

Reviewer 1 Comments

This paper examines the relationship between satellite-observed sea ice and large-scale wind patterns using an EOF analysis and ERA5. The paper is well placed within the broader field and the discussion of the role of short-term variability in the context of longer-term changes is sound. The insight that different polynyas respond differently to similar large-scale anomalies is valuable. The methodology is useful for understanding distinct modes of SIC variability in polynya regions and their association with large-scale wind patterns. I feel that quite a bit more work is needed to physically interpret the results robustly, i.e. link EOFs and MCA back to physical processes.

See below:

1. Without e.g. composites (ideally lagged composites) of absolute conditions occurring during the high/low phases of different EOF modes it's quite difficult to reach a physical interpretation.

E.g. northerly wind anomalies in the Ross could be associated with either actual northerlies (potentially driving sea ice compaction) or weak southerlies (reduced divergence).

For example, in the paper (e.g. L201) there is description of physical features such as jets in Figure 3Ad, but the Figure shows the association between winds and SIC, rather than physical jets. Similarly, it's difficult to understand the interpretation of low-sea-ice conditions in mode 2 if thermodynamic factors play a role as no thermodynamic fields are shown.

Response:

We thank the reviewer for this thoughtful comment regarding the physical interpretation of the EOF-associated circulation patterns. We agree that anomaly fields alone may not uniquely distinguish between true flow reversals and variations in the strength of the prevailing circulation regime. To address this concern, we have now included composite maps of the absolute near-surface winds during the positive and negative phases of the leading EOF modes (Fig. S5 and S6). These additional diagnostics help clarify the background circulation states and support our interpretation that the identified anomalies primarily reflect changes in the strength and positioning of the prevailing flow regimes over the Ross Sea sector.

We also acknowledge that lagged composite analysis could provide further insight into the temporal evolution and potential causality of the circulation–sea ice relationships. However, the primary objective of the present study is to identify the dominant modes of coupled SIC–circulation variability and their associated large-scale atmospheric structures, rather than to investigate the lifecycle or predictability of individual events. Therefore, the analysis is intentionally focused on simultaneous relationships, while lagged evolution is acknowledged as an important avenue for future work.

In addition, we agree that the regression patterns represent circulation anomalies associated with SIC variability rather than direct diagnostics of jet structure or intensity. Accordingly, we have revised the manuscript throughout to avoid overinterpretation and now refer more generally to circulation anomalies associated with the Ross Ice Shelf Air Stream region.

Finally, we agree that thermodynamic fields would be necessary to fully evaluate the role of thermodynamic forcing mechanisms in driving the observed SIC variability. Since the primary focus of this study is on the large-scale dynamical circulation patterns associated with SIC variability, we have moderated the discussion related to thermodynamic influences and clarified that such interpretations remain speculative within the scope of the present analysis.

2. Based on the methodology I would caution against the use of ‘extreme wind events’ in this paper. Papers describing extreme wind events usually focus on e.g. specific percentiles or thresholds. Here, the focus is on the daily maximum wind speed which may not be extreme. The name ‘ExWinds’ should be updated accordingly (maybe ‘MaxWinds’?).

Response:

We appreciate this important point. In response, we have replaced the term “ExWinds” throughout the manuscript with “MaxWinds” to better reflect the nature of the metric used. We agree that the term “extreme wind events” may imply a threshold-based definition that is not applicable here.

3. Related to the above, what’s the justification for using daily maximum wind speeds (potentially noisier) compared to daily mean?

Response:

We thank the reviewer for raising this point. We use daily maximum wind speed because our focus is on the strongest wind forcing occurring within each day. In polar regions, short-lived but intense wind bursts can be highly effective in driving rapid sea-ice divergence, polynya opening, and related ice-dynamic responses, even if they are not sustained throughout the day. Daily mean winds may smooth out these short-duration peaks and therefore underestimate the instantaneous forcing most relevant to sea-ice motion and deformation.

We have revised the manuscript to make clear that this metric is intended as a proxy for peak daily forcing rather than a formally defined extreme.

4. Several figure improvements are needed:

1. All Figures’ DPI is quite low.

We have revised the figures with improved resolution where possible and also includes clearer labeling and captions.

2. The BWCJ mechanism is central to the Mode 3 interpretation. Can you provide a suitable composite or at least local wind vectors as in Figure 3B over the BWCJ region?

We also note that barrier winds are difficult to resolve at the 0.25° horizontal resolution used here, and we have added text acknowledging this limitation. Where feasible, we have also included additional local wind-vector information over the BWCJ region to better support the discussion of Mode 3.

3. Figure 3B does not have panel labels or a descriptive caption (are the wind vectors MCA spatial patterns?). Figure 4 caption should refer to panel labels.

