Response to RC1:

This research paper explores the global mechanisms that lead to variations in landfast ice around the Antarctic continent. This is an interesting submission and one which could make a strong contribution, but one which also needs revision. I outline below how such a revision should be undertaken.

We sincerely thank you for your careful reading and insightful comments.

As a broad comment, the focus of the investigation is on the variability breakdown via EOF analysis. As some background at an early stage in the paper, it would be very valuable to preset time series of the TOTAL fast ice area for the four seasons and their trends.

Thank you for your suggestion. I have added the time series plot of the total fast ice extent anomalies for all four seasons (Figure 1), along with a description of this figure.

Lines 43-46: In these opening lines in connection with the importance of the study it would be beneficial to reference the very recent studies of ⋯

Luke G. Bennetts and Nathan J. Teder, 2025: Fast ice: The last line of defence for weakened Antarctic ice shelves. Nature Geoscience, 18, 574-575, and

Nathan J. Teder, Luke G. Bennetts, Phillip A. Reid, Robert A. Massom, Jordan P. A. Pitt, Theodore A. Scambos and Alexander D. Fraser, 2025: Large-scale ice-shelf calving events follow prolonged amplifications in flexure. Nat. Geosci., 18, 599-606.

Regarding your suggestion to include recent recommended references on lines 43-46, I have carefully studied them and incorporated relevant citations into the revised version.

Line 50: The year of publication is 2023, not 2024.

The incorrect publication year on line 50 has been corrected in the revised version.

Line 51: 'shrink' (sp.)

The spelling mistake on line 51 has been corrected in the revised version.

Line 53: The text here cites 'Crocker & Wadhams, 1989a' (and also below). However, no related '1989b' is cited or in References. From the context here I suspect the authors may have wanted to also cite the paper ⋯

Crocker GB, Wadhams P (1989) Modelling Antarctic fast-ice growth. J. Glaciol. 35: 3-8 doi: 10.3189/002214389793701590.

If this is correct please to make appropriate changes.

Regarding line 53, it was my oversight to include an extra "a" after the year. I had not intended to cite a reference "1989b." This has been corrected in the revised manuscript.

Line 56: Paper is missing from References. Maybe you are referring to Shuki Ushio's

2006 paper 'Factors affecting fast-ice break-up frequency in Lützow-Holm Bay,

Antarctica.' Annals of Glaciology, 44, 177-182, doi: 10.3189/172756406781811835.

I have added the missing references to the reference list in the revised manuscript.

Lines 72-74: On this important point, include references to ….

Li et al. 2021: Trends and variability in polar sea ice, global atmospheric circulations, and baroclinicity. Ann. NY Acad. Sci., 1504, 167-186,

Pezza, A.B. et al., 2012: Climate links and recent extremes in Antarctic sea ice, high-latitude cyclones, Southern Annular Mode and ENSO. Climate Dyn., 38, 57-73, doi: 10.1007/s00382-011-1044-y,

Irving, D. & co-authors 2016. 'A new method for identifying the Pacific-South

American pattern and its influence on regional climate variability', J. Climate 29, 6109–

6125.

Regarding your suggestion to include the recommended readings on lines 72-74, I have carefully studied them and incorporated relevant citations into the revised content (Simmonds and Li, 2021; Pezza et al., 2012; Irwing and Simmonds, 2016).

Line 74: Another paper missing from the References! I imagine that 'Clem et al., 2017'

is referring to

Kyle R. Clem, James A. Renwick and James McGregor, 2017: Large-scale forcing of the Amundsen Sea low and its influence on sea ice and West Antarctic temperature. Journal of Climate, 30, 8405-8424, doi: 10.1175/JCLI-D-16-0891.1.

For the missing references mentioned on lines 74, I have added them to the reference list in the revised manuscript.

Lines 121-124: Strongly suggest discussing the EOFs first, and then the PCs. The EOFs are the more fundamental aspect. Similarly, flip the two columns in Figs. 1 & 2.

