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

Anonymous Referee #1

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Review of "Interactions between Antarctic sea ice and large-scale atmospheric modes in CMIP5 models" by Schroeter et al.

General Comments

This paper analyses observed Antarctic sea ice extent, sea-level pressure from the ERA-Interim reanalysis, and the CMIP5 models to assess linkages between atmospheric forcing and sea ice in both the real world and in simulations. The paper demonstrates observed linkages between sea ice variability and atmospheric forcing in different regions/seasons, and proceeds to consider how well the models capture these linkages. It also investigates how well the atmospheric variability in the models reproduces that in the reanalysis.

My overall impression of this paper is overwhelmingly positive. It contains important new material and is executed well. The results have far-reaching implications for the study of Antarctic climate, both modelled and observed. I congratulate the authors on having produced such a nice piece of work. I have a long list of suggestions for improvements in areas where I did not follow the interpretation of the results. I listed these as major revisions because there are quite a few of them, but I don't think I need to re-review the paper as I trust the authors to incorporate my points where they think appropriate.

Specific Comments

P1L15: I am unconvinced that the ocean is a dominant driver of retreat variability, and this paper shows atmospheric influences on retreat variability that are at least as important as those on advance. See comments below.

P1L19: the simulations only have an amplified SAM in terms of fraction of variability contained; the SAMs in the models could be of accurate absolute magnitude relative to observations??

P3L1: 'divergent' implied ice divergence to me

P3: There is a GRL paper in press by Kwok et al. "Linked trends in the South Pacific sea ice edge and Southern Oscillation Index" that suggests a link between SOI and the winter ice edge in the south Pacific.

P4L5: and other places: What happened to September?

P4L20: When this sentence says total ice area, it sounds like the definition of ice area (the area integral of ice concentration), not ice extent (the total area of ocean with ice concentration 15% or above). Which do the authors mean?

Section 3: I found this section very hard to follow. When I read section 4 and saw the plots, a lot of the details became clear, but only then, and I spent a lot of time trying to ingest section 3 before I moved on. For example, it was frequently unclear whether

time series were being detrended for each grid cell or for some sort of sector-wide timeseries, or whether a correlation was between a sector timeseries and a map of timeseries or another sector timeseries, etc. My suggested solution would be to only present the very basics of what data are being used in the methods section, and then to more fully explain the method underlying each figure in the results section 4.

P5L10: significance

P5L26: Why a square root cosine weighting on a grid with uniform latitude spacing?

P6L13: The EOFs from the different ensemble members are averaged together to be correlated with SIE. Which SIE? I would have thought that each ensemble member would have its own EOFs and its own SIE, so they can be directly correlated for each ensemble member?

P7L5: Is the difference in ASL-advance and SAM-retreat due to the position of the ice edge, further north at the start of retreat than it is at the start of advance?

P7L12 and others: The wording needs to be very precise. I think the finding is that the ASL is the dominant driver of *interannual variability* in sea ice advance in the A/B seas, not that it is the driver of ice advance per se. Please check this throughout the paper.

P7L21: see above! The ice in this region is definitely subjected to large-scale atmos influence, though I agree that it appears that its interannual variability is not. . .

P7L23: I do not agree that the patterns are similar.

P7L26: and SAM?

P8L11: I do not see SAM-ice interactions for Hakon.

P8L12: I do not see the PSA pattern in either sector during retreat.

Figure 2: Caption mentions lines at r=+/- 0.4 which do not appear. It would be better to

add lines showing rˆ2=+50% and rˆ2=+80%, as referred to in the text. I don't think negative values should be shown with dotted lines, since any negative correlation would be a very bad thing. Can the plot limits be set to +/-1? Can the dots be coloured like in Figure 3 so we can see which models are bad?

P8L17: It might be worth clarifying that a high correlation shows that the regional patterns are similar, but the magnitude of the relationship can still be way off in the model?

