RC2

This study looks at the connections between large scale circulation of the atmosphere and the semi-permanent subtropical eastern ocean low cloud decks. To do this the authors examine the relationship between a measure of the static stability of the atmosphere (estimated inversion strength, EIS) and low cloud cover (LCC) over the major eastern subtropical low cloud cover regions. To do this, the authors use software that can track wave and wave-like features in meteorological reanalysis. Lagged compositing is used to evaluate the synoptic/large-scale patterns controlling EIS variability on timescales from synoptic through to interannual. The authors find that circulation patterns, particularly those due to extratropical Rossby waves, influence the LCC and EIS in different ways, suggesting that the EIS is not always a direct influence on LCC especially on synoptic to subseasonal timescales. Rossby wave ridges in the midlatitudes impact EIS largely through the free tropospheric temperature rather than the SST. These appear to be enhanced by local orography. SST changes play a large role in influencing EIS and LCC on interannual timescales.

The paper is interesting and attacks a longstanding question of why EIS is such a strong predictor of LCC on a range of timescales, particularly seasonal to interannual. The approach taken of grounding the meteorological influences on LCC as part of the extratropical and tropical wave patterns in the atmosphere is quite novel and will be of significant interest to those wishing to understand how the meteorological environment influences low clouds. That said, I don't really understand what the MODES software is doing exactly, or why it is really necessary. The use of it makes it difficult to interpret the findings in a more straightforward meteorological framework. Apart from some confusion I had with some of the grammatical expressions, I did find the manuscript to be interesting and compellingly structured, breaking the timescales of EIS and LCC variability into synoptic, subseasonal, interseasonal and interannual timescales. Compositing is used to evaluate the meteorological structures that are associated with anomalously high LCC and EIS conditions. The authors show that examining LCC-EIS correlation on the seasonal cycle hides an important difference between SST warming upstream and LCC. Removing the annual cycle makes this more apparent. The manuscript will be of interest to readers of ACP, and I suggest the authors consider my mostly minor comments before revising.

Thanks a lot for these general comments. We use MODES because it can decompose different wave types, so that we can see dynamical components more clearly. We also use composite analyses, which are performed in physical space without using MODES, to test the results. However, those tests are not put in the manuscript to disturb readers from the key information. Only additional information given by the composite analyses is shown in the manuscript. Please see the test in the response letter to RC1.

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

SPECIFIC COMMENTS:

Line 5: What does interseasonal mean when the record is deseasonalized?

Here, “interseasonal” refers to anomalies on timescales from 3 months to 1 year after removal of the climatological seasonal cycle. It can be, for example, the enhanced monsoons from climatology. The phrase “after deseasonalization” has been changed to “climatological seasonal cycle removed”. (Line 8)

Line 17: The STBL is not always well-mixed. Most of the Sc on Earth is probably not in a well-mixed PBL.

Thanks for this comment. The “well-mixed” has been removed. (Line 29)

Line 22-24: These statements are not necessarily contradictory. What controls Sc in the current climate need not be the same as what controls climate changes.

Yes, we agree. Here, we mean that the processes underlying the EIS–Sc relationship need to be understood, rather than assuming that the same relationship applies under different climatic conditions. We have rephrased the original sentences (Lines 35-39).

Line 24/25: Provide evidence to support this.

Some references supporting this have been added. (Line 43)

Line 119: Is the "Sc area index" the same as the LCC?

It is $\mathcal{I}$, which represents either LCC or EIS in the stratocumulus area. For each of the five regions and each of the four timescales, we calculated the regression between χ (global circulation) and local LCC or EIS. This section has been rewritten due to the comments of RC1. (Section 2.4)

Eqn. 4: What if 1000 hPa is below the surface?

Thanks for bringing this up. To avoid the potential use of below-surface values, we have replaced the 1000 hPa winds and temperature used in Eqn. 4 with 10 m winds and 2 m air temperature, respectively. The revised results (the left panel of Fig. 9) are consistent with the conclusions from the original results.

Line 176: EIS has to be controlled by T700 on synoptic and subseasonal timescales given that SST (which controls the near surface temperature) does not vary strongly on these timescales.

