Major comment:

Recommend labelling Figure 3 to be Figure 1. The discussion of Figure 1 is primarily based on Figure 3, thus shifting order can help the readers a lot if specific schemes are shown first.

In response to your recommendation, we have changed the order of the figures to enhance the readability and understanding of our manuscript.

Section 2 largely lacks a detailed description of precipitation observation. Lines 103-104 are too limited for the interpretation/evaluation of Figure 6. What are the observed precipitation frequency and intensity time resolution; what is precipitation frequency defined; what are the data quality controls and raw data processing processes (acknowledge the mention of the 90th outlier removal in a later section); how large the uncertainties are; what is the precipitation phase; why using the MEAN precipitation to relate to the MEDIAN of CCN; can Figure 6 has variations/ranges shown, e.g. in shading, together with the mean/median value of each month.. Briefly mentioning some of the info above would help validate Figure 6 given using rain gauge data itself can be an advantage compared to the reanalysis data.

We have addressed your concerns by providing additional details on the precipitation data (e.g., lines 113 to 119) in the revised manuscript. Specifically, we clarified the frequency of precipitation and explained why we use the mean precipitation, in contrast to the use of the median for CCN. Since 88% of the time (hours) there is no precipitation, the median value is 0, which makes the mean more appropriate for capturing the range of precipitation events. In contrast, the median was used for CCN data to mitigate the influence of outliers.

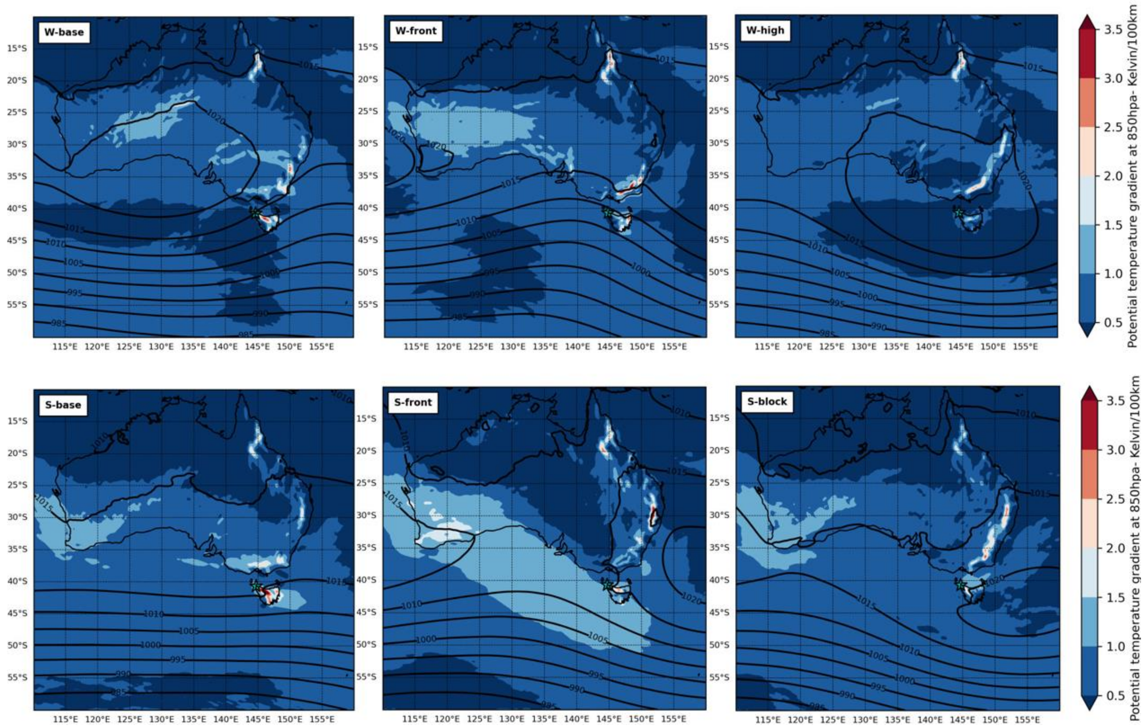
Additionally, we have updated Figure 6 (now Figure 5) to incorporate all cases within our two baseline clusters. We now include the 25th and 75th percentiles for CCN data, providing a clearer depiction of the distribution. For precipitation, we added the 95th percentile, though the 5th percentile remains at 0, which would yield an infinite log value.

