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Discussions

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

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The authors would like to thank the two anonymous reviewers who have made thoughtful and insightful comments on this paper. Below, we provide a comment-by-comment response to each reviewer.

Reviewer 1

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.

Response – The sentence in the abstract has been revised to remove the reference to

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the ocean being a dominant driver during retreat.

From: Atmospheric influence on sea ice is known to be strongest during its advance, with the ocean emerging as a dominant driver of sea ice retreat; therefore, while it appears that models are able to capture the dominance of the atmosphere during advance, simulations of ocean-atmosphere-sea ice interactions during retreat require further investigation.

To: Atmospheric influence on sea ice is known to be strongest during its advance, and it appears that models are able to capture the dominance of the atmosphere during advance. Simulations of ocean-atmosphere-sea ice interactions during retreat, however, require further investigation.

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??

Response – The word ‘amplified’ has been removed to avoid confusion; the absolute magnitude of the models relative to the observations is now discussed in the results section.

P3L1: ‘divergent’ implied ice divergence to me

Response – ‘divergent’ has been replaced with ‘contrasting’.

From: The divergent sea ice trends of the Amundsen/Bellingshausen and Ross Seas are associated with the deepening of the ASL in recent decades (Turner et al., 2013b).

To: The contrasting sea ice trends of the Amundsen/Bellingshausen and Ross Seas are associated with the deepening of the ASL in recent decades (Turner et al., 2013b).

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.

Response – reference has been included in the paper:

The high-latitude atmospheric response to ENSO is linked to sea ice anomalies in the Amundsen, Bellingshausen, Ross and Weddell Seas (Karoly, 1989; Harangozo, 2000; Kwok and Comiso, 2002; Yuan, 2004; Stammerjohn et al., 2008; Bernades Pezza et al., 2012), with recent work indicating that trends in the south Pacific ice edge during winter can be explained by changes to ice drift and surface winds resulting from a positive trend in the Southern Oscillation Index (Kwok et al., 2016).

P4L5: and other places: What happened to September?

Response – According to the calculations of Raphael & Hobbs (2014), sea ice in the different sectors around Antarctic stops advancing during August, instead maintaining the winter maximum throughout September before beginning its retreat in October. The only exception was the King Hakon VII sector which reached its maximum later than the others and began its retreat one month later; however, to compare like with like, we used the majority advance period for all sectors in this study. None of the sectors had an extended minimum, which is why the end of retreat and start of advance do not have a gap.

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?

Response – We mean sea ice extent here; as stated in the manuscript, we use the 15% sea ice concentration isoline. "Area" has been replaced in the text by "cover" to avoid confusion.

From: From the regridded data, sea ice extent (SIE) was calculated from the total ice area for each degree of longitude, bounded by the coast, and the 15% sea-ice concentration isoline.

To: From the regridded data, sea ice extent (SIE) was calculated from the total sea ice cover for each degree of longitude, bounded by the coast, and the 15% sea ice concentration isoline.

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.

Response – The method section has been substantially revised to more clearly explain the steps taken for each part of the analysis in order to reduce confusion.

P5L10: significance

Response – This has been updated in the manuscript.

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

Response – As stated in the manuscript, cosine weighting is used to account for the change of longitude distance with latitude. The cosine weighting is essentially an areal weighting, thus each grid cell has equal influence in the EOF analysis.

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?

Response – This is an error in the manuscript. The individual model plots in Figure S2 should show the individual ensemble member EOFS. The Taylor diagram does actually show the individual ensemble member EOF against the same ensemble member SIE, not the model average as written. The text has been changed to reflect this, and Figure

S2 has also been updated.

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?

Response – This is an interesting idea; probably only answerable by looking at extensively at patterns of sea ice concentration rather than extent. This is beyond the scope of this paper, but is worthy of further analysis.

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.

Response – The wording in the paragraph has been changed to reflect this, and it has been checked throughout the paper.

