RC1

GENERAL COMMENTS:

Overall recommendation: Not acceptable in its current form / requires major revision.

The study explores the large-scale dynamical processes that drive variability in subtropical stratocumulus. Stratocumulus clearly play a key role in Earth's radiative budget and climate feedbacks. And most previous studies on the drivers of stratocumulus have focused on the climatological-mean or seasonal cycle. Ding et al. 2026 propose instead to explore the processes that govern stratocumulus on timescales ranging from the synoptic to interannual. In this sense, the paper is both needed and important.

However, I think the analysis design and treatment of the data are overly and unnecessarily complex and opaque. As a result, I think many of the results are difficult to interpret. I also worry about reproducibility. My own research is focused on the analyses of climate phenomena. But I nevertheless had a very hard time understanding how many of the results were generated and what is being shown in many of the figures. I think it would be effectively impossible to reproduce the results without relying on the MODES code. The methodology leaves the data very little‘ room to breathe' before it is analyzed.

I appreciate the analyses involved a lot of time and effort. But I think the paper requires extensive major revision before it can be considered for publication. I suggest the authors revise the paper with a focus on simplifying the analysis, reducing the reliance on existing code packages, and ensuring reproducibility. It would be OK to keep some of the results based on the MODES code. But those results should be supported by results based on more standard and straightforward analysis tools. I've provided an example below.

The authors appreciate the reviewer's careful attention to reproducibility. We consider these important because this paper tries to remind people of some missing aspects when considering the large-scale conditions for stratocumulus variability. The suggested tests help us show the robustness of the conclusions.

We don't want the technical details to distract readers from the key information, so we didn't include our test of MODES in the manuscript. We did the test using composite analyses of the original ERA5 fields (independent of using MODES), and they support the conclusion found via MODES. We only included the composite results which bring additional information in the manuscript. As the reviewer raising the concern of reproducibility, we'd like to put an example of our previous test here. Below shows the PV' distribution from 4 days ahead to the composite day of high EIS or LCC. The plotting setup is the same as Figure 3 in the manuscript. We can also see the extratropical Rossby wave trains and the anticyclone above Sc areas at lag 0. Figure R1 was based on running-mean high-pass filtering of the deseasonalized series (<15 days). We also performed the direct regression using Butterworth filtering as suggested, and put the results in the specific response, point 1. Both methods support the findings in our manuscript.