Section 3 serves as the most fundamental part of this paper, which can go further into depth. Recommend adding wind field and front/ridge location on Figure 3 to, 1) help the description of Figure 4 which refers to the consistent surface wind direction and speed at CGO, 2) help the reader see the relative locations of the surface front system and the CGO, which is important for convergence/divergence flow and can be used to explain the Radon concentration as well as the spread extent in history trajectories (related to the wind speed according to Line 181). Recommend the description of inversion from altitude, depth, and strength perspectives, and a more quantitative description can be added in for lines 150-165. For example, it can start from one cluster, mentioning the specific numbers of inversion layer location and depth, and maybe also the estimated inversion strength, boundary layer depth, potential cloud layers locations, where is the referred FT, and how large the Relative Humidity is through the FT. Numbers are vital for intra-comparison in Figure 4 between clusters. Also, in Line 161, "the relative humidity is greatest for W-front suggesting heavier precipitation rate", the small gap between the two temperature profiles illustrates a more humid condition but does not necessarily suggest a heavier precipitation rate. The mention of open and closed MCC by cross-citing is wonderful and can be shifted here from Section 4.1 which is about precipitation.

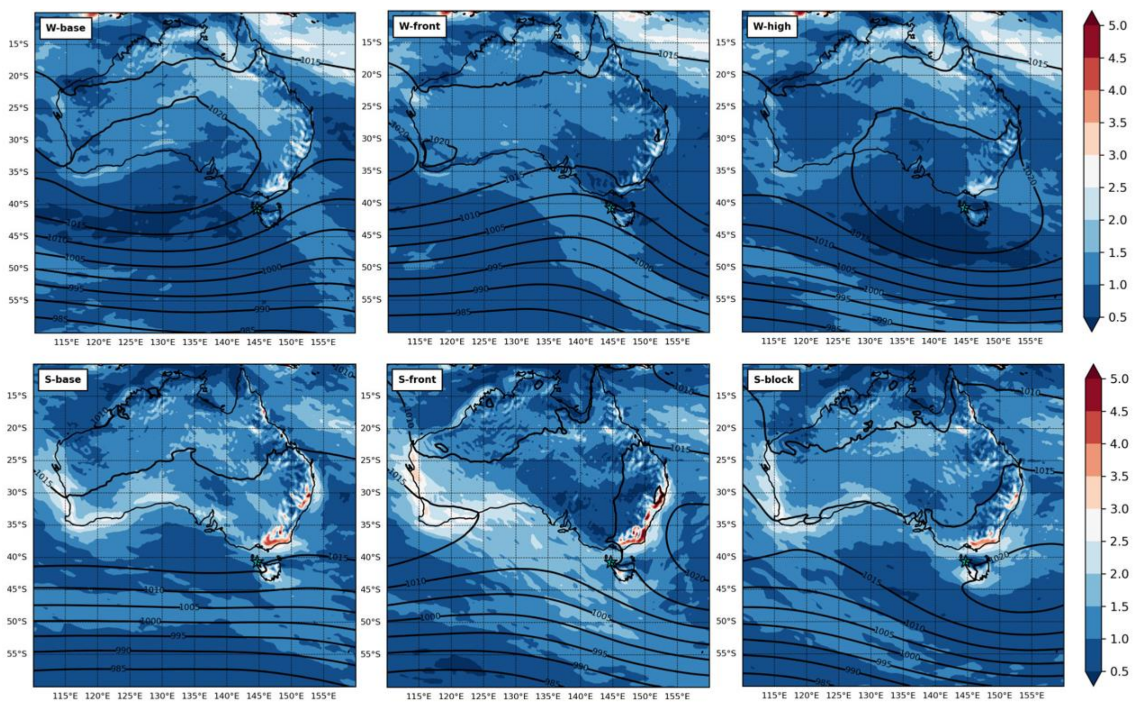
We have added wind vectors (10m wind) to the figure (New Figure 1) to provide clearer visualization when comparing the composite soundings with the composite MSLPs.

