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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aircraft measurements of aerosol and trace gas chemistry in the Eastern North Atlantic" *by* Maria A. Zawadowicz et al.

Anonymous Referee #1

Received and published: 1 December 2020

The paper presents results from two campaigns of aircraft measurements over the eastern North Atlantic based out of the Azores. Vertical profiles of non-refractory aerosol composition, CCN, and trace gas composition are reported and found to confirm previously reported results for the North Atlantic (e.g., both local and long-range transported emissions contribute to the aerosol population in this region). As indicated in the comments below, the addition of references to previously reported results would enhance the paper. The profiles of gas phase species are a strong contribution since there are few similar measurements over oceans. The paper should be published after the comments below are addressed.

Lines 62 – 63: This statement ("...this view (the CLAW hypothesis) has been debated as primary sea salt aerosols....have been hypothesized to be a more robust source

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of CCN than DMS-derived aerosols") is an inaccurate and oversimplified summary of Quinn and Bates (2011). There are at least three factors that prevent DMS-derived sulfate from being involved in a climate feedback mechanism – 1) there are additional sources of CCN in the MBL (e.g., sea spray), 2) nucleation of new particles from DMS sulfur occurs in the free troposphere prohibiting local feebacks, and 3) the connection between increased CCN and changes in aerosol-cloud interactions is more complicated than depicted in Charlson et al. (1987).

Lines 109 – 111: NAAMES took place in the western North Atlantic region – west of 30W.

Lines 123 – 124: Perhaps rephrase this statement as "A unique feature of the ACE-ENA aircraft deployments is the seasonally-resolved measurement IN THE EASTERN NORTH ATLANTIC. . ." as NAAMES also had seasonally resolved aircraft flights but in the western NA.

Section 3.1.1 and throughout: To avoid confusion, it should be pointed out that total particulate sulfate measured with the AMS is non-sea salt sulfate.

Lines 190 – 209: It would be interesting to add a comparison with NAAMES seasonal sulfate values. See Saliba et al., JGR, 125, doi: 10.1029/2020JD033145 (2020) and Sanchez et al. Sci. Rep. 8, 3235, doi: 10.1038/s41598-018-21590-9 (2018).

Lines 210 – 220: Seasonal concentrations of DMS and MSA from NAAMES could be compared to the values measured here. See Quinn et al., JGR, 124, 14240 – 14261, 2019. In addition, the MSA to non-sea salt sulfate ratio measured here during the summer (<10%) should be compared to previously reported ratios in remote marine regions during the spring/summer.

Line 229 – 230: This result (MSA does not account for the majority of the particulate sulfate mass in the MBL) is not new and should be noted as such by providing appropriate references.

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Line 236: Figure 9 is mentioned before Figures 5 to 8.

Lines 256 – 257: What was used to designate RF #9 and #19 as having lower and higher influence from long range transport?

Lines 275 – 276: Are measured levels of isoprene and monoterpene consistent with this statement, i.e., are they large enough to provide the third source of organic aerosol at ENA? Based on Figure 11, there is no significant surface (marine) source of isoprene even in summer.

Lines 278 – 286: The result that methanol concentrations are larger than DMS in the summertime should be provided with a caveat that reflects the results shown in Figure 11, i.e., methanol concentrations are higher aloft ($\sim$2000 m) while DMS concentrations are lower near the surface.

Lines 297 – 298: The winter surface source of sulfate appears to only be significant in RF 34. What would a winter source of sulfate be? DMS concentrations should be quite low. Figure 11b indicates that DMS surface concentrations were low during RF 34.

Line 327: 0.1 or 0.13%S?

Figures S3 and S4: There appears to be two different populations showing up in Figure S3d and S4d. What is causing this split response between CCN and AMS SO4? It looks like it could be partially responsible for the low r^2 values.

Lines 345 – 349 and Fig. S5: It is really difficult to see that winter concentrations of chlorophyll are higher than summer concentrations in this figure.

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