The panels labels are now added

4. It’s not clear whether Figure 3Ad,e,f is or should be different from the right panels of Figure 3B. Figure 3B Modes 1 and 2 look similar to Figure 3A(d,e) but Mode 3 looks completely different to Figure 3A(f)?

Thank you for pointing this out. Figures 3A(d–f) and the right-hand panels of Figure 3B are related but not identical diagnostics. Figure 3A(d–f) presents the circulation patterns associated with the identified modes in the large-scale wind field, whereas the right-hand panels of Figure 3B show the composite local wind vectors over the Ross Sea sector. The

close similarity for Modes 1 and 2 reflects the fact that these modes are primarily associated with large-scale synoptic circulation anomalies that are well represented in the 0.25° ERA5 data. In contrast, Mode 3 is interpreted as being linked to the Barrier Wind Coastal Jet (BWCJ) mechanism along the Transantarctic Mountains. Because barrier winds are mesoscale features confined to narrow coastal regions, they are not fully resolved in the present dataset. We have revised the manuscript to clarify this distinction and to explicitly note the resolution limitations for capturing such localized coastal jets.

Specific comments

1. L113 Square root of the cosine of latitude?
We have corrected the text as suggested.
2. L191 ‘PCs associated 3’?
We have revised the sentence to refer explicitly to “mode 3.”
3. L212: No 3B(e) is marked on the figure. Also ‘Northwest-southeast pattern’ please clarify using e.g. northwesterly, southeasterly
Thank you for this observation. The figure has been revised, with all relevant panels clearly labeled. We have also clarified the wind-direction terminology for improved readability.
4. L219: Stammerjohn et al. (2008) in the references gives a DOI: <https://doi.org/10.1016/j.pocean.2008.02.002> which leads to a paper called “Statistics from Lagrangian observations”. I cannot find any paper entitled “Sea ice in the Southern Ocean: The Weddell Sea and Ross Sea”.
Thank you for identifying this reference issue. We have corrected the citation and updated the reference list accordingly.
5. L210 This paragraph needs supporting evidence to interpret the Mode 2 pattern. I think it’s important to help readers understand how weak winds conditions can be associated with low sea ice states (it’s intuitive to understand how strong winds can be associated with low sea ice). I feel that composites of physical quantities are needed. Is it thermodynamic factors (e.g. increased shortwave absorption, low cloud cover) as you propose, or perhaps memory from previous high-wind states?

Response:

We thank the reviewer for this insightful comment. We agree that the physical interpretation of low sea-ice conditions associated with comparatively weak wind anomalies in Mode 2 is less straightforward than in strong-wind regimes. Our intention was not to imply a definitive attribution to specific thermodynamic mechanisms, but rather to discuss plausible contributing processes that may accompany the observed circulation patterns.

To avoid overinterpretation, we have revised the text to clarify that the proposed thermodynamic influences, including enhanced shortwave absorption and cloud-related effects, remain speculative within the scope of the present analysis. We also acknowledge that persistence or memory effects

associated with preceding circulation states could contribute to the observed SIC anomalies. However, a detailed assessment of these mechanisms would require additional thermodynamic and lagged composite analyses, which are beyond the primary focus of this study. The manuscript has been revised accordingly to emphasize that our results primarily identify large-scale dynamical associations rather than establish direct process-level attribution.

6. It may be worth looking at cutting down on acronyms a bit as there are many to keep track of – RS for Ross Sea is probably not needed. Similarly, no need to define AP as an acronym.

We agree and have simplified the terminology accordingly. “RS” has been replaced with “Ross Sea,” and “AP” has been removed where unnecessary.

7. L228 Barrier wind corner jet mechanism (BWCJ): it’s hard to discern this from the wind vectors (which are presumably anomalies anyway so not clear how the real wind field looks).

We appreciate this comment. We have revised the discussion to make clear that the barrier wind corner jet mechanism is inferred from the broader circulation context and local geography, rather than directly resolved in the wind-vector fields alone. We also emphasize that the available resolution limits the explicit representation of this feature

8. L248-249 Not sure what is meant by this localized analysis? Do you mean that the BWCJ doesn’t appear when you look at local wind vectors?

We have clarified this point in the revised manuscript. The local wind vectors are derived from ERA5 10 m u and v components averaged over the day, whereas the “MaxWinds” metric captures the daily maximum wind speed. Because the daily-mean wind vectors smooth short-lived variability, they may not fully represent a barrier wind structure, although they do reproduce the broader patterns associated with Modes 1 and 2. We have revised the text to make this distinction explicit.

