Response to reviewer #2

Manuscript: Thermodynamic and dynamic drivers underlying extreme central Arctic sea ice loss
Authors: Zhenlin Li, Fei Huang, Jian Shi, Ruichang Ding, Shumeng Zhang
Number: egusphere-2025-3594

Dear reviewer,

We previously provided a general response to your comments. We have revised the manuscript thoroughly based on your suggestions and now offer a more detailed response. We sincerely appreciate your constructive criticism and advice again.

Sincerely,

Zhenlin Li
On behalf of all the authors

Black text: Reviewer's comments

Blue text: Authors' response

Italic text: Revised content in the manuscript

1 General comments

The study conducted by Li et alii investigates events of major sea ice concentration decrease in the Central Arctic. To do so, they use an Empirical Orthogonal Function decomposition of standardized sea ice concentration (SIC) anomalies, and use a set of complementary analyses, sea ice budget, temperature tendency analysis, sea ice dynamic decomposition and Rossby wave source identification, to determine the drivers of the two dominant modes of the SIC anomalies. The topic is of interest, though already extensively studied, and the study could contribute to the overall understanding of how sea ice in the Arctic evolves, in particular for the next few decades, during which the Central Arctic sea ice will become more and more vulnerable. Moreover, the large range of complementary analyses to determine the sources of sea ice loss are at first read enticing.

Unfortunately, the methods at the heart of those analyses and the data used to conduct them raise some serious concerns and I therefore have doubts as to whether they can support the claims from the authors. But more importantly, the study never addresses the potential role of the ocean to drive the ELSEs, while it is now considered as the first driver of sea ice loss. Moreover, it does not seem to disentangle the trend from the EOF modes, while it is likely hidden in the two dominant modes.

I do my best to detail my concerns below and to support them convincingly. I suspect addressing them properly will require a significant amount of work, including a total change of the methodology used. It should also lead to a significant change in the results.

We would like to thank you for the valuable comments and suggestions, which have greatly helped us improve the quality of our manuscript.

2 Major comments

Reanalysis data fluxes should not be used to close a budget

An important caveat of reanalysis products is that they are not physically consistent. Indeed, when assimilating observational data into the model state, spurious fluxes are introduced, breaking the conservation of some properties, including momentum and mass: in reanalysis, "the system state estimate can undergo jumps, implying implicit non-physical sources, and rendering very difficult the physical interpretation of the time-evolving state. Methods have been employed to smooth out the discontinuities over finite times, but still leaving artificial imbalances in the solution." (Wunsch & Heimbach, 2007). While reanalysis products provide the best estimate of the state of the climate, they should not be used to calculate budgets, as they cannot physically close them. A better, physically-consistent alternative to reanalysis would be State Estimates products, but those are costly to compute (e.g. ASTE, Nguyen et al., 2021) and typically not available for the kind of investigations conducted here.

Unfortunately, this study relies on reanalysis products, ERA5 and JRA55 to calculate a temperature budget, and PIOMAS to close a sea ice budget. This is a major issue, especially considering that the most important terms of the budgets (diabatic heating for the temperature; thermodynamics for the sea ice) are calculated by making the assumption that those budget are closed and that the residuals therefore correspond to the wanted term. Uncertainties are difficult to evaluate in reanalyses, and so it remains unknown whether using those data while significantly alter the results on the spatial and temporal scales considered here. But in doubts, I believe we have to make the assumption that the unphysical flux produced by data assimilation might not be negligible. Therefore, the method proposed here to evaluate the thermodynamical and dynamical contributions to sea ice loss is not sound. Note that this is a bit less worrisome for the sea ice budget, as the dynamical term is actually estimated by observations and not a reanalysis, but the thermodynamical term should still include not only the "real" thermodynamics but also a (hopefully small) spurious term related to the correction of the sea ice thickness by assimilation of observations into the PIOMAS H_{eff}.

This is a highly noteworthy issue, which we have addressed with additional discussions in the Method and Discussion sections.

We have added the following content in the Methods section.

Previous studies proposed using SIC budget to diagnose sea ice changes, in which the advection term, divergence term, and residual term (including thermodynamic processes and mechanical processes related to ridging and rafting) constitute the total SIC budget (Holland and Kimura, 2016; Holland and Kwok, 2012). Schroeter et al. (2018) replaced the SIC budget with the sea ice volume budget, thereby enabling SIT changes to include mechanical processes like ridging and rafting and making the residual term directly represent thermodynamic processes; this method has been widely used (Bi et al., 2023; Ding et al., 2025; Lukovich et al., 2021).

The sea ice volume budget analysis requires SIC, SIM, and SIT data, among which SIC and SIM are derived from NSIDC observational data in this work to ensure the robustness of ELSEs in the diagnostic process. Previous studies have indicated that the simulation bias of SIT dominates the accuracy of Arctic sea ice simulation and prediction (Massonnet et al., 2018). Therefore, SIT data is crucial for the diagnostic analysis of sea ice budget. Given that SIT cannot be directly observed by satellites, we use the SIT reanalysis data from PIOMAS here.

It should be noted that the combination of observational and reanalysis data for budget diagnosis involves the equation closure issue. Previous studies proposed the Lagrange multiplier algorithm to address the issue (Mayer et al., 2018). Existing research has used this method to conduct sea ice budget analysis based on NSIDC and PIOMAS data (Ding et al., 2025; Lukovich et al., 2021). They found that the difference between the original budget terms and those corrected by the Lagrange multiplier algorithm is minor, indicating the sea ice budget diagnostic method combining NSIDC and PIOMAS data here is reliable.

We have added the following content in the Discussion section.

Although this study has used observational data as much as possible, it should be acknowledged that the methodological limitations of this study stem from the reanalysis datasets: the non-physical fluxes introduced by data assimilation in reanalysis products make them unsuitable for accurate budget calculations (Wunsch and Heimbach, 2007). Although physically consistent State Estimate products are more optimal alternatives, such products are often associated with high computational costs (Nguyen et al., 2021). Furthermore, for the spatiotemporal scales and variables focused on in this study, no publicly available State Estimate products currently exist. In this context, the ERA5, JRA55, and PIOMAS datasets remain widely recognized as reliable data sources for analyzing large-scale climate *and sea ice variations and their underlying mechanisms. This study follows the approach of previous studies by treating the residual term as the diabatic heating term (Yanai et al., 1992; Yao and Sun, 2016). Precisely because we recognized the potential systematic errors inherent in this method, two independent datasets were used for cross-validation to maximize the reliability of the results. It is important to clarify that the primary objective of this study is not to provide precise quantitative estimates of thermodynamic and dynamic contributions, but rather to analyze the differences in their relative importance and spatiotemporal distribution characteristics within the framework of currently widely used datasets, so as to explore clues to the physical mechanisms driving Arctic sea ice loss. Despite the inevitable systematic errors caused by data limitations, numerous studies have confirmed that reanalysis data can still provide valuable references for analyzing the mechanisms of Arctic sea ice change.*

The Temperature tendency does not account for the ocean or sea ice

Equation (1) in section 2.4 equates the temperature tendency to the advection, the adiabatic heating and the diabatic. The tendency, the advection and the adiabatic heating are computed using the ERA5 reanalysis (see above for a major caveat of using this data for a budget). The last term, arguably the most important (I regret the authors did not show the comparison of all the terms), is estimated by considering that it is equal to the residuals of the budget. This could be true if 1. the reanalysis could be used to close the budget (I have argued above that it is not the case) and 2. if it was the only term missing. Unfortunately, I believe that the heat flux at the surface is not accounted for, in this equation. Indeed, sea ice or ocean are important heat sources or sinks and are therefore likely to provide an important heat flux. In equation (1), it is implicitly in the residual, but the text describing the equation makes me think that the authors are not aware of it: "The diabatic heating rate can also be directly estimated by summing the large-scale condensation heating rate, convective heating rate, vertical diffusion heating rate *[this would be the sensible heat flux between atmosphere and ocean/sea ice]*, solar radiation heating rate *[did the authors account for upward solar radiation proportional to the albedo of the surface? it seems not]*, and longwave radiation heating rate *[another heat flux for which sea ice or ocean need to be accounted for, but with no mention of it in the text]* based on the JRA-55 datasets." This is particularly worrisome as this study focuses on sea ice, but the equation is never used to link temperature tendency to sea ice! And in some cases (including during ELSEs), we can expect this flux to be the first order driver of the temperature tendency. Therefore this budget is not closed and, unless I missed something fundamental in the methods, what the authors consider as the diabatic heating is actually not the diabatic heating alone.

Because of those two major concerns, the results described in this study cannot be fully trusted. Many of those results are overall consistent with the scientific literature (e.g. the dominating importance of the thermodynamics over the dynamics in the sea ice budget, Le Guern-Lepage & Tremblay, 2023 or the importance of the "diabatic term" in the temperature tendency, over the other terms). But some other results are a bit at odds, to the best of my knowledge, e.g. the prominent importance of latent heat flux, which is rather supposed to be one or two orders of magnitude smaller than radiative and sensible heat fluxes (note that this could actually be related to another methodological issue, see last major comment).

The discussion of the budget diagnosis using reanalysis data is provided in the response to the first Major Comment.

Although the direct interactions between sea ice and the atmosphere are not explicitly decomposed in this study, the diabatic heating term in the equation, calculated as a residual in the total heat budget, inherently incorporates sensible heat flux, latent heat flux, and radiative flux. This indicates that the thermodynamic forcing imposed by the ocean/sea ice surface on the atmosphere via turbulent and radiative processes has been fully included in the diagnostic framework of the temperature tendency equation. Therefore, even though individual components are not explicitly separated, the total diabatic heating term used in this study adequately represents the key physical processes of heat exchange. **We have revised** "*The first term on the right side of the Eq. 1 represents the horizontal advection term, the second term denotes the combined effect of convective and adiabatic heating, and the third term represents the contribution of diabatic heating*" **to** "*The first term on the right side of the Eq. 1 represents the horizontal advection term, the second term denotes the combined effect of convective and adiabatic heating, and the third term represents the contribution of diabatic heating, which implicitly includes the sensible heat flux, latent heat flux, and radiation terms*".

The revision regarding the role of latent heat flux is provided in the response to the seventh Minor Comment.

What is the role of the ocean in the sea ice loss?

This says it all. The ocean is a complete blind-spot of this study, while it now explains over half of the sea ice melt in the Central Arctic (e.g. Carmack et al., 2015, Oldenburg et al. 2024).

Thank you for this very constructive and insightful comment. We fully agree with you that oceanic processes have increasingly played a critical role in recent Arctic sea ice changes. In the revised manuscript, we have added a dedicated discussion to emphasize the importance of oceanic heat forcing and the "Atlantification" process, and have clearly stated that oceanic contributions represent a key limitation of the present study.

The following content has been added to the Discussion section.

The budget diagnosis presented in Fig. 4 of this study incorporates all influencing factors and indicates that thermodynamic contributions account for approximately 70% of the sea ice variations in the central Arctic region. Meanwhile, the high correlation coefficients (-0.77 and -0.91) between surface air temperature anomalies and sea ice variability modes underscore the significant influence of atmospheric processes (Fig. 5), yet do not fully explain all thermodynamic contributions. Given the increasingly recognized role of oceanic heat forcing in the Arctic (Carmack et al., 2015; Docquier and Koenigk, 2021; Oldenburg et al., 2024), this suggests that oceanic thermodynamic processes constitute the remaining key contribution. A prominent example is the widely discussed "Atlantification" process in recent years: the increasing presence of warm, saline water from the North Atlantic within the Arctic Ocean and the subsequent cascading climatic responses (Årthun et al., 2012; Polyakov et al., 2017). Existing studies have demonstrated that Atlantification is steadily advancing eastward and poleward (Polyakov et al., 2017; Shu et al., 2021), and the latest observational evidence further indicates that its impacts have extended into the Makarov Basin (Polyakov et al., 2025). This signifies that sea ice changes in the central Arctic may have entered a new phase profoundly influenced by oceanic processes. This indeed represents a limitation of the present work and will be a key focus of our future research.

We acknowledge that our current analysis does not explicitly include oceanic processes, and we have pointed out that a quantitative evaluation of oceanic impacts will be a major focus of our future work. We hope this clarification and additional discussion adequately address your concern.

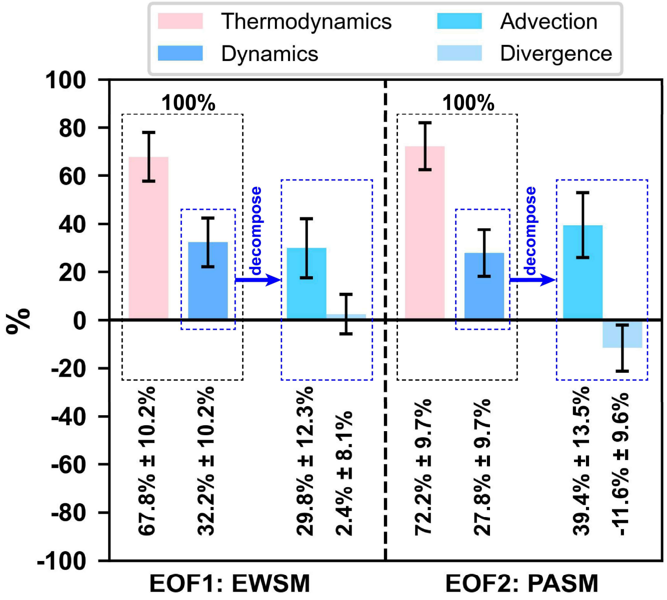


Figure 4. Effective SIT budget for the EWSM (left) and the PASM (right). Error bar represents the range of 1 SD from the mean value.

