The Cryosphere Discussions

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Reply on RC2

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-AC2, 2022

Comment from Reviewer #2:

Review of 'Antarctic sea ice regime shift associated with decreasing zonal symmetry in the Southern Annular Mode' by Schroeter et al.

Overview: This paper uses smaller temporal subsets of the Southern Hemisphere sea ice observations to demonstrate that there are changes in sea ice variations (anomalies and trends) through time that occur since 1979. Importantly, the trends weaken in time, and the earlier part of the satellite record (with the exception of record low years in 2017 and 2022) contain the majority of low sea ice extent / area anomalies. The study relates these changes to changes in the structure of the Southern Annular Mode by analyzing EOFs of SLP and how these change in time, and using a gradient of these anomalies to connect changes in the meridional winds to sea ice concentration.

The paper is interesting, and provides new insight on sea ice variability and its connections to atmospheric (and to some extent, SST) variability. However, it is sometimes unclear what is exactly being shown in the paper, and (from what I can tell) it often masks important seasonal variations as well as discussing relationships that are not likely statistically significant due to the smaller sample size. I recommend a minor revision to add in these details to improve the paper substantially.

Main comments:

More detail is needed throughout – particularly if the analysis is done on annual means or monthly anomalies, or something else (Fig 3, Fig 5-7). More details are needed on the calculation of the SAM gradient, as well as some demonstration of the relationship with the meridional wind and its changes through time. Important seasonal deviations from the annual mean or annual cycle (the latter for sea ice especially) are needed throughout. (some details are provided in the listed minor comments below)

While the shorter time period helps to understand changes in anomalies or trends, there is essentially nothing done in the manuscript to discuss any statistical significance. I suspect as the time period / sample size decreases, the relationships are not statistically significant. In my view, this needs to be discussed – and changes that are statistically significant need to be emphasized. While I realize the paper is about the large variability – it needs to be made clear that this large variability dampens the ability to detect *meaningful relationships beyond noise, especially at smaller temporal scales (smaller sample sizes).*

Thank you for pointing this out. We agree that inclusion of measures of statistical significance where possible improves the discussion. Our intention is to incorporate a method of calculating linear least-squares regression coefficients in order to account for temporal autocorrelation, by modifying the calculations of residual variance and standard error to use an effective sample size based on lag-1 autocorrelation coefficients (Santer et al. 2000). A couple of sentences on the methodology would of course be included in the methods section. Statistical significance would be calculated for all regression coefficients in all figures, and contour plots also included so as to overlay statistically significant regions with stippling to aid interpretation. The text and captions would also be updated accordingly. We believe this strengthens our analysis: initial attempts indicate that, for example, the statistically significant regionally-opposing trends of SIA occur early in the satellite record, and in 30-year samples from 1988 onwards, the Ross Sea trend becomes statistically insignificant and weakly reverses sign while the Amundsen Sea trend reverses sign and becomes statistically significant in the most recent samples.

Greater detail will also be included for the methodology of diagnostics and analysis, particularly the SAM gradient. We accept that this is unclear at times in the text. We are also happy to include seasonal perspectives, whether in place of or alongside annual perspectives where necessary, or as supplementary material.

Minor comments

L25: A recent paper by Turner et al. (2022) may be worth citing here as well (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022GL098904)

Thank you for supplying this reference, this will be included.

L69. 72: change 'data is' to 'data are'

The wording will be changed as suggested.

Fig 1a – I think you have the SIA and SIE lines labeled incorrectly, SIA should be larger than SIE

Actually, by standard convention, SIE is always larger than SIA, because SIE counts the total grid cell area of any cells containing at least 15% concentration, whereas SIA only counts the ice-covered fraction of any grid cell (see Notz, D., 2014. Sea-ice extent and its trend provide limited metrics of model performance. The Cryosphere 8, 229-243, doi:10.5194/tc-8-229-2014, introduction: paragraph 4).

Fig1b-e, and discussion on lines 79-83: Perhaps it is better to call these 'anomalies' rather *than deviations, since from the word deviation I immediately think of standard deviation (a measure of variance – which is clearly not what you are showing), rather than a difference from the mean*

No problem, we'll change the wording throughout the paper to be "differences" from the mean instead to avoid confusion, only using "deviation" in relation to standard deviation as suggested.

L83: again, from statistical standpoint, suggest changing to: '...the long-term mean of both SIA and SIE is more reflective of conditions during the earliest years of the satellite observations' as you are not showing the statistical measure of skew

No problem, we'll change the wording to be "dominated by" rather than "skewed towards" so as to avoid negative connotations.

Fig. 2 – again, SIA should be greater than SIE (SIE does not include ice with less than 15% concentration), so something is off in the labeling here

See response above; SIE is always larger than SIA.

Fig 2b- It doesn't appear that the trends in sea ice variability are statistically significant, and indeed, if there is any decrease it is because the higher variability at the begging and lower variability at the end of the timeseries – much of the timeseries shows relatively consistent (at least for such a highly-variant sea ice!) year to year variation.

Agreed. We're not arguing that there is a statistically significant trend in variability (which we will explicitly state in the text for clarity). Figure 2b is intended as both an illustration of the generally high variability and, as you say, that it is high at one end and low at the other (Figure 2c). This contrasts with the annual average anomalies of Figure 2a, in which the anomalies are low at one end and high at the other, because of the cancellation effect of the high variability in early years and the agreement between key regions in more recent years.

