The second metric is clear, but what is the first metric? This becomes not very clear by structure. Try to use the expression "the first metric" before "a second metric".

12) p.11, l.15:

The start of the sentence is misleading because to me it sounds like a definition of the advance season. I would suggest to start with: "In the advance season the modeled sea ice trends diverge ..."

13) In contrast to the rest of the manuscript, I find the conclusion section a bit weak. I think it hides some major findings that are more clearly stated in the results section. I would also love to see that the last sentence/paragraph contains the major conclusion(s) of or the overall benefit from the present paper, rather than an outlook as it is currently done. To me, this leaves the impression the results of this paper are not important which is not true.

14) Fig.1 and Fig.S1 (captions): I would prefer red dotted/blue "contours" or "isolines" instead of just "lines".

15) Fig.2 (caption): The authors mention dotted lines at 0.4 and -0.4. I cannot find them in the figure.

16) Fig.5: It would be very helpful for the reader if the authors would use the same

color for each model as in Fig.3. I cannot see a reason for not doing so.

Technical comments:

p.2, l.19: "i" is missing in comparatively

p.3, l.33: remove one "boundaries"

p.5, l.10: significance instead of "significant"

Fig.6 (caption): a dot is missing after "retreat"

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Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-200, 2016.

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