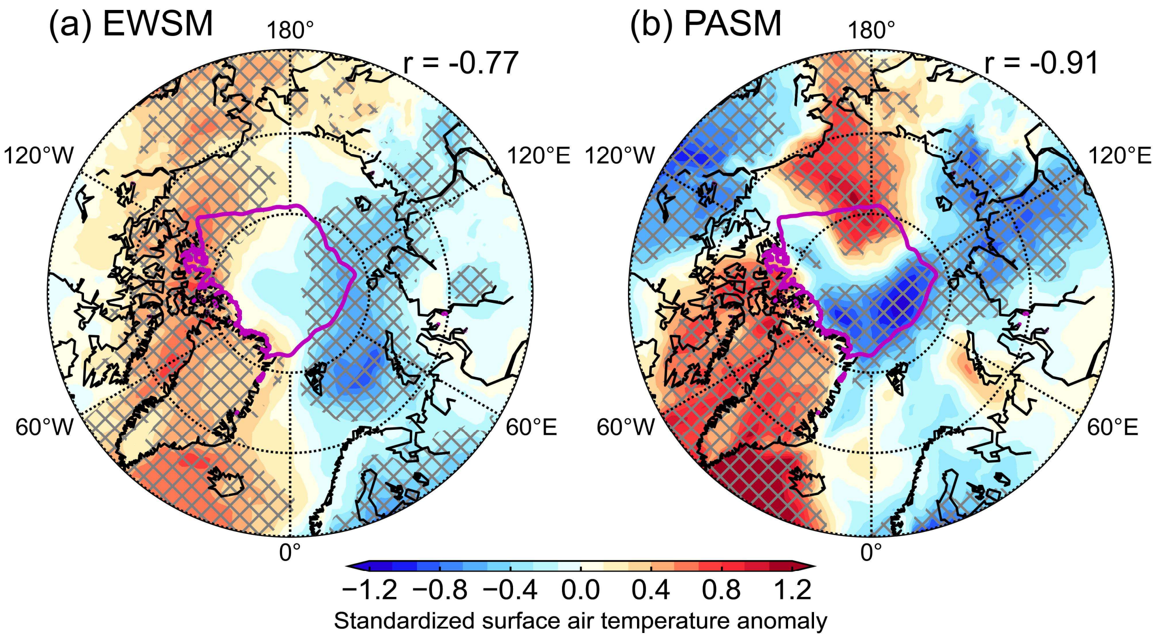


Figure 5. Composite differences in standardized surface air temperature anomaly between the positive and negative phases of **(a)** the EWSM and **(b)** the PASM. Magenta curve represents the central Arctic. The spatial correlation coefficients with the corresponding spatial patterns in the central Arctic are shown in the upper right corner. The grey crossings indicate areas with significant difference at the 0.05 significance level based on Student's t-test.

Are trends of sea ice concentration included in the EOF modes?

In the preprocessing steps before decomposing the sea ice concentration into EOFs, the sea ice anomalies are computed by removing the climatology. But no trend seems to be removed. Considering the major changes that the sea ice is undergoing in the Arctic, I would expect the trend to be the dominant mode of the EOF decomposition. The authors first briefly mention that indeed the first mode of the non-normalized anomalies "spatially manifest as significant SIC anomaly signals along the Arctic marginal seas and the edge of the central Arctic" (l.220). The authors claims this is due to the summer signal; my guess is that this should also include the overall trend. The authors then normalize the anomalies to give equal weights to other seasons. But I would not expect this normalization to remove the trend. Yet, I am surprised to not see any mention of it when analysing the EOF decomposition of the standardized anomalies. Is that because it only appears in the third or higher order mode? Or because the method does indeed remove the trend? Or because the trend is actually not a major mode of variability? If the latter, this would be a major result that should be discussed. If not, I suspect it should be hidden somewhere and needs to be analysed and discussed. Moreover, in general, EOF decomposition studies tend to first remove the trend. I believe this needs to be done here as well. Note that this is not straight-forward for sea ice concentration, as this typically leads to sea ice concentrations above 1 at the beginning of the period of interest, and that a trend needs to be computed for each day-of-year (e.g. Richaud et al., 2025 for an example of day-of-year trend calculations for atmospheric variables).

A comparison confirms that detrending does not affect our conclusions, as the long-term trend of sea ice concentration in the central Arctic is weak. Your suggestion has improved the robustness of our results. We have supplemented Fig. 3 and Fig. S8 and added related discussions in the manuscript.

To further verify the robustness of these modes to methodological choices, we examined the effect of SIC detrending on the EOF results. Results show that the spatial patterns and variance contributions of EWSM and PASM remained nearly unchanged regardless of detrending (Fig. S8). Notably, after detrending, PASM became the third mode, while the Centrally Uniform Mode shifted from the third to the second mode (Li et al., 2026). The fourth mode was also highly similar before and after detrending (Li et al., 2026; Fig. S8).

In summary, the long-term trend did not alter the spatial patterns of the dominant EOF modes, but only swapped the order of the second and third modes. This indicates that the EOF-derived modes mainly reflect internal variability of the central Arctic sea ice, rather than signals driven by the long-term decline. Over the past 30 years, the long-term trend in SIC over the central Arctic has been weak, at approximately -1% per decade (Fig. 1c), much weaker than that in the marginal ice zone (Fig. 3), further supporting this conclusion. In the following sections, we present results without detrending.

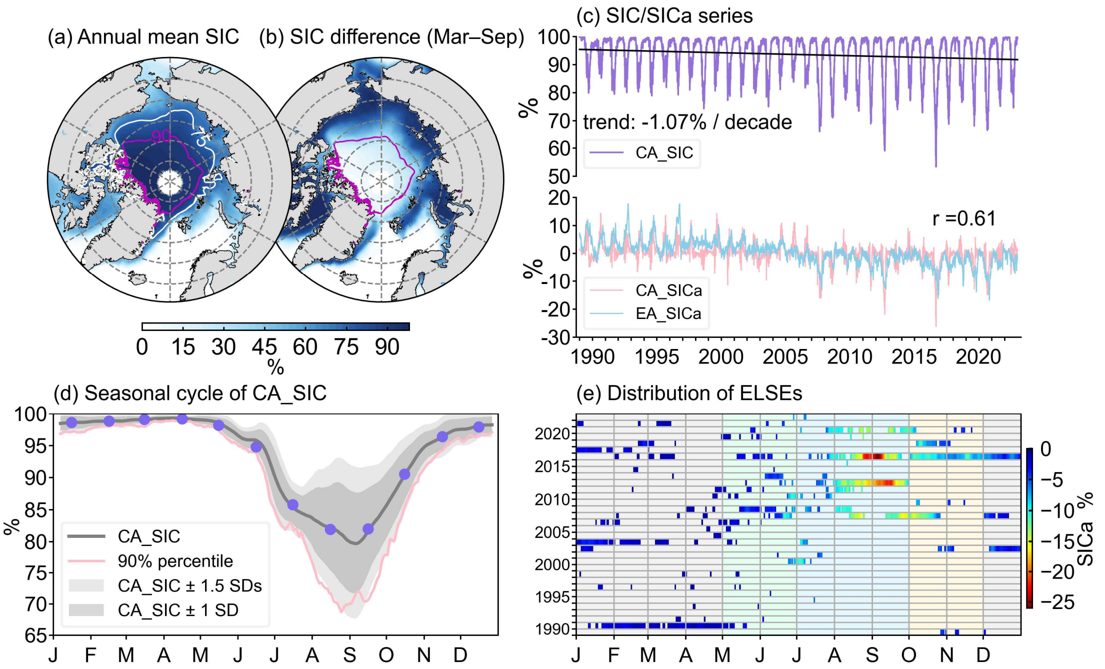


Figure 1. (a) Climatological SIC from 1989 to 2022 and **(b)** its seasonal difference (i.e., March minus September). Magenta and white curves represent the 90% and 75% SIC contours, respectively. **(c)** Time series of central Arctic SIC (CA_SIC), central Arctic SIC anomaly (CA_SICa), and entire Arctic SIC anomaly (EA_SICa). The black solid line represents the annual linear trend of CA_SIC. Letter r denotes the correlation coefficient between CA_SICa and EA_SICa. **(d)** Climatological seasonal variation of CA_SIC (gray curve and purple circles) and the 90th percentile of CA_SIC (pink solid line), smoothed using an 11-day moving window centered on each target day. The dark (light) gray shading represents the ranges within 1 (1.5) SD of CA_SIC. **(e)** Seasonal and interannual distribution of ELSEs in the central Arctic. Gray, green, blue, and golden fillings represent the winter, spring, summer, and autumn seasons defined by SIC seasonal variation according to d). Shadings represent the magnitude of SIC anomaly.

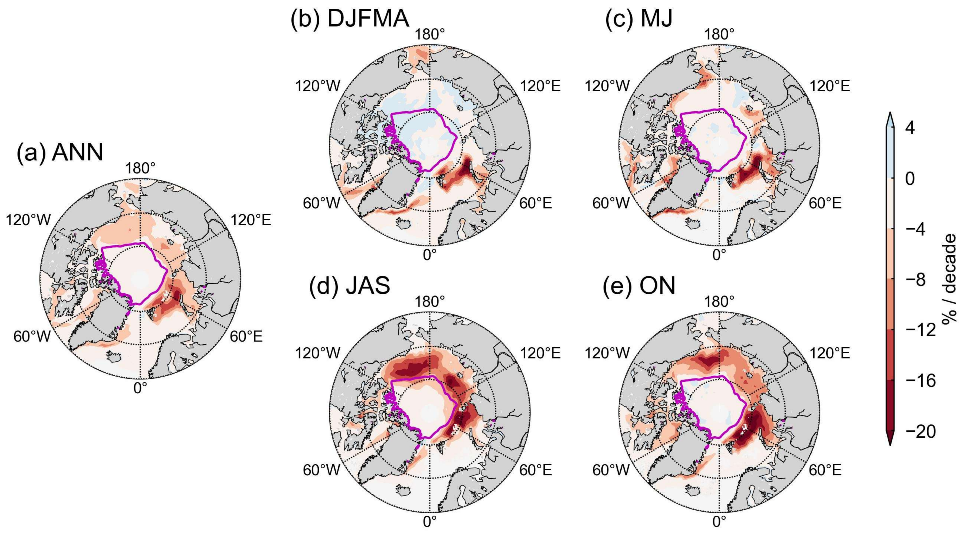
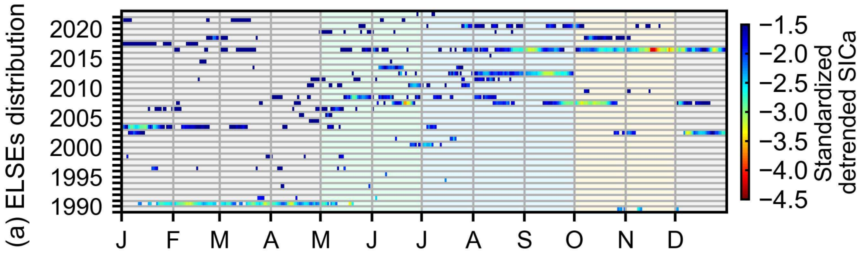
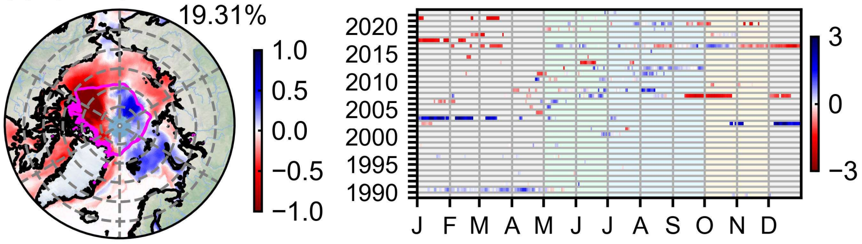


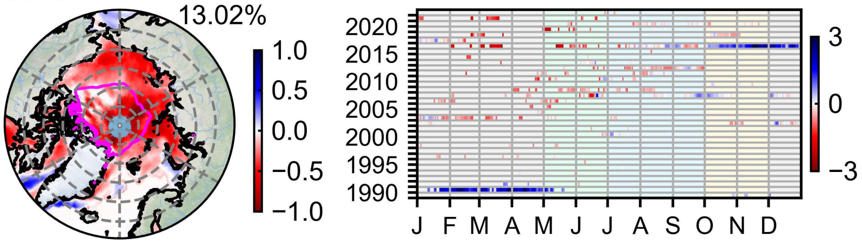
Figure 3. Long-term trend of SIC during 1989–2022 for **(a)** annual mean, **(b)** winter, **(c)** spring, **(d)** summer, and **(e)** autumn. The seasons are defined by SIC seasonal variation according to Fig. 1d.



