Dear Reviewer 1,

We have carefully read and thoroughly evaluated each of your comments on our manuscript. We fully recognize that your constructive, in-depth suggestions are critical to enhancing the scientific rigor, narrative clarity, and overall academic impact of this work. Accordingly, we have systematically addressed every single comment, with detailed point-by-point revisions implemented in the resubmitted manuscript.

We are sincerely grateful for the time and effort you have dedicated to reviewing our work, as well as your invaluable, insightful feedback that has significantly strengthened this study.

Format specification for this response letter: Comments from the reviewer are presented in black font, our point-by-point responses in blue font, and revised manuscript text excerpts in green font.

This article presents an exploration of the processes underlying a documented recent timescale transition in Bering Sea ice area from interannual to decadal variability. The mechanisms, as suggested by the authors, are a negative feedback wherein wind field anomalies resulting from sea ice area anomalies (due to localized atmosphere-ocean heat flux changes) induce changes in wind field divergence which impact subsequent sea ice area, and a positive feedback wherein wind field anomalies resulting from sea ice area anomalies impact oceanic heat transport which then impacts subsequent sea ice area. A change in the relative prevalence of these processes over time is suggested as an explanation for the transition from interannual to decadal variability.

Weibo: We greatly appreciate this encouraging and insightful comment. This is exactly the core takeaway we hope to deliver to our readers, and we are very pleased that this key message was clearly and effectively conveyed in our manuscript.

The ideas presented are scientifically interesting and the overall structure of the article is reasonably good. The quality of the figures is good overall. I think some of the points in Section 3 could be de-emphasized as I discuss below, for clarity and brevity, since it appears to me that the more substantial points are in Section 4. The dynamical/thermodynamical reasoning in the manuscript is reasonable to me, but I have some questions about the robustness of some of the arguments used to substantiate this reasoning. Furthermore, I found it was often difficult to ascertain in the article where there are novel findings and where findings are being cited/reproduced from previous work to provide context for the article's findings. Because of this, I think the manuscript would require some substantial revisions before being suitable for publication.

Weibo: We greatly appreciate your positive assessment of the overall manuscript. We acknowledge your concern regarding the overlap between our manuscript and our prior published work (Wang et al., 2023) – including re-presented figures – which was originally included to maintain contextual continuity and improve readability. We fully agree with your comment, and have comprehensively reorganized and streamlined Section 3 in the revised manuscript.

In addition, we have implemented targeted revisions to Section 4.2. Specifically, we have re-elaborated the underlying driving mechanisms for the transition of the dominant temporal scale of sea ice variability from interannual to decadal, from the perspective of anomaly intensity. Full details of these revisions are elaborated in the subsequent point-by-point response and the corresponding section of the revised manuscript.

My general comments are included below, followed by line-by-line comments and technical corrections.

General comments:

A portion of this manuscript is closely connected to results from previous work by the authors; e.g. Wang et al. 2022, Wang et al. 2023, and Wang et al. 2024 a,b (as cited in the article references). Some of the panels in the figures are also found in previous work, and although the studies are referenced, it is not always clear which figure panels contain new work. I appreciate that the authors want to build up context from previous work to situate the current findings, but this makes it difficult for me to ascertain where the novel results in this article are located. I will try to indicate in my line-by-line comments where I see this happening but overall, I think many parts of Section 3 need revision to make it clearer where figure panels and results are reproduced from previous work.

In general, parts of Sections 3.1 and 3.2 appear to be somewhat redundant to me; I think they could be condensed or partly moved to supplements. Because the timescale transition has been documented in previous work, I do not think as much detail is necessary in establishing the context except for the specific novel results on the specific timing of the transition, as applicable.

Weibo: We fully agree with your comment, and have comprehensively reorganized and streamlined Section 3 in the revised manuscript, with detailed modifications as follows:

- First, we have removed redundant figures presenting results from our prior work: the original Figure 3 has been deleted entirely, and only subplots e, f and g from the original Figure 4 have been retained and integrated into the updated Figure 3.
- Second, we have merged the original Section 3.1 and Section 3.2, and substantially cut down redundant text that overlaps with our previous work, including the content in original Lines 202–216 and Lines 231–242.
- Finally, to further highlight the original research contributions of this work, we have added a new paragraph in revised Lines 243–251 to explicitly elaborate the novelty and advances of our study.

