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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Effects of continental emissions on Cloud Condensation Nuclei (CCN) activity in northern South China Sea during summertime 2018" *by* Mingfu Cai et al.

Anonymous Referee #2

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Cai et al. presented a comprehensive observational dataset from a shipborne measurement over the South China Sea, near Guangzhou, China. As the authors argued in the manuscript, the survey is conducted in the area with a complex source profiles from different anthropogenic influences in the marine background. Although the dataset is worth publishing in this very reason, the presentation and the data analysis in the manuscript can be substantially improved.

The manuscript is quite difficult to follow as it is just describing a lengthy dataset by correlating each other. It is highly unclear what is the main scientific conclusions of the data analysis. This issue is well represented in the lengthy abstract of the manuscript. It

is just way too long, which makes difficult to grasp the scientific merits of data analysis. I strongly recommend the authors to remind themselves a couple of main scientific findings that they hope to come across in the manuscript for the revision.

Speaking of main scientific findings in the manuscript, the presented discussion does not support them very well. For example, the analysis for the different air masses, came across during the cruise, should be developed further more thorough fashion. I would present available ground data either concentrations of emissions from the different region to discuss their characteristics to elucidate how the chemical evolution affects the outflow to evaluate whether the observational result makes sense or not. For example discussion about the presence of sulfate over the South China Sea (line numbers between 269 to 271) can certainly go further by discussing upper end DMS emission rates and whether the assumption can account the observed SO2. Another example is in line 342. CO is an obvious long lived tracer for pollution, therefore the correlation of CO with parcels # is not surprising. I would recommend the authors to discuss further more process level aerosol chemistry evolution than these rather one dimensional comparisons of observables. It is even more troubling by attributing biomass burning sources as presented in lines between 403 to 405. I would recommend to take full advantage of your wealthy dataset and back trajectory analysis to solidly argue the origin of the observed airmass of bio mass burning.

In conclusion, I would recommend for a major revision of the manuscript to highlight a couple of major scientific findings and present more process level analysis to highlight those findings.

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