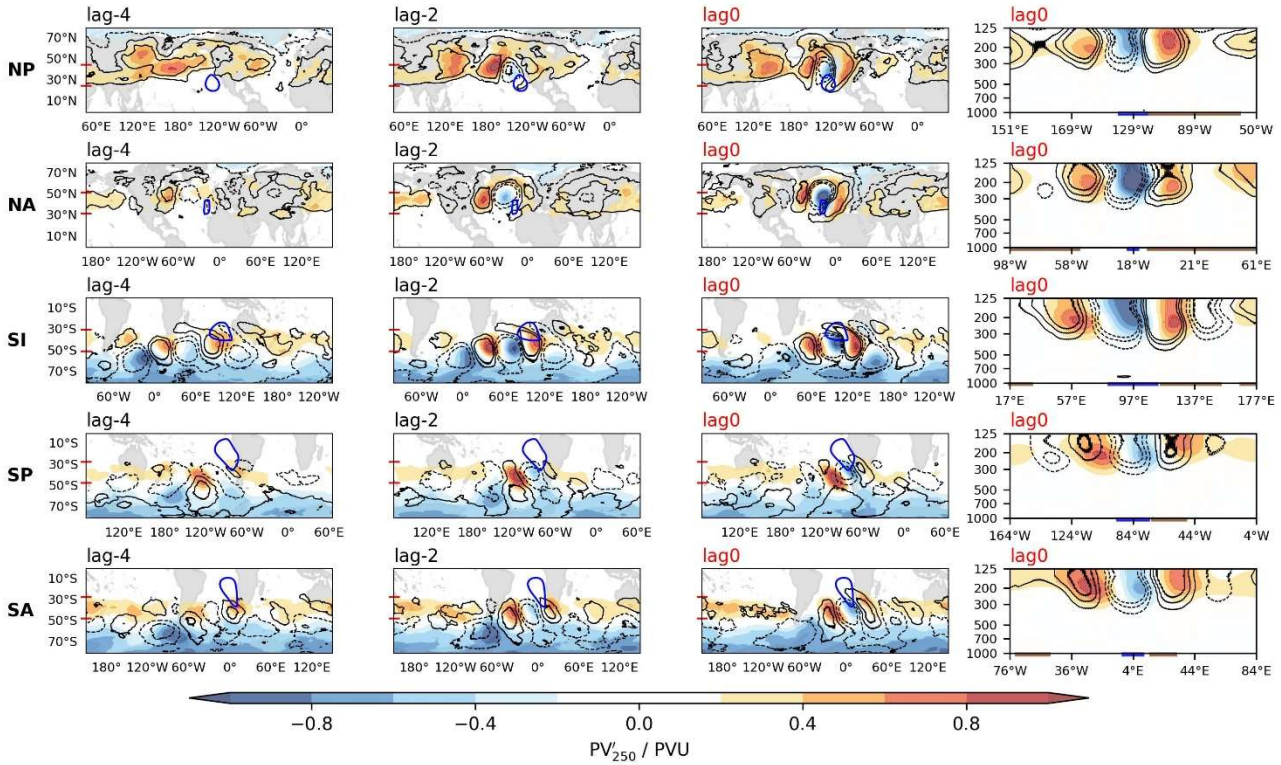


Figure R1. Potential vorticity (PV) anomalies associated with composite high-EIS (shading) and high-LCC (contour lines) cases. The left three columns show PV anomalies at 250 hPa at lags of −4, −2, and 0 days relative to the composite days. The rightmost column shows cross-sections at lag 0, averaged over the latitude bands indicated by the red ticks on the maps. PV anomalies are multiplied by −1 in the Southern Hemisphere.

MODES is based on the 3D normal mode expansion method described by Kasahara and Puri (1981). This software is openly available, and it has been used to analyze ECMWF operational forecasts on a daily basis since 2014. We use it because it separates wave types and wavenumbers in 3D within a clear dynamical framework. Direct regression can reveal the associated patterns, but the patterns may contain contributions from multiple dynamical components. MODES allows us to distinguish these components and see their spatial relationships, particularly the spatial relationship between Rossby and IG modes in this study.

Here we summarize what MODES does to make it more transparent. At each time step, MODES decomposes the global ERA5 wind and geopotential fields using Fourier functions in the zonal direction, Hough functions describing their joint meridional structure, and vertical structure functions in pressure coordinates. The vertical structure equation is solved using the reference static stability calculated from the analytical temperature profile of Jablonowski and Williamson (2006). Its eigensolutions provide the vertical basis functions and the corresponding equivalent depths. The horizontal basis functions are eigensolutions of the linearized shallow-water equations on the sphere. The expansion coefficients, $\chi_n^k(m,t)$, are then obtained by projecting the original ERA5 fields onto these 3D basis functions.

We regress $\chi_n^k(m,t)$ (describing global circulation at each normal mode) getting from MODES onto local $\mathrm{EIS/LCC}(t)$ separately. $\chi_n^k(m,t)$ denotes the time series of the expansion coefficients at zonal wavenumber k, meridional mode index n and vertical mode index m.

Then we reconstruct all the regressions into Rossby modes and IG modes, and project the respective regressions back to physical space. The accuracy of this reconstruction has been tested in previous studies (e.g., Žagar and Franzke, 2015; Chen et al., 2025) and was also checked in this study: the reconstructed fields reproduce the original fields. Because both regression and reconstruction are linear, reconstructing the regression coefficients is mathematically equivalent to regressing the reconstructed fields onto the same index. Following the reviewer's suggestion, we further compared the reconstructed regression patterns with those obtained by directly regression method.

All line numbers cited in this response letter refer to the PDF version of the revised manuscript with tracked changes.

A FEW SPECIFICS:

- One of the main conclusions is that stratocumulus are primarily influenced by extratropical wave processes and that "instead of the tropical thermodynamic framework, we find an extratropical dynamics control EIS variability." This is a strong conclusion and I worry it derives at least partially from the treatment of the data by the MODES package (since it reformats the data into normal modes of the equations). Reproducing the results based on simple linear methods would go a long way to addressing this concern.

For example, consider eddy activity at 500mb. The authors could 1) form Z*500 by removing the zonal mean from the daily data; 2) time filter Z*500 into the frequency bands of interest using a Butterworth (or similarly robust) filter; and 3) regress or correlate the resulting time series onto indices of Sc incidence as a function of lag. The results would be simple to reproduce and interpret, and if the conclusions of the study are robust, they should yield maps similar to those shown in Fig. 3.

Thanks a lot for suggesting this test. We followed the suggested procedures. To note, in the second step, we applied Butterworth filtering to the deseasonalized daily EIS/LCC time series and to the deseasonalized daily Z^*_{500} field at each grid point.

A comparison of Butterworth and running-mean filtering is shown below, using EIS in NP as an example. We agree that Butterworth filtering provides a sharper separation among timescales. The next step is to test whether the conclusions are sensitive to the filtering method.