The authors categorize December SIA years into five categories based on normalized values which is a reasonable approach, but I wonder at how representative the heavy-ice year composites are, since only ~5 years are included. Since much of the analysis seems focused on the extremes, I wonder if the authors could consider reducing the number of categories to simplify interpretation and streamline the results (i.e. reduce to 3 categories, either combine the low/high categories into the normal category, or group them with the extremes).

Weibo: Following your valuable and targeted recommendation, we have systematically classified the observational years into three distinct groups: normal years (NM), heavy-ice years (HI), and light-ice years (LI). This classification enables a more robust, differentiated analysis of sea ice variability under different background conditions, which markedly improves the clarity and physical interpretability of our results.

To fully integrate this classification into the manuscript, we have comprehensively updated Figures 5, 7 and 10, with all panels and statistical analyses fully aligned to the three categories. We have also completed full corresponding revisions to the supporting text: specifically, we have removed outdated and redundant content in original Lines 303–310 and Lines 343–346, and thoroughly revised the core analysis, interpretation and narrative in original Lines 310–319 and Lines 346–357 to be fully consistent with the new classification framework. All revisions are fully traceable in the revised manuscript.

The information flow argument for causal influence is broadly reasonable to me; given that the SLP captured by EOF3 appears to align with the SLP patterns during extreme high and low sea ice years, the statistically significant causal influence of SIA12 on EOF3 makes sense to me, although this influence appears to have a somewhat small magnitude. The relationship to wind divergence during extreme events is also fairly convincing. I have one question on this point: why is the same causality analysis not carried out for other quantities such as SST and heat transport? Would we not expect them to also be causally linked to SIA12?

Weibo: Following your comment, we calculated the Liang–Kleeman information flow from December sea ice area anomalies (SIA_{12}) to December northward heat transport，since this study focuses specifically on the coupling relationship between NHT and sea ice, with the results illustrated in Figure 1. At the sea ice edge, the

Liang–Kleeman information flow shows significantly negative values, indicating that SIA12 exerts a constraining effect on NHT.

The primary reason for this behavior is that the wind field represents only one component in the calculation of northward heat transport. Other key factors, including sea surface temperature (SST) and sea surface dynamic height, also contribute to the total NHT and may provide additional information flows. Wang et al. (2022) previously investigated the relative contributions of SST, wind field, and sea surface dynamic height to northward heat transport using Reynolds decomposition, and clearly demonstrated that NHT is primarily modulated by SST and wind forcing. These findings have already been fully documented in their published work.

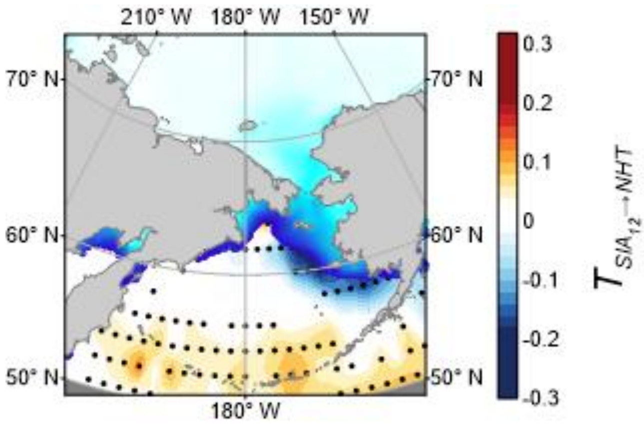

Figure 1. Liang-Kleeman information flows from SIA_{12} to December northward heat transport (NHT)

In response to your comment, we have revised Figure 10 (original Figure 11) and incorporated a new panel (c) as shown in Figure 1. In addition, a new paragraph describing these results has been added in the revised manuscript between Lines 437~442: 'The influence of wind fields on sea ice manifests not only through wind divergence but also via its modulation of NHT. We computed the Liang–Kleeman information flow from December SIA to NHT. Along the sea ice edge, the information flow metric $T_{SIA12\rightarrow NHT}$ exhibits significantly negative values, indicating that SIA12 exerts a constraining influence on NHT variability. Notably, this negative value approaches −0.1, with a smaller magnitude than $T_{SIA12\rightarrow WDIV}$, which may arise from the modulating effect of SST and sea surface height on northward heat transport. Indeed, Wang et al. (2022) demonstrated using Reynolds decomposition that wind fields and SST contribute comparably to the variability of NHT.'

