Interactive comment on " The impact of aerosol size-dependent hygroscopicity and mixing state on the cloud condensation nuclei potential over the Northeast Atlantic" by Wei Xu et al.

This manuscript presents the CCN closure study over the Northeast Atlantic Ocean. By assuming different mixing states and size-resolved κ values, the measured and estimated N_{CCN} were compared. I do think the results are quite interesting and important for the model work, as the author points out in the conclusion. However, I strongly recommend the author be more careful about the data evaluation, analysis, and interpretation. The following major comments must be satisfactorily addressed before consideration for publication.

Major comments:

1. In Section 2.2.1, SMPS measured particle number size distribution only covers the particle size up to 500 nm. Once you used the integrated particle number concentration from SMPS-measured size distribution, particle number concentration in the size range larger than 500 nm ($N_{>500nm}$) are not considered. $N_{>500nm}$ are CCN at supersaturations >0.20%. From Fig. 2, even the particle larger than 500 nm is not measured, it is clear that $N_{>500nm}$ cannot be neglected. This is also related to the interpretation of slopes in Fig. 4 & 5.

   One solution is to fit the larger Accumulation mode. Based on such a method, the $N_{>500nm}$ can be estimated.

2. In Section 2.3, the observation was categorized into Clean-H, Clean-L, Polluted-H, Polluted-L and mixture in between. Why did you use the wind direction, rather than be backward trajectory? It is mentioned the pollution is long-range transport anthropogenic pollution from Europe. Therefore, backward trajectories should work better than the wind direction.

   Besides, after classification, it is better to show the wind rose plot; boxplot or frequency distribution of BC, N_{30} and/or meteorology data during Clean-H, Clean-L, Polluted-H, Polluted-L, Mix-H and Mix-L in the supplement. Reviewers and readers will have a better understanding of the classifications. It also helps your interpretation afterward.

   The "H" and "L" are classified by the biological activity seasons; therefore, it is needed to show the difference of biological activity during "H" and "L". For example, you can

show the Chlorophyll-a and DMS concentration over different seasons. There are free and easy access data from NASA.

Why WS>3m s^{-1} is one of the criteria of the clean sector? When BC<15 ng m^{-3}, WD from 190 to 300 and WS<3 m s^{-1}, will it be classified as which sector?

3. Some of the criteria and numbers are very arbitrary, lack evidence to support.

   For example, in Lines 194-201: First, as far as I know, the CPC model 3010 TSI detection limit is 10 nm particles. Second, SMPS upper limit is 500 nm, whereas CPC depends on your inlet cut size. To make it clear, as CPC covered a broad size, N10/Ncpc should be smaller than 1. In a normal distribution, $\mu \pm \sigma$ covers 68% and $\mu \pm 2\sigma$ covers 95%. I could not under why 84.15^{th} quantile. Why "extra 10% of uncertainty was allowed due to typical uncertainty of particle counting"? Here you mean the measurement uncertainty of SMPS?

   Lines 203-207: Why are 1.1*N30 and 1.2*N30 used as the limit?

   Line 216: Why "uncertainty of the AMS based on κ to be lower than 20%"? How do you get 20%?

4. Some of the statements and interpretations are too strong without supporting evidence.

   For example, in Line 311, Do you think wet removal is one of the "cloud processing" or not? I am guessing what you are trying to say is that higher concentration of accumulation might be due to strong condensation growth and/or free troposphere entrainment during wintertime.

   Lines 327-329: Based on your classification criteria, Polluted and Mix both feature higher BC mass, I presume the air masses during these periods are from the land. Why did Mix-H show greater similarity in number size distribution to Clean-H? Winds during Polluted-L are from 35° to 135°, whereas winds during Mix-L are from 135° to 190° and from 300° to 35°. It is not accurate to say the prevailing winds are similar during Polluted-L and Mix-L.

Minor comments:

Line 152: Since you have not introduced the method F, it is better to delete this sentence here.

Line 210: Please check the unit of BC in Table 1.

Line 306: “Fig. 2 shows” change to “Figure 2 shows”.

Line 353: Did you use air mass back trajectory? As suggest in the major comments, the back trajectory is important for your classification and data interpretation.

Lines 366-368: In the classification standard, you only mentioned the wind direction at the measurement site. Wind direction cannot tell the air mass during Clean-L from the ocean rather than from continental. As I said in the major comment, the backward trajectory might be a better classification criterion than wind direction.

Line 452: Change to “Fig. 5”.