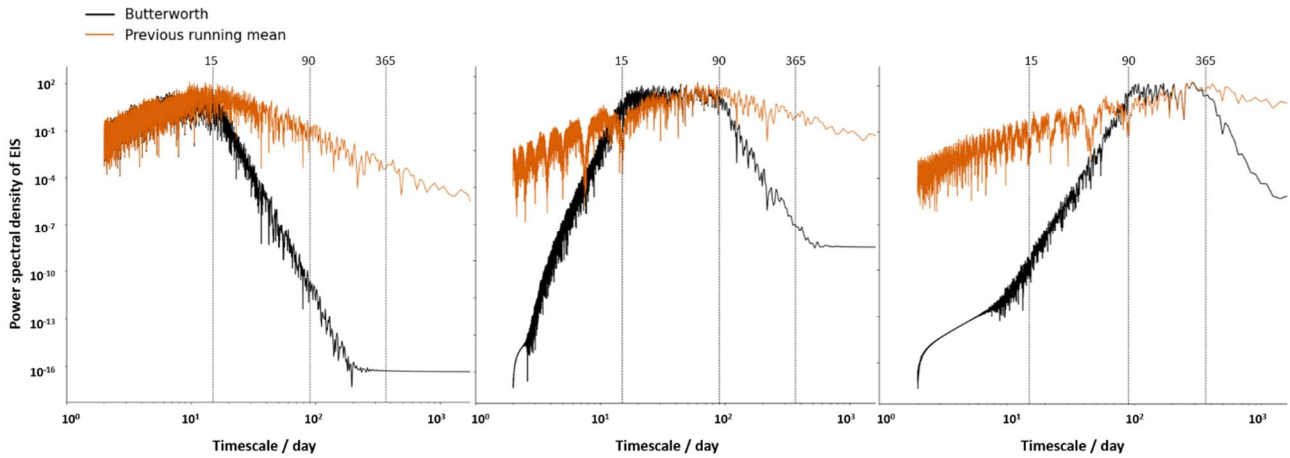


Figure R2. Power spectral densities of EIS (averaged over NP area) series obtained using Butterworth (black) and running-mean (orange) filters for the <15-day, 15–90-day, and 90–365-day ranges (left to right).

The results on the synoptic (<15 days, Figure R3), subseasonal (15-90 days, Figure R4), and interseasonal (90-365 days, Figure R5) timescales using the suggested regression method. The plotting setup is similar to that of Figure 3 in the manuscript. Note that the cross-section column is removed as Z^*_{500} is a 2D filed at 500 hPa. Comparing Figure R3 with Figure 3 in the manuscript, Figure R4 with Figure S11 in the supplement, and Figure R5 with Figure S12 in the supplement shows that the qualitative conclusions remain unchanged. To

clarify, Figures 3, S11, and S12 show regressions at 250 hPa, where the Rossby-mode perturbations are stronger than 500 hPa, as reflected in the vertical cross-sections.

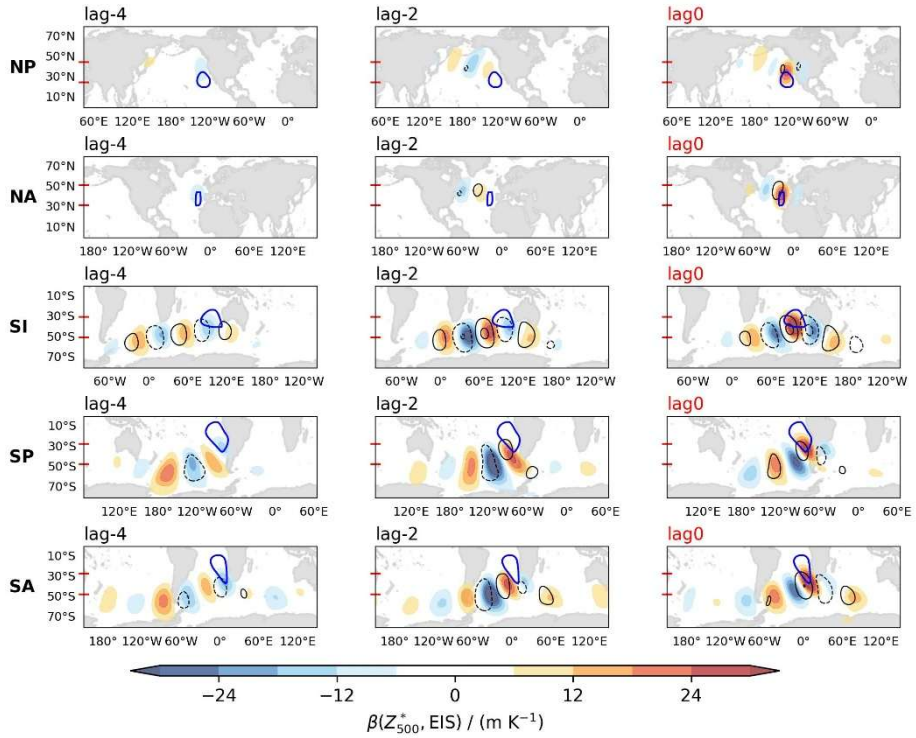


Figure R3. Lagged regressions of 500 hPa eddy geopotential height (Z^*_{500}) onto EIS (shading) and LCC (contour lines) in the five Sc areas at lags of −4, −2, and 0 days. The daily fields and indices are deseasonalized and Butterworth-filtered to retain variations < 15 days. Negative lags indicate that circulation leads the indices.