The reasoning around Figure 13 seems tenuous to me. The authors present time series of sea surface temperature, wind divergence, and heat transport in Figure 13, and discuss the trends in these quantities, using the differing trends to provide an interpretation for the change from interannual to decadal variability due to the dominance of differing contributing factors over the years. The physical explanation does seem reasonable to me, and earlier analysis in the manuscript does seem to substantiate it. However, the use of trends in Figure 13 weakens the argument for me because the trends are quite small and furthermore, they do not meet the 95% confidence level (I assume due to the high interannual variability). This is particularly apparent to me for the SST, where the authors report a decline followed by an increase as per the trend lines drawn in the plot, but equally, I could see a claim to be made for a decline from 1980 to ~2013, followed by a later increase, or also a relative lack of trend overall from 1979-2023, depending on where one would choose to place the discontinuity in trend lines. The heat transport is also highly variable and could also be interpreted to e.g. have a declining trend since ~2014. I would be more convinced if the authors could show that the relative magnitude of the driving factors changes, of which I am not convinced from Fig 13. I would appreciate if the authors could justify their inclusion of this analysis or make the argument using more robust data.

Weibo: We acknowledge the validity of your comment. We recognize that attributing the time-scale shift to long-term trends is wrong in the original manuscript. The primary driver underlying the observed time-scale transition is, in fact, the amplitude of atmospheric and oceanic forcing relative to its climatological mean.

In line with the clarifications above, we have deleted the content in Lines 509–514 of the revised manuscript, and added the following description in Lines 514–525: "Concurrently, SST anomalies were muted prior to 1994 but exhibited pronounced large-amplitude variability thereafter. The near-surface zonal wind showed a post-1994 shift, with a prolonged positive anomaly during 2005–2014 and a sustained negative anomaly over 2015–2022. During 2016–2020, the mean SST anomaly reached 1.09 °C, and these large anomalies, coupled with persistent negative zonal wind anomalies, drove a sustained positive anomaly in northward oceanic heat transport. In contrast, wind divergence anomalies showed strong variability before 1994 but became substantially muted afterward, with only large-amplitude, episodic fluctuations in a small number of years.

From the EOF decomposition, the January sea ice area increment anomaly can be expressed as,

$$\Delta SIA = PC1 \times EOF1 + PC2 \times EOF2 + \sum_{i=3}^{n} PCi \times EOFi$$

This decomposition demonstrates that interannual variability in January ΔSIA anomalies is dominated when the PC2 exhibits enhanced deviations from its climatological mean. Correspondingly, decadal variability in ΔSIA emerges as the amplitude of the PC1 relative to its climatological mean increases. These inferences are consistent with the aforementioned variability in SST and wind divergence. Prior to 1994, wind divergence anomalies exhibited stronger amplitude fluctuations, whereas after 1994, northward heat transport anomalies displayed markedly larger amplitudes. "

With the discussion of horizontal sea ice convergence/divergence, a consideration that is missing to me is sea ice thickness. With sea ice convergence comes thicker ice which can have thermodynamic and dynamic impacts, e.g. stronger inhibition of air-sea heat fluxes and impacts on ice motion. I would appreciate hearing at least a brief mention of what impacts the authors could expect from thicker ice.

Weibo: Following your constructive and targeted comment, we have supplemented a dedicated description of the effects associated with increasing sea ice thickness in Lines 549–551 of the revised manuscript: 'Finally, we note that sea ice thickness was not included in the present analysis, owing to the scarcity of long-term observations and substantial uncertainties in satellite-based thickness retrievals across marginal ice zones. Sea ice acts as a thermal insulator between the ocean and atmosphere, with its thickness directly modulating the magnitude of air–sea heat fluxes. Sea ice convergence drives dynamic thickening, which dampens heat fluxes, suppresses ocean–atmosphere heat exchange, and promotes descending atmospheric motion, potentially accelerating the HI feedback loop in Figure 9. However, the impact of sea ice thickness variations on air–sea heat exchange is likely considerably weaker than that driven by the presence or absence of sea ice itself, and the extent to which it modulates regional sea ice variability remains to be rigorously quantified via targeted in situ observations and process-based numerical simulations'

I will leave this point to the authors' discretion, but I would find it helpful to have a more specific title (e.g. mention that the article is examining the role of mesoscale processes in the timescale transition).

Weibo: Following your constructive suggestions, we have revised the title of the manuscript to: "Mesoscale Ice-Atmosphere-Ocean Coupling Processes Drive Interannual-to-Decadal Timescale Shift of Bering Sea January Sea Ice Variability"

Specific comments:

- L105: Why was ERA5 relegated to the supplement rather than NCEP? To my understanding it's a more contemporary dataset than the NCEP product with a higher resolution, which is particularly helpful when considering processes such as wind where capturing small scales is important

Weibo: During the initial draft preparation in 2024, our analysis primarily relied on NCEP 2 data. Subsequent to manuscript submission, reviewers recommended supplementing the analysis with ERA5 data to validate the robustness of our findings. Accordingly, we incorporated the ERA5-derived results into an Supplementary Materials. Notably, the outcomes derived from both datasets exhibit fundamental consistency. Consequently, no further modifications to the core analysis were implemented.