9. Figure 4: please explain what the hatching/stippling denotes. Also, because the regressions are computed from daily ASO data concatenated across years, the underlying time series are serially correlated and the nominal sample size will overstate the effective degrees of freedom. Please clarify how the p-values were calculated (e.g., using an effective sample size adjustment). If no autocorrelation treatment was applied, please revisit the significance masking accordingly.

Response:

We thank the reviewer for this important comment regarding the statistical significance assessment. In the revised manuscript, we have clarified that the hatching/stippling in Figure 4 denotes regions exceeding the chosen statistical significance threshold for the regression analysis.

We agree that the use of daily ASO data may introduce serial autocorrelation, which can reduce the effective degrees of freedom and potentially lead to overestimation of nominal significance levels when using standard statistical tests. The original analysis did not explicitly apply an effective sample size correction for temporal autocorrelation. We have therefore revised the manuscript text to clarify this limitation and moderated the interpretation of statistically significant regions accordingly.

In addition, we have updated the figure caption and associated discussion to emphasize that the stippling should be interpreted as indicative of robust spatial associations rather than strict pointwise significance under fully independent sampling assumptions.

10. L268–271: ‘weak ExWin anomalies’ indicates winds close to the seasonal mean, not necessarily weak absolute winds. I suggest avoiding interpreting near-zero anomalies as ‘subdued in speed’ unless you show composites of absolute ExWin. Unless you mean negative anomalies?

We agree and have revised the wording to refer to near-zero wind anomalies rather than implying weak absolute winds. The text has been adjusted accordingly to avoid ambiguity.

11. L283: Perhaps avoid using Cape Colbeck as it’s not a widely known geographic marker.

We appreciate the suggestion. We have revised the wording to use a clearer geographic reference that will be more familiar to readers.

12. L287: The figures are not really sufficient to interpret the BWCJ

We thank the reviewer for this important observation, we do agree that the present figures do not directly diagnose the Barrier Wind Convergence Jet (BWCJ) structure or intensity. Our intention was to describe circulation anomalies that are spatially consistent with variability in the BWCJ region, rather than to claim a direct observation or explicit dynamical characterization of the jet itself. To avoid overinterpretation, we have revised the relevant text throughout the manuscript to more carefully describe these features as circulation patterns associated with the BWCJ sector.

13. L300 This paragraph’s wording (‘dominated by a small set’) conflicts with the earlier acknowledgement that EOF1-3 leave 63% of SIC variance unexplained. I may have misunderstood so please qualify what is meant by ‘dominated’ or soften the claim.

We agree that the term ‘dominated’ may overstate the variance explained by the leading EOF/MCA modes, given that EOF1–3 account for approximately 37% of the SIC variability. We have therefore revised the text to soften the interpretation and clarify that these modes represent the leading coherent patterns associated with Ross Sea polynya variability rather than the entirety of the variability

14. L304: please explain somewhere the use of ‘accumulation’ in this paper and how you observe it?

We thank the reviewer for this helpful suggestion. We have now moved the detailed discussion of study limitations from the Conclusions to the Discussion section in order to keep the Conclusions more concise and focused on the primary findings and implications of the study.

We also clarified the use of the term ‘accumulation’ throughout the manuscript. Specifically, we now define accumulation as regions of relatively enhanced SIC interpreted as sea-ice convergence or retention within a coastal sector associated with circulation-driven advection and compaction, rather than a direct observation of ice mass increase or thickness

15. I suggest moving discussion of limitations to the Discussion section and keeping the conclusion concise.

We appreciate the reviewer’s suggestion. In the revised manuscript, discussion related to study limitations and interpretational constraints has been moved to the Discussion section, while the

Conclusion section has been streamlined to focus primarily on the main findings and broader implications of the study.

16. References section: I see many references listed which don't appear in the text. I also see e.g. 'n.d.' for dated papers and typos e.g. "Academic Press.sss" and the Turner et al. citation.

We thank the reviewer for carefully checking the reference list. The references section has been thoroughly revised to remove unused citations, correct formatting inconsistencies, update missing publication information, and fix typographical errors including those noted by the reviewer.

Technical corrections

- L29 double comma - *We thank the reviewer for carefully identifying this typographical error. The double comma has been corrected in the revised manuscript.*
- L198/L199 ExWin? Or ExWinds? See other references to it as well. - *The terminology has now been revised and standardized to "MaxWinds" throughout the manuscript for clarity and consistency.*
- L208 WinVec? *Thanks for this suggestion. "WinVec" has been replaced with "wind vectors" in the revised manuscript for improved readability.*
- L246 periods - *The punctuation has been corrected as suggested.*
- L311 erroneous full stop – *The erroneous full stop has been removed.*
- Consistency of eg. Localised, localized... - *The terminology has now been standardized to "localised" throughout the manuscript.*
- L635 typo –*The correction has been made in the revised manuscript.*