We explored using the composite field to define a front based on the gradient of potential temperature or equivalent potential temperature of the 850 hPa (Thomas and Schultz, 2019) but believe the results can easily be misinterpreted. As the clustering algorithm is centered on CGO, the physical coherence of the meteorology decays with distance from CGO. In particular, the signal for the front (at any threshold) decays rapidly at higher latitudes over the storm track, where there is great variability in the location of the cyclone. Similarly constructing a composite of frontal locations becomes dispersed at high latitudes. Even near CGO the frontal position – and orientation (east/west vs north south) – experiences a high degree of variability. The summer baseline composite – when the subtropical ridge is at these higher latitudes - is particularly variable.

We can put the trough (and ridge) axes onto the MSLP, but this is different than the frontal position.

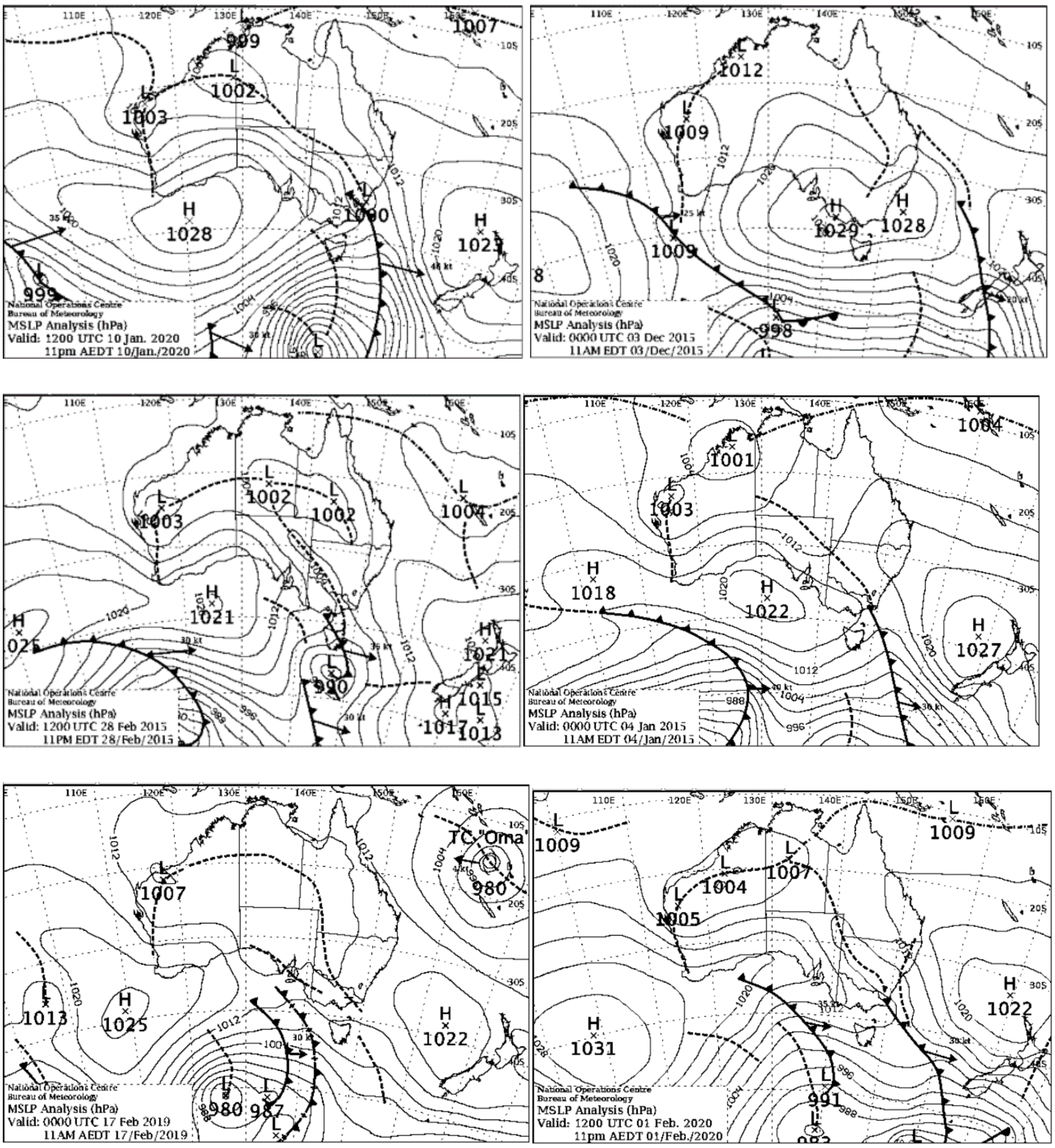


Potential temperature gradient at 850 hPa (°K/100km) for 6 clusters



Equivalent potential temperature gradient at 850 hPa (°K/100km) for 6 clusters

Here, we present an example of six cases within the S-base cluster, illustrating the significant variability in frontal locations within the same cluster. This diversity highlights why creating a composite of these frontal positions is not feasible.