- L106: Reanalysis products are not observations, please take care with not referring to them as such here and throughout the article

Weibo: In accordance with your comment, we have revised the entire manuscript by changing "observation dataset" to "reanalysis datasets". Relevant revisions can be found in Lines 104, 327, and other locations throughout the revised manuscript.

- L188: Please cite the “several studies” that have substantiated this transition

Weibo: In the revised manuscript, we have added two references at Line 189: Wu and Chen (2016) and Yang et al. (2020).

- L190: The motivation for the use of IMFs here is not entirely clear to me; has the onset of the transition not been documented sufficiently in previous work?

Weibo: To demonstrate the motivation, we added the following sentence in Lines 188–191: ‘While several studies have definitively substantiated the timescale transition of sea ice from interannual to decadal variability in the Bering Sea (e.g. Wu and Chen, 2016 and Yang et al., 2020)—a result corroborated by our calculations (Figure 2a–d), attention has largely been restricted to the maximum sea ice extent. The specific onset month marking the emergence of these characteristics remains to be determined.’

- L198-200/Figure S5: it’s unclear to me how this figure illustrates the timescale transition mentioned in the text, since those figures are just time series, and they do not include any sort of analysis of variability. If this is a result shown more clearly in previous work, I request that the authors state this more explicitly.

Weibo: These figures provide limited additional insight to the main conclusions of this study. We have therefore removed the corresponding text in Lines 188–200 in the revised manuscript. Furthermore, Figure S5 in the supplementary materials has also been deleted.

- Figure S2, re: the wavelet transform: how reliable can some of these results be given that the edge effect influence extends quite far into the time series?

Weibo: The edge effect in wavelet analysis is inherently unavoidable. Based on the above considerations, we have abandoned wavelet analysis in the main text of the manuscript and instead directly extracted annual, multi-year, and decadal signals using MODWT, which allows for a direct comparison of the strengths of these three signals. The results presented in Supplementary Materials T1 merely illustrate that the decadal signal became dominant after 1995, as revealed by wavelet analysis, and this conclusion is less affected by the edge effect.

- Figure 4: Could you explain why the MODWT is unable to separate multiyear and decadal variability? Is it a matter of the time series not being long enough?

Weibo: The MODWT is applied here, as it enables robust separation of multi-year and decadal signals from the raw time series. In the revised manuscript, we have superimposed the multi-year and decadal signals in Figure 3 (original Figure 4 in the initial submission). This superposition is performed to explicitly highlight that the interannual signal of the PC1 is relatively weak, which is directly opposite to the dominant interannual variability characteristic of the PC2.

- Figure 5 a): the coloured dots were initially somewhat confusing to me since they do not line up with the ice categories: consider changing them so that each category has a different colour or simply having all dots the same colour. The caption says 4 categories but there are 5.

Weibo: Following your constructive suggestions, we have fully revised the original Figure 5, which is now presented as Figure 4 in the revised manuscript. Specifically, the original colored data points have been replaced with gray points. In addition, we have systematically classified the observation years of the full study period into three categories: heavy ice years, normal ice years, and light ice years.

- L252-253: "only a single interannual variation feature is evident" could you clarify what you mean here? Is this a result from Wang et al. 2023?

Weibo: We have removed this paragraph from the revised manuscript, as the original text contained ambiguous phrasing and insufficient logical coherence. A fully revised, restructured discussion of this topic is now provided in Lines 217–230 of the revised manuscript.

- L260: Please rephrase, you say "significant" but the correlation not marked as statistically significant until after 2010 as per the green dots in Fig 5b

Weibo: Following your constructive comment, we have revised this sentence at Line 274 of the revised manuscript to: "a significant negative correlation between December SIA_{12} and the PC1 becomes apparent after 2010".

- L370: Maybe better to say the results are discussed further in Wang et al. 2022 rather than saying "corroborated" since that study is not entirely independent from this one (Figure 9b appears to have the same information as Fig 4b from that article)

Weibo: Following your constructive suggestion, we have revised this sentence accordingly. The fully revised text is now presented at Lines 389–391 of the revised manuscript, as follows:

"Our study builds on this work to further quantify the impact of warm advection on sea ice, and clarifies the regulatory role of December sea ice on warm advection."