From: This indicates that the ASL is the dominant large-scale atmospheric driver for the Amundsen/Bellingshausen sector during the period of ice growth...

To: This indicates that the ASL is the dominant large-scale driver of interannual sea ice variability for the Amundsen/Bellingshausen sector during the period of ice advance...

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. . .

Response – The wording in the paragraph has been altered.

From: Rather, sea ice in this region during ice advance is more likely driven by alternative factors such as synoptic-scale weather systems, intrinsic variability, or the ocean. During retreat, the positive correlation pattern in the Weddell sector is similar to the pattern in the Ross/Amundsen sector during the same season, exhibiting an ASL component but not the zonally symmetric SAM component (Figure 1f).

To: Rather, the variability of sea ice in this region during ice advance is more likely driven by alternative factors such as synoptic-scale weather systems, intrinsic variability, or the ocean. During retreat, the positive correlation pattern between sea ice variability in the Weddell sector with atmospheric variability over the Amundsen and Bellingshausen Seas indicates the influence of the ASL (Figure 1f).

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

Response – The sentence has been revised to more accurately described the correlation pattern in the Weddell Sector.

From: During retreat, the positive correlation pattern in the Weddell sector is similar to the pattern in the Ross/Amundsen sector during the same season, exhibiting an ASL component but not the zonally symmetric SAM component (Figure 1f).

To: During retreat, the positive correlation pattern between sea ice variability in the Weddell sector with atmospheric variability over the Amundsen and Bellingshausen Seas indicates the influence of the ASL (Figure 1f). The inverse sign of the correlations compared with ASL influence in the Ross/Amundsen sector during the same season indicates that as the atmospheric circulation pattern deepens, sea ice extent in the Weddell Sea decreases.

P7L26: and SAM?

Response – The reference to SAM has been deleted.

From: This reflects the implied circulation of the ASL and SAM in this region, where stronger southerly winds over the Ross Sea result in the northward transport and reduced melt of sea ice in this region and stronger northerlies over the north of the Antarctic Peninsula confining ice in the Weddell Sea and increasing melt (Liu et al., 2004).

To: This reflects the implied circulation of the ASL in this region, where stronger southerly winds over the Ross Sea result in the northward transport and reduced melt of sea ice in this region and stronger northerlies over the north of the Antarctic Peninsula confining ice in the Weddell Sea and increasing melt (Liu et al., 2004).

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

Response – The pattern reflects the non-annular component of the SAM, which is more commonly discussed as the ASL. The text has been updated to reflect this.

From: The King Hakon VII sector during advance (Figure 1g) shows a pattern that is weakly reminiscent of that observed in previous studies which have linked sea ice in this sector to the SAM (Turner et al., 2015a). However, during retreat the SAM-like pattern disappears (Figure 1h), indicating that the region becomes more sensitive to other factors such as weather and a small ENSO forcing as suggested by Matear et al. (2015).

To: The King Hakon VII sector during advance (Figure 1g) shows negative correlations over the Amundsen and Bellingshausen Seas, indicating the influence of the ASL on sea ice variability in this sector. However, during retreat the pattern disappears, with no large-scale atmospheric influence on sea ice variability visible (Figure 1h). This suggests that variability in retreating sea ice in this region is more sensitive to other factors such as weather and a small ENSO forcing as suggested by Matear et al. (2015).

From: In summary, large-scale atmospheric circulation patterns do not appear to be a dominant force in all sectors and seasons. The ASL is the dominant force in the Ross/Amundsen and Amundsen/Bellingshausen sectors during advance and the Weddell sector during retreat, while SAM-sea ice interactions occur in the King Hakon VII sector during advance and in the Ross/Amundsen sector during ice retreat. The PSA pattern occurs in the Amundsen/Bellingshausen sector and to a smaller extent in King Hakon VII during retreat.

In summary, large-scale atmospheric circulation patterns do not appear to be a dominant driver of sea ice variability in all sectors and seasons. The ASL is the dominant force in the Ross/Amundsen, Amundsen/Bellingshausen and King Hakon VII sectors during advance and the Weddell sector during retreat. SAM-sea ice interactions occur in the Ross/Amundsen sector during ice retreat.