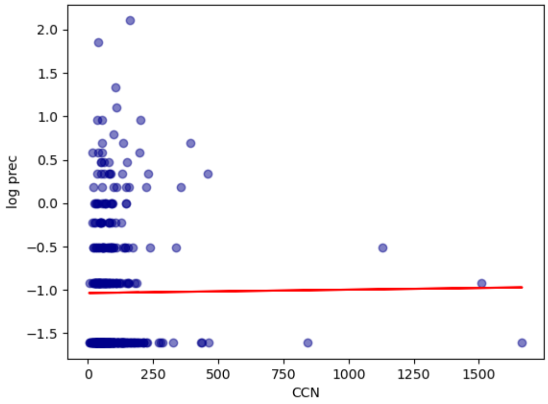
We have revised lines 163-184 to include the specific measurements of inversion height and estimated inversion strength (EIS). Furthermore, as suggested, we have relocated the discussion comparing the baseline clusters with the open and closed MCCs to this section (lines 170-176) to better align with the synoptic meteorological differences within our clusters. Moreover, we have removed the mention of relative humidity from this section, as you correctly pointed out that higher relative humidity does not necessarily correlate with higher precipitation. Instead, we have incorporated the discussion of relative humidity into Section 4.1, where we discuss the precipitation in greater detail. In this section, we have quantified relative humidity and total precipitable water to better support our discussion (e.g., lines 226-236)

Thomas, C. M., and D. M. Schultz, 2019: What are the Best Thermodynamic Quantity and Function to Define a Front in Gridded Model Output?. Bull. Amer. Meteor. Soc., 100, 873–895, https://doi.org/10.1175/BAMS-D-18-0137.1.

Section 4: reading until now, I recommend combining Figures 1 and 2 in one figure to show the consistency between each other, and adding one new figure showing the correlation plot between Nccn and Radon concentration and that between Nccn and Precipitation, this can be a key figure for the follow-up discussions in Section 4-6. Looks like the correlation has been done already given some of the P-values are provided, plots may better show how related they are.

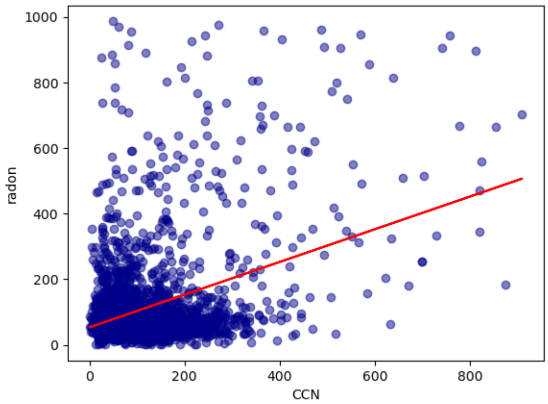
As per your suggestion, we have combined Figures 1 and 2 into a single figure to better illustrate the consistency and relationship between the seasonal migration of the subtropical ridge (STR) and the clustering results (now called Figure 2).

Regarding the additional correlation plots between N_{CCN} and Radon concentration and between N_{CCN} and Precipitation, we have generated these plots: the correlation coefficient between Precipitation and CCN is very low (0.007) and not statistically significant (p = 0.9). The scatter plot does not show any clear trend, likely due to highly intermittent nature of precipitation over the region as discussed in Alinejadtabrizi et al., (2024). We also looked at the lagged correlations of the precipitation and CCN, but it remains statistically insignificant.



Scatter plot illustrating the correlation between CCN and precipitation in base line clusters.

While the correlation between Radon and CCN is statistically significant (correlation coefficient = 0.4, p ≈ 0.0001), the scatter plot does not provide a clear visual representation that would effectively contribute to the discussion in the main body of the manuscript. The significant correlation suggests an association, but due to the scatter in the data, the relationship is not strong enough to warrant inclusion as a primary figure.