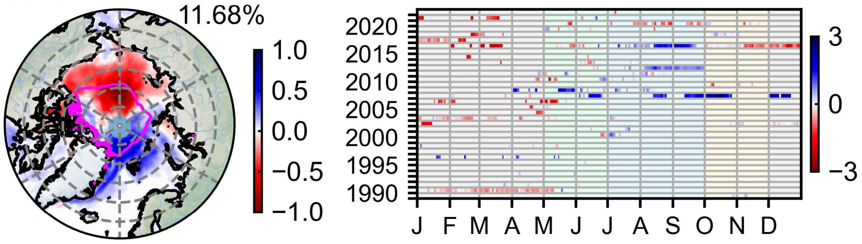
(b) Spatial pattern and PCs of EOF1



(c) Spatial pattern and PCs of EOF2



(d) Spatial pattern and PCs of EOF3



(e) Spatial pattern and PCs of EOF4

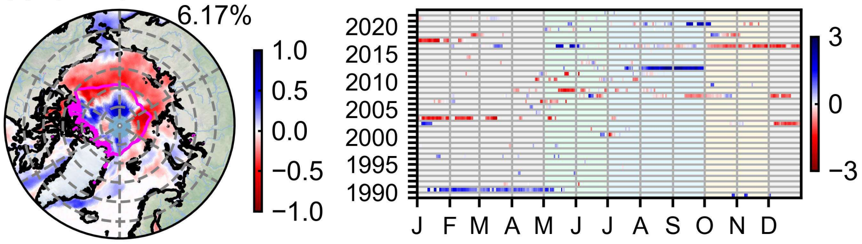


Figure S8. (a) Same as Fig. 1e but for standardized detrended results. Principal components (right) and regression fields of principal components against entire detrended Arctic SIC anomalies (left) for **(b)** EOF1, **(c)** EOF2, **(d)** EOF3, and **(e)** EOF4. Principal components have been standardized. The variance contribution is displayed at the top right corner of the regression fields. The magenta curve represents the central Arctic.

Sea ice divergence is not the same as Helmholtz divergent term

In section 2.5, the sea ice dynamical term is decomposed into an advective and diverging term (eq. 3). Then in section 2.6, a Helmholtz decomposition of the (sea ice) velocity field is done, computing the divergent and rotational term. The text leads to think that the diverging term of eq. 3 and that of eq. 7 are equivalent. This is not the case and was (still is) very confusing to me. Those kinds of Helmholtz decompositions are typically done in rheological studies, but this is not the case here. Moreover, the text gives the impression that since dynamics can be decomposed into advection and divergence, and since the velocity field can be decomposed into divergence and vorticity, then the advection is equivalent to the vorticity: “By decomposing the standardized SIM fields into divergence (Fig. 9a and b) and vorticity (Fig. 9c and d) components, it can be observed that the sea ice advection primarily drives the anomalies in sea ice motion” (l. 312). This is obviously not the case. I do not understand why the advection was not directly computed, or if it was, why it wasn’t shown and relied upon, rather than going through the rotational/vorticity. In any case, the Helmholtz decomposition does not bring anything to the study and I would suggest to drop it.

Thank you for the correction. We have removed all analyses related to Helmholtz decomposition to simplify the analytical framework, and revised and supplemented other relevant content accordingly.

During both EWSM and PASM, sea ice in the thick ice zone of the Canadian Arctic Archipelago and northern Greenland exhibits outward drift. In EWSM, the outward drift of thick ice is further modulated by the combined effects of the Beaufort Sea high anomaly and the Kara Sea low anomaly (Fig. 7a), facilitating its transport toward the Eastern Hemisphere. In PASM, influenced by the pan-Arctic high-pressure system and the high-pressure anomaly near Alaska (Fig. 7c), SIM bends inward toward the central Arctic in the Pacific sector and is subsequently advected into the Atlantic sector by the clockwise circulation (Fig. 9b). Overall, among dynamic processes, the advection term plays a dominant role in regulating sea ice variations during both EWSM and PASM (Fig. 4).

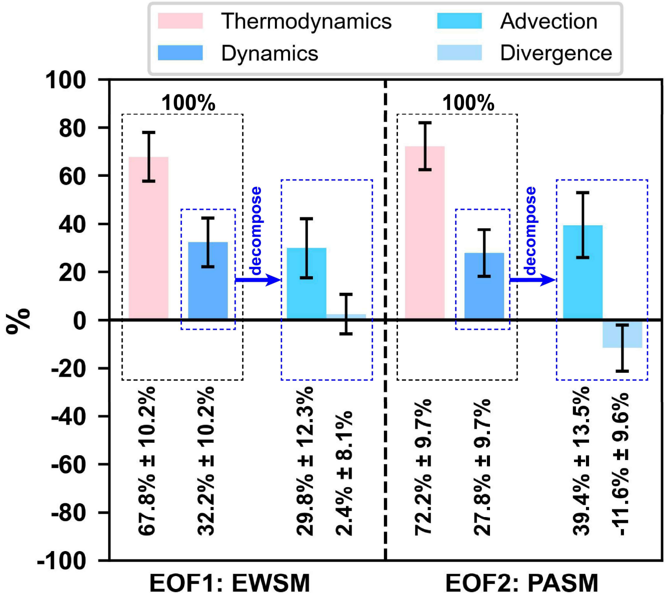


Figure 4. Effective SIT budget for the EWSM (left) and the PASM (right). Error bar represents the range of 1 SD from the mean value.

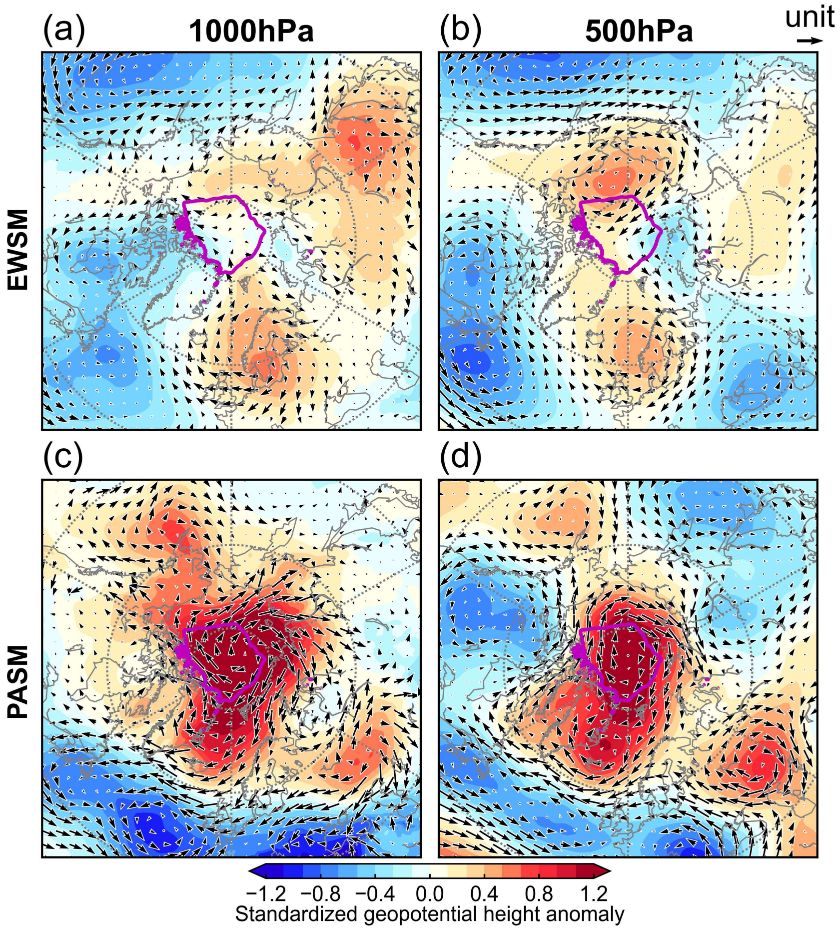


Figure 7. Composite differences between the positive and negative phases of the standardized geopotential height (shadings) and wind (arrows) anomalies for **(a, b)** the EWSM and **(c, d)** the PASM. The left and right columns show results for the 1000 hPa and 500 hPa pressure levels, respectively. The magenta line denotes the central Arctic.

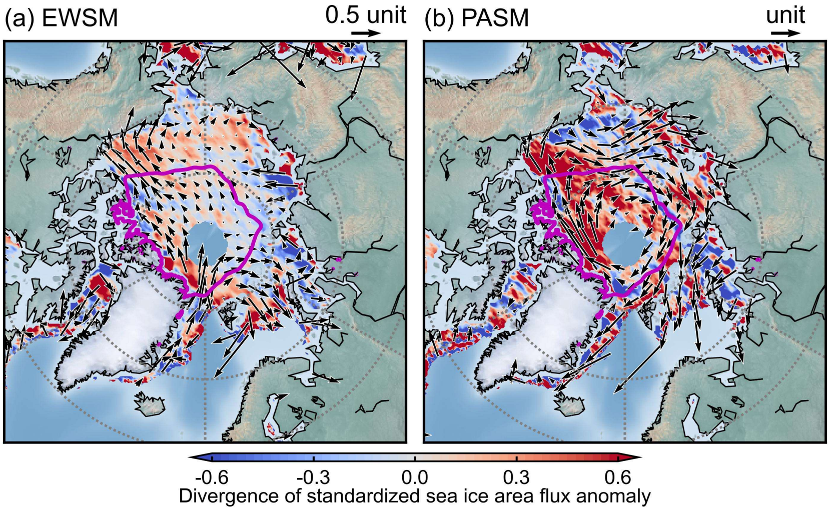


Figure 9. Composite differences between the positive and negative phases of the standardized sea ice area flux anomalies for **(a)** the EWSM and **(b)** the PASM. Arrows indicate the sea ice area flux anomalies and shadings represent their divergences. The magenta line denotes the central Arctic. Note that the scales of the arrows differ.

Many of the assertions are not supported by the analyses

"The above analysis suggests that local surface temperature anomalies caused by local diabatic heating anomalies are the important factors in the formation of EWSM and PASM." (l.243): I do not see how this sentence is supported. It suggests that atmospheric (surface) temperatures drive the two EOF modes found in the study, on the basis that the spatial patterns of the composite temperature match the EOF mode patterns. But it could very well be (and I would guess likely is) the opposite, with the ice pattern driving the temperature. This is one example, amongst many, of a causal link claim made by the authors that could very well be the other way around. And that reversed causality is never explored or mentioned. Other examples include l. 285-286, l. 317-318, l.330-332, l.341-342, l.343-345 (list not exhaustive). Moreover, some other claims in the Conclusions and Discussions section do not seem to be really demonstrated in the paper: "Under thermodynamic dominance, both the EWSM and the PASM trigger water vapor and cloud feedbacks to sustain and enhance their development" (l.386-387). Section 4.2 does discuss this but does not provide any result to prove it and Figure 7 just gives some vague (not convincing to me) spatial coherence between the different metrics. Same with l.394: "the convergence and divergence of SIM exert a minor positive contribution to the EWSM": I could not find any substantial result in the main text that support this.

Proving the causal link is complex, requires using some causality methods (e.g. Liang-Kleeman), and seems outside the scope of the study. Nonetheless, the claims of the authors are a bit too assertive to my opinion, and a more nuanced view on the direction of the links needs to be taken into account.

We agree that establishing strict causal relationships requires more dedicated causality analysis, and that some statements were assertive. We have revised the manuscript to soften the causal language and adopt more cautious and nuanced expressions across the relevant contents.