It is important to note that the sign of EOFs is arbitrary. If the sign is changed so does

the sign of the PC and, e.g., 'positive trends' become 'negative trends'. Broadly

speaking the four EOF1s have similar structures, but also large differences in the key regions around 180 deg and from -90 deg to -10 deg. I presume the sign of the patterns

was taken because they 'look similar', but it would be of interest to calculate the spatial cross-correlations of the four patterns. This would maybe be a path to make a more physically meaning statement about the relative changes.

Regarding the response to lines 121-124: We are very happy to accept your suggestion to flip the two columns in the figure. Concurrently, accepting another reviewer's suggestion, we have numbered the subplots to make the narrative clearer. Through your advice and further reading, I have gained a deeper understanding of the physical meaning of the EOF decomposition. Consequently, in the revised manuscript, I have made significant revisions to the explanation of EOF1 and PC1. For example, I added statements such as "the leading EOF (EOF1) represents the spatial pattern corresponding to PC1. It shows how much and in which direction the variable changes at each location when PC1 increases by one standard deviation, thereby illustrating the relative magnitude and sign of variability across different spatial regions. " Furthermore, following the description of EOF1, I have added and elaborated on the content of calculating the cross-correlation coefficients between the four spatial modes.

Lines 130-⋯: Related to the point above, the EOFs essentially identify the regions of (high) variability, rather than 'positive anomalies' etc. Please word these passages more appropriately.

Also, at line 130 (and 139, ⋯) change 'significant' to something like 'sizeable'. Only use the former word when referring to results of a statistical test of the null hypothesis.

For lines 130 and 139, I have given serious consideration to your suggestions and revised the corresponding expressions in the manuscript. As mentioned in my previous response, I have developed a deeper understanding of the meaning of EOF decomposition and have more cautiously changed the vocabulary, for example, changing 'significant' to something like 'sizeable', and changing 'positive anomalies' to 'high variability', etc.

Line 144-145: Here the text speaks of the lack of statistical significance of the trends of the PC2 series. Similar test should be conducted for the PC1 series. One might guess that the winter and spring trends could be significant, placing any discussion on this on firmer ground.

Response to lines 144-145, I have described the statistical significance of PC1 in the revised manuscript.

Lines 152-155: I am not sure that Fig. 3 is particularly useful in the paper, especially at the trends in PC2s are not significant (and a similar comment probably applies to most/all of the PC1s). Suggest deleting it.

Response to lines 152-155, I have accepted your suggestion and removed Figure 3

Line 167-169: The SST structure in MAM is suggestive of ENSO. But note that in the far eastern Pacific the significant regressions move off the Equator, and into the NH.

The other three seasons in Fig. 4 show only scattered 'significant' areas, and probably less that the 5% of the globe that you would expect by chance. The regions of significance around Antarctica in JJA and SON are regions covered by sea ice. What does 'SST' mean here?!

Interesting to note that the eastern Pacific SSTA Shigeru Aoki in the 2017 paper (lines 77-79 above) showed strong ENSO structure with greater anomalies just to the SOUTH of the Equator in April and December (Figure 4 in the paper). (Also see the comment made in the paper at line 292-294.)

For lines 167-169, it is mentioned in the conclusion section that significant SST signals in the equatorial Pacific are only present in MAM, while a subtropical dipole pattern appears in other seasons. SST around Antarctica in JJA and SON denotes the sea surface temperature near the sea ice margin.

Also, I have explained in the revised manuscript that the reason for the difference from the anomalous region in the tropical Pacific in the Aoki (2017) paper is that this study divides the data by season rather than by month, leading to the discrepancy.

Lines 176-179: The role of the 'Amundsen Sea Low' here is very interesting. Perhaps emphasise the points being made here by referencing study of Fogt, R. L., A. J. Wovrosh, et al., 2012 - (The characteristic variability and connection to the underlying synoptic activity of the Amundsen-Bellingshausen Seas Low. J. of Geophysical Research, 117, D07111, doi: 10.1029/2011JD017337) and commenting that the region is strongly tied to the location of the maximum cyclone system density and minimum cyclone central pressures, and is tied up with the SAM.

