The Cryosphere Discussions

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Reply on RC1

Serena Schroeter et al.

Author comment on "Antarctic sea ice regime shift associated with decreasing zonal symmetry in the Southern Annular Mode" by Serena Schroeter et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-151-AC1, 2022

Reviewer Comments #1:

The manuscript presents an interesting analysis of recent changes in Antarctic sea ice, illustrating well the non-stationary behavior of the sea ice variability patterns and the difference in trends if the 42 years of the satellite period is split in different parts. Many analyzes have been devoted to the satellite records of sea ice concentration but by decomposing the series in different regimes, the authors are able to present an original and useful point of view. They discuss intriguing changes between the first and the second half of the period and they propose a discussion of the possible origin of theses changes. They do not answer all the questions raised by their diagnostics but the interpretation is interesting and will certainly stimulate more work on the subject. I thus recommend the publication of this manuscript in The Cryosphere. Some suggestions for a revised version are presented below that, to my point of view, would strengthen the impact of the results presented.

I have first three general points

1/ If I understand well, the main point in the discussion is the modification of the link between SAM and the sea ice concentration (Figure 6), associated with the reinforcement in the recent year of the non-zonal component of SAM (Figure 5). What I am missing at this stage is a clear connection with the total changes in ice extent (Figures 1 and 2 for instance). In other words, the manuscript demonstrates a decreased zonal symmetry in SAM, as mentioned in the title. It suggests qualitatively how this impacts the sea ice concentration as the patterns seems to fit with generally an enhanced north-south flow where sea ice extent decrease/increase. However, the fit is not always good and the authors do not quantify how much of the observed changes in the ice extent can actually be attributed to the shift in SAM and this weakens their conclusion. Additional diagnostics or analysis are thus required for me to quantify the proposed links and the impact of the decreasing zonal symmetry of SAM on Antarctic sea ice.

It is true that the fit is not always good – we state within the text that several regions in which SAM asymmetry increases do not show corresponding sea ice anomalies (Haakon VII, Dumont D'Urville etc). In addition, only the coastal, western part of the Weddell Sea shows an agreement with the changes in SAM, due to its position in between the cyclonic/anticyclonic patterns. We will reword this section to more clearly state that the agreement between the patterns of sea

ice trends and asymmetry of SAM is only important in regions where SAM is already known to be important – i.e. Ross, Amundsen, Bellingshausen, Weddell seas – and that the other sea ice regions (which have little to no clear signal among the running trends) continue to be dominated by cyclonic activity or other ocean/atmosphere drivers.

We (the authors) understand that perhaps the first part of the results section and the final discussion as well need to make clearer that it is not our intention to detect and attribute regional changes as a proportion of total sea ice change. In showing Figure 1 and 2, our main intention is to highlight the importance of a regional perspective in understanding how Antarctic sea ice variability and trends have changed over time, as it is very common for studies to use circumpolar total metrics or sector averages which may mask that trends or patterns of variability can (as seen in Figure 3) shift spatially as well as temporally.

2/ The origin of the shift in SAM is not discussed. I understand that it is not the subject of the paper. However, the reader would be interested to know if this could be due to multi-decadal variability in the atmospheric circulation, a response to the greenhouse gas forcing, recovery from the ozone hole, or any other mechanism. I would thus suggest to add a paragraph in the final section, at least to present the different hypotheses.

Agreed – we are more than happy to include a paragraph or two as needed to more thoroughly discuss the shift in SAM and its potential drivers, and agree that this would add much-needed context for the discussion.

3/ For several diagnostics, I was not totally sure of the diagnostics that is displayed. As this is key for understanding the paper, I would recommend to add more details on the way they are produced. In particular, for figure 2b, that would be useful to explain exactly how regional variability is computed. For figure 4 and 7, regressions of SIA anomalies and SST are mentioned but I am not sure with what SIA or SST is regressed, maybe with time? Can 'regression' here be considered equivalent to 'trend'? If this is the case, I think that it would be useful to specify it. Figure 6 mentions a gradient of 15yr samples of average EOF1 zonal anomaly. I guess it is the zonal gradient and thus the derivative of the plot of Fig. 5d but I am not totally sure.

Thank you for pointing this out. We agree, and will expand upon the methodology section, figure captions, and within the text as needed to clarify the calculation of each diagnostic and its physical meaning.

I have also a few specific points.