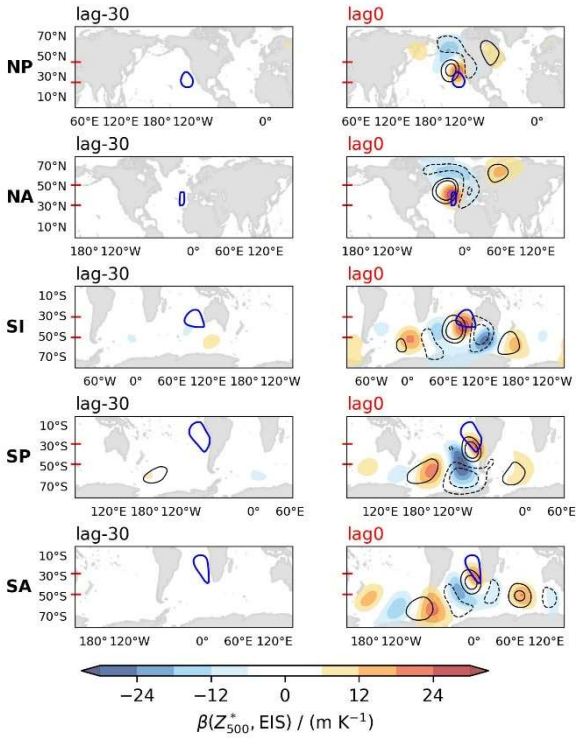


Figure R4. Lagged regressions of Z^*_{500} onto EIS (shading) and LCC (contour lines) in the five Sc areas at lags of −30, and 0 days. The daily fields and indices are deseasonalized and Butterworth-filtered to retain variations between 15-90 days.

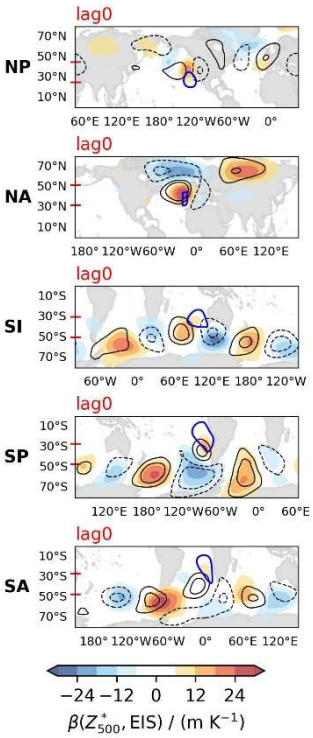


Figure R5. Regressions of Z^*_{500} onto EIS (shading) and LCC (contour lines) in the five Sc areas. The daily fields and indices are deseasonalized and Butterworth-filtered to retain variations between 90-365 days.

- The study feels like it is motivated by a 'tool looking for an application'. If the authors keep the MODES results, I think they need to demonstrate what is gained from the MODES software that cannot be inferred from more straightforward analysis techniques.

Some explanations have been put in the general response above.

Our initial motivation for using MODES is to ask whether the monsoon-desert mechanism could help explain the summer peak of Sc in some areas. The Rossby and Kelvin responses to monsoon heating discussed by Rodwell and Hoskins (1996, 2001) made normal-mode decomposition a suitable framework for examining this possibility. Although this mechanism is being argued for the subtropical highs, it hasn't been tested for their adjacent Sc areas. We tested and found that this mechanism is not suitable for explaining Sc variability (thus didn't emphasize it in the manuscript), while the extratropical Rossby modes are more pronounced.

MODES is useful because our findings concern the separate dynamical components and their relationships, such as the spatial relationship between Rossby and IG modes, and the contributions of different wavenumbers. Direct regression can show the combined pattern, but does not explicitly separate different wave types. A technically straightforward analysis does not necessarily provide a straightforward dynamical interpretation. We've added some explanations in the introduction (Lines 81-85) and conclusions (Lines 487-492).