- Figure 11: Please specify that dots indicate statistical significance in the caption

Weibo: Following your suggestion, we have added a statistical significance note to the caption of revised Figure 10 (original Figure 11) at Lines 445–448 of the revised manuscript: “Black dots in both panels denote statistically significant correlations at the 95% confidence level ($p < 0.05$).”

- L427/fig 12b: Minor phrasing consideration: the anticyclonic circulation only appears substantial north of the Bering Strait towards the East Siberian Sea

Weibo: Following your constructive suggestion, we have revised the corresponding sentence in the revised manuscript (at Line 455) to:
“a strong anticyclonic anomaly only appears substantial north of the Bering Strait towards the East Siberian Sea”

Technical corrections:

- L60: are -> is
- L131: “within annual” -> “within the annual”
- L295: "drivers" -> "drives"
- L307: anomalies.. -> anomalies.
- L311: I think this should be S3 not S1?
- L360: During -> during
- L379: influence -> influences
- L406: missing numbers for PC2 and PC3 in math text
- L415: exert -> exerts
- L480: exacted -> extracted?
- L491: the -> The

Weibo: We greatly appreciate your detailed comments on the language and grammar of our manuscript. Following your suggestions, we have systematically corrected all grammatical errors throughout the full text, with line-by-line revisions to improve the clarity, fluency and academic readability of the manuscript. For brevity, these language-specific changes are not individually listed in this response letter.

Dear reviewer 2,

We sincerely thank you for your positive and insightful comments on our manuscript, as well as your detailed and constructive suggestions. We have carefully and comprehensively revised the manuscript in strict accordance with each of your comments, and hereby present the following detailed point-by-point responses to address all of your recommendations for your review and further assessment.

Format specification for this response letter: Comments from the reviewer are presented in black font, our point-by-point responses in blue font, and revised manuscript text excerpts in green font.

The change in Arctic sea ice area is a hot topic. Authors elucidated the changes in sea ice area (SIA) in boreal winter and amplified variance of decadal fluctuations in the wintertime SIA increment (ΔSIA). The manuscript is well-written and has good novelty. Some concerns should be addressed before acceptance of the manuscript I suggest minor revisions. Some grammar errors need to be revised. My detailed comments are organized as follows:

1 Sea ice concentration budget holds significant potential for elucidating the aforementioned competitive processes, as it enables the quantitative decomposition of sea ice concentration changes into distinct physical components—including sea ice drift (encompassing transport and convergence dynamics), thermal melting/freezing, and mechanical redistribution. Given this analytical capability, the authors are encouraged to clarify why the sea ice concentration budget was not employed in the present study.

Weibo: We greatly appreciate your valuable suggestion to incorporate a sea ice concentration budget analysis, which is an excellent approach to better disentangle the relative contributions of competing physical processes driving sea ice changes. As you noted, this framework enables us to quantitatively decompose SIC variations into distinct components, including advection, divergence, and thermodynamic melting/freezing.

This analysis was indeed included in our initial study design, as we aimed to use the SIC budget to deepen our understanding of the physical mechanisms governing sea ice variability in the study region. However, during the data preparation phase, we encountered critical limitations with the availability and quality of sea ice drift data from the National Snow and Ice Data Center (NSIDC) microwave remote sensing products. Our systematic evaluation revealed substantial spatial inconsistencies between the satellite-derived sea ice drift data and the SIC dataset, particularly in regions with low sea ice concentration (as shown in Figure 1). These mismatches would introduce non-negligible biases into the calculation of dynamic terms in the SIC budget, directly compromising the accuracy and reliability of the budget analysis, especially for the early winter period that is the focus of our study. For this reason, we made the decision to exclude this analysis from the current manuscript.