We **revised** "*This suggests that a decrease of SIC in the central Arctic has a facilitating effect on subsequent marginal Arctic SIC changes over the next two months*" **to** "*This suggests that a decrease in SIC in the central Arctic may facilitate the subsequent decline in SIC in the marginal Arctic over the next two months*"; We **revised** "*The above analysis suggests that local surface temperature anomalies caused by local diabatic heating anomalies are the important factors in the formation of EWSM and PASM*" **to** "*The above analysis suggests that local surface temperature anomalies caused by local diabatic heating anomalies may play an important role in the formation of EWSM and PASM*"; We **revised** "*Hence, the water vapor and cloud feedbacks, induced by variations in water vapor and low clouds, facilitate the formation and development of the EWSM and PASM*" **to** "*Hence, the water vapor and cloud feedbacks associated with variations in water vapor and low clouds appear to favor the formation and development of the EWSM and PASM*"; We **revised** "*This indicates that the convergence and divergence of sea ice positively contribute to the formation of the EWSM"* **to** *"This is consistent with the minor positive contribution of sea ice convergence and divergence to the formation of the EWSM*"; We **revised** "*Furthermore, the Arctic Amplification underwent an interdecadal transition in 2002 (Wang et al., 2017), which might be a result of the unique dynamical processes of the EWSM coupled with thermodynamic effects*" **to** "*Furthermore, the Arctic Amplification underwent an interdecadal transition around 2002 (Wang et al., 2017), which might be linked to the unique dynamical processes of the EWSM coupled with thermodynamic effects*"; We **revised** "*This leads to deformation and even fragmentation of sea ice in the Pacific sector. Consequently, the formation of wind ridges and ice leads is favored, with thinner ice being more likely*" **to** "*This favors deformation* *and even fragmentation of sea ice in the Pacific sector. Accordingly, these conditions are conducive to the formation of wind ridges and ice leads, with thinner ice being more likely*"; We **revised** "*Under thermodynamic dominance, both the EWSM and the PASM trigger water vapor and cloud feedbacks to sustain and enhance their development*" **to** "*Under thermodynamic dominance, both the EWSM and the PASM are accompanied by consistent water vapor and cloud anomalies, which may act as positive feedbacks to favor the maintenance and amplification of these modes*".

We have also added a discussion on the limitations regarding causal directionality to further clarify the physical linkages rather than one-way causation.

The following content has been added to the Discussion section.

While this study identifies key physical processes of co-variation between sea ice and the atmosphere, it is important to acknowledge its limitations in establishing causality. The analyses presented are primarily diagnostic and reveal robust statistical correlations and spatial configurations associated with specific sea ice modes. However, the tight coupling within the sea-ice-atmosphere system implies inherently bidirectional feedbacks. Statements implying a direct causal link should be interpreted with caution. Determining the directionality of the mechanisms proposed here would require targeted numerical experiments or advanced causality analyses (Granger, 1969; Liang, 2014, 2016), which are beyond the scope of the current observational study. Future work should focus on applying these methods to validate and refine the causal pathways suggested by our findings.

Many important aspects of the methodology are missing

The description of the methodology at the moment does not allow to reproduce the results. For example, the temperature tendency description (section 2.4) does not mention if this is for surface (2m) temperature, atmosphere-integrated temperature), boundary layer temperature or else. See also above for the lack of description of the trends of sea ice concentration, and other variables as well, if any trend is accounted for. The calculation of the climatology is not sufficiently described (see also minor comments on a suggestion to smooth it). None of the units of the terms are ever given (if they had been, it would have become obvious that the different "divergent" terms are not the same). The temporal threshold for the detection of ELSEs is not given. Looking at the figures, there might not be any, which then is a potential point of improvement of the study (see minor comment). Finally, references are missing for nearly all important equations used in the method.

We have addressed and revised all issues raised in the comments.

1. The specific type of temperature data used has been clarified in the revised manuscript. The following explanation has been added to Eq.1: *T denotes temperature, and here we use 1000-hPa temperature.*
2. For the issue of detrending, please refer to Major Comment 5.
3. For the issue related to climatology calculation, please refer to Minor Comment 3.
4. For the issue of units, please refer to Minor Comment 7 and the first suggestion in the Figures comments.
5. For the issue of the temporal threshold for ELSEs detection, please refer to Minor Comment 4.
6. For all issues related to the figures, please refer to all suggestions in the Figures comments.
7. For the issue of missing references for important equations, please refer to Minor Comment 1.

3 Minor comments

- Many equations are not referenced, such as the temperature tendency equation, the Helmholtz decomposition, the diabatic heating rate calculation, etc. I know those are classic equations, but they can take alternative forms depending on the field of interest, and therefore a quick reference towards other papers using those equations in the same way would be relevant.

We have added appropriate references for these classic equations.

*The variations of air temperature T can be diagnosed using the temperature tendency equation (**He and Black, 2016**), as shown in Eq. 1.*

*The diabatic heating rate can also be directly estimated by summing the large-scale condensation heating rate, convective heating rate, vertical diffusion heating rate, solar radiation heating rate, and longwave radiation heating rate based on the JRA-55 datasets **(Luo et al., 2023)**.*

*Sea ice area flux, F_{SIC}, is given by Eq. 5 **(Kwok, 2009)**.*

*This paper also accounts for sea ice deformation rate ϵ, including the strain rate invariants divergence ϵ_D and shear ϵ_S **(Bouchat and Tremblay, 2017; Marsan et al., 2004)**.*

- Sea ice observation data: why only start in 1989? Sea ice concentration and motion data are available starting in 1979, which would give another decade of precious data on an else relatively short time series. This would give a more robust analysis.

You are absolutely correct. Available SIC data began in 1979. However, between 1979 and 1987, the area north of 84.5°N was subject to a satellite data Pole Hole Mask, which is too large for the central Arctic region studied in this paper. Furthermore, there is a major data gap in the period from December 1987 to January 1988. Considering these factors, the data selected for this study start from 1989.

The data user guide is available at https://nsidc.org/sites/default/files/documents/user-guide/nsidc-0051-v002-userguide.pdf.

We have briefly described the rationale for selecting the data period: *Although sea ice observations date back to 1978, the period from 1987 to 1988 contains a significant data gap. Furthermore, between 1978 and 1987, the region north of 84.5°N was affected by a large satellite Pole Hole Mask covering the central Arctic study area. To ensure both temporal continuity and spatial completeness, this study takes 1989 as the starting year.*

- Climatology calculation: the methods are not very explicit on the way the climatological mean is computed. It seems to be simply the mean of each day of the year, I suspect over the whole time series. A justified choice of the baseline would be good: 1979-2007 would avoid the recent decline period; or on the contrary only take the last 30 years to have the most recent behaviour, or the whole period? See Smith et al. (2025) for an in-depth discussion of why baseline are important. On top of that baseline aspect, it is conventional to smooth out the climatology when using daily data, by using a window around the considered day-of-year, yielding the advantage of increasing the sample size (see the MHW field of research, e.g. Hobday et al., 2016). This does not seem to have been the case in this study, when looking at Fig. 1.d).

The description of baseline calculation and smoothing procedure has been added, and Fig. 1d has been revised.

The following content has been added in the Methods section.

The climatological mean is calculated as the daily average over the entire study period, which provides a consistent reference framework for identifying extreme events. This choice is justified by the need to capture the long-term variability of sea ice conditions without obscuring the cumulative impacts of climate change. As emphasized by Smith et al. (2025), fixed baselines are optimal for retaining the link between historical thresholds and physical responses. To improve the robustness of the daily climatological mean and increase the effective sample size for each day of the year, an 11-day moving window centered on the target day is applied to smooth the daily climatological SIC field (Hobday et al., 2016).

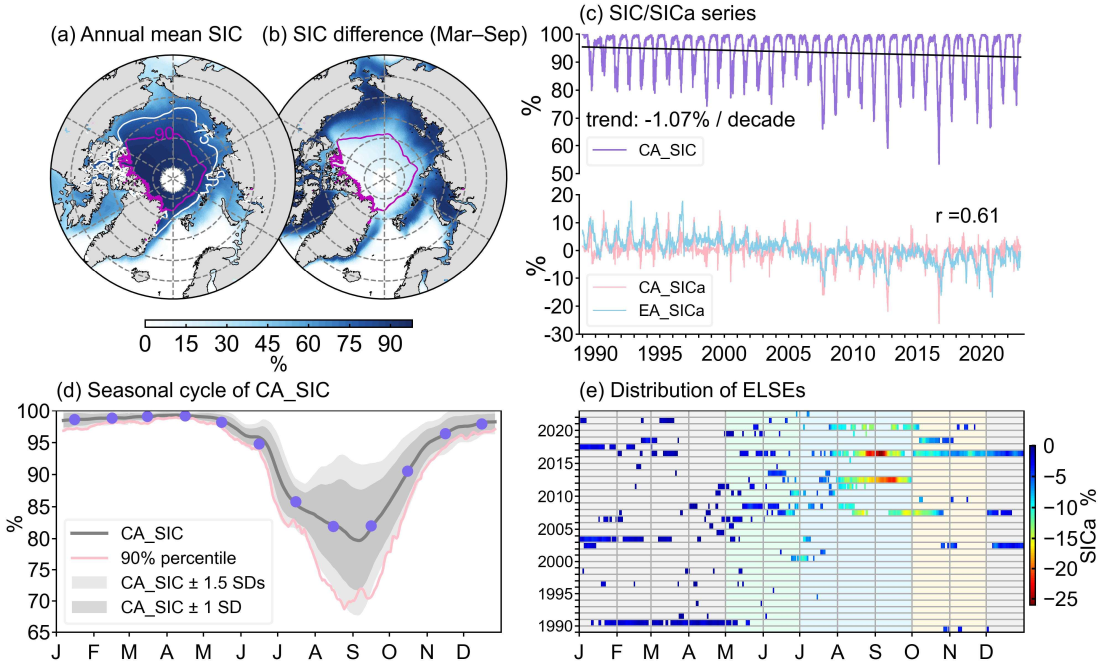


Figure 1. (a) Climatological SIC from 1989 to 2022 and **(b)** its seasonal difference (i.e., March minus September). Magenta and white curves represent the 90% and 75% SIC contours, respectively. **(c)** Time series of central Arctic SIC (CA_SIC), central Arctic SIC anomaly (CA_SICa), and entire Arctic SIC anomaly (EA_SICa). The black solid line represents the annual linear trend of CA_SIC. Letter r denotes the correlation coefficient between CA_SICa and EA_SICa. **(d)** Climatological seasonal variation of CA_SIC (gray curve and purple circles) and the 90th percentile of CA_SIC (pink solid line), smoothed using an 11-day moving window centered on each target day. The dark (light) gray shading represents the ranges within 1 (1.5) SD of CA_SIC. **(e)** Seasonal and interannual distribution of ELSEs in the central Arctic. Gray, green, blue, and golden fillings represent the winter, spring, summer, and autumn seasons defined by SIC seasonal variation according to d). Shadings represent the magnitude of SIC anomaly.

- ELSE definition: there does not seem to be any temporal threshold on the detection of the ELSEs. In other word, a sea ice concentration below the 1.5 standard deviation threshold for 1 day would count as an ELSE, as would an event lasting a year. No discussion is brought on that aspect, and it seems to me that this requires some thinking and a different definition could yield different results. Considering the different temporal scales of the atmosphere, the ocean and the sea ice, the choice of the temporal threshold would give more or less weight on events that are likely to influence larger scale dynamics. I would recommend to filter out events shorter than a specific threshold, to be justified (e.g. 10 days? 1 month?). This would likely change the results in section 3.3: why is the 2008 event included as a significant one for the PASM mode, but not 2003?

We fully agree that distinguishing between transient anomalies and persistent events is critical in defining extreme events, as the latter are far more likely to exert a significant influence on large-scale climate dynamics. Our decision to adopt a definition without a duration threshold was motivated by the aim of performing an exploratory analysis. Specifically, this approach aims to capture all sea ice anomaly days below the statistical threshold initially and without omission, and to prevent the exclusion of potential signals from short-lived extremes due to an a priori persistence threshold.

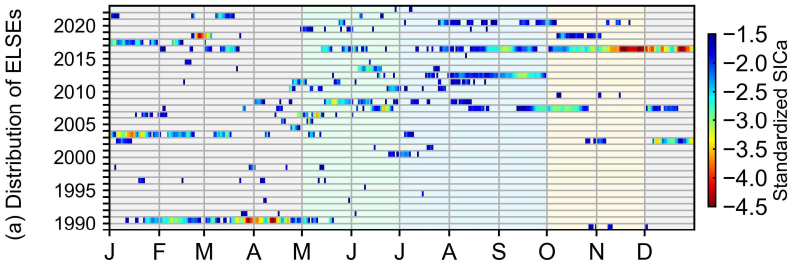
We noted this issue early in the study and briefly discussed it in the Discussion section: *By selecting ELSEs without temporal threshold based on daily data, the entire development process of an extreme case may not be fully captured. Nevertheless, it effectively explores the spatial distribution and commonalities in the causes of different types of ELSEs.*

- On the same aspect of ELSEs definition, I was very surprised to see that 2012 is not included in the list of ELSEs that are related to the EWSM and PASM modes, while it is the observational record of sea ice low. Considering its spatial pattern, I would expect it to be maybe in a PASM positive phase, but also with some EWSM negative contribution (wild, vaguely educated guess ;)). That does not seem to match Fig. 2, and I am curious as to why it is not in phase opposition with 2007 and why it is not more prominent. The study should at the very least discuss this aspect, considering the importance of 2012 in the recent sea ice evolution. Similarly, 2007 is also a very important sea ice low event: a couple of sentences on how it fits in the EOF modes story would be valuable.

