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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 #2

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In the paper "Estimating a relationship between aerosol optical thickness and surface wind speed over the ocean" by P. Glantz, D. E. Nilsson, and W. von Hoyningen-Huene, the authors shown a positive correlation between Sea WiFS aerosol optical thickness (AOT) and ECMWFs 10-m wind speed. The authors claim that "50% of the enhancement seems to be due to hygroscopic growth of the marine aerosols and the remaining part due to increase in the sea salt particle mass concentrations, caused by a wind driven water vapor and sea salt flux."

Indeed, there are a number of field studies confirming the increase of sea salt aerosol loading with the increase of near-surface ocean wind speed value. However, given the low sensitivity of satellite observations to the aerosol signals at very low AOT cases, I am not convinced that the positive correlation between Sea WiFS AOT and ECMWFs

wind speed shown in this study is a refection of a real physical process specified, but is rather to a number of artifactual reasons. These include either cloud contaminations or changes in ocean surface reflectance patterns due to wind speed changes-both of which have already been shown to hamper MODIS. It is not proper to apply this approach to aerosol climate forcing studies without exploring these artifacts, and I am afraid that I cannot recommend publication in any form until these issues are resolved. Such revisions I feel would be extensive to the paper and hence I recommend rejection. Major concerns are presented in detail below.

Major Concerns:

(1) Satellite visible channels have low sensitivity to aerosol signals when aerosol loading is low (e.g. AOT $<$ 0.1). For the AOT range (AOT $<$0.15) used in this study, Sea WiFS AOT values could be significantly affected by the change in surface characteristics. In fact, the variation in surface characteristics could contribute to a significant portion of the relationship between Sea WiFS AOT and ECMWFs wind speed for two reasons. First, as the authors mention, an increase in white foam formation could contribute to the observed relationship. But, this paper only shows that white foam formation is less important in satellite aerosol studies for 5-8 m/s wind speed range-when production is weak. Second, the change in glint patterns as a function of 10-m wind speed affects satellite observed radiances not only at glint angles but also at non-glint angles as well. This is likely dependant on the difference in the wind versus wave vectors. At such very low AOTs uncertainties in the lower boundary layer need to be well quantified. I suggest that a very comprehensive validation with AERONET as a function of wind speed be included in this analysis. What does the scatter plot of figure 8 look like? What happens if the validation time period is increased to a year?

(2) Sea WiFS has 8 channels ranging from 402 to 885 nm. Due to a lack of IR channels, the authors attempt to exclude cloudy pixels using only a visible threshold plus some AOT variation tests. However, I am not convinced that the proposed cloud screening approach works efficiently for partially cloudy pixels, especially for pixels with very low

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cloud fraction or thin cirrus, while the presence of clouds might correlate with wind speed. Considering wind speed and cloud cover can easily correlate under these