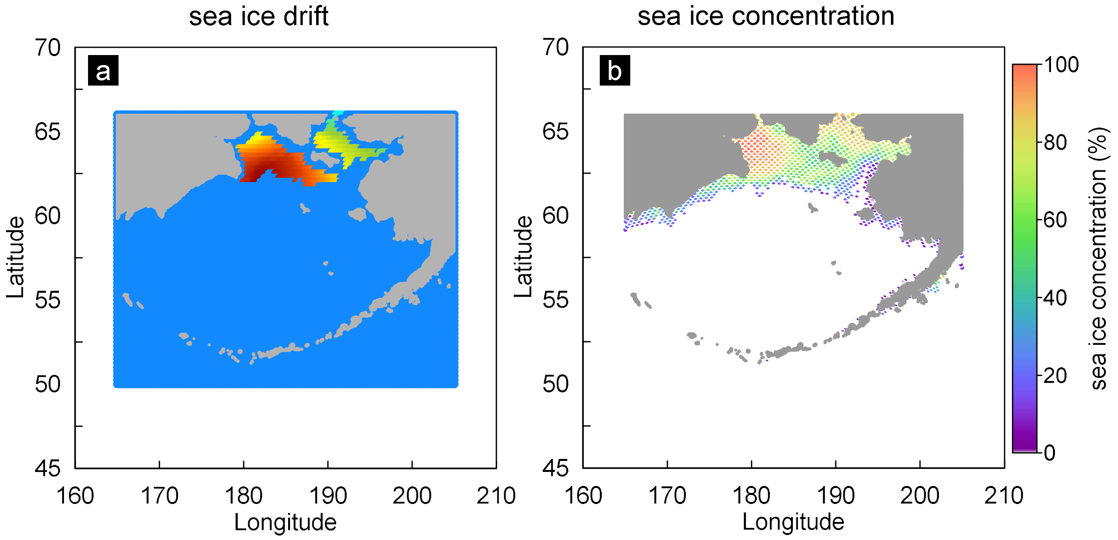

Figure 1. Spatial distributions of sea ice parameters on 31 March 1979 over the study domain: (a) zonal component of sea ice drift velocity, and (b) sea ice concentration (SIC). Blue shading in Panel (a) and white areas in Panel (b) denote regions with missing observational data.

2 What is the role of atmospheric thermodynamic forcing—such as the radiative effects of water vapor and clouds—in the proposed competition mechanism? These factors are known to exert significant influences on sea ice thermodynamic processes.

Weibo: We greatly appreciate your constructive inquiry regarding our consideration of atmospheric thermodynamic forcing, particularly the radiative effects of water vapor and clouds. In response to this comment, we have performed a regression analysis of Precipitable Water (PW) and Total Cloud Cover (TCC) against the second principal component (PC2) time series from our EOF analysis, covering the period 1979–2022, with the results presented in Figure 2. Notably, the PC2 time series exhibits a significant inverse correlation with December sea ice area (SIA), meaning that a higher PC2 index corresponds to reduced December SIA.

Figure 2 demonstrates that increases in PW and TCC over the Bering Sea are positively associated with the PC2 index, which covaries positively with December SIA. This relationship reveals a critical positive ice-radiation feedback loop modulated by atmospheric effects: the lower the December SIA, the higher the PW and TCC, and further suppresses sea ice area in the subsequent January. The impact of these two processes on sea ice evolution can be fully attributed to their longwave radiative warming effect on the sea ice-ocean system. Specifically, reduced sea ice area enhances ocean-to-atmosphere moisture transport, and the resultant PW increase amplifies the atmospheric longwave greenhouse effect, slowing the rate of sea ice formation in early winter. Meanwhile, elevated TCC suppresses outgoing longwave radiation from the sea surface, enhancing surface warming and further inhibiting sea ice growth.

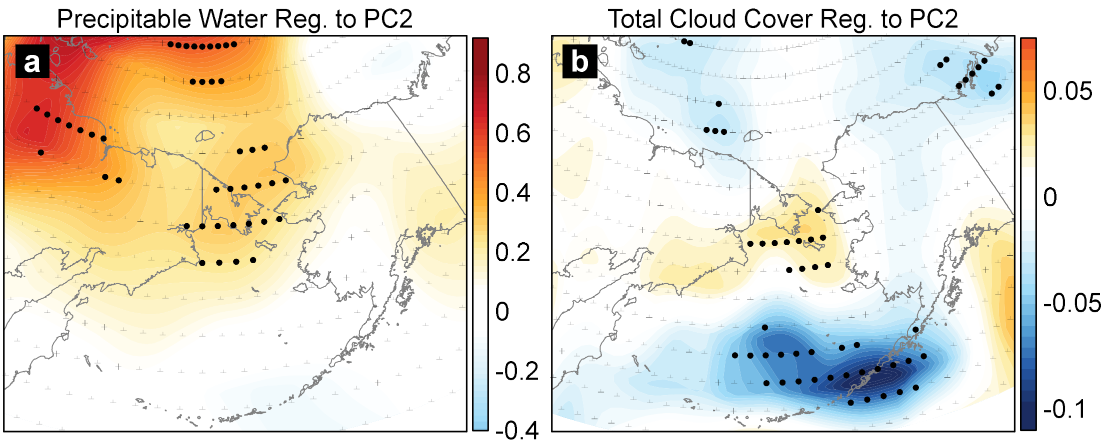

Figure 2. Regression maps of (a) precipitable water (PW) and (b) total cloud cover (TCC) regressed against the second principal component (PC2) index over the Bering Sea. Black dots in each panel indicate values statistically significant at the 95% confidence level, as determined via a Student's t-test.