Yes, we agree that the dominant role of T700 in EIS variability on synoptic and subseasonal timescales is expected. Fig. 1 is intended to provide background information for the subsequent analysis on different timescales. This paragraph has been rephrased to explicitly describe this expected behavior (Lines 251-255).

Figure 1: Abscissae are not labeled. Should say "days". Also labeling on abscissa should be 10^3 for the rightmost value, not 10.

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 areas and with labels). (Figure 1)

Line 206: I am unclear how the waveguide assertion is deduced from Fig. 3. The Rossby wave pattern does not seem to move along these jets - they often cross the jets.

Thank you for this comment. We agree that Fig. 3 does not provide sufficient evidence for the waveguide interpretation. We have therefore removed the waveguide discussion and the causal interpretation between the weaker synoptic variability in SP and SA and the role of jets. The revised text describes only the spatial relationship between regions, subtropical jets, and geopotential height perturbations (Lines 290-292). The polar-front jet markers have been removed in Figures 3 and 4.

Line 221: Does Myers and Norris show that EIS and subsidence are not correlated? The authors should provide more specific info on this. Fig. 9 in Myers and Norris appears to show that EIS and subsidence are correlated positively.

Myers and Norris (2013) found that EIS can increase Sc at fixed subsidence, while subsidence can decrease Sc at fixed EIS. Here, our intention was to discuss the distinct cloud responses in the context of the spatial offset identified here, which suggests that increases in EIS need not be accompanied by stronger area-mean subsidence over the Sc regions. The paragraph has been rewritten to clarify this distinction. (Lines 306-313)

Line 305: I don't see an 80% difference in Fig. 9b. Please explain what is meant here. Do the authors mean 8%?

The approximately 80% difference refers to the relative increase in the peak-to-trough range of LHF in high-LCC cases compared with high-EIS cases over lags of −6~0 days. It is calculated as $\frac{[\max(\mathrm{LHF}_{\mathrm{high-LCC}})-\min(\mathrm{LHF}_{\mathrm{high-LCC}})]-[\max(\mathrm{LHF}_{\mathrm{high}})-\min(\mathrm{LHF}_{\mathrm{high-EIS}})]}{\max(\mathrm{LHF}_{\mathrm{high-EIS}})-\min(\mathrm{LHF}_{\mathrm{high-EIS}})}$. We have revised the text (Lines 413-415).

Line 312: The decoupling induced by increased LHF involves increased entrainment, which then suppresses mixing through the entire PBL, but this would take time.

Thanks for this comment. It has been added to the sentence. (Lines 424-426)

GRAMMATICAL ISSUES:

Line 21: "contradictory" rather than “contradict"

Line 100: "type" rather than “types"

Line 122: "with deep convection", rather than "with a deep convection”.

Line 133: "mass" not “mas"

Line 209: "Close to”.

Thank you very much for the above grammar suggestions. We’ve made these corrections.

Line 263-264: Grammar. What does approaching Sc mean? Approaching the climatological region of peak Sc?

Yes, we have replaced “Sc” with “the Sc area” and added a clarification where the term is first mentioned (Line 49).

Line 308: What does "from the west poleward" mean? That would be from the southwest?

In the Northern Hemisphere, it means from the northwest; in the Southern Hemisphere, it means from the southwest. We have revised the wording to “from the west and poleward side of the Sc area”. (Line 420)

Line 336: This is not a sentence. Please adjust.

Line 337: I do not understand what is meant by "the preference of regions still shows". I can't follow much of this paragraph to be honest. Please consider rephrasing.

Answering the above 2 points together. By “the preference of regions”, we meant that LCC is more closely associated with upstream SST warming, while EIS is more closely associated with local SST cooling. We have rephrased the sentences. (Lines 459-463)

Line 359-360: I can't follow this. What does it mean to say a variable prefers winter conditions? It is high in winter? Low?

Thank you for raising this question. We meant that high-EIS events identified from the deseasonalized time series occur more frequently in winter than in summer. Removing the climatological annual cycle removes the mean seasonal differences but does not eliminate seasonal differences in anomaly variability or event frequency. The sentence has been rephrased. (Lines 503-505)