- Section 2.4 and Eq. 2 are overly terse and I don't think provide enough information to reproduce the results. Understanding Section 2.4 would require considerable familiarity with the source paper (Zagar et al. 2015). Likewise, why use the SciPy Python package (version 1.14.1) to compute coherence? Cross spectral analysis is straightforward to code using FFT. From the text, it is not clear how one might reproduce the results. How are degrees of freedom and significance assessed?

We've rephrased section 2.4 and added more explanation to Eq. 2.

For the coherence computation, we updated it using the suggested explicit coding via FFT. The plots are identical to those obtained using the SciPy Python package. Please see the comparison below. The respective Methods introduction has been updated in the manuscript (Section 2.7).

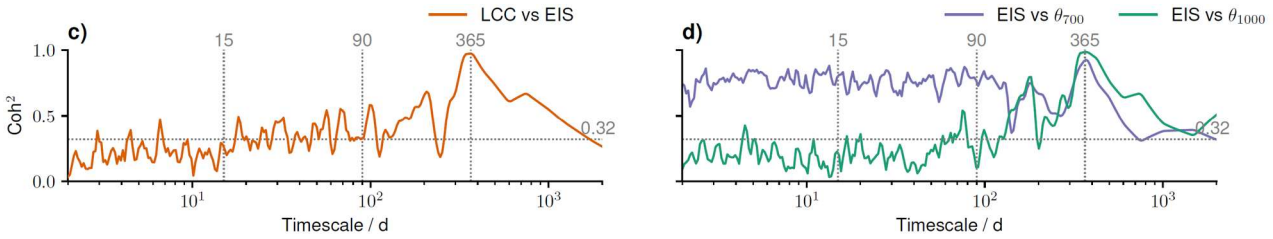


Figure R6. Coh^2 calculated from the SciPy Python package.

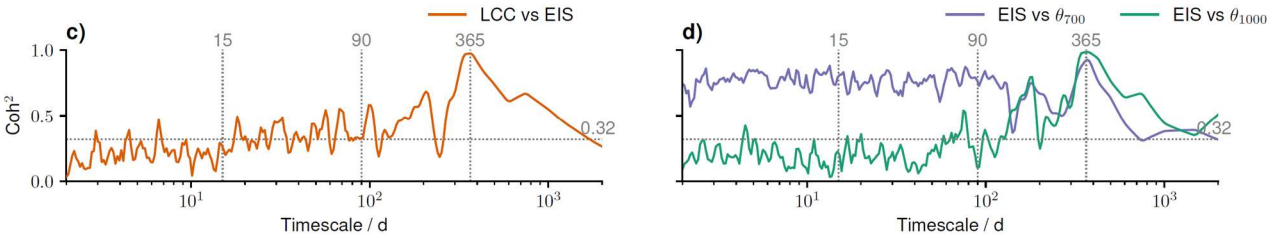


Figure R7. Coh^2 calculated from the explicit coding using FFT.

The significance assessment method has been updated (see the response in "SOME MORE DETAILED COMMENTS – Figure 1" below). Here we answer the reviewer's question about how the fixed value (i.e., 0.32) was assessed in the original manuscript: for the daily time series spanning 1987-2019, the total length is 12053 days. Using a 3000-day window with 50% overlap, there are seven complete segments ($12053 \div (3000 \times 50\%) - 1 \approx 7$). Treating the seven segments as independent gives nominal spectral degrees of freedom of $7 \times 2 = 14$, with the factor of two accounting for the real and imaginary Fourier components. Then the 90th percentile significance threshold is estimated as $1 - (1 - 0.9)^{\frac{1}{7-1}} \approx 0.32$, and the 95th percentile threshold as $1 - (1 - 0.95)^{\frac{1}{7-1}} \approx 0.39$. These thresholds are calculated using Eq.7, Miles (2011). They were rough estimates based on the assumption of independent segments. The updated method for calculating the thresholds does not rely on that assumption.

- Running-mean filters are generally not considered appropriate for precise time filtering. They are fine for display and simple low-pass filtering. But the sidelobes that result from

the Fourier Transform of the boxcar window function render them unsuitable for the fine-scale filtering done in the analysis (<15 days; 15-90 days; 90-365 days). I suggest using, say, a Butterworth filter or something along those lines. It is widely used, easy to apply, and has much smaller sidelobes.

Thank you. Please see the response in "A FEW SPECIFICS – 1st point" above. Changing the filtering method doesn't change our findings. It supports the robustness of these qualitative findings to the filtering method. We have kept the original filtering method in the manuscript and added a description of this test (Lines 134-136).