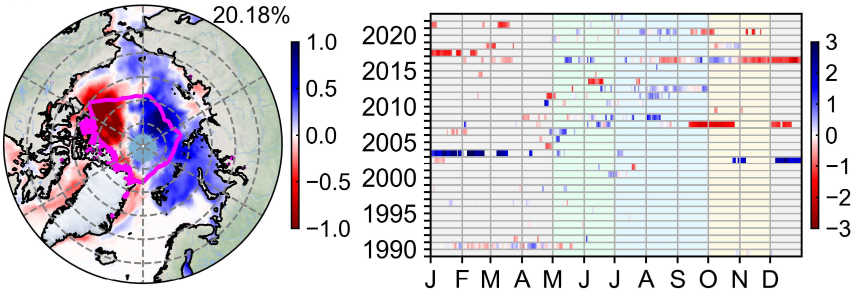
Thank you for your valuable suggestions. The years 2007 and 2012 are well-known for their extremely low Arctic sea ice extent. It is essential to discuss the relationship between the sea ice retreat characteristics in these two years and the EOF modes presented in this study.

The following content has been added to the Discussion section.

The years 2007 and 2012 are well-known for record-low Arctic sea ice extent (Parkinson and Comiso, 2013; Stroeve et al., 2008). As shown in Fig. 2, ELSEs in 2007 were most prominent during September–December, characterized by a combination of a negative EWSM phase and a positive PASM phase. This pattern is clearly visible in Fig. S16a: negative SIC anomalies in the central Arctic between 60°E and 160°W, together with positive anomalies between 60°W and 160°W, contributed to the negative EWSM phase. Meanwhile, strong negative SIC anomalies in the central Arctic between 60°E and 160°W, combined with positive anomalies over the northern Canadian Arctic Archipelago and northeastern Greenland, contributed to the positive PASM phase. Thus, the sea ice minimum in 2007 can be regarded as a combined mode of EWSM and PASM. ELSEs in 2012 were most significant during August–September, exhibiting a weak positive PASM phase with almost no typical EWSM signature (Fig. 2). In Fig. S16b, the central Arctic was dominated by negative SIC anomalies across both the Eastern and Western Hemispheres, showing little evidence of EWSM. Weak positive SIC *anomalies north of the Canadian Arctic Archipelago and Greenland, together with strong negative SIC anomalies in the Pacific sector, resulted in a weak positive PASM phase. The year 2012 is not only famous for its record-low sea ice extent but also for intense summer cyclone activity. A previous study indicated that an extreme cyclone was a key driver of pronounced sea ice melt in the Chukchi Sea during the summer of 2012 (Tian et al., 2022), while its impact on the Atlantic sector was relatively weak. However, 2012 featured frequent cyclones, with typical tracks moving from northern Eurasia toward the Pacific sector. These cyclones enhanced thermodynamic–dynamic coupling, accelerating sea ice retreat across the Arctic, particularly in the marginal seas (Lukovich et al., 2021). Consequently, the spatial pattern of ELSEs in 2012 cannot be well characterized by EWSM and PASM alone (Fig. S16b).*



(b) Spatial pattern and PCs of EWSM



(c) Spatial pattern and PCs of PASM

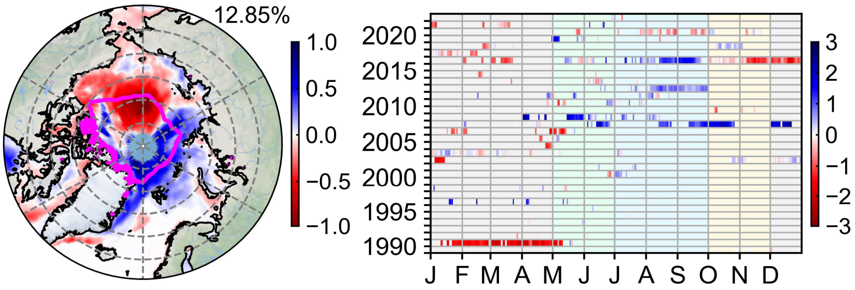


Figure 2. (a) Same as Fig. 1e but for standardized results. Principal components (right) and regression fields of principal components against entire Arctic SIC anomalies (left) for **(b)** the EWSM and **(c)** the PASM. Principal components have been standardized. The variance contribution is displayed at the top right corner of the regression fields. The magenta curve represents the central Arctic.

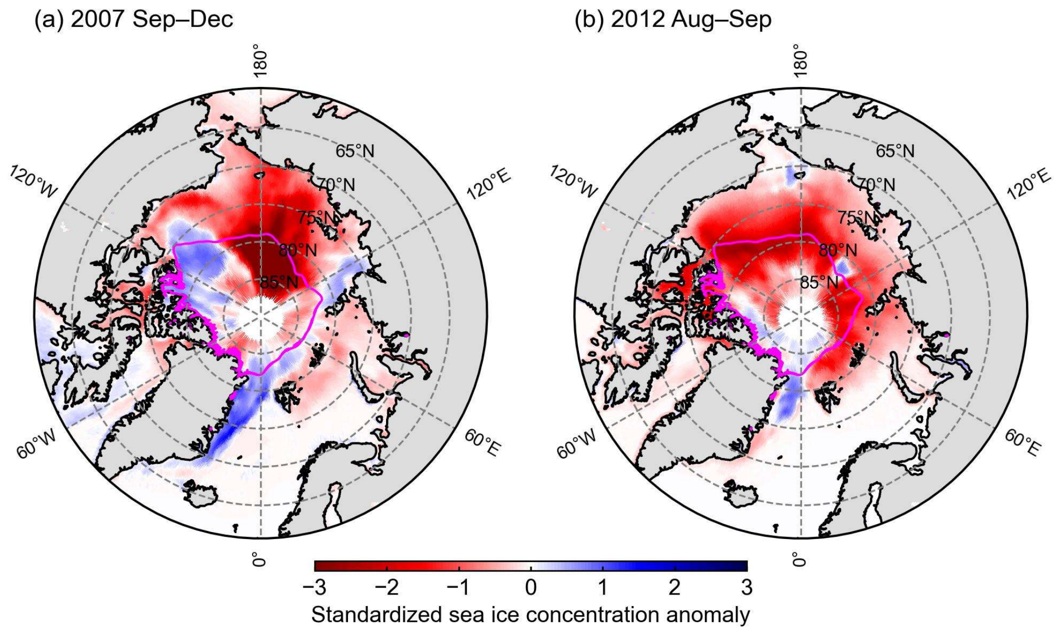


Figure S16. Composite standardized SIC anomalies of ELSEs for **(a)** September–December 2007 and **(b)** August–September 2012.

- The text only mentions the first two modes, which seem to explain 33% of the total variance. This means the other modes are also important. It seems that the authors would like to discuss the other modes in another study, but I think it would be important to at least mention how quickly the explained variance decreases with the other modes. Moreover, the fact that the first two modes explain “only” 33% of the total variance of sea ice concentration standardized anomalies seem to indicate that the EOF decomposition might not be the best approach to explain the variability of sea ice. A discussion on this aspect seems important.

We have added a brief discussion on the variance contributions of the EOF modes: *The explained variance decreases gradually with higher-order modes, with EOF3 and EOF4 accounting for 8.31% and 5.43% of the total variance, respectively (Li et al., 2026). Although the first two EOF modes collectively explain only 33.03% of the total variance, such a moderate cumulative explained variance is common in Arctic sea ice studies (Yu and Zhong, 2018). The first two modes still represent the dominant and physically meaningful patterns of the ELSEs in the central Arctic.*

- Figure S7 shows the composite differences of the standardized anomalies for the different heat fluxes. First, decomposing the radiation flux into solar (shortwave) and thermal (longwave) radiation would be valuable. Second, no description of how those composite differences and anomalies are computed, leaving the reader guessing the methodology. Is that the standardized anomalies of each flux? Or is it the composite anomalies of the fluxes but for the EOF modes of the standardized anomalies (of sea ice)? No units, no labels are given on the colorbar to help me decide. But I suspect it is the first option, looking at the colorbar values. If that’s the case, it means each flux is normalized by its own standard deviation. But then we certainly cannot compare those fluxes together! The absolute value of the latent heat flux could be (and likely is, to the best of my knowledge) orders of magnitude smaller than the absolute value of the radiative fluxes: we have no information on that aspect in this manuscript. If that is the case, the claim that "Fig. S7 indicates that latent heat flux anomalies are prominent in the formation of the EWSM and PASM" (l.249-250) could be wrong, unfortunately.

We appreciate your suggestions and corrections. We have revised Fig. S7 (**now Fig. S10**) and corrected the corresponding text. We **revised** "*Furthermore, Fig. S7 indicates that latent heat flux anomalies are prominent in the formation of the EWSM and PASM*" **to** "*Furthermore, Fig. S10 indicates that downward longwave radiation flux anomalies are prominent in the formation of the EWSM and PASM*".

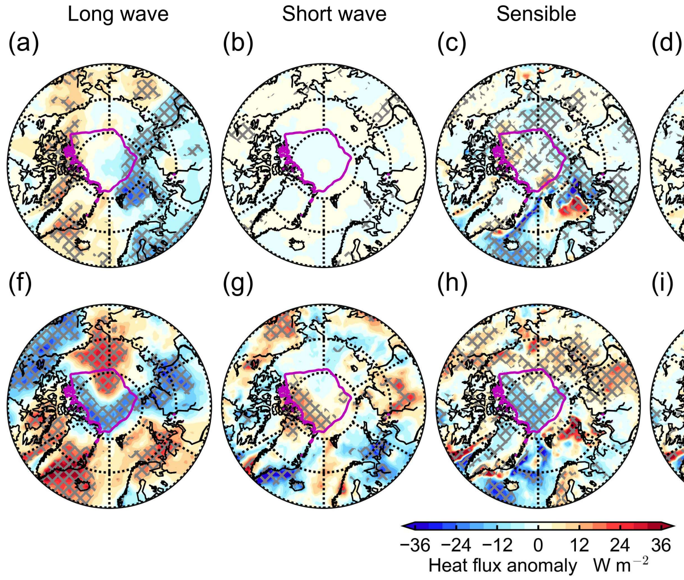


Figure S10. Composite differences between the positive and negative phases of the anomalies for **(a–e)** the EWSM and **(f–j)** the PASM for **(a, f)** surface downward long wave radiation flux, **(b, g)** surface downward short wave radiation flux, **(c, h)** sensible heat flux, **(d, i)** latent heat flux, and **(e, j)** total heat flux. Positive values indicate downward flux. The magenta lines denote the central Arctic. Grey crossings indicate areas with significant difference at the 0.05 significance level based on Student's t-test.

- On a related topic, the reader misses critical information to understand how the SIM anomalies are standardized: are U&V standardized by the total velocity SD, or by U SD for U and by V SD for V? I suspect the first option, as the second would not make any sense and would prevent any comparison, but some weird patterns on Fig. 8 (e.g. on the Siberian Shelves for 8.b) cannot take off my mind that the second option might be used. Please provide the necessary details.

We adopted the first method. As you pointed out, the second method is methodologically flawed, specifically, as it would lead to erroneous sea ice drift directions. We have provided the necessary clarification in the revised manuscript: *Unless necessary, the subsequent results will primarily present the standardized anomaly fields. Note that when standardizing SIM variables, standardization should be applied to the total velocity rather than to the U and V components separately.*

- Divergence: "There is an out-of-phase relative variation of D between the Eastern and Western Hemispheres composited for EWSM mode" (l. 315). As already mentioned in the major comments, I am very confused by the use of sea ice divergence in the sea ice (thickness) budget and in the Helmholtz decomposition; there is also a third “divergence” used in this study, defined by eq. 6 and written DF. This is not the same divergence as the other two, since it uses SIC, instead of Heff for the sea ice budget divergence and no sea ice for the Helmholtz decomposition. Yet, it does not seem to be distinguished in this section. I suspect that the DF and the ice thickness divergent term might be similar, but they would still show some differences.

As stated in the response to the fifth Major Comment, we have removed the analysis of Helmholtz decomposition. Furthermore, to avoid confusion, we have added a comparison between the sea ice budget diagnosis and DF in the methodology section: *Note that, in contrast to the sea ice budget analysis, the* D_F *term here is calculated using SIC. The use of sea ice effective thickness in the sea ice budget has been justified previously. In comparison, SIC is adopted here because it is computationally efficient and fully aligned with our analytical objectives. Although minor discrepancies exist between results derived from sea ice effective thickness and SIC, the two approaches provide distinct yet complementary perspectives for interpreting the EOF modes.*

- The whole section 6 on the Rossby wave source and teleconnection seems out of place. It does not use the same approach (why not use the composite differences as done for all other aspects?), it is not clearly linked to the EOF modes, Fig. 11 only shows the Pacific side of the Arctic (why not the rest) and its contribution to new scientific knowledge is not obvious to me (I believe the Pacific role in the Rossby wave generation is well known and that the spatial pattern of the T-N wave activity flux has also already been documented extensively). Therefore, I do not understand the contribution of this section to the general scientific knowledge. But I admit that this is a bit far from my domain of expertise and I might simply not be knowledgeable enough to understand its significance. I let the other reviewer(s), the editor and the authors judge on that aspect.

