

Interactive comment on “Large increases in N_{cn} and N_{ccn} together with a nucleation-modeparticle pool over the northwestern Pacific Ocean in the spring of 2014” by Juntao Wang et al.

Anonymous Referee #2

Received and published: 25 January 2019

General comments: The manuscript by Wang et al, “Large increases in N_{cn} and N_{ccn} together with a nucleation-mode-particle pool over the northwestern Pacific Ocean in the spring of 2014” reports CCN and CN concentration of maritime aerosols. As the authors said, CCN concentration itself of Asian outflow has not reported as literatures current years. The main result doesn't necessarily surprise; high concentration of CCN over their observation region is predictable from knowledge by previous observation reports for coastal and leeward area around East Asia and models. However, their fundamental data and reports for some events are valuable as observation reports. On the other hand, I was confused by the manuscript because some important information for representativeness and characteristic of the observation data were unexplained or

Printer-friendly version

Discussion paper



added later. This structure and title might mislead the readers to different image from author's assertion before read all the manuscript. Also, some topics were seen as lacking in explanation because the other possibilities were considered insufficiently. I have some question and comments to clarify the manuscript.

Specific comments:

1) Introduction: Authors said "direct observational data of aerosol particles and CCN in number concentrations remain limited in the remote atmosphere over the NWPO and the last spring observation can be traced back to 1996" as the motivation of the study. This is almost true, but this explanation can give different image of no recent studies for CCN in the region because they did not reference the relating studies in their introduction. There are several studies for CCN properties based on observation in similar air mass conditions. I think that CCN concentration tends to be seen as no urgent information because CCN concentration according to various conditions of supersaturations and CN concentration can be modeled by using accurate kappa values. What is the advantage of the direct observation of CCN concentration? Please clarify specific original point and information added to previous knowledge of CCN in remote sea of the East Asia regions.

2) Sections 3.1 and 3.2: Although air mass of the observation period tended to be affected from continental outflow, air mass in same region could be affected from marine air according to meteorological condition. The adequacy and meaning of discussions of continental input and estimation of kappa value depends on air mass tendency of the observation. I think that information of air mass tendency (Figure S3) should be explained before (or with) these discussions.

3) Section 3.2: Duseck et al. (2006) evaluated the correlation of CCN concentration estimated using constant composition or size distribution with the observed CCN; their evaluated point was different from this study. Correlation (R) is unnecessary to be well because aerosol species can have variation. Although authors used a size of "good

[Printer-friendly version](#)[Discussion paper](#)

correlation (best R?)", slope=1 should be treated as most important if the aim was estimation of kappa, for discussion of both 0.4%SS and 0.2%SS. Also, some studies pointed a possibility of biased condition of air mass to the result of good correlation at constant compositions. Air mass condition of the analyses is important information to read implications of good correlation in this study. I think that data BB event was exclude in the analysis should be pointed in the manuscript. (Also, what is a rule of "suspected either BB or dust aerosols"? LEVO concentration?)

4) Section 3.2: For high Nccn/N60 at low CCN concentration, only effect of BB and dust was pointed in this study. Did you consider the other possibilities? Low Nccn can be observed at low Ntotal, low activation ability or both. In the case of Ntotal, there are possibilities of effect of diluting, transport of clean air mass and scavenging process etc. Because scavenging process can preferentially remove aerosols having high ability as CCN, the high Nccn/N60 and low CCN concentration can be observed. How were the Ntotal and the meteorological conditions?

5) The observation was conducted over marine, but comparatively near the continent of East Asia. Authors also suggested effect of continental input strongly. Therefore, I think that their observation result is valuable as "aged" air mass of continental pollution (after a few days) than aerosols over remote marine. Did the CCN properties (concentration and ability) in this study have difference to that of coastal area in East Asia (upwind area) by previous CCN studies? (Schmale et al. (ACP, 18, 2018) compiled recent CCN studies including information of CCN concentration around East Asia, which may also be useful to compare to this study.)

6) Section 3.3: I was confused; which did they assume temporal change of same air mass or regional difference? Although this section discussed mainly change by Hop-pel effect, the difference of number-size distribution can include but only not effect by atmospheric process but also difference of origin of air mass. In this manuscript, many "increase" and "decrease" was used (e.g. P6L30, P7L6, L8, L9 etc.), especially in this section. However, I think that those without temporal change should be replaced "be

[Printer-friendly version](#)[Discussion paper](#)

high” etc. In addition, Fig.1 is difficult to understand temporal change and representativeness (fraction to all period) of the size distribution. Also, in some case, averaged distribution of 2-mode distribution having different peaks can become 3(or 4)-mode distribution. It would be better to add temporal variation of number-size distribution (e.g. to Fig. 2). This is also helpful to show the accuracy of their data screening.

7) Sections 3.4 and 3.5: I was interested in the discussions, but data base on their observations seemed to not be enough to support the hypothesis that air mass was affected from upper layer. Cannot O3 data be used in this discussion?

8) I felt that the title was not sound right. This study did not observe direct relations between increase in CCN and CN and nucleation-mode particles. Also, temporal increase of CCN and CN was not shown in this study.

Technical comment and minor issues

Figure 1: The spots of map can be seen as fixed point. If the data included that during moving of ship, please add the ship track. In addition, legend is unclear and confusable with data spots. Also the direction should change.

Section 2.1: Please clarify where the inlet set. Also, did the data considered particle loss in tube?

Section 2.3: Was the data using screening only FMPS? CN seemed to be no data in period 2.

Section 3.2: Accuracy of kappa estimation depends on size classification. Please show how many bins of the analyzed size range.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-1089>, 2019.

Printer-friendly version

Discussion paper