Response to lines 176-179, I thank you for the good suggestion. Having read the Foght et al. 2012) reference thoroughly, I have cited it and added an explanation in the revised manuscript regarding the part where the ASL leads to increased cyclones and influences fast ice extent.

Lines 183-185: Figure 7a is not mentioned or referred to in the paper. However, its very interesting structure shows a strong wavenumber three. This mode has a large influence on subantarctic conditions (e.g., Irving & co-authors 2015: A novel approach to diagnosing Southern Hemisphere planetary wave activity and its influence on regional climate variability. J. Clim., 28, 9041-9057). Some additional text on this aspect would be very valuable.

Response to lines 183-185, thank you for your suggestion. I have carefully read this and a related paper, cited them, and added an explanation of the ZW3 structure and

effects in the interpretation of Figure 7(a) in the revised manuscript.

Line 309: Unbalanced parentheses.

The unbalanced parentheses on line 309 have been corrected in the revised version.

Response to RC2:

General Comments:

This research establishes a teleconnection relationship between equatorial sea surface temperatures and Antarctic landfast ice using a principal component analysis. This article and the relationships established within would contribute to our understanding of the drivers of Antarctic landfast ice variability. However, this manuscript needs revisions, specifically with regards to the interpretation of the Empirical orthogonal functions and principal components and what the represent. I have outline specifics and other revisions necessary.

We sincerely thank you for your careful reading and constructive comments. I deeply apologize for the formal and detailed errors and have made corrections accordingly, while continuing to review the manuscript carefully. In response to your suggestion, I have added content describing the data processing methods in Section 2 of the manuscript. Below are my point-by-point responses to your detailed suggestions:

Regarding the meaning of EOF decomposition, I have studied it in depth and made extensive revisions in the revised manuscript.

Figures – Pannels of figures need to be individually labelled i.e. Figure 1a such that in

text referencing is clearer. EX. Line 121 left panels of Figure 1 should be Figure 1a/c/e/g. This should occur both on the figures and in the text referring to the figures.

I have accepted your suggestion and numbered the subplots in the figures within the revised manuscript. Also, following another reviewer's advice, I have placed the spatial modes on the left and the temporal modes on the right.

Interpretation of PCs and EOFs are incorrect. Interpretations of both PCs and EOFs are referred to as trends in fast ice anomalies in time (PCs) and space (EOF). EOFs describe the spatial patterns of variability while the PCs indicate how strong the EOF is expressed each year. Adjustment to the interpretations of the PCs and EOFs is needed.

Specific Comments:

Concerning the interpretation of PC and EOF, I have deeply recognized my initial misunderstanding. Through reading relevant literature, I have gained a better understanding of EOF decomposition. I now realize that the values in the EOF spatial mode represent the rate of change in different longitudinal regions when the PC changes by one positive standard deviation. I have added explanations and made substantial revisions on this point in the revised manuscript.

Line 31 – Fast ice is the short version. It is ok to use throughout the article but there

should be a mention of the full "Landfast ice" somewhere.

The full term "Landfast ice" is now clearly stated in the first sentence of the Introduction.

Figure numbers have been corrected in the revised manuscript.

Line 33 - The maximum width of Antarctic fast-ice zone is approximately 200 km, covering an area of up to 600,000 km2. This sentence is not clear as the 200 km is referencing a single width a while the 600,000 is referencing the seasonal maximum circumpolar summed value. Could be improved by adding specifics such as "Fast ice has been observed to extend 200 km from the Antarctic coastline. Fast ice reaches its annual maximum extent during October with an average extent of ~601,000 km2."

Line 33 has been modified according to your suggestion.

Line 51 – I am unfamiliar with the term "fast ice shrisk" and could not find any reference to this. Possible typo. If not, please define this as it is not common vernacular.

My sincere apologies for the spelling error on line 51; it has been corrected to "fast ice shrink."

Line 54/55 - The atmospheric forcing factors also influence the variability of Antarctic fast ice across time scales ranging from microscale to synoptic, regional, seasonal, and interannual. "regional" is not a time scale.