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

Response – We've removed this sentence and now only refer to the ASL and SAM in this summary.

From: ... The ASL is the dominant force in the Ross/Amundsen and Amundsen/Bellingshausen sectors during advance and the Weddell sector during retreat, while SAM-sea ice interactions occur in the King Hakon VII sector during advance and in the Ross/Amundsen sector during ice retreat. The PSA pattern occurs in the Amundsen/Bellingshausen sector and to a smaller extent in King Hakon VII during retreat.

To: ... The ASL is the dominant force in the Ross/Amundsen, Amundsen/Bellingshausen and King Hakon VII sectors during advance and the Weddell sector during retreat. SAM-sea ice interactions occur in the Ross/Amundsen sector during ice retreat.

Figure 2: Caption mentions lines at r=+/- 0.4 which do not appear. It would be better to add lines showing rËĘ2=+50% and rËĘ2=+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?

Response – Lines have been revised to show r = 0.7 and 0.8. Plot limits have been changed to +/-1.0. Dots have been coloured using the same colour scheme as in Figure 3.

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

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model?

Response – following sentence has been added to to this paragraph for clarification.

‘These comparisons only measure the extent to which the observed spatial pattern was replicated in the models, not whether the magnitude of the interactions in the models is similar to that of the observations.’

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.

Response – In this case, the ‘explained’ refers to the amount of variance in the data to which each pattern corresponds. The comparison between the models and the observation is only to show the difference between how much variance in one is ‘explained’ by a particular pattern compared to how much is ‘explained’ by another. However, this has been replaced in the text by the term ‘accounted for’ to try to avoid confusion.

P8L29: I think this should say ‘advance’ not ‘retreat’

Response – Correct; this has now been updated in the text.

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?

Response – The text has been updated to remove the ambiguity. The intention of the sentence was to explain that having a better representation of atmosphere-ice interactions does not necessarily mean the same model will also produce a sea ice trend closer to observed trends.

From: There does not appear to be a strong relationship between either higher pattern correlation values (indicating close agreement between the model correlation maps and that of the reanalysis), or the proximity of model SIE trends to observed SIE trends in each sector and season.

To: There does not appear to be a strong relationship between higher pattern correlation values (indicating close agreement between the model correlation maps and that of the reanalysis) and the proximity of model SIE trends to observed SIE trends in each sector and season.

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?

Response – The intention of this sentence is merely to point out that if a model produces a reasonable representation of interannual variability in the relationship between SIE and SLP, it doesn't necessarily also produce a reasonable sea ice trend. The sentence has been replaced for clarification.

From: This suggests that the ability of the models to simulate correlations between SIE and SLP that reflect observed correlation patterns does not necessarily mean that models also produce SIE trends that reflect observed SIE trends.

To: These results suggest that a model with an interannual sea ice-atmosphere interaction pattern that closely represents the observed pattern will not necessarily also produce realistic sea ice trends.

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?

Response – This paragraph has been updated to more clearly state that the individual members are indeed used, not the ensemble mean.

From: The EOF analysis was then conducted on the historical ensembles of each model, revealing the forced climate response of the models.

To: The EOF analysis was then conducted on the individual ensemble members of each model, revealing the forced climate response of each model member.

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.

Response – We have examined the absolute variance of the principal component corresponding to each EOF (Figure 6). This shows that the model SAM and PSA variability is far too weak compared to observed variability. This has been added to the text.

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

Response – We have rounded the variance-explained to 2 significant figures. Since this is a gross empirical metric, we believe that this is a suitable level of precision to report.

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?

Response – Originally, two-axis plots were used to show both these metrics; however, it was not easy to see the spread among the model ensemble members in both directions using this type of plot. The authors decided instead to use a Taylor diagram, which is a clearer method of comparing the outcome of multiple ensemble members at once using the two different metrics.

