The study by Humphries et al. (2022) reports two years of recently acquired CN and CCN measurements on Macquarie Island, located in the mid-latitudes of the Southern Ocean. While this data set is still too short to study certain aspects and/or validate certain hypotheses, and the absence of size distribution data is also a limitation, these observations are very valuable as they contribute to characterise the aerosol in a region of the globe which remains poorly documented. In particular, as the authors point out, the possibility of documenting the seasonal variation of the concentrations is a real plus compared to campaign data, which only concern short periods of time and often target the same seasons; the combination with the other data sets explored also makes it possible to document the spatial variability of the particle concentrations in this region, which is particularly sensitive to natural (and in particular marine) aerosols and representative of pre-industrial conditions. I therefore recommend the publication of this paper, which is otherwise well written. I would, however, suggest restructuring section 3 to improve clarity. In particular, I would recommend merging sections 3.1 and 3.4.1 on the one hand, and 3.2 and 3.4.2 on the other hand, as they deal with the same topic, while keeping all the figures since they provide a complementary view. This would make it possible to group the information together and avoid repetition as well as seemingly contradictory conclusions, such as that in L372-373 ("These data all fall within the ranges of those observed at KCG.") and 466-468 ("Voyage data agrees well with station data at mid-latitudes, however this isn't the case at the northern latitudes where voyage data are significantly higher than station data from KCG.")

I also have some specific comments listed below (authors are invited to reflect the proposed suggestions/comments on the abstract and conclusion where appropriate).

P4, Methods: the data sets are well described but because of their number and the different treatments applied to "clean" them, it remains difficult in the end to get a clear overview of data availability for the different sites/campaigns. Since the Macquarie Island data are central to this study, I would suggest at least moving Figure A4 into the main text, to illustrate the explanations in Sections 2.1.1 and 2.1.2.

P6, L150-151 : « hourly statistics were calculated for each supersaturation": to clarify, as a result of the measurement sequence and data filtration, the hourly statistics are calculated for each supersaturation on the basis of 7 min of measurement (from 2/11/2016), is that right?

P7-8, L195-198: can the authors give a brief indication of the frequency of occurrence of data from the baseline sector? This would help understanding why the amount of data finally involved in the statistics (as indicated in figures 4-5 and A6-A7) is so limited in relation to the length of the period studied (11 years).

P8, L200-201: « The station has a significant sea ice presence around the station": this part of the sentence should be reworded.

P10, L271: isn't it after CAPRICORN1 voyage?

P11, Results and Discussion, general comments:

- I would recommend balancing the interpretation of certain observations by taking into account the variability associated with them or the magnitude of the differences observed; in particular, the repeated use of the terms "substantially" and "significantly" seems to me to be sometimes inappropriate (e.g. L344, L467, L504, L516, L519);
- Unless I am mistaken, the authors do not discuss the possible impact of 1) the difference in length of the datasets of the different stations and 2) the time lags between some datasets (e.g. SIPEXII in 2012, Cold Water Trial in 2015 vs. MQI in 2016-2018) on the observations, and, in relation to these aspects, a possible impact of e.g. interannual variability? Can the authors comment on this point?

P11, L309: « Note that the seasonal cycle is plotted over 18 months to show the maxima and minima clearly, is presented in Appendix Figure A1a”: extra "is" should be removed.

P11, L310: « with the highest concentrations observed at the northernmost station, KCG. »: I do not fully agree with this statement since while the median CN concentration at KCG is often above that of other sites, this is not the case in February, when the concentrations at SYO are higher (visible on both the median and quartiles). I would simply suggest to slightly modify the sentence: « with the highest concentrations often / most of the time observed at the northernmost station, KCG ».

P11, L315-316: For KCG and SYO: looking at Figure 2.b, I would say that the concentration increase is already initiated in August (rather than September, as indicated in the text); this is especially the case for KCG, and seems more consistent with L387 (« This is in contrast to the CN minimum, which only reaches its minimum in June and July. »).

P11, L315-316: « Springtime increases begin almost simultaneously in September at both KCG and SYO, although the increase at MQI is comparatively”: the connection between the two parts of the sentence does not seem appropriate.

P13, L348-349: « and is likely the explanation of the difference observed between the stations during wintertime across the three stations”: this part of the sentence should be reworded.

P11-14 : Although explanations are given by the authors on the differences observed between the three sites, in particular in relation to their geographical position and configuration (small island vs. more extensive coastline), the singularity of the peak observed in February at SYO and the fact that these high concentrations are not found during the campaigns makes me curious: is this peak observed every year with a comparable amplitude, or are the observations in figure 2.a largely influenced by one or several years in particular?

P15, L394-395 : « the significant spike in CCN0.5 concentrations at MQI in May and June”: from figure 2.c, I would say that the CCN0.5 peak is only seen in June; similarly, with the exception of CCN0.45, the increase in concentration for the other supersaturations is not striking in May in Figure A2, but again more marked in June.

P15, L424-426: « Activation ratios of unity can be interpreted as all available CN data can serve as CCN, meaning the accumulation and larger Aitken modes are dominant, and species are typically water soluble. Lower activation ratios mean either the composition is largely organic species, or a strong nucleation or Aitken mode is present, or both. ». This formulation seems to me to be oversimplified / incomplete since, beyond the intrinsic properties of the particles (size, chemical composition), supersaturation is a parameter to be taken into account in the capacity of the particles to serve as CCN: for a high supersaturation, it is not excluded that a dominant Aitken mode may be the source of a significant number of CCN.

P16, L445: « Observations in the high latitudes in summer fall into the range of 0.6 to 0.8, similar to many of those classified as mid-latitude.”: it is not just 4 out of 10 points? Similarly, the use of "many" at L443 seems to me to be too strong.

P16, L446: couldn't this hypothesis be tested by an air mass back-trajectory study? (Similarly at L410, couldn't the possibility of long-distance transport from Australia be assessed by checking air mass back-trajectories?)

P17, L461-463 : « This is consistent with previous observations (Humphries et al., 2016, 2021a) which found that concentrations within the sea ice region around the Antarctic continent (…) were found to exhibit distinctly different aerosol properties to observations on the continent itself.” : wording should be checked.

P19, L512-513: «concentrations in the northerly sector are drastically higher than those observed at KCG. This is likely a result of the small sample size (only 77 hourly points) ». There is a difference in the 75th percentile concentrations in summer of the same order as that observed in autumn (219 vs. 320 cm-3 in summer, 119 vs. 244 cm-3 in autumn) which is not discussed. Is the size of the campaign dataset (although larger than in autumn) suspected in this case too of explaining at least part of these observations?

P20, L527-528: « Overall, it can be concluded from these data that long-term stations are good representations of their respective latitudinal bands. » This conclusion does not seem to me to reflect previous discussions, in which notable differences between station observations and campaign data at equivalent latitudes are highlighted, particularly for CCN0.5.

P20-21: I would suggest moving L527-543 to the conclusions.