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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimating a relationship between aerosol optical thickness and surface wind speed over the ocean" *by* P. Glantz et al.

Anonymous Referee #3

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The paper presents an interesting analysis comparing AOT derived from SeaWiFS and surface wind speed derived from ECMWF over a patch of the North Pacific during September of 2001. A power fit of AOT to wind speed is derived for the time period with a correlation coefficient of 0.92. This result indicates there is promise in this approach for determining sea salt AOT from surface wind speed. As the authors point out, it remains to be seen how applicable this approach will be for other marine regions. There are major issues with the paper, however, that need to be addressed before it is published in ACP. The largest issue is the method chosen for "validating" the results. Rather than using direct measurements of sea salt mass, hygroscopic growth of sea salt, and AOT from ships, a modeling approach was used. In this approach, sea salt mass was calculated from a wind speed dependent source function and growth factors

were assumed (and not specified in the text). It would be a much stronger paper if direct measurements were used for validation. The latter approach may require identifying a different location or locations for the study where measurements are available. One ideal location would be the ocean south of Tasmania which was the focus of ACE-1. Because of the choice of validation that is used, there is very little support for statements in the paper that claim "approximately 50% of the enhancement in AOT is due to hygroscopic growth of the marine aerosols." To be able to make this claim, the model itself needs to be validated with measurements. Including this result is a stretch not supported by the analysis. Instead, the paper should focus on the relationship shown in Figures 4a and 6 for areas of the ocean where direct measurements are available. A final critical issue is the disagreement between the result of this analysis (that sea salt AOT can be derived from surface wind speed) and previously reported results showing poor correlations between local wind speed and sea salt mass (see, for example, Figure 17 of Lewis and Schwartz, AGU, 2004). A discussion that addresses this issue should be added. Additional issues are listed below.

p. 11624, line 25: Should be Durkee et al., 1986.

p. 11627, line 6: Should be "Ěare also shown in Figure 1", not in Figure 2 as implied by the sentence structure.

p. 11628, last paragraph and Figure 4: What is the correlation coefficient for AOT and 10 m wind speed for the 17 km grid boxes used in the analysis? There is a resemblance between the two parameters as stated although there also appear to be differences. For example, wind speed is low to the west and east of 160deg W while AOT is low across 160deg W and higher to the west and east.

p. 11629, first lines: It is stated that "Ěweak or no correlations occur between the two quantities over some areas." Does this refer to the area of the study region or to the global ocean? If this methodology is recommended on a global basis for retrieving sea salt AOT from surface winds (as defined by ECMWF analysis at 10 m), the correlations

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should be proven for a broader region of the globe.

p. 11629, line 15: It appears that sea salt mass in the accumulation and coarse modes is based on a wind speed dependent source function given by Gong et al., 1997. This approach involves circular logic. In other words, wind speed is used to generate sea salt mass and then sea salt mass is correlated with wind speed determined from the ECMWF analysis. A stronger approach would involve using measured sea salt mass concentrations that were measured directly rather than inferred from wind speed.

p. 11629, line 19: What value was used for GF(RH = 80%) of accumulation and coarse mode seasalt? Were the values used size dependent?

p. 11630, equation 3: What is meant by "NH4SO4?" Is it (NH4)2SO4 or NH4HSO4? What is its source and why is it dependent on surface wind speed? Are you assuming a flux of DMS out of the ocean and subsequent oxidation to SO4? If so, what is the source of the NH4?

p. 11630, equation 3: In equations 1 and 2, RH1 refers to the mean RH for the lowest wind speed range - presumably for the entire study region. In equation 3, what do RHi and BLHi refer to?

p. 11630, equation 3: How were GFi and GF1 for the total aerosol determined? Figure 4: Exactly how where the mean and standard deviation of AOT derived in Figure 4b? Is this a result of binning the values from Figure 4a? Figure 6: It is not clear what the difference between the stars and the squares is. Do the stars not include ammonium sulfate while the squares include both sea salt and ammonium sulfate?

Figure 8: In the text it is stated that satellite retrieved AOT were estimated over ocean areas near Lanai and Coconut Islands because of issues with aerosol retrievals over land. A stronger analysis would also incorporate shipboard measurements of AOT (e.g., as reported by Smirnov et al.).

Figure 8: A scatter plot and correlation coefficient should be shown for the satellite and

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Aeronet retrievals of AOT for each island site.

p. 11636, lines 14 - 22: It is acknowledged that surface reflectance due to high wind speeds could introduce an artifact into the present analysis. Several references from the 1980 to 1995 time periods are listed which all showed this effect to be small. These references are a bit unsatisfactory, however, since they do not directly apply to the satellite sensor or analysis used here. Is there direct evidence for SeaWIFS that reveals the relationship between surface reflectance under high wind conditions and artifact AOT?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11621, 2006.