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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol measurements during COPE: composition, size and sources of CCN and IN at the interface between marine and terrestrial influences" *by* J. W. Taylor et al.

Anonymous Referee #1

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The study investigates cloud condensation nuclei (CCN) and ice nucleating particle (INP) characteristics in the marine-terrestrial region over the southwest peninsula of the UK in 2013. A comprehensive data set of aerosol chemical and microphysical parameters was acquired during several flights. One of the main results includes specific CCN number concentrations at 0.1 and 0.9 % supersaturation during clean and polluted conditions. Another result is that significant knowledge gaps still exist in predicting INP concentrations as the comparison of INP concentrations with existing parameterizations shows. The paper is written clearly, and the research and data analyses have been carried out very carefully with great attention to details. This work is a worthwhile contribution to the body of literature on CCN and INP, especially with respect to

the marine-terrestrial interface and the differences between natural and anthropogenic CCN and INP contributions. I recommend publication with minor revisions.

General comments:

The authors introduce the greater topic of convective cloud formation and linkages to flash floods on the southern UK. However, the actual results of the COPE study are not interpreted in relation to this theme. To further improve this work, I recommend adding a more direct description of how COPE relates to convective cloud formation in the introduction, and a respective paragraph in the discussions section.

As far as I am aware, ice nuclei are referred to ice nucleating particles (INP) in the current literature. I recommend changing this throughout the manuscript.

Specific comments:

p. 1, l. 16: specify if the concentration are given for STP or ambient conditions.

l. 20: What was the supersaturation at cloud base?

l. 22: It is not clear what the authors mean by "Marine organic aerosol (OA) had a similar mass spectrum to sea spray". What type of spectra are you comparing: only OA spectra in marine air masses to only sea spray spectra? I would expect that the OA spectrum does not show fragments of sodium and chloride (and other salts) while the sea spray spectrum is dominated by salts. Some clarification is needed.

p. 2, l. 2: The region is not particularly vulnerable because of convective cloud formation and flash floods. The region is particularly vulnerable because there is human activity where flash floods occur. In other words, if nobody lived there, there would be no vulnerability. Rephrase.

p. 3, l. 14f: Include a reference or explanation for the quantification of the uncertainty.

l. 19: a reference is missing.

Section 2.1 Aircraft Measurements: Information about the inlet system is missing. Did you use a pressure controlled inlet? If yes, for which instruments, if not how did you treat/correct the data?

p. 4, l. 6: which densities did you use to calculate the volume?

p. 5, l. 4: As far as I am aware, the recommendation is to use the latest reference for HYSPLIT as noted on their webpage. Further regarding the back trajectory analysis, did you run individual trajectories only or ensembles for each release time? If you did not run ensembles, how can you guarantee that the single trajectory is representative and accurate enough? Some more information on how you performed the analysis and why is needed.

l. 19: I would not state "any long-range transported" aerosol was washed out, but rather "most". Air masses that have experienced precipitation are not completely free from aerosol.

p. 6, l. 24: Information is missing on how you determined nss-chloride. We know from the literature you cite and further references that the AMS does measure a fraction of sea salt. So the signal at the chloride fragment m/z must be partially originating from sea salt.

p. 10, l. 5f: Was there no coarse mode from sea salt that was measured? More complete information on the size distribution is needed here.

l. 7: From what I see in the figure, I cannot agree with the statement that all distributions were open at the lower end, e.g. in panel f, the red curve is not open (also in others). This needs some more detailed discussion. In case I misinterpret the meaning of "open" a clearer description is needed.

p. 13, l. 4-9: Is there any particular reason why you did not apply kappa-Köhler theory? It seems only logical to derive a kappa value from the AMS measurements to compare it with previously determined kappa values.

l. 29: The first factor determining CCN concentrations is the CN number concentration rather than the size distribution.

Fig. 1: I suggest to use the color code for altitude information and to insert numbers to indicate the age of the air mass.

Technical comments:

p. 1, l. 28: The sentence doesn't make sense grammatically.

p. 3, l. 8:. "." Is missing after "al", also in some other occasions

p. 10, l. 3: There is a grammatical error in the sentence.

Table 1: Include information on the year.

Fig. 7: include information on what the solid and dashed lines represent in the captions.

Fig. 8: A legend explaining what the rectangles and triangles are would help the reader to understand the message more easily and quickly.

Fig. 9: The small graphs in each panel are not explained.

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