Scatter plot illustrating the correlation between CCN and radon in base line clusters (for the CCN and radon concentration less than 1000)

We have discussed these relationships in more detail within the manuscript (e.g., line 272) to provide further clarity. Given the lack of strong visual clarity in the plots, we have opted not to include them in the main body.

How is Radon distributed through the troposphere, do the air mass coming from the boundary layer and FT above the Australian continent have the same Radon concentration? If not, the altitude of air mass back history would be of importance here, before any discussions about staying over terrestrial in history for long is related to the higher Radon concentration.

We are unaware of any observations of the vertical distribution of radon through the atmosphere, either over the Southern Ocean or over Australia, that would be suitable for this climatological study.

The largest concern though comes from the interpretation of Figure 7 and the descriptions in Section 4.2. This needs further clarification. The log10 refers to the log10 scale of the "W-base trajectories subtracted from the S-base"? Does this mean the subtraction results have to be positive to be able to be "logged"? How would the back trajectory starting from 2500m above the GCO be directly useful for surface CCN from the understanding of FT entrainment? I assume the logic is that, if the FT air masses originate more from above the Australian continent, then the FT may contain more aerosols and thus can be a strong source for the surface CCN budget. However, are there any conditions that have to be met so that the 2500m air subsides into the boundary layer? What are the roles of the two below scenarios respectively, 1) an air mass originates from above the Antarctic at, for example, 2500m, and gets transferred into the boundary layer of GCO, and 2) as Figure 7 shows, some air masses from above the Antarctic during the winter travel and arrive at the 2500m right above the GCO. In particular, the 2500m starting level shows more FT continental aerosol information but how can this be used for the surface/below clouds CCN budget discussion without details of discussion of "processes right above the GCO between the surface and 2500m" such as mixing/exchanging and cloud processing? For Line 249, in Kang et al. (2022), FT CCN was quantified using the UHSAS measurement in the FT, and for surface sources, only the wind-oriented primary CCN is quantified in the budget. However, the logic in this paragraph originates from (line 242) whether the less pristine are caused by/related to the biological production in the summer. Could Kang's (2022) paper be used here to support/debate the conclusion? Instead, Kang's conclusion about FT entrainment influences the NCCN more can be heavily related to the surface biological production of, e.g., DMS.

We fully respect and understand the need for further clarification in interpreting Figure 7 and the associated discussion in this section. We have addressed your comment by splitting it into smaller parts, and by doing so, we hope to enhance the clarity of this section.

The log10 transformation is applied to the individual normalized trajectory counts (frequencies) for both S-base and W-base clusters before performing the subtraction. Therefore, the operation being represented is the difference between the logarithms of the trajectory frequencies (log10(S-base) - log10(W-base)). Mathematically, this is the same as the log of the ratio of the frequencies. We have adjusted the colour bar caption in this to clarify this process and to avoid any further confusion. Additionally, following suggestions from another reviewer, we have included a similar plot for the boundary layer back trajectories, which show consistent patterns—air parcels originate more frequently from lower latitudes in summer, while in winter, they primarily originate from higher latitudes.

The original Figure 7 cannot be used to distinguish between these two scenarios that you raised, as it only pertains to parcels at 2500 m over CGO at the start of the back trajectories, not simultaneous parcels in the free troposphere and the boundary layer. We accept that we have no knowledge of either free troposphere entrainment or potential cloud processing that could affect any relationship between free troposphere and surface CCN concentrations. You are correct that we are simply exploring the potential role of free troposphere transport within the framework of this synoptic typing.

As you correctly pointed out, our assumption is that entrainment from FT occurs, and now the question is about the sources of air masses in the FT level. If these air masses originate more frequently from above the Australian continent, they likely contain higher aerosol concentrations, which could significantly contribute to the surface CCN budget. However, determining the exact conditions under which the 2500 m air entrain into the boundary layer required the cloud processing and entrainment information which is not available over the CGO. We further, examined plots showing the distribution of frequency of the 72-hour back trajectories' heights (added as supplementary materials-Figure A1) which demonstrate the subsidence of air parcels, however, ERA5 do not explicitly capture entrainment processes as they are sub-grid scale phenomena.

Therefore, our analysis relies on the assumption that FT entrainment acts as a potential source contributing to the observed seasonality of CCN based on the available datasets. We added lines 287 to 295 in an effort to make this discussion clearer.