- Some of the conclusions appear to derive from visual inspection of the results and lack quantitative support. For example, the PDO is listed in the abstract: "On interannual timescales, planetary-scale Rossby waves coupled with a negative PDO-like (Pacific Decadal Oscillation-like) sea surface temperature pattern increase EIS". As far as I can tell, the conclusion is drawn from visual inspection of Fig. 10 and is never quantitatively tested. The PDO also exhibits most of its power on decadal timescales, which is beyond the scope of the data length in the paper. Perhaps it would be more robust to work with ENSO rather than the PDO?

We agree that identifying circulation patterns visually does not provide a quantitative test of their influence by itself. MODES helps distinguish the dynamical components contributing to these patterns, but the distinction between our main findings and the interpretations drawn from them needed to be clearer.

Regarding the PDO, we used "PDO-like pattern" to describe a spatial SST anomaly pattern, rather than to say the formally defined PDO events. We define the "-PDO-like structure" as upstream warming in the western part of the ocean basin together with cooling in the east, including the Sc area. Similar configurations occur near all five Sc areas (Figure S14 in the supplement). Our intention to use "PDO" was to emphasize the extratropical pattern rather than an equatorial ENSO signal, although ENSO can be a reason for this pattern. We've further clarified this term in the manuscript (Lines 352-355).

A key conclusion is that EIS is more closely associated with local conditions, while LCC is more closely associated with upstream conditions. We also assessed this distinction quantitatively by comparing the R^2 values of multi-linear regressions using local or upstream relative vorticity, geopotential height, and SST as predictors. These comparisons support the local–upstream distinction. The fits using the more closely associated predictors are reported in Supplement Section 10: the dynamical predictors account for about 76% of EIS variability and 36% of LCC variability, increasing to 45% for LCC when EIS is included as another predictor. These values have been mentioned in the conclusion (Lines 507-510).

An implication from the above conclusion is that SST configurations with opposite anomalies locally and upstream can favor simultaneous increases in EIS and LCC through different processes. This shared influence could increase their correlation in the seasonal cycle and on interannual timescales. The "PDO-like pattern" visual detected on those timescales could be the reason for much higher EIS and LCC correlation compared to other timescales. A

very preliminary supporting analysis is presented in Ding (2026, Figure 5.4), but this remains an interpretation requiring further studies. We've revised the text to clarify this proposed interpretation from information given by the plots (Section 5.3).

- Why is the analysis period limited to an endpoint of 2019? Why not include data through 2025?

Thank you for raising this concern about the analysis period. We initially selected 1980-2019 to cover four complete decades. This followed the common practice in climate analyses of selecting a period spanning more than 30 years, from the beginning of a decade to the end of the most recent complete decade. We then changed the starting year to 1987 to match the consistent-quality daily satellite LCC record from CLARA-A3 and used the same period for EIS.

The regression results for EIS change little when we change the period from 1980-2019 to 1987-2019, indicating limited sensitivity to the change in period. Further evidence for this limited sensitivity is shown in the response to the next bullet point ("SOME MORE DETAILED COMMENTS – Figure 1").

SOME MORE DETAILED COMMENTS:

- Figure 1: What are the units on the x-axis of the coherence plots? Do the results show coh^2? How many degrees of freedom are assumed in the significance calculation? I recommend reproducing the coherence in Fig. 1c and 1d in subsets of the data to ensure reproducibility.

Firstly, I'm sorry that I've put the wrong version of Figure 1 in the submitted manuscript, as the results shown there use only the mean time series across the NP, NA, and SI areas, and the labels are missing. That was a version for our own test, and it has now been updated to the correct version (using the mean series across all five areas and with the labels). The correct version before we updated the method is shown in Figure R6 in this response letter. And yes, the results show Coh^2.

We've updated the significance calculation method from a fixed estimation to a statistical calculation: for the two series used to calculate coherence, we retain the Fourier amplitude at each frequency and independently randomize the phases 2000 times. The 90th percentile of the resulting 2000 coherence values is used as the significance threshold (dotted line) in the updated plots. The dashed lines represent the respective 95th percentiles.

Thanks for recommending testing the coherence in subsets. We use the periods 1987-2002 and 2003-2019 to compare with the results in the full period (1987-2019). For the shorter period, the previously used 3000-day window is too large. Hence, we use the 1500-day window for each period in this test (Figures R8-R10). But in the updated Figure 1 of the manuscript, we keep the 3000-day window.

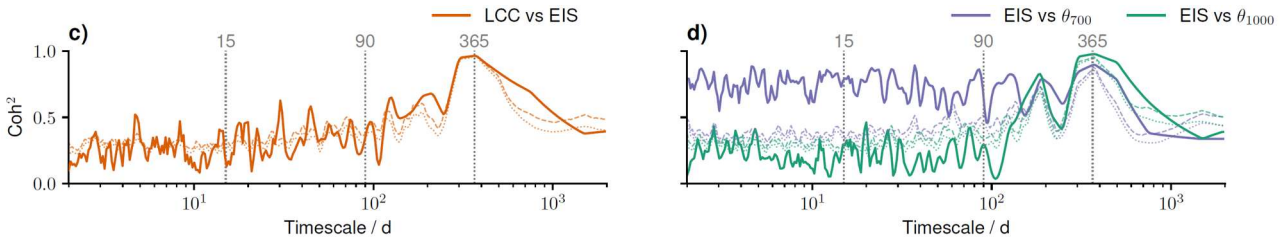


Figure R8. Results for the period 1987-2002, using 1500-day window size.

