

## ***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 #1**

Received and published: 25 February 2020

Cai et al. present particle composition and hygroscopicity measurements from the South China Sea during a ship campaign in summer of 2018. Their measurements indicate that the particle number, size, composition, and CCN activity vary dramatically depending on wind direction and other meteorological parameters. The authors used HYSPLIT to determine if the sampled air mass traveled over continental pollution sources. The results reported in this study are not the most novel but are useful for the community due to the lack of field measurements. This paper fits with ACP and should be published once the authors address the below comments.

Major comments:

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Line 170: The authors mention that they removed abnormal measurement spikes from their data set. They attribute these spikes to potential interference from their ship or other ships' emissions. This seems rather arbitrary way of throwing out data without more concrete reasoning. Can the authors provide chemical analysis of particles found in these spikes? Does the composition match what is expected for ship exhaust? Also, does the wind direction correlate with when ship emission would impact the container of instruments? How long did these spikes last? How many spikes in the data were there (hard to tell from the Figure S1)? Were the spikes just removed or averaged out? It would be helpful to actually reference the supplemental figures in the main text so the reader knows what the raw data of NCN looked like.

The authors point out that shipping emission likely contribute significantly to particles in the South China Sea (line 120). Why then would the authors throw out observations from shipping emissions? In short, the authors must better justify why these spikes in data were thrown out and to know more definitively what was causing these spikes.

Line 270: authors attribute high sulfate content to ship emissions instead of oxidation of DMS from ocean. Can the authors estimate sulfur emissions from the ocean from previous studies and compare that to what they saw? This would provide more solid evidence that the elevated sulfate content is due to ship emissions than from ocean DMS.

Minor Comments:

Abstract seems unnecessarily long. Would be more readable if it were shortened to include the main point of the paper.

Line 21: high temporally and spatially resolved

Line 82: how was mixing state important for CCN? If it's important, why did the authors not address how mixing state may impact their measurements/conclusions?

Line 102: unclear the sentence starting with Furthermore.

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Line 148: delete both

Line 149: comma before which

Line 161: “, which is listed in Table 1,”

Line 213: remove firstly

Line 299: please use parallel phrasing for this sentence.

Line 385: was likely

Line 405: if the particles are from biomass burning, do the authors observe any of the classic biomass burning tracers (such as K)?

Table 2: the values have too many significant figures given the uncertainties of the measurement.

Figure 2d) what is the black line? And which lines go to which y axis?

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-1041>, 2020.

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