First, the core objective of Section 6 is to dynamically link the sea ice anomaly modes over the central Arctic identified in this study to the source regions and propagation pathways of mid-latitude waves. This work thus builds a bridge between the two research perspectives of “local sea ice-atmosphere interactions” and “mid-latitude teleconnections”, which carries certain implications for future research.

Second, we chose the continuous case from 2002 for demonstration, primarily considering the prominent temporal propagation characteristics of waves. In fact, the results derived from the composite difference method are nearly identical to those in Fig. 11, which further validates the representativeness of the selected typical case (**Fig. R1**). We **revised** “*Considering that the most pronounced EWSM signal during the winter of 2002 and the propagation speed of Rossby waves, we focus primarily on the autumn and winter of 2002*” **to** “*Considering that the most pronounced EWSM signal occurs during the winter of 2002 and given the temporal propagation characteristics of Rossby waves, we primarily present the results for the autumn and winter of 2002. Results obtained using the same positive–negative phase composite analysis as above are nearly identical (not shown)*”

Finally, Fig. 10 shows that the T-N wave flux propagates eastward from the North Pacific. The RWS is significant at the 0.01 level based on Student’s t-test only over the North Pacific (**Fig. R2**). We therefore believe that the RWS in the North Pacific acts as the primary source of the T-N wave flux shown in Fig. 10. To emphasize the meaningful signals and minimize distraction, we present only the North Pacific domain in Fig. 11. In consideration of your comment, we have added textual descriptions related to Fig. 11 in the revised manuscript to minimize reader confusion: *Figure 10 shows that the T-N wave flux propagates eastward from the North Pacific. The RWS is significant at the 0.01 level based on Student's t-test only over the North Pacific; thus, only the North Pacific is shown in Fig. 11.*

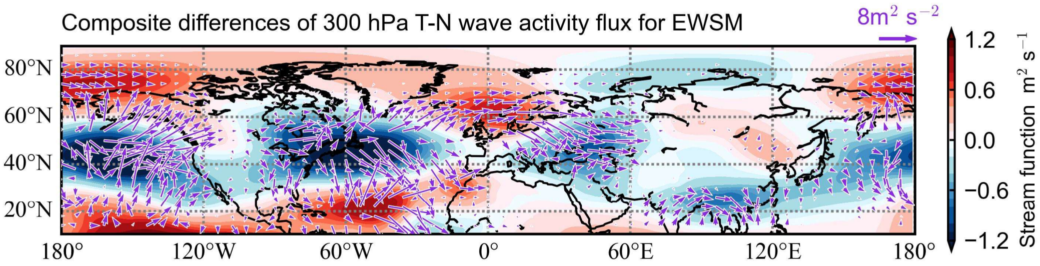


Figure R1. Composite differences between the positive and negative phases of 300 hPa T-N wave activity flux for the EWSM. Shading represents the stream function.

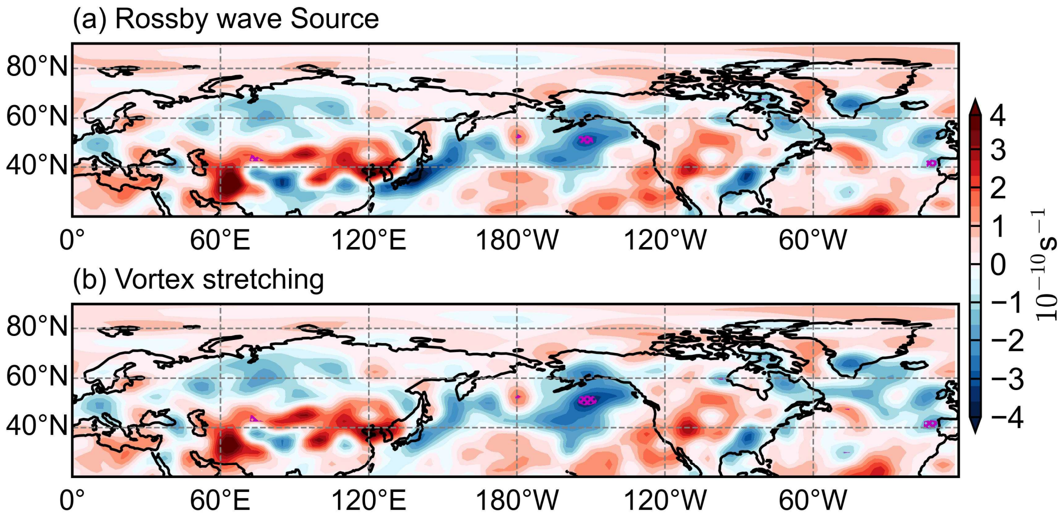


Figure R2. Same as Fig. 10a, b but for 20°N–90°N, 180°W–180°E. Magenta crossings denote areas with significant differences at the 0.01 significance level based on Student's t-test.

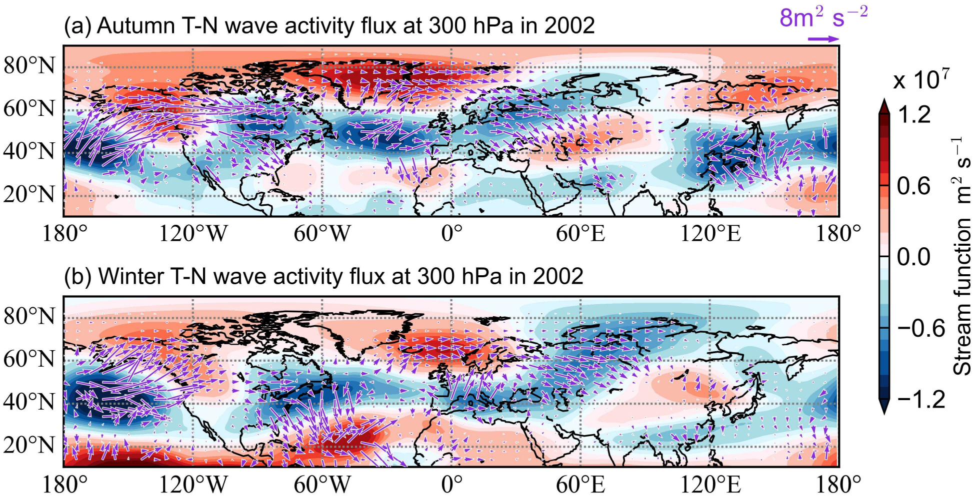


Figure 10. (a) Autumn (September–November) and **(b)** winter (following December–January) T-N wave activity flux (arrows) at 300 hPa in 2002. Shading represents the stream function.

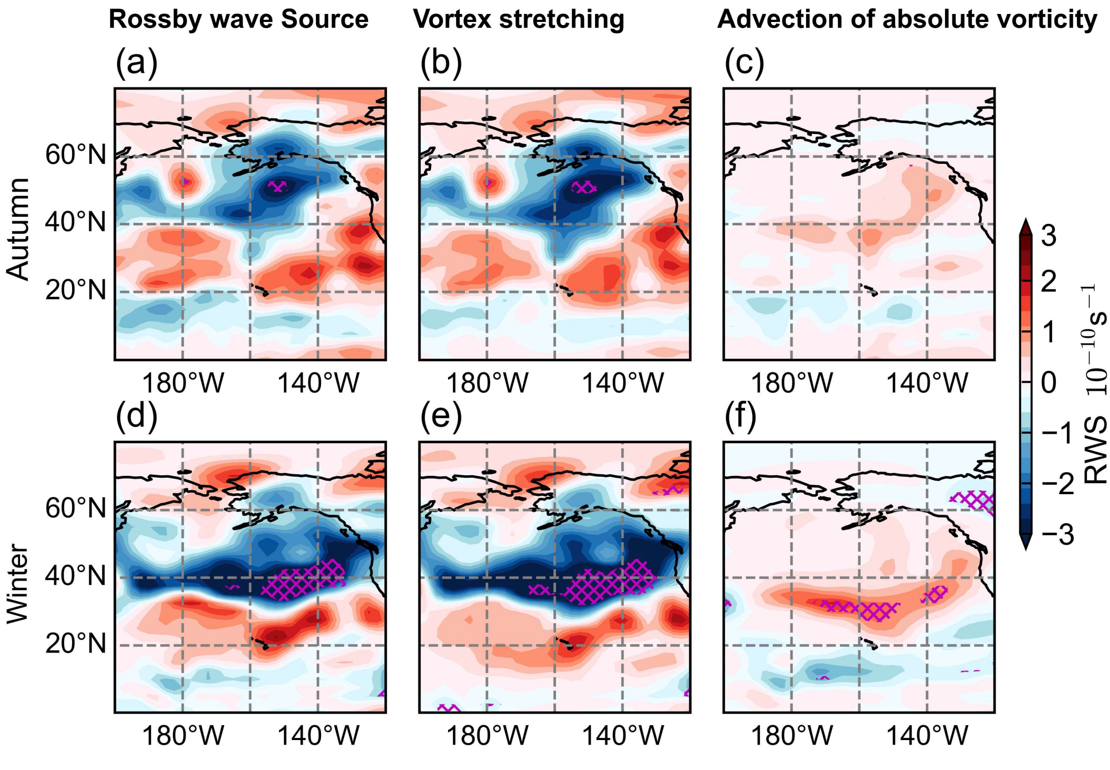


Figure 11. (a) RWS at 300 hPa, **(b)** the vortex stretching component, and **(c)** the advection of absolute vorticity component in autumn of 2002. **(d)–(f)** are same as **(a)–(c)**, but for the winter of 2002. Magenta crossings denote areas with significant difference at the 0.01 significance level based on Student's t-test.

- Oceanic discussion (l. 413-435): This is the first and only time that the ocean is considered in the story. Unfortunately, the Sverdrup balance is not valid for the Arctic (e.g. Timmermans & Marshall, 2020), and the inflow of warm Atlantic water is governed by a complex set of atmospheric and oceanic interactions. Moreover, the Sverdrup balance is brought up in an attempt to explain discrepancies between Fig. 5b and S10e. The issue is that those are not showing the same thing at all! Fig. S10e shows surface temperature anomalies (in K) while Fig. 5b shows the diabatic rate (which should be in K/s); comparing that latter figure to the temperature tendency anomalies could be done, but not to the actual temperature anomalies. Hence, we should not expect a similarity between Fig 5b and S10e. Regarding the oceanic heat transport in the Arctic, a good source of information is Docquier & Koenigk (2021) and Docquier et al. (2021). Check also Polyakov et al. (2023). Many other papers investigate the role of heat inflow into the Arctic and would be a much more reliable and convincing source of explanation of Atlantification than the proposed hand-wavy Sverdrup balance that is not applicable.

We acknowledge that the discussion lacks sufficient scientific justification. The meridional gradient of the Coriolis parameter is weak in the Arctic, rendering the traditional Sverdrup balance inapplicable. We have removed the erroneous and inadequately justified inference and revised the discussion in this section by incorporating the role of oceanic processes. **Please refer to Major Comment 3 for the revised content.**

4 Suggestions, technical details and typos

4.1 Text

- Use of “vorticity” (e.g. l. 313), “rotational” (e.g. l. 154) and “nondivergent” (e.g. Fig. 9) names for the same term: please pick one term and stick to it. Same for divergent vs. irrotational.

We uniformly adopt the terms “vorticity” and “divergent”. In fact, the relevant revisions have been deleted in response to the ninth minor comment concerning Helmholtz decomposition.

- l.10: remove “First,”

Removed.

- l.14: “which highlight common characteristics of sea ice variations.” We could argue that this is not really the case, and that concentration anomalies are simply a convenient metric to observe, but that ice thickness would be much better to really understand sea ice variation.

We intended to show that the standardized anomaly field highlights similar spatial patterns in absolute SIC changes of different magnitudes, and this sentence is removed to avoid reader confusion.

- l. 28, “approximately twice the global average”: the most recent estimates rather suggest 3 to 4 times (e.g. Rantanen et al. 2022)

The restructured content is “*Amidst the background of global warming, the Arctic is warming at a* *rate roughly twice the global average—a phenomenon widely known as Arctic Amplification (Pithan and Mauritsen, 2014; Serreze and Barry, 2011). However, recent studies have indicated that this amplification has been underestimated: both observational and modeling studies now reveal that the Arctic is warming at a rate about four times (even potentially exceeding eight times during winter) faster than the global average (Chylek et al., 2022; Davy and Griewank, 2023; Rantanen et al., 2022)*".

- l. 33, "This rapid warming in the Arctic has led to a significant decline trend in Arctic sea ice ": one could argue that the decline in Arctic sea ice has led to the rapid warming, more than the opposite (albedo positive feedback). Please nuance this sentence.

We nuance the sentence: *This rapid warming and the significant declining trend in Arctic sea ice are closely coupled (Cai et al., 2021a; Nghiem et al., 2007; Park et al., 2020; Roach and Blanchard-Wrigglesworth, 2022; Sumata et al., 2023; Yang and Magnusdottir, 2018; Yu et al., 2020; Yu and Zhong, 2018).*

- l. 47: Voosen (2020) seems like a journalistic piece, not a scientific article. While it is an interesting one, please provide a peer-reviewed paper. Moreover, I could not find any part of that (short) piece that talks about a dynamic and thermodynamic coupling... was that LLM-generated?