In response to the comment regarding the Kang's paper, we have also clarified our discussion of Kang et al. (2022) in the revised manuscript. We acknowledge that Kang et al. (2022) emphasize the importance of free troposphere entrainment from the biogenic productions, however, we focussed on the entrainment from continental sources. We have revised our paragraph to reflect this nuance (e.g., lines 275 to 278)

In general, Figure 5 is about the advection history. While Figure 7 is also an advection history (FT though), it is analyzed as a local source for the surface CCN budget at GCO. Then the gap question would be, how efficient are the advections and the local FT entrainment? Without the filling of this gap, Figure 7 only talks about "the potentials" of FT air mass feeding the surface CCN.

As we mentioned in a previous comment, we do not have direct data on entrainment or advection to assess the efficiency of advection and local FT entrainment over CGO. Nor can we get this from the reanalysis. Our analysis was conducted based on the assumption that entrainment is occurring, with the focus on understanding the differences in the origin of the air masses. As you correctly pointed out, this approach does not allow us to confirm the role of FT entrainment in feeding the surface CCN. Therefore, all we can discuss is the potential contribution of FT air masses to surface CCN. We tried to clarify this in lines 287 to 295.

Section 4.3 looks quite independent from the other part of the paper, in particular since the Macquarie Island data are not shown/heavily discussed. Suggest a removal of this section/shift to supporting materials so that the most important scientific question (precipitation/FT transportation) can be focused on using the 11 year data from the GCO.

We agree with your reservation about this section and have removed this section from the revised manuscript.

Maybe briefly mention the reasons for some methods that are used. Why is 72h chosen for back trajectories instead of 35h or 120h? Why is 2500m chosen as a reference for FT? Why are two-tail-student-t-tests used for precipitation while Whitney-U tests used for CCN and radon?

We selected 72 hours for the back trajectories to demonstrate the connectivity between our study area and both lower latitudes (the continent) and higher latitudes (Antarctica). Additionally, 72 hours aligns with the typical time scale between cyclones in the Southern

Hemisphere, where approximately two cyclones pass through each week. We added a note in this regard in lines 190 to 193.

We selected 2500 m as the free troposphere level based on field observations from Lang et al. (2021), which reported that boundary layer clouds, specifically open and closed MCCs generally form below 2.5 km. By choosing 2500 m, we aimed to ensure that our analysis captures air parcels above these shallow cloud formations. We have clarified these points over line 280.

We used the two-tailed Student's t-test for precipitation because we analyzed the mean precipitation, and the t-test is appropriate for comparing means between two groups. For CCN and radon, where we analyzed the medians, the Mann-Whitney U test was used as it is more suitable for medians. We have added a note in this regard in lines 226 and 212 respectively.

Minor comments:

• *Suggest making the abstract only one paragraph, with more quantitative descriptions. For example, what is the referred "deeper boundary layer" (line 9); specifically how do STR moderate the advection of air masses (line 15).* We have modified the abstract accordingly.

• *Line 49-50, "since new particle formation is rare in the MABL" turns out to be a fundamental basic assumption for this study, which will need citations to support. Zheng et al. 2021 actually states that there are observed new particle formation (NPF) in the remote MABL. Zheng et al. 2018 indeed mention that the NPF events within remote MBLs like the ENA are infrequent, but this is only done through citing other papers, which should be cited instead.* Related references have been added.

• *Figure 2 caption, what are the hollow circles on the plot?* They are the outliers. We added this to the figure caption.

• *Line 60 grammar check.* The paragraph has been revised for better clarity and flow.

• *Line 63 grammar check.* The paragraph has been revised for better clarity and flow.

• *Line 40, ".. marine biological sources predominantly govern Nccn during the summer…, ,multiple elements contribute to the CCN throughout the year…" recommend specifically mention what are the multiple elements.* Done (line 38).
• *Similarly Line 43, "various other sinks and sources influence the CCN budget" what the "various other".* Done (line 41).
• *Line 43, "coarse mode sea salt", coarse mode normally refers to aerosols larger than 1um, delete the "coarse-mode", sea salt plays a crucial role in CCN…* Done (line 38).
• *Line 43-44, recommend adding in related publications about SO aerosols, for summer: Fossum et al. 2018 Scientific Reports, about seasonal variations of CCN. Humphries et al. 2023 ACP, Niu et al. 2024 JGR-A, etc.* Done (line 42).