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Dr. Graham Feingold,
Editor
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Dear Dr. Feingold:

We sincerely appreciate this new opportunity to revise and improve our manuscript on "On the relationship between mesoscale cellular convection and meteorological forcing: Comparing the Southern Ocean against the North Pacific".

In the revised manuscript, we have made modifications to address your questions and suggestions.

Below are our point-to-point responses to each of your comments.

Sincerely,
Francisco Lang

***Editor***

1) I have some difficulty with the fact that the results are not always clear on differences between how processes play out in the subtropics vs. the mid latitudes in SO and NP. A clear example is that of the SCT, which is a subtropical phenomenon driven by warming SST and may include a role for drizzle (e.g., Yamaguchi, Eastman). The discussion on lines 370-377 mixes subtropical-related work (Eastman) with mid-latitude work (Abel) leading the reader to assume that the mechanisms are the same. Another example of a transition is that of a pocket of open cells which occurs via drizzle, without a clear change in meteorology. A more careful categorization/organization of the role of processes driving transitions is required. I believe this is in line with Reviewer 1's major comments (1) and (2).

R: Thank you for your suggestion. We have revised the paragraph to more clearly distinguish between the processes influencing the subtropics and mid-latitudes. In this revision, we clarify that in the subtropics, the transition from closed to open MCC clouds often occurs under uniform meteorological conditions and is driven by strong winds and intense drizzle, as outlined by Eastman et al. (2022) and Yamaguchi et al. (2017). In contrast, over the mid-latitudes, precipitation significantly influences the transition from closed to open cloud formations during MCAOs, primarily due to the decoupling of the boundary layer, as demonstrated by Abel et al. (2017) and Tornow et al. (2021). We apologize for the lack of clear distinction in the previous version of our manuscript.

2) While I believe there is merit in a monthly data analysis, I am not content with your response to reviewer 1 regarding the choice of monthly rather than daily data. I don't understand the response: "Calculating hourly frequency of occurrence is not feasible because hourly data represents Open and Closed MCC as integer numbers, lacking physical meaning, and serving solely to distinguish between cloud types." If the 'integer' indicates that an open or closed MCC occurred, why would it not be useful for calculating a freq of occurrence? I also don't follow the logic in the next sentence:

"Furthermore, larger timescales, such as daily frequencies, do not provide a robust sample size for calculating frequencies and correlating them with the meteorological parameters. Thus, we believe that the most robust correlations are derived from monthly frequency of occurrence."

First, daily frequencies are shorter timescales, and these would increase the sample size, though probably also increase the 'noise'. Please provide a clearer explanation of your choice of monthly cycles in the text.

R: Our apologies for the lack of clarity in our previous response. In our initial approach to calculating correlations, we used monthly averages, based on the assumption that longer time scales would effectively filter out noise, as suggested by the editor. Furthermore, we aimed to maintain consistency with prior studies, such as those by McCoy et al. (2017) and Muhlbauer et al. (2017), which also analyzed monthly correlations between the meteorological variables used in this study and the frequency of MCC clouds.

Nonetheless, we have calculated the correlations using daily frequencies of MCC clouds and daily averages of the $M$ index, EIS, SST, and near-surface wind speed. The results showed that, while daily correlations exhibit more noise compared to those on a monthly scale, resulting in lower correlations, their spatial patterns still align with the monthly correlations. We have included the daily correlations in the Supplementary Material (Figure S3).

**Other comments:**

Did you intend to remove the sentence prior to the last? The information appears to be the same.

R: Our apologies for the confusion. We rephrased the sentence in the previous revision as requested by referee #1.

Line 48: Somewhere here please also mention POCs — also a transition of closed to open, with no distinct change in meteorology.

R: Thank you for your suggestion. In the introduction, we have noted that in the subtropics, the lack of substantial meteorological differences between open and closed MCC clouds suggests a predominant role for the precipitation mechanism.

Line 49: please clarify that the transition via drizzle discussed by Yamaguchi occurs during advection over warmer ocean temperatures.

R: We have revised the sentences to indicate that one mechanism driving the transition is advection over warmer waters, where drizzle leads to the breakup of closed MCCs into open MCC clouds, as described by Yamaguchi et al. (2017).

Missing important references to ACTIVATE (NW Atlantic) results (e.g. work of Tornow).

R: We have included a sentence about the work of Tornow et al. (2021), which uses a case of MCAO within the Aerosol Cloud meTeorology Interactions oVer the western ATlantic Experiment (ACTIVATE) campaign.

Please clarify what the correlation is between.

R: We have rephrase the sentence to clarify that the correlations is between closed MCC and EIS.

**References:**

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Muhlbauer, A., McCoy, I. L., and Wood, R.: Climatology of stratocumulus cloud morphologies: microphysical properties and radiative effects, Atmospheric Chemistry and Physics, 14, 6695–6716, https://doi.org/10.5194/acpd-14-6981-2014, 2014.

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