3 Section 2.2b: Would alternative decomposition methods (e.g., Ensemble Empirical Mode Decomposition, EEMD) produce similar results to the one currently used? Validating the findings against EEMD would help confirm the robustness of the analysis.

Weibo: We greatly appreciate your valuable suggestion to explore alternative time series decomposition methods, such as Ensemble Empirical Mode Decomposition (EEMD), for the analysis of our PC time series. In direct response to this comment, we have applied the EEMD method to decompose the PC time series into interannual, multiyear, and interdecadal variability components, with the results presented in Figure 3.

Both the EEMD analysis and the Maximal Overlap Discrete Wavelet Transform (MODWT) employed in the original manuscript consistently show that PC1 primarily captures multiyear and interdecadal variability, whereas PC2 is dominated by interannual variation patterns. The high consistency between these two independent decomposition approaches validates the robustness of our core findings, and demonstrates that the choice of decomposition technique has negligible impact on the identification of the dominant timescales of Bering Sea SIA variability.

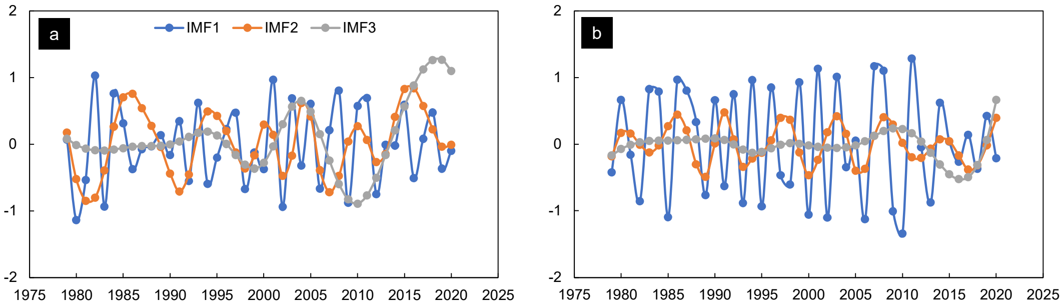

Figure 3. Ensemble Empirical Mode Decomposition (EEMD) analysis of the normalized December PCs time series derived from Bering Sea sea ice variability: (a) decomposition of the PC1 into three distinct timescale components: interannual, multi-year, and decadal signals; (b) corresponding EEMD decomposition results for the PC2.

4 Figure 2i ‘increament’->’increment’.

Weibo: We have made targeted revisions to the manuscript, with the specific modification detailed in Line 254 of the revised manuscript.

5 Figure 3: Have you conducted sensitivity tests on the moving window parameters? The robustness of the results presented in this figure relies heavily on the stability of the moving window settings.

Weibo: We greatly appreciate your inquiry regarding the sensitivity of our moving correlation analysis to the choice of moving window length. To directly address this concern, we have conducted a comprehensive sensitivity test using a range of window lengths (7–13 years) for the moving correlation analysis between December ΔSIA and its IMF1–IMF3 components, with the results presented in Figure 4.

Our results confirm that the Moving Correlation Coefficient (MCC) exhibits highly consistent and robust characteristics across all tested window lengths, from the 7-year to 13-year window, verifying that our findings are not sensitive to the choice of window size. In addition, our analysis reveals that the statistical significance of the interannual variability signal in ΔSIA exhibits a gradual weakening trend over the study period. By contrast, the multiyear signal shows a pattern of initial increase followed by a decrease, while the decadal signal has become increasingly prominent, particularly after 2010.

After careful re-evaluation, we have removed this section from the revised manuscript for two well-considered reasons. First, the core conclusion of this section—the interannual-to-decadal regime shift in the SIA over the Bering Sea—has been thoroughly documented and verified by multiple prior studies in the field, and thus does not require redundant restatement in our manuscript. Second, the scientific narrative and analytical content of this section present nearly complete overlap with Figure 2 in the revised manuscript, which would lead to unnecessary repetition.