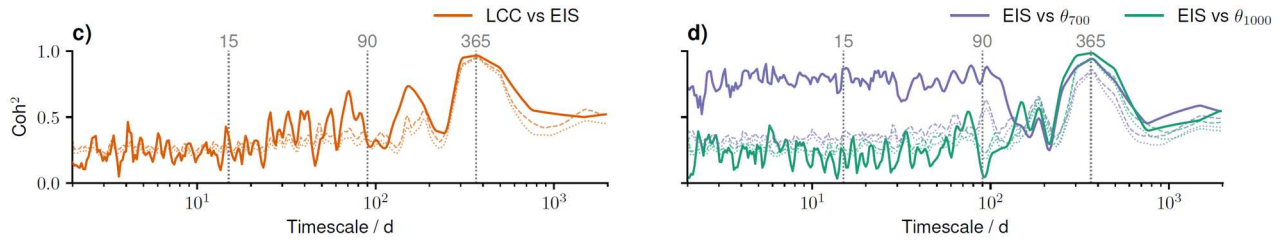


Figure R9. Results for the period 2003-2019, using 1500-day window size.

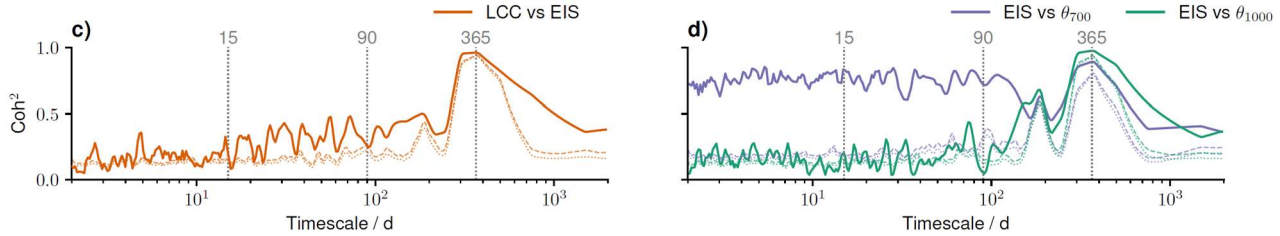


Figure R10. Results for the period 1987-2019, using 1500-day window size.

As we only suggested reading the qualitative information from Figure 1 in the manuscript, the above plots support the robustness of the qualitative timescale-dependent relationships to the period selections. We further tested this robustness by comparing the results from the original series and the detrended series using the 3000-day window size for the full period (results are shown below). The results are almost identical.

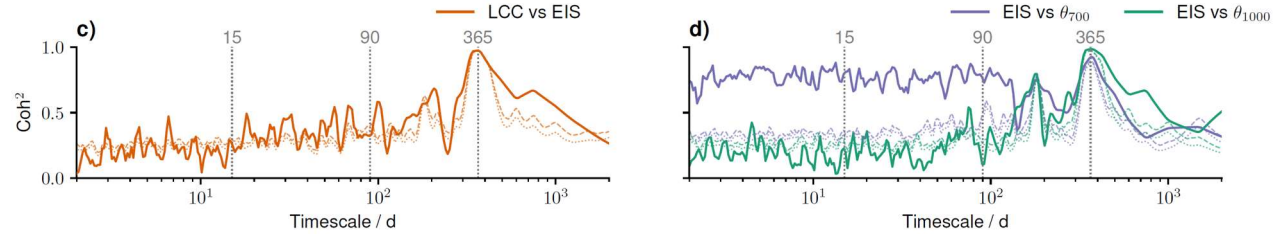


Figure R11. Results for the original series in the period 1987-2019, using 3000-day window size.