P8L25 and others, e.g page 10: I realise it is statistical convention, but the use of the word 'explained' is inappropriate here. This is just showing how well the models match the observations – the models are not explaining anything in this case.

P8L29: I think this should say 'advance' not 'retreat'

P9L16: I don't understand the 'either. . . or. . .' construction of this sentence. Is it supposed to say that there is no relationship between higher pattern correlation and veracity of model trends? Can this claim be made quantitative?

P9L22: Is the implication that the model SLP trends must be wrong? Or perhaps the model SIE and SLP patterns are spatially correlated well, but with the wrong magnitude in the correlation?

P9L30: Taking the ensemble mean EOFs does indeed reveal the forced climate response – but doesn't this complicate the comparison with ERA-Interim? The real climate is a single ensemble member, not an ensemble mean, so shouldn't ERA-interim should be compared to the population of ensemble members, not its mean?

P9L32: Similarly to the above relations between SLP and SIE, pattern correlations will reveal whether the models have a relatively strong SAM relative to the model PSA, for example, but will not detect if that SAM variability is far too weak or strong relative to the real observed SAM variability. I think this should be mentioned explicitly.

Figure 4: EOF1 explains exactly 36% of the variance in (a)?

Figure 5: I wondered if there is a concrete rationale for these being quarter-circle Taylor plots rather than just two-axis square plots like in figures 2 and 3?

P10L20: The different ensemble members' PSAs show different pattern correlations to the ERA-Interim PSA. Could this be a real result, in the sense that not just the variability but also the different modes of variability can differ between ensemble members as a result of internal variability? If so, does it make sense to judge the models too harshly against the observed PSA pattern, since that is after all just one ensemble member? If not, how does this happen in the models and not in reality?

Figure6: Could reduce the y-limits from +/- 1.2?

P11L5: I did not fully understand the argument in this paragraph. The observed relationships in Figure 6 all fit within the envelope defined by the simulations, so my default interpretation of the plot is that reality is indeed one member of the ensemble defined by CMIP5. I think the argument is that there are good physical reasons why the (single-member) observed relationships have the spatial distribution that they do (?), and this is independent of internal variability (?), so we should expect most of the simulated relationships to follow this spatial distribution (?), or perhaps at least the multi-model mean relationship should follow it (?). Also, the figure shows the envelope and mean from the simulations, but not the standard deviation, which I think is what we need to assess whether the models are wrong.

P11L8: This paragraph seemed very unclear to me and I think needs rewriting and breaking into two paragraphs. 1) The first half of the paragraph says that the models have accurate SLP-SIE relationships during advance but do not capture the observed trends during advance, but this is not explored further until a few comments at the end of the paragraph. It seems to me that this paradox could be due to either the magnitude of the SLP-SIE relation being wrong in the models (it is only a pattern correlation that is good) or the model SLP trends being wrong. The latter would be unsurprising given the poor state of the model SLP EOFs 2&3. 2) The second half of the para-

graph appears to argue that in the real world the importance of atmospheric variability is diminished during retreat, but it is not (figure 1). It is the veracity of the models in reproducing atmospheric-driven ice variations that is diminished during retreat (figure 2). This could be due to model errors in any of the mechanisms mentioned, but the paragraph seems to be suggesting that the mechanisms per se reduce the effect of atmospheric variability, which is not the case. In any case, only the atmosphere-induced fraction of the variability is under consideration in this paper, not the entire variability. It may be the case that ice-climate feedbacks have an important role here. During retreat, any variability in ice cover due to winds will be amplified by melting feedbacks (e.g. albedo causes low ice to melt faster, causing lower ice). I would speculate that it is hard for models to accurately represent such feedbacks, and as a result their SLP-SIE relationships are less reliable during retreat than advance.

P12L2: I think the models underestimate the role of PSA (figure 5) in atmospheric variability?? And I am not convinced about the modelled role of PSA (figure 6).

P12L13: This sentence is worded in a very complex way and would probably be better placed in the paragraph discussed above in comment P11L8.

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