Response to RC2: MS No.: egusphere-2026-1250

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RC2: This manuscript investigates the relationship between persistent synoptic-scale atmospheric features and recent Antarctic sea-ice anomalies, with particular emphasis on the low sea-ice years 2016, 2021, and 2023. The authors develop a reduced-order coupled atmosphere-sea-ice model using reanalysis data and apply the FEM-BV-VAR framework together with a non-stationary transition-matrix approach to identify persistent background states of varying duration. The manuscript argues that persistent synoptic patterns strongly influence the rate and spatial structure of sea-ice variability. During periods of relatively weak synoptic variability, a large-scale Antarctic warming pattern becomes the dominant influence on sea-ice retreat. The topic is interesting and potentially important. However, in its current form, I do not think the manuscript is yet ready for publication. My main concern is that the physical interpretation remains insufficiently developed, and several key methodological and diagnostic issues need to be addressed before the conclusions can be considered robust.

Authors: We thank the reviewer for the effort taken to examine the manuscript.

Comments:

1. The most important weakness of the manuscript is that the results are primarily presented as statistical or pattern-based associations, but the underlying physical processes are not sufficiently explained. At present, the analysis supports physically plausible relationships, but it does not yet provide a sufficiently clear process-based interpretation of how the identified synoptic states lead to sea-ice growth or retreat. The paper would be much stronger if the authors could provide more detailed discussion of the relevant physical mechanisms, for example through thermodynamic, dynamic, or regional process diagnostics.

Authors: The paper has been revised to better emphasise and quantify a process-based interpretation of the mechanisms by which the identified synoptic states lead to sea-ice growth or retreat. This includes additional references to recent literature and earlier process based modelling studies by some of the authors and additional quantitative measures of the contribution of relevant teleconnections to the observed persistent states. To facilitate this there has also been some reorganisation of the methods to appendices with additional technical details as requested by another reviewer. A key point in applying regression learning to data is that physical processes are inferred based on the methodological approach. Our approach is to infer plausible physical relationships based on the manifestation of persistent synoptic states. Where an emergent structure has a significant pattern correlation to a canonical teleconnection pattern we argue it is reasonable to assume that teleconnection is dominant over the period identified given the averages are over reanalysed daily anomalies and the reference patterns are the commonly accepted descriptors of the particular teleconnections. Thus, our approach allows us to study multiple teleconnections and their influence at once rather than studying the individual influence of each teleconnection.

2. Related to the previous point, the manuscript often goes beyond what the analysis can firmly support. The current framework is useful for identifying covariation and persistent background states, but it does not by itself establish strict causal attribution. Some conclusions therefore appear overstated. The authors should be more cautious in distinguishing between statistical association, dynamical interpretation, and formal attribution.

Authors: Here we are applying optimisation methods to fit a stochastic model to reanalysis data then applying Markov transition matrices to affiliations of each successive data point to states determined from an optimal linear vector autoregressive model. We make no claims regarding causality. We note that a future study could use the generated Markov model to measure predictive information allowing for estimates of Granger causality; we only conjecture around the physical implications of identified persistent atmospheric events preceding a sea-ice loss event. We have carefully revised our paper to ensure there is no confusion about this point.

3. The manuscript refers to a "climatological anthropogenic warming pattern" as a dominant influence during periods of weak synoptic variability. However, the present study does not perform a formal attribution analysis separating externally forced and internally generated variability. Therefore, this terminology is too strong in the current context. The authors should either soften this statement or provide stronger justification for using such wording.

Authors: We agree on this point. We have revised the manuscript to soften the statement simply noting there are periods of sea ice loss coincident with periods of relatively quiescent winds which are also coincident with anomalously warm surface temperatures.

4. Data: The study uses daily SIC from NCEP, but the comparison with NSIDC appears to be limited to monthly-scale analysis. Monthly comparison alone is not sufficient, especially given that the manuscript focuses on synoptic events. The authors should provide a more detailed validation of the daily SIC fields, including spatial patterns and event-scale behavior, rather than relying only on monthly anomaly comparison. For atmospheric fields, it is not clear how robust the results are to the choice of reanalysis. The manuscript should discuss whether the same persistent patterns and event structures would be obtained using ERA5 instead of NCEP, or at least assess the differences between the two products for the key variables used in the analysis. At present, the sensitivity of the conclusions to the reanalysis dataset remains unclear.