Thank you for the reminder. The word "regional" on line 54 has been deleted.

Line 55/56 - Missing a reference for wind speed and direction. Also, Leonard et al. 2021 which you cite above also says storms and cyclones affect fast ice variability.

"Wind direction and speed" have been added to line 55.

Line 101-109 – Because a lot of the emphasis and frankly the results are reliant on the EOF analysis and the preparation of the data prior to the EOF analysis, it would be nice to have more details regarding how the fast ice data was prepared prior to the EOF analysis (EX. What resolution, you mentioned anomalies, any detrending, that sperate EOF analysis is done on each season). If the data was not detrended prior to EOF analysis EOF1 is potentially representative of the linear trend of fast ice extent during each season where if the PC has a positive trend longitudes within the EOF with a positive value had a similar positive trend in fast ice extent where negative eigen values represent a negative trend.

I have added content describing the data processing methods in Section 2 of the manuscript：

We detrended the fast ice boundary time series together with the sea surface temperature and atmospheric variables. We then removed the monthly climatological mean for each respective month to derive anomaly fields for all variables. Finally, the data were

grouped into seasons as follows.

Line 114-117 – Since you define the month triplets and use them in Table 1 instead of winter, summer etc. the text and table should use the same reference scheme. i.e. just use MAM or austral summer.

Since the seasons are defined in Section 2, subsequent text uses expressions such as "austral summer."

Line 112-Section 3.1 – The left panels of Figure 1 are not representative of trends in fast ice anomalies. These are principal components and given the y axes are from -3 to 3 I believe the data was standardized prior to PCA. As stated elsewhere, information regarding the data preparation prior to EOF analysis would be key to understand what the result mean. This makes the PCs unitless. Same with the EOFs which are dimensionless. In this analysis the EOF describe the where and the relative effect of the PC is expressed (The special pattern of the variability) while the PC described the strength of the EOF expression interannually. The interpretation of these data and figures needs to be changed to accurately represent the figures.

Response to line 112 - Section 3.1: As mentioned earlier, I have made extensive revisions and rephrased the explanation of the EOF decomposition results in the revised manuscript. Additionally, I have modified the figures to include the units (°) for the EOF and PC.

Section 3.1 – The along the front of the Ross ice shelf there is very little fast ice. Interpretation of variability in the Ross Sea should be done with caution.

I am especially grateful for you pointing out the fact that fast ice is rarely present in the Ross Sea. We have removed the statements regarding fast ice in the Ross Sea in the revised manuscript.

Line 152-155 – It is unclear how the data shown in Figure 3 was calculated. Please explain the calculation, indicate what the total linear trend is for each season, and how these values vary across longitudes.

Regarding the content on lines 152-155: Thank you for your query. After careful consideration, we concluded that Figure 3 lacks substantive meaning. We have deleted this figure and its related descriptions from the revised manuscript.

Section 3.2 – This section contains a lot of information. The information needs to be reduced to the important information relevant to the take home message. Or it needs to

be organized differently such that the reader understand which variable is important where and during which season. At present all the information is there but is not easily trackable.

I have supplemented the content in section 3.2 of the revised manuscript.

Figure 7-9b – What is the area defined as the fast ice region. The regression was done over multiple years I assume and the fast ice edge varies between years.

The fast ice extent is defined as the latitudinal difference between the outer edge of the fast ice and the Antarctic continental coastline. The fast ice extent described in Figures 7-9b refers to the first spatial mode (EOF1) from the EOF decomposition of the fast ice.

Figure 7-9b – the left y-axis is not latitude; it is the PC value.

The unit of the PC values is degrees of latitude (°), hence the y-axis uses 'latitude'.

Technical Corrections:

Line 46 – on line 45 you say "Secondly" then the next sentence is "Second". I assume the "Second" should be "Third". This would cause the "Third" on line 48 to be "Fourth".

Line 110 – Double period to end the sentence.

Line 116 – asutral winter should be austral winter.

I deeply apologize for the formal and detailed errors and have made corrections accordingly, while continuing to review the manuscript carefully.