1/ In many places, starting in the abstract, the authors mentioned that the long-term trend is skewed towards the earliest years (line 9; line 83, line 211). Maybe 'skewed' can give the feeling that the estimate of the long-term trend is wrong or biased. I would personally preferred 'dominated by the changes in the earliest year' or something equivalent that is more neutral.

No problem, we can rework these sections to remove any potential negative connotations of wording choice.

2/ Line 113. The eastward shift of the anomalies is interesting for me. The authors interpret the changes in patterns as an increase in the meridional exchanges related to SAM but would it be possible that a part of the signal is due to and eastward shift of the pattern or to the advection of some anomalies by oceanic currents. On this subject, maybe a link with the very recent study of Morioka et al. (2022) would be interesting

Morioka, Y., Iovino, D., Cipollone, A. et al. Decadal Sea Ice Prediction in the West Antarctic Seas with Ocean and Sea Ice Initializations. Commun Earth Environ 3, 189 (2022). https://doi.org/10.1038/s43247-022-00529-z

Thank you for the reference, we will closely examine the aforementioned study and incorporate it into the text both in the section listed and also in the final discussion. It is entirely possible that the signal in this region is partly due to oceanic current anomalies – we do not claim SAM is responsible for the entirety of anomalous sea ice in any region. Indeed, the Morioka et al. (2022) study does state in the conclusions that the weak 6-10yr prediction skills in the Ross Sea could be (at least in part) due to the influence of the SAM in this region on decadal timescales, so fits well with our discussion of what drivers may be shifting these patterns of variability.

3/ Line 244. It is indeed counterintuitive and I do not follow well the argument here. Line 243, it is said that increasing meridional flow over the sea ice zone is driving spatially heterogeneous anomalies. If I understand well the sentence, a higher overall variance of the total sea ice extent would be due to a 'greater agreement across regions of high-magnitude changes'. If this is the case, that would be important to quantify this.

We agree that this is not clearly worded and that this paragraph requires rewording. Figure 2b and c show that, compared with the widely-opposing anomalies in early years of the satellite record, zonal sea ice variability since about the mid-2000s is much more moderate, and variability further drops in the most recent years because the anomalies in previously strongly-opposing regions now mostly agree. While this might indicate a driver that produces a spatially heterogeneous sea ice response, the variability in recent samples is low but still not zero. The trend magnitudes in the most recent samples are quite small compared to the early samples, and the only statistically significant regional trends in the 1991-2020 and 1992-2021 samples are small increasing trends in the Amundsen and Dumont D'Urville seas, not any of the larger decreasing trends in the Ross or Weddell seas. Spatial heterogeneity is still therefore present, but the magnitudes are low in the context of the long-term shift in these trends, and the spatial pattern is almost the opposite of the earliest (1979-2008) pattern. Since the SAM change has not been a sudden change, but rather a gradual shift, the sea ice response would also be gradual – especially since there may be multiple factors affecting sea ice variability on shorter timescales, as you've pointed out above. We (the authors) intend to make it much clearer in the discussion text that the changes in SAM only strongly affect the regions where SAM variability dominates sea ice variability, and that the changes in SAM (from largely annular, where the flow pattern encouraged ice motion outwards from the western Ross Sea and cyclonic thereafter, to more wave-3-like, where the flow pattern favours outward flow from the eastern Ross Sea/western Amundsen Sea and southward flow in the western Ross) have been gradual (and hence so have the sea ice changes).

4/ Line 247. Why is it assumed that the changes that are underway are likely to continue? The response of sea ice to wind changes is usually relatively fast. If it is because it is assumed that the asymmetric flow pattern will continue to intensify, this should be explained in more details here.

Agreed. It is based on whether the asymmetric flow pattern continues or intensifies, and an enhanced discussion of SAM trends, potential drivers and variability (as you've already suggested) should aid this substantially. The response of sea ice to wind changes is indeed quite fast, but as mentioned above, the SAM change is not sudden. We (the authors) will also make it clearer that SAM is not the only – or even most important – driver of change, especially outside of the Ross/Amundsen/Bellingshausen/Weddell, so especially around East Antarctica these wind changes may not provoke much sea ice response at all.

5/ Figure 5. The caption does not seem to correspond to the figures. The period 1979-2001 is mentioned in the figures (panel a) but not in the caption. Is the difference (panel d not c) between (a) and (b) or between (b) and (c) ?

Apologies – somehow this outdated caption was missed during proof-reading. The caption will be corrected. Thank you for bringing this to our attention.