We appreciate your suggestions and have cited another scientific paper ***(Wang et al., 2020)***. This sentence is not LLM-generated. Although the coupling of thermodynamic and dynamic effects is not directly stated in this journalistic piece, the positive feedback loop formed by processes such as wind-induced sea ice deformation, wave-driven sea ice fragmentation, increased open water, and Atlantification mentioned therein precisely represents the outcome of the coupling of thermodynamic and dynamic effects. Therefore, this conclusion is summarized by us based on the journalistic piece.

- l. 51: "Arctic sea ice concentration" or "extent"

We have revised "Arctic sea ice" to "Arctic sea ice concentration".

- l. 54: Sticker et al. (2025) would be a good, recent addition here
- l. 61: Hoffman et al. (2025) would be a good, recent addition here

Thanks for the valuable references you suggested. We have cited them.

- l.63, "the reduction of Arctic sea ice is spatially heterogeneous, which is attributable to the spatial variation of thermodynamic and dynamic processes driven by atmospheric and oceanic circulation. [...] The trend of sea ice reduction is notably significant in the marginal seas along the Eurasian and the North American coast, while the sea ice from north of Greenland and Canadian Arctic Archipelago to the pole remains relatively stable". I agree that atmospheric and oceanic circulation play an important role in generating spatial variability, but not those described by the text. The difference between shelves and northern part is simply due to astronomical considerations (less solar radiation close to the pole than further south, e.g. Maksym 2019) ... Please rephrase.

Our original aim was to show that sea ice decline is pronounced in the Arctic marginal seas, but relatively weak in the central Arctic as defined in this study. The spatial heterogeneity we emphasize is not simply greater melting in the southern Arctic than in the northern Arctic. In fact, the spatial pattern of Arctic sea ice decline does not follow a simple latitudinal ring-like distribution, largely due to atmospheric and oceanic circulations. We appreciate your comment; our original statement was unclear, and we have revised this paragraph by incorporating your point on solar radiation.

Revisions are as follows.

Additionally, the reduction of Arctic sea ice is spatially heterogeneous. The trend of sea ice reduction is notably significant in the marginal seas, while the sea ice from north of Greenland and the Canadian Arctic Archipelago to the pole remains relatively stable (Fig. 4c of Roach and Blanchard-Wrigglesworth 2022). The difference between the shelves and the northern part arises primarily from astronomical factors: solar radiation is lower closer to the pole than at lower latitudes (Maksym, 2019). Moreover, spatial variations in thermodynamic and dynamic processes driven by atmospheric and oceanic circulations also play an important role (Cai et al., 2021b; Spreen et al., 2020; Sumata et al., 2022; Wu and Ding, 2023).

- l.71, “the perennial sea ice in the central Arctic has begun to undergo extreme reductions in recent years”: “recent” is subjective, but it has been a few decades by now, so I would remove the “begun [...] in recent years”

Revised.

- l.76, “This is because the sea ice in the central Arctic region is predominantly multi-year thick ice, and the absolute value of sea ice variation in winter and spring is relatively small”. It is also (and maybe primarily) because the winter and spring sea ice extent is strongly geographically constrained by the surrounding continents, and that there is therefore less degree of freedom (e.g. Maksym, 2019). Please rephrase.

Thanks for your suggestion. We have rephrased it: *This is primarily because the winter and spring sea ice extent is strongly geographically constrained by the surrounding continents, and therefore there is a lower degree of freedom (Maksym, 2019). Another reason is that the sea ice in the central Arctic region is predominantly multi-year thick ice, with relatively small absolute variations in winter and spring.*

- l. 179-186: This is a good introduction, though maybe a bit too detailed. You could shorten to only highlight the relevant definition.

The text has been condensed. We have removed “*Politically, Van Pelt et al. (2017) defined the waters north of the exclusive economic zone boundaries of the five Arctic coastal states as the Central Arctic International Waters. To reveal the distributional features of Arctic cyclones, Kong et al. (2024)*

defined the cyclone-active region within the Arctic Ocean, the Alpha Ridge area (78°N–90°N, 150°E–180°–90°W), as the central Arctic"

- l.187, "climatological SIC": is that annual mean? seasonal? day-of-year? Maybe worth considering a time-varying definition. See also minor comment for smoothing suggestion, to have a more statistically robust definition. If it is annual (as Fig 1 seems to suggest), it needs some discussion, as it will induce a significant ELSE detection bias between winter and summer.

This value represents the annual mean, and the relevant sentence has been revised: *This study applies the methodology of Huang et al. (2021) to define the central Arctic as the area where the* ***annual mean*** *climatological SIC from 1989 to 2022 exceeds 90%.* We define the study area as the region where the climatological sea ice concentration exceeds 90%, and this region remains fixed throughout the analysis. The objective of this study is to investigate the variability of sea ice in regions with generally high sea ice concentration, rather than to focus on a dynamically changing region where the sea ice concentration is above 90% at all times. Since the area with summer sea ice concentration above 90% is very small and provides limited physical information, we did not adopt a time-varying definition. This choice is justified by the discussion of Fig. S3.

Regarding the smoothing suggestion, please see the response to the third minor comment.

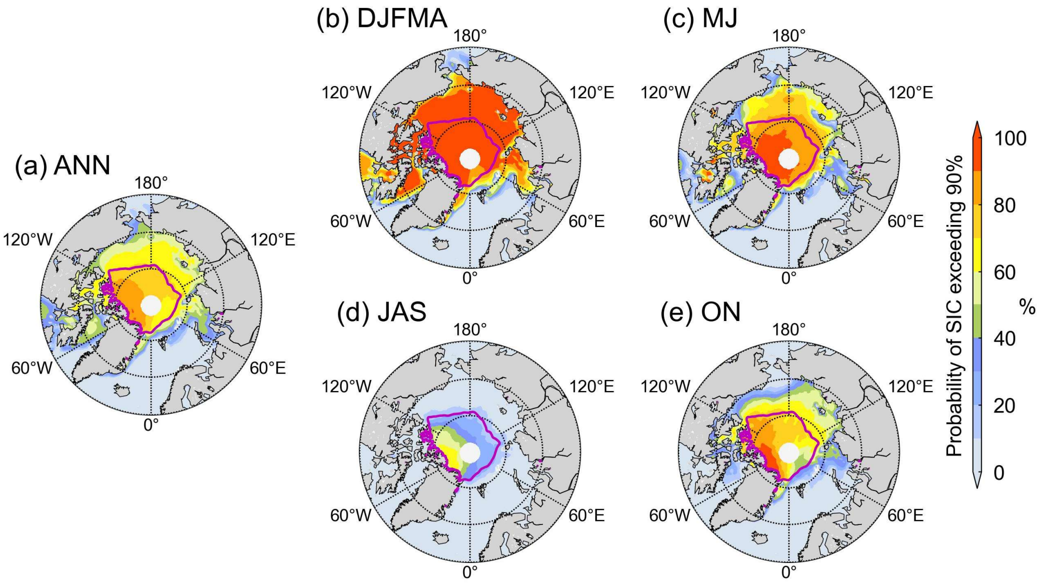


Figure S3. Probability of SIC exceeding 90% for **(a)** annual mean, **(b)** winter, **(c)** spring, **(d)** summer, and **(e)** autumn. The seasons are defined by SIC seasonal variation according to Fig. 1d.

- l.193, "which validates the rationality of the definition of the central Arctic in this study": except for JAS, for which the probability of SIC>90 % represents a small fraction of the Central Arctic. See above suggestion to use a time-varying definition.

Please see the response to the last suggestion.

- l.225-226: please provide references for those EWSM and PASM: are those names yours? Or do they come from other studies? Do they match other studies?

These two terms were first explicitly proposed and named by us in this study to describe the two specific sea ice retreat modes identified here. We discussed the consistency between the characteristics of sea ice retreat in key years and the two modes; see also Minor Comment 5.

- l. 241: how is the spatial correlation computed? Why is it only computed for air temperature and not for the diabatic heating, heat fluxes, etc? Also, see major comments: correlation is not causation.

The spatial correlation coefficient is obtained by flattening the two spatial fields into one-dimensional series and computing their Pearson correlation coefficient. The spatial correlation coefficient here measures the degree of similarity between the spatial patterns of the two variables. Diabatic heating, heat fluxes, and other related variables are analyzed as physical drivers of surface temperature changes. Their spatial patterns are clearly consistent with the EOF modes. As they would not add new insights to the main discussion, we chose not to present these results in detail to maintain focus and conciseness.

Regarding causation issues, please see the response to Major Comment 3.

- l. 354, "Considering that the most pronounced EWSM signal occurs during the winter of 2002 [...]"

Revised.

- l. 454-455, "The unpresented EOF3 and EOF4 in this study are primarily related to anomalous temperature advection from mid-latitudes, with significant increases in dynamic contributions.": I don't really understand the distinction made between dynamic and thermodynamic, then. To me, the advection of heat would lead to a change in thermodynamics, not dynamics, which would rather be controlled by momentum fluxes, not heat fluxes. So this sentence seems self-contracting to me.

We apologize for the ambiguity. The former and latter parts of the sentence represent two main conclusions rather than a causal relationship. The text has been revised for clarity. We **revised** "*The unpresented EOF3 and EOF4 in this study are primarily related to anomalous temperature advection from mid-latitudes, with significant increases in dynamic contributions. Their atmospheric circulation characteristics are markedly different from those of EOF1 (the EWSM) and EOF2 (the PASM), as will be discussed in future work*" to "*The unpresented EOF3 and EOF4 in this study are primarily related to anomalous temperature advection from mid-latitudes and SIM. Moreover, compared with EOF1 and EOF2, dynamic contributions become more important in EOF3 and EOF4. Their atmospheric circulation characteristics are markedly different from those of EOF1 and EOF2 (Li et al., 2026)*".

4.2 Figures

- All: please provide labels + units on all colorbars and don't hesitate to also add title above the different panels to make sure the reader can quickly understand what they are looking at. Try to be consistent and homogeneous between figures, keeping the same latitude boundaries, projections, row/column orientation, etc.

Thank you for this valuable suggestion. We have added the necessary descriptive information to all figures to improve readability. Note that most fields shown in the manuscript are standardized anomalies, thus units are not required for the colorbars in these figures.

We have standardized the figure formatting as much as possible. Figure 7 is an exception, as it is necessary to display a larger spatial domain for the wind and geopotential height fields.

Nearly all figures have been revised and are not shown here individually.

- Fig. 1: The colormap for panels a and b is not sequential, consider using another one; please mask regions where there is no ice, instead of plotting the 0% SIC. Panel e (and left panels in figure 2) are great! I would suggest making those a bit wider to see better.

Thank you for your suggestions. Figures 1 and 2 have been revised accordingly.

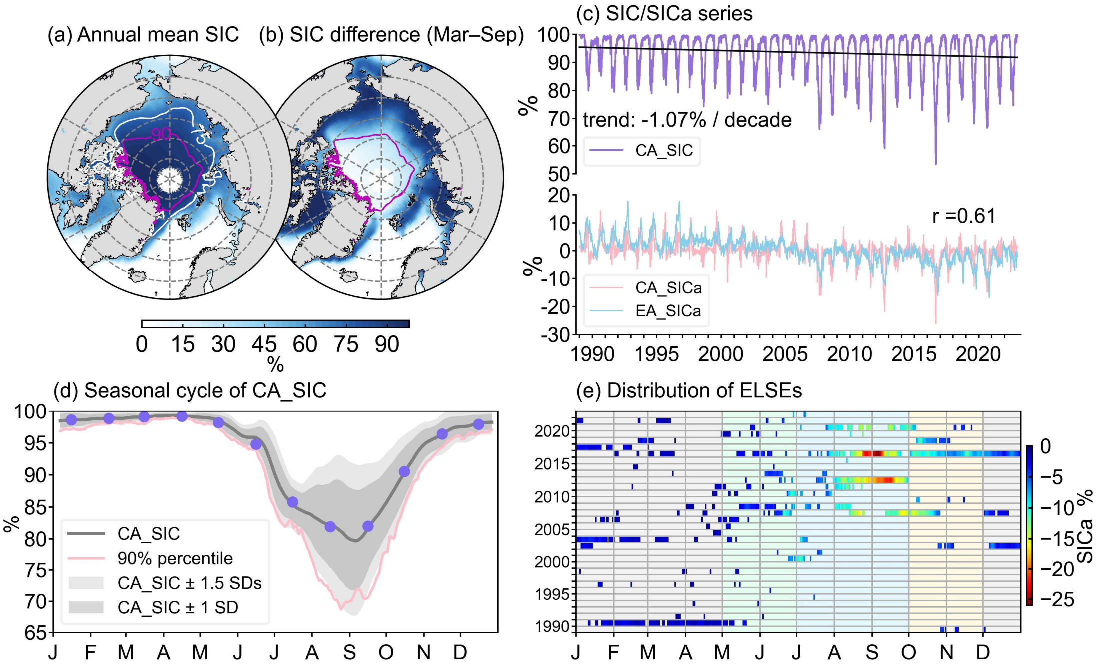
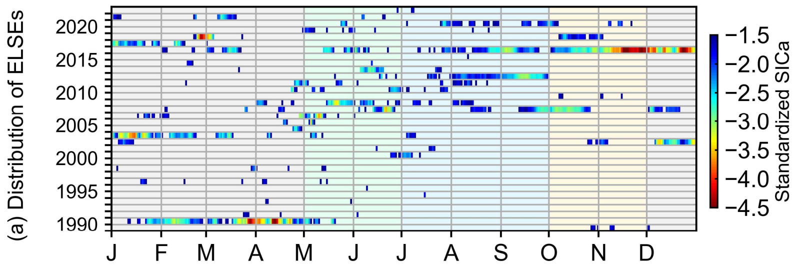
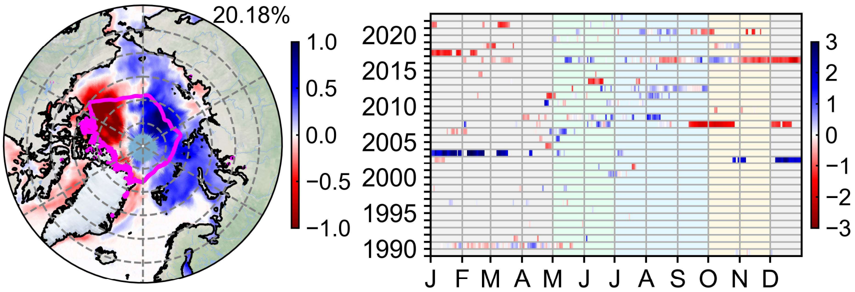


