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Interactive comment on “Hygroscopic properties of organic aerosol particles emitted in the marine atmosphere” by A. Wonaschütz et al.

Anonymous Referee #2

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Results on the aging of an organic plume in the marine atmosphere and impacts on aerosol CCN activity and hygroscopic growth are presented. The paper provides a unique perspective on aging processes and time and should be published once the concerns listed below have been addressed. I found the paper structure to be distracting from the main points. As written, the CCN and GF measurements are described first, then aerosol chemical composition is presented, and then the CCN and GF measurements are discussed in the context of the composition measurements. It would be much simpler (and easier to read) if composition were described first to lay a foundation for the CCN and GF results. In addition, this is a very complete data set with multiple measures of several parameters. It would be nice to see comparisons that would reveal the consistency between different measurements. For example, com-

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parisons between different measures of size distributions (AMS vs. SEMS-OPS) and organic matter concentrations (AMS vs. FTIR). See specifics in the comments below.

p. 11921, line 7: change to “increased at LONGER plume ages”

p. 11927, lines 18 -27: How well did the AMS and combined SEMS-OPS-APS size distributions agree? Did the agreement indicate that a collection efficiency for the AMS of 0.5 was reasonable?

p. 11928, lines 1 – 9: How well did the AMS and FTIR organic matter agree? Again, this would lend insight into the collection efficiency of the AMS and also uncertainty associated with calculated WIOM concentrations.

p.11930, lines 23 – 24: How was the organic mass fraction calculated – based on total mass measured by the AMS?

p. 11931, line 9: Figure 11 is mentioned out of order.

Figure 6b: Are 30 nm growth factors shown? It is difficult to see the difference between the blue and black symbols used for the two different particle sizes.

p. 11935, lines 20 – 26: What is the physical reason for why primary plume particles would activate at a 0.25% supersaturation but not at 0.87%? Were the measurements at difference supersaturations made sequentially so that 0.25% was sampled before 0.87%?

p. 11936, Lines 5 – 23: This discussion would be easier to follow if values of d_{act} and d_k were put into a table or figure. Also, details on how the Kelvin diameter was calculated should be given.

p. 11940, lines 15 – 16: Is this supposed to say that there is a higher peak in hydroxyl in the fresh smoke than in the oil that was used to generate the smoke?

p. 11941, lines 20 – 24: This sentence should be re-written to say that that newly-formed particles after aging contributed to CCN concentrations. As currently written it

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appears to say that CCN inactive particles contributed to CCN concentrations.

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