Fig 3 – are these EOFs based on all monthly anomalies, or for the annual mean? Either way, it masks the important seasonal cycle in sea ice and therefore makes it difficult to interpret. In the same way your paper argues that the climatological (time) mean is biased toward the earlier part, these EOFs represent conditions of sea ice that are only observed for a small portion of the annual cycle. They don't appear to be a robust representation of the dataset. It would be better to look at conditions perhaps at sea ice max / min, or seasonal averages instead.

Thank you for this comment. The EOFs are calculated from monthly anomalies. The methodology section will be detailed further to clarify the calculation of EOFs. We agree that the analysis would benefit from a seasonal perspective here, and would include seasonal average EOFs (perhaps surrounding the minimum and maximum as suggested, or the most robust representation as possible based on monthly EOFs).

Fig 3 – the correlation has shifted perhaps, but again due to the very small sample size there is not a statistically significant shift in the correlation magnitude.

Is this in reference to Figure 3c? The pattern correlation results of each 15-year EOF against the most and least recent EOFs is intended to show the gradual change over time; however, if this is not useful, we can remove this panel in favour of a more detailed seasonal perspective as suggested in the previous comment.

Fig 4 – the challenge in interpreting the changes in these long term trends (again masking the annual cycle) is that a lot of the sea ice is gone in the Bellingshausen and King Hakon seas in recent years – which naturally would decrease the variance and weaken the spatial heterogeneity. I'm not entirely convinced from these figures that it is a sea ice regime shift rather than just a complete (or near complete) removal of much of the summer sea ice in these regions and a lengthened ice-free season. The pattern seems to be preserved in the Ross Sea and Weddell Seas, areas with the most ice (even in recent years) in the austral summer seasons.

Thank you for this comment. We agree that inclusion of seasonal trend patterns here as contour plots with statistical significance stippling overlaid would aid interpretation and add important seasonally compensating information to the discussion. An initial test of this clearly shows gradual versus abrupt shifts in the rolling trend patterns, and also shows a somewhat wave-3-like pattern in trend contours in recent samples during JJA and SON. The removal of summer ice/lengthened ice-free season is an important point, and one we would explore further by examining seasonal or monthly anomalies and trends.

Fig 5 – is this for annual means? If so, you are masking the role of ozone depletion that has a strong seasonal footprint on the SLP anomalies (which aren't really reliable over Antarctica, but I suspect you'd have a similar depiction in surface pressure anomalies) – this should be noted

No, we do not calculate the EOFs from annual means, the SLP EOFs are calculated on detrended, cosine-weighted monthly anomalies from 15-year running monthly means. This will be given a more detailed description in the method section for clarity.

Fig 5 – the caption needs redone as it does not describe the paneling in the actual figure – there's no 5e in the caption (it is incorrectly described as 5d)

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

L157 – the SAM structure may be more zonally asymmetric in the annual mean, but it is more symmetric in the summer according to Fogt et al. (2012) – likely due to the role of ozone depletion mentioned above

We agree that this section would benefit from a seasonal perspective on changes to SAM symmetry, and would seek to add this to the existing figures and discussion.

Fig 5e – can you confirm that the zonal anomalies sum up to zero for each 15 year period? The integration almost looks negative for the later part (red lines), but it could be my eyes tricking me.

Confirmed – every sample sums to zero across the longitudes.

Fig 6 – how is this gradient calculated exactly and it is smoothed in some way spatially?

We agree that this needs to be more clearly stated in the methods section. EOFs are calculated from 15-year samples of detrended anomalies that have been weighted by the cosine of latitude (to compensate for meridional convergence near the poles). In all samples, the symmetric SAM component is then calculated as the zonal mean of EOF1, while the asymmetric SAM component is calculated as the zonal anomalies which are then averaged over the latitudes -70°S:-55°S (encompassing much of the sea ice zone through to 55°S as the region of maximum amplitude for ZW3). The gradient is then calculated simply as the difference between the most and least recent patterns, divided by the number of samples. We will consider whether this is the most appropriate metric to show this; a regression coefficient of the 15-year ASAM samples shows a similar pattern but offset (for example with a trough in the eastern Ross rather than slightly to the west, and a peak over the Bellingshausen instead of the Amundsen), which more clearly captures the edges of the intensified wave pattern.

Fig 6, lines 170-176: Is there are way to show these relationships differently, and to demonstrate some of level of statistical significance? Correlations of SIE anomalies with meridional winds or something similar? You talk about things being in agreement (with meridional winds in particular), but none of this is directly shown other than the difficult to interpret connections with the SAM gradient.

Thank you for this point. We will consider whether meridional wind trends can be used as well in order to show statistical significance, and as stated above will also consider whether the SAM gradient alone is an appropriate metric to show this change.

L220-229: Worth adding into the discussion here the recent paper by Fogt et al (2022) who also discuss a regime shift in Antarctic sea ice in the 20th century – consistent with the lower SIE in the mid 20th century near the start of the satellite observations (https://www.nature.com/articles/s41558-021-01254-9)

Thank you for supplying this reference, we will include this as suggested.