Figure 1. (a) Climatological SIC from 1989 to 2022 and **(b)** its seasonal difference (i.e., March minus September). Magenta and white curves represent the 90% and 75% SIC contours, respectively. **(c)** Time series of central Arctic SIC (CA_SIC), central Arctic SIC anomaly (CA_SICa), and entire Arctic SIC anomaly (EA_SICa). The black solid line represents the annual linear trend of CA_SIC. Letter r denotes the correlation coefficient between CA_SICa and EA_SICa. **(d)** Climatological seasonal variation of CA_SIC (gray curve and purple circles) and the 90th percentile of CA_SIC (pink solid line), smoothed using an 11-day moving window centered on each target day. The dark (light) gray shading represents the ranges within 1 (1.5) SD of CA_SIC. **(e)** Seasonal and interannual distribution of ELSEs in the central Arctic. Gray, green, blue, and golden fillings represent the winter, spring, summer, and autumn seasons defined by SIC seasonal variation according to d). Shadings represent the magnitude of SIC anomaly.



(b) Spatial pattern and PCs of EWSM



(c) Spatial pattern and PCs of PASM

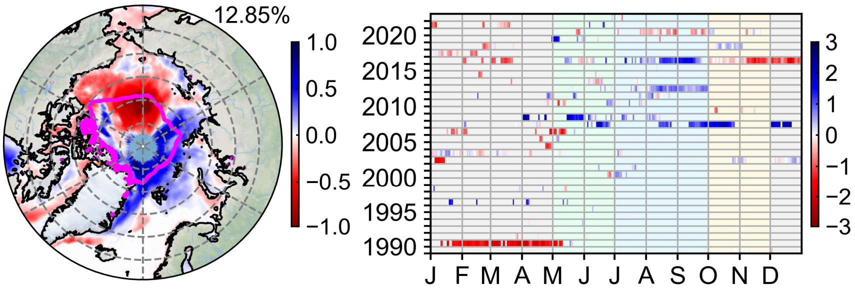


Figure 2. (a) Same as Fig. 1e but for standardized results. Principal components (right) and regression fields of principal components against entire Arctic SIC anomalies (left) for **(b)** the EWSM and **(c)** the PASM. Principal components have been standardized. The variance contribution is displayed at the top right corner of the regression fields. The magenta curve represents the central Arctic.

- Fig. 3: this figure is a bit confusing because at first glance, it is not clear that adding advection and divergence leads to the Dynamics term. Please consider stacking them to reducing their width and adding transparency to make it more obvious that there are only two terms and that you decompose one of them into two.

Thank you for this helpful suggestion. We have attempted to stack the bars as recommended. Unfortunately, after stacking, the Dynamics term becomes difficult to visualize clearly, the error bars overlap and are hard to distinguish, and the text labels for the relative contributions cannot be properly aligned. For these reasons, we have instead added clear labels in Fig. 3 (**now Fig. 4**) to help readers quickly and easily understand the relationship between the different terms.

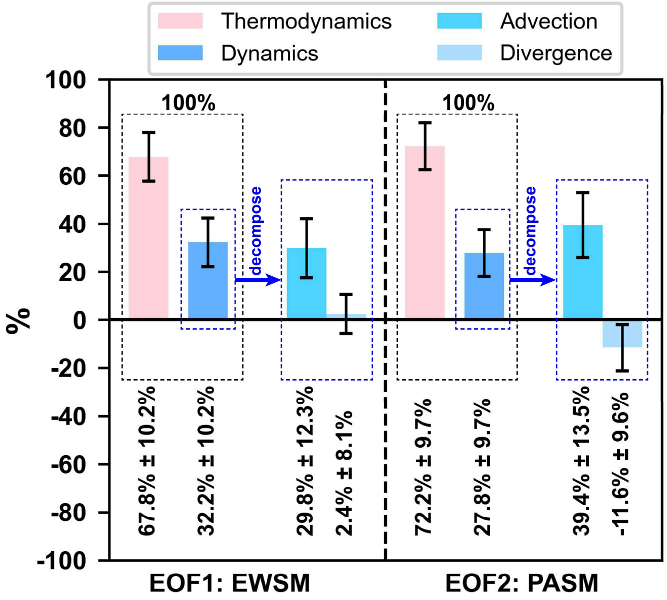


Figure 4. Effective SIT budget for the EWSM (left) and the PASM (right). Error bar represents the range of 1 SD from the mean value.

- Figures 6 and 9: I would suggest to transpose the figures, putting the EWSM and PASM as rows instead of columns, to match the other figures (e.g. Fig 7 and those in SI). Keeping the same convention would allow the reader to be able to skim through and compare the figures in a more intuitive way.

This is a valuable and detailed suggestion. Figures 6 and 9 (Fig. 6 is now Fig. 7 and Fig. 9 has been removed) have been transposed. The corresponding figure captions and in-text figure references have been revised accordingly.

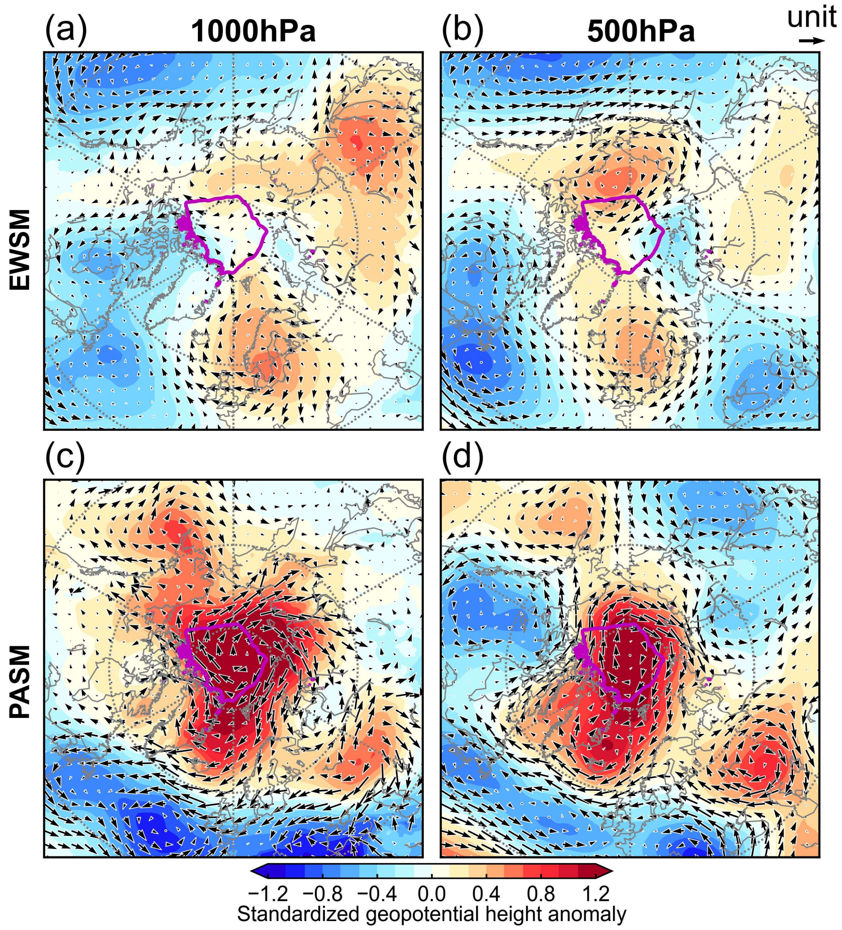


Figure 7. Composite differences between the positive and negative phases of the standardized geopotential height (shadings) and wind (arrows) anomalies for **(a, b)** the EWSM and **(c, d)** the PASM. The left and right columns show results for the 1000 hPa and 500 hPa pressure levels, respectively. The magenta line denotes the central Arctic.

- Figs. 10 and 11: Why change the projection? Other figures use a North Polar Stereo projection while 10 and 11 are cylindrical (?).

We initially plotted these two figures using the North Polar Stereographic projection, consistent with other figures in the manuscript. However, the latitude–longitude grid data become overly condensed at high latitudes under the North Polar Stereographic projection. In particular, the visualization of vector arrows is less clear than with the cylindrical projection. For example, in **Fig. R3a**, the vector arrows tend to overlap and become unclear near Greenland when using the North Polar Stereographic projection, which obscures the wave flux structure.

We fully understand your concern about maintaining projection consistency to help readers quickly compare figures. Nevertheless, to ensure the clearest visualization of wave flux characteristics, we prefer to retain the cylindrical projection in Figs. 10 and 11.

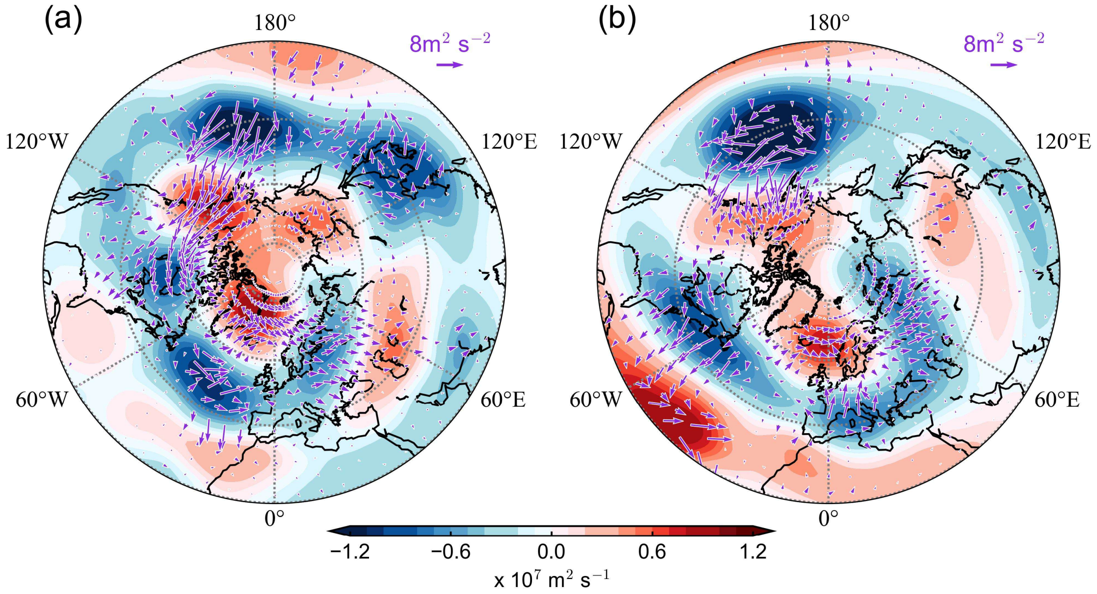


Figure R3. Same as Fig. 10 but in the North Polar Stereographic projection.

- Fig. 11: Why not plotting the whole hemisphere? At least for a) and d).

Please see the response to the tenth Minor comments.

- Fig. 12: Great schematic! Not sure it is very colorblind-friendly, but I like it. I am unfortunately not convinced by the content, because of all the reasons detailed in the major comments...

All figures were checked using the "Coblis—Color Blindness Simulator" provided by *The Cryosphere* prior to submission. We are glad you like the schematic and hope the revised version of our manuscript will convince you of its suitability.

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