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?

Response – The differences between the spatial representation of the PSA modes in the difference ensemble members suggest that they may change upon multidecadal timescales. We have updated the text here to include this caveat.

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

Response – Limits have been reduced to +/- 1.0.

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 (singlemember) 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.

Response – We have added lines depicting the 1.96 standard deviation (equivalent to the 95% confidence interval for a Normally distributed ensemble for infinite degrees of freedom.

Note that given: a) the strength of the observed correlation pattern (which is large compared to the standard deviation), b) its acknowledged importance in the literature, and c) the length of correlation period (approximately 3 decades), it is highly unlikely that the differences between ensemble members could be explained by internal variability alone.

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 paragraph 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.

Response – The paragraph has been broken into two sections as suggested, with the advance sea ice-atmosphere interactions discussed first and then retreat separately to avoid confusion. It is true that complex ice-ocean feedbacks are probably difficult for models to represent; however, those ice-ocean feedbacks are equally complex (if not more so than) during advance (for example, the entrainment of sub-mixed layer into the mixed layer from brine rejection). Although incredibly important, those feedbacks don't therefore explain why advance should necessarily be better represented than retreat in the models.

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).

Response – The sentence has been updated to reflect the underestimation of the PSA.

From: However, during retreat, historical simulations overestimated the relative importance of the SAM and PSA in terms of atmospheric variability as well as the relative influence of these modes on SIE.

To: However, during both sea ice advance and retreat, the majority of historical simulations overestimated the relative importance of the SAM and underestimated that of the PSA.

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

Response – This sentence has been reworded to reduce its complexity.

Reviewer 2 – Variability

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.

Response – Through addressing the comments of Reviewer 1, the text throughout the results has been revised to more specifically discuss interannual variability, and this hopefully reduces the confusion in the rest of the paper. The revised results and discussion are quite clear that the paper is discussing variability rather than trends.

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.

Response – The sentence in the abstract has been revised to remove the reference to the ocean being a dominant driver during retreat.

From: Atmospheric influence on sea ice is known to be strongest during its advance, with the ocean emerging as a dominant driver of sea ice retreat; therefore, while it appears that models are able to capture the dominance of the atmosphere during advance, simulations of ocean-atmosphere-sea ice interactions during retreat require further investigation.

To: Atmospheric influence on sea ice is known to be strongest during its advance, and it appears that models are able to capture the dominance of the atmosphere during advance. Simulations of ocean-atmosphere-sea ice interactions during retreat, however, require further investigation.

p.4, l.5: Is there a reason why September is not considered?

Response – According to the calculations of Raphael & Hobbs (2014), sea ice in the different sectors around Antarctic stops advancing during August, instead maintaining the winter maximum throughout September before beginning its retreat in October. The only exception was the King Hakon VII sector which reached its maximum later than the others and began its retreat one month later; however, to compare like with like, we used the majority advance period for all sectors in this study. None of the sectors had an extended minimum, which is why the end of retreat and start of advance do not have a gap.

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.

Response – The method section has been updated to clearly state that CMIP5 monthly historical data is used. Cross-correlations between ERA-Interim reanalysis SLP and NSIDC sea ice extent have been run also for the period January 1979-December 2005, and have yielded largely the same results (now attached as Figure S1). There is no substantial difference between the shorter and longer timeseries in the observations. Therefore, we decided not to lengthen the historical ensembles, given the significant extra work required to concatenate RCP4.5 onto 73 historical ensembles.

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.

Response – We use the same linear detrending method for all datasets, including the reanalysis dataset, as it has been used widely both for ERA-Interim and other datasets, for example: âĂć Bracegirdle, T. J.: Climatology and recent increase of westerly winds over the Amundsen Sea derived from six reanalyses, International Journal of Climatology, 33, 843-851, 2013. âĂć Bromwich, D. H., Nicolas, J. P., Monaghan, A. J., Lazzara, M. A., Keller, L. M., Weidner, G. A., and Wilson, A. B.: Central West Antarctica among the most rapidly warming regions on Earth, Nature Geoscience, 6, 139-145, 2013.

p.5, ll.10-11: Related to 4) I wonder whether monthly data is sufficient to detect autocorrelation in the SLP and SIE data.