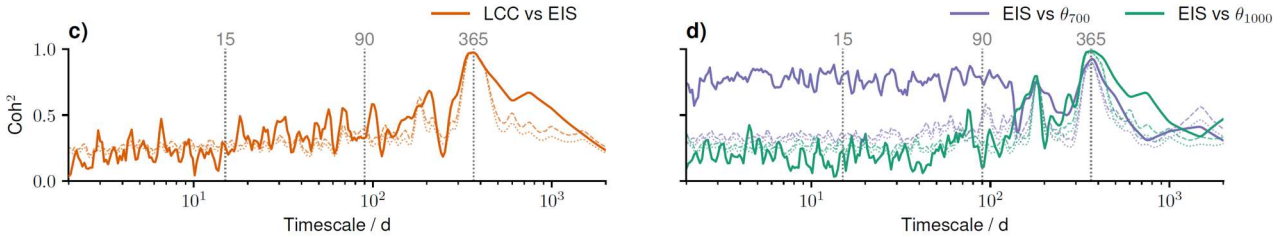


Figure R12. Results for the detrended series in the period 1987-2019, using 3000-day window size.

Therefore, the selection of periods doesn't influence our qualitative findings in the text (Lines 241-266), and the description is adjusted for the new plots.

- Figure 2: What exactly is being shown here? Is this the amplitude of wave activity integrated over the entire globe associated with variations in LCC on the indicated timescale? If so, why the globe and not the regions of interest? The text and caption do not provide enough information to properly interpret the results, or to reproduce them.

Figure 2 shows the zonal wavenumber distribution of the squared modal regression coefficients, weighted by gD_m, for the Rossby modes associated with LCC variability at lag 0. The factor g is gravitation and D_m is the equivalent depth of vertical mode m. Weighting by gD_m expresses the contributions of different vertical modes on a common energy scale before summation. For each zonal wavenumber k, we sum $gD_m|\mathcal{R}_n^k(m, t=0)|^2$ over the Rossby meridional modes n and vertical modes m. The spectral peaks indicate the dominant zonal wavenumbers contributing to the circulation patterns shown in Figures 3, S11, and S12.

It does not show the wave activity of the original field, but the wave patterns associated with regional LCC variability extend beyond the Sc area, including upstream. Restricting the analysis to the Sc area would omit much of this structure, while our aim is to identify the contribution of global wavenumbers associated with LCC variability for each area. Note that contributions can be compared across wavenumbers within each curve, but not in absolute magnitude across curves. We've revised the introduction of this calculation (Lines 190-194) and the description of Figure 2 (Lines 270-276).

- Figure 3: "We see that the circulation controlling synoptic EIS and LCC variability is an extratropical Rossby wave". It is true that the results in Fig. 3 appear as Rossby waves. As noted above, I worry the wavelike nature of the results is mandated by the MODES software package. Do you recover similar features if you simply correlate, say, Z500* with variations in clouds in the respective regions? If not, why not?

Yes, we recover similar features of that method. Please see Figures R3-R5 above.

- Figure 6: The text states "Hence, tropical perturbations are not the direct driver controlling the interannual variability of Sc." The conclusion seems to be drawn from visual inspection of the results rather than a statistically significant relationship.

This statement has been removed (Lines 356-359).

- Figure 7. The wave activity flux is a useful tool for diagnosing wave/mean flow interactions. But that is not the focus of this paper, and I don't understand how the flux

results add to the conclusions of the current study. The saturation of the shading in Fig. 7 is hard to follow.

Thanks. The wave activity flux diagnostics were intended to show the westward-directed flux over the topography and to help interpret the locally amplified geopotential anomaly over the stratocumulus area compared with the open-ocean area (S6 in the Supplement). However, we agree that topographical amplification is not the focus of this paper, and this detailed discussion may distract readers from the key findings. We have therefore removed the wave activity flux diagnostics from both the Methods section and the plots. We've also reduced the saturation of the shading in Fig. 7 and S6.

- Figure 10. I do not think the evidence in Fig. 10 is sufficient to warrant the conclusion "Figure 10 suggests that it might be due to some shared influence by low-frequency circulations".

We agree that Figure 10 alone cannot establish this explanation. It shows that the circulation and SST patterns associated with high-EIS and high-LCC share broad features but differ in the locations of the anomalies. We propose that these shared patterns may contribute to the strong EIS–LCC correlation by affecting the two indices through different processes. We have revised the text to distinguish this interpretation from the observed patterns and to clarify that further quantitative testing is needed (Section 5.3).

- The text would benefit from proofreading. There are a lot of typos. Here are a few examples but there are many more.

Line 21: "later studies with contradict results"

Line 28: "Regarding the large-scale mechanism"

Line 41: "Above discussion highlights"

Line 42: "there are several questions remain"

Line 59: "1987-01-01–2019-12-31" Why not just spell out Jan 1987-Dec 2019?

Line 103: "The tropical trapped"

Line 342: "Instead of the tropical thermodynamic framework, we find an extratropical dynamics control EIS variability."

Line 189: "Figure 2 shows the zonal wavenumbers spectra"

Line 263: "When a Rossby ridge approaching Sc"

Thank you for pointing them out. We've made these corrections and checked other typos through the manuscript.