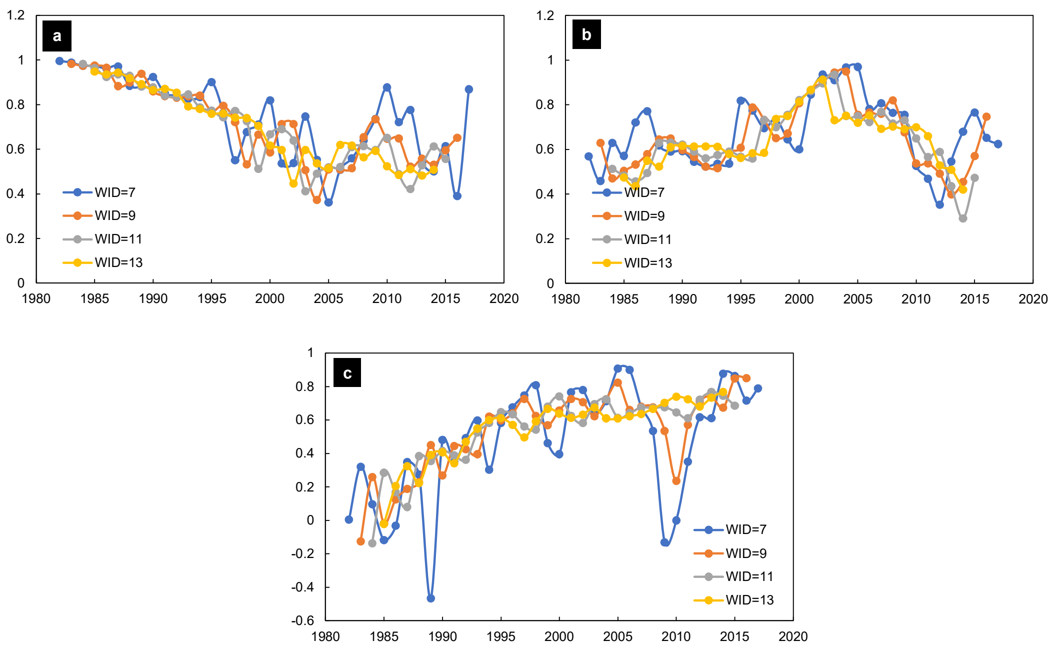

Figure 4. Moving correlation coefficients (MCCs) between December ΔSIA anomaly and its first three intrinsic mode functions (IMF1–IMF3), calculated with different moving window lengths. Panels show MCCs between ΔSIA and (a) IMF1, (b) IMF2, and (c) IMF3, respectively. Lines in blue, orange, gray, and yellow correspond to moving window lengths of 7, 9, 11, and 13 years, respectively.

6 Certain figures require additional beautification to improve their presentation quality, including Figures 4, 5, and 7.

Weibo: In accordance with the journal's formatting guidelines and to improve the clarity, readability, and consistency of our results presentation, we have comprehensively revised, standardized, and optimized all figures in the manuscript. This includes targeted updates to the layout, labeling, and visual quality of Figures 4, 5, and 7, with all revised figures fully integrated into the updated manuscript.

7 Authors should emphasize further the implication of this study.

Weibo: In accordance with your comments, we have revised the final paragraph of Section 5 (Conclusions) (Lines 585–596 of the revised manuscript) to highlight the potential applications of our study. The revised text is as follows: 'The ecological and biogeochemical consequences of prolonged heavy or light sea ice regimes in the Bering Sea are multifaceted, most notably the degradation and seasonal disappearance of the cold pool over the eastern Bering Sea shelf, and the poleward shift of subarctic groundfish communities. Mirroring the shift in sea ice variability, the population dynamics of key Bering Sea ecological species have transitioned from interannual to decadal-scale fluctuations, with direct impacts on commercial and subsistence fishery yields. This regime shift poses profound adaptive challenges for indigenous communities and commercial fishing enterprises reliant on the stability and predictability of Bering Sea fishery resources. Under ongoing global warming, persistent upper-ocean warming in the Bering Sea means that sea ice cover is highly unlikely to revert to a predominantly interannual variability regime. Instead, the Bering Sea will likely remain prone to prolonged periods of heavy or light sea ice, locking in a shift in fishery resource fluctuations from interannual to decadal timescales. Consequently, fishery-dependent communities and management bodies must adapt to this new decadal-scale variability regime. While this shift offers greater multi-year predictability of sea ice and fishery conditions, it also presents substantial adaptive challenges associated with decade-scale shifts in fishery productivity and species distributions.'