Authors: A detailed comparison of differences between NSIDC and NCEP is beyond the scope of this study, however, we note that the NSIDC website states "NSIDC currently uses the National Centers for Environmental Prediction-National Center for Environmental Research (NCEP-NCAR) Reanalysis 1 for assessments of atmospheric circulation and temperature". On that basis we assume that NNR1 is a sufficiently reliable and consistent dataset to base our study, and thus it is the most appropriate to consider in the first instance. In terms of differences between the NNR1 atmosphere versus ERA5 or other products, there are many varied reanalysis intercomparison studies, many relevant to this study are focussed on particular variables e.g., the intercomparison study of surface air temperatures over Antarctica [5], or for the Southern Hemisphere troposphere over the pre-satellite record including as in Appendix A of [9]. It is not unreasonable to expect differences in the metastable states between different products. For example [1] report major artifacts in ERA5 2-m air temperature trends over Antarctica prior to and during the modern satellite era which would undoubtedly lead to differences to those reported here. However, attribution of differences in reanalysis requires understanding the assimilation systems and underlying model biases [11, 2, 8, 4], and relationships between teleconnections [3] such that a detailed intercomparison that includes attribution of the cause of any differences between products is not within the scope of this study.

5. The manuscript spends considerable space describing the methodology, while the physical interpretation of the results is comparatively brief. I suggest streamlining the methodological description in the main text and moving some technical details to an appendix or supplement, in order to leave more room for discussion of the physical implications of the identified patterns.

Authors: Agreed. The FEM-BV-VAR methodology has been moved to an appendix to address another reviewer's request for more details on the methodology and to streamline the main body of the manuscript. Additional analysis and diagnostics have been added to improve physical interpretation and correspondence to recent literature.

6. The identified events have very different durations. This raises an important question: are the events with different lengths dynamically comparable? Do they represent similar mechanisms, or are they fundamentally different types of processes being grouped within the same framework? The authors should discuss whether the same interpretation applies across events of very different duration, and whether duration itself carries physical significance.

Authors: The duration of given events is only an indication of their relative persistence. For example, two distinct events of similar amplitude and persistence associated with the same statistically stationary state may exhibit quite distinct local dynamics. This is a key point and can easily be understood from the perspective of PCA or k-means. As an example, an EOF is an invariant pattern associated with a statistically stationary state that explains a specified fraction of the total variance. FEM-BV-VAR performs a global optimisation to assign sequential data instances for given embeddings to a finite number of locally stationary states. This means the patterns themselves are not invariant but time evolving with a probability that any particular data instance resides in one or more metastable states. Hence in post processing patterns are determined directly from the reanalysis anomalies. Therefore the correct interpretation is that varying local synoptic processes are being grouped within a common global clustering framework. That is locally diverse dynamics can have common global affiliations to given teleconnections.

7. The manuscript focuses strongly on atmospheric driver, but the role of the ocean and sea-ice dynamics is not sufficiently considered. Given the current understanding of Antarctic sea-ice variability, it is important to clarify to what extent the identified atmospheric patterns act alone, and to what extent oceanic forcing or sea-ice dynamical processes may also be essential. At minimum, the limitations of excluding these factors from the main analysis should be discussed more explicitly.

Authors: As the revised title, "Covariations between persistent synoptic features and Antarctic sea ice via unsupervised regression learning" suggests, this work is solely focusing on the role of the atmosphere on case studies of recent record low sea-ice events. Given the paucity of subsurface ocean observations of sufficient density to adequately constrain reanalysis of subsurface ocean dynamics, coupled model simulations are required and beyond the scope of the current study. We now include additional discussion of such modelling studies for example the earlier general modelling studies of [10] & [6] and the most recent study of record low sea ice using an ocean model constrained by observed surface forcing [7]. A natural follow-up study could include applying this methodology to ocean-SIC coupled data to find specific oceanic drivers on longer timescales. This, however, would require more careful consideration of the ocean data used as psuedo-observations as mentioned above.

8. The wind vectors in the figures are very difficult to read. In their current form, I cannot clearly determine their direction or magnitude. The figure design should be improved. Moreover, some figures appear to lack units. All plotted variables should include clear and consistent units in the color bars, captions or axis labels.

Authors: We have modified the figures to be more legible with regards to the arrows and checked all units are accounted for.

References

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