Response – We're only interested in autocorrelation in this case insofar as it affects statistical significance tests. As we are using monthly data in the study, it is only appropriate to consider the autocorrelation at monthly timescales.

The authors mention the similarity of their approach to that of Raphael and Hobbs (2014) in the method section and the similarity of theirs 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.

Response – We have updated the text to clearly refer to the Raphael and Hobbs (2014) study in the Introductory section, Method section and Discussion so as to most clearly differentiate between the predecessor study and this one.

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.

Response – This is an error in the manuscript. The individual model plots in Figure S2 should show the individual ensemble member EOFS. The Taylor diagram does actually show the individual ensemble member EOF against the same ensemble member SIE, not the model average as written. The text has been changed to reflect this, and Figure S2 has also been updated.

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).

Response – The text has been updated to include the weakening of the pattern.

From: During the retreat season, the correlation pattern remains in a similar area but contracts northwards and towards the Ross Sea (Figure 1d).

To: During the retreat season, the correlation pattern remains in a similar area but weakens, contracting northwards and towards the Ross Sea (Figure 1d).

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.

Response – As described in the text, the correlation pattern for Figure 1f shows the non-annular component of the SAM, the ASL, but not the annular component of the ASL. The ASL pattern is inverse to that of the non-annular component of the SAM pattern in the Ross/Amundsen sector during the same sector. Physically, this is well known, as the implied circulation of the ASL results in increased southerly winds over the Ross Sea and northerly winds over the Antarctic Peninsula and Weddell Sea (e.g. Turner et al. 2015).

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

Response – A separate paragraph has been inserted here for East Antarctica.

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.

Response – We have double checked this, and altered the Figure to make the results clearer as a result of comments from Reviewer 1. These numbers are indeed correct; in order for the model to obtain an r2 score of 50%, it needs to have a pattern correlation of 0.7 or higher, not 0.5 (which would only obtain an r2 score of 25%). Dotted lines at 0.7 and 0.8 have been added at the urging of Reviewer 1 in order to make this clearer, and we hope this assists with the comments of Reviewer 2 as well.

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“.

Response – The text has been updated here to avoid confusion.

From: Correlation values close to 1 indicate good representation of the spatial pattern of the observation-based atmospheric mode in the models, while values near 0 indicate little resemblance between them. A second metric was created by dividing the amount of atmospheric variance explained by the model EOF by the amount of variance explained by the observation-based pattern, creating a ratio of the percentage of variance explained.

To: The results are explained using two metrics. The first metric, correlation values, is used to indicate the strength of the simulated representation of the spatial pattern seen in the reanalysis. A correlation value close to 1 indicates good representation of the pattern, while values near 0 indicate little resemblance between the two. A second metric was created by dividing the amount of atmospheric variance explained by the model EOF by the amount of variance explained by the observation-based pattern, creating a ratio of the percentage of variance explained.

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 ..."

Response – The text has been updated as suggested.

From: The advance season is when the CMIP5 models sea ice trends diverge most significantly from observed trends, particularly in the Ross/Amundsen and Amundsen/Bellingshausen sectors where the highest-magnitude change is also observed (Hobbs et al., 2015; Hobbs et al., 2016).

To: In the advance season, the modelled sea ice trends diverge most significantly from observed trends, particularly in the Ross/Amundsen and Amundsen/Bellingshausen sectors where the highest-magnitude change is also observed (Hobbs et al., 2015; Hobbs et al., 2016).

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.

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To me, this leaves the impression the results of this paper are not important which is not true.

Response – The conclusion section has been reorganised and rewritten in parts to more clearly state the conclusions and their importance in context of other academic literature.

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

Response – This has been updated as suggested.

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

Response – Figure 2 has been updated to show dotted lines at 0.7 (r2=50%) and 0.9 (r2=80%) for ease of interpretation. At the advice of Reviewer 1, the dotted lines have not been extended to negative correlations.

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.

Response – The colour scheme here has been updated to match the other coloured plots.

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“

Response – All these technical comments have been implemented in the text.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-200/tc-2016-200-AC1-supplement.pdf

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

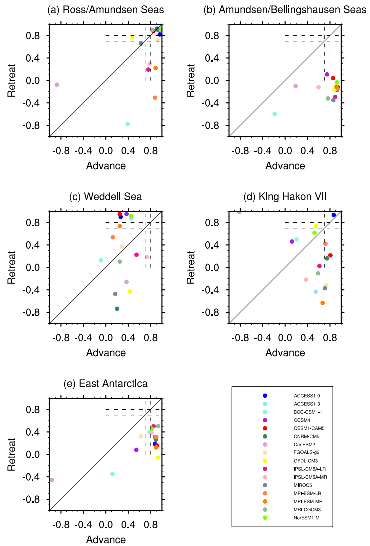

Fig. 1. Changes to Figure 2 (adding lines, changing axes and adding coloured markers)

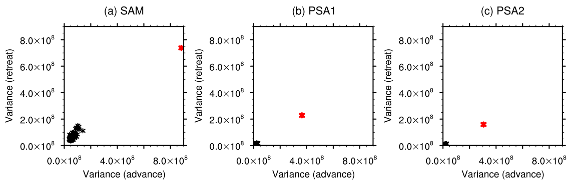

Fig. 2. New figure (Figure 6) - comparison of absolute variance of EOFs between ensemble members and ERA-Interim

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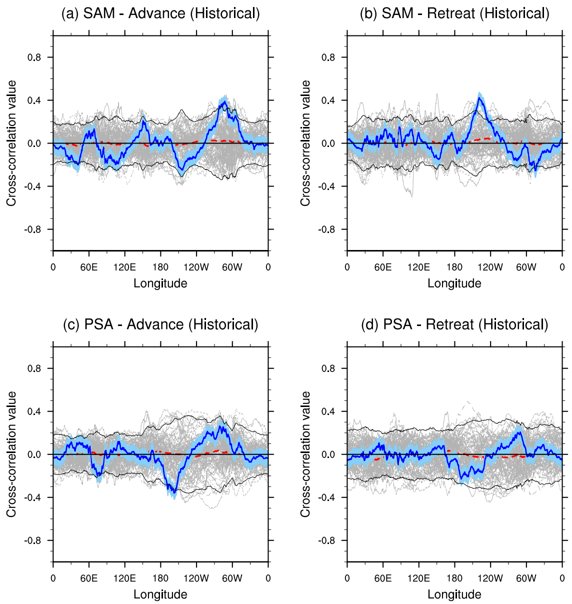

Fig. 3. Changes to Figure 6 (now Figure 7), including the standard deviation lines

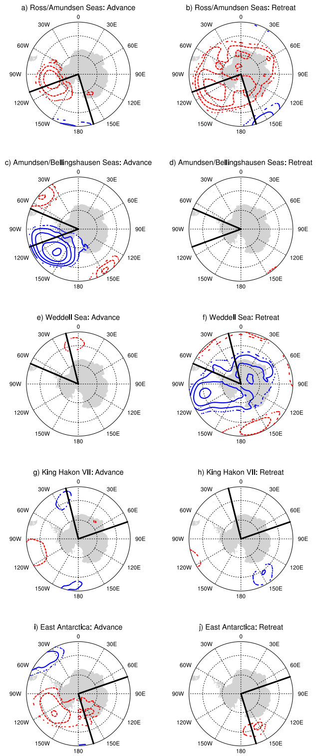

Fig. 4. New Figure S1 - cross-correlation of ERA-Interim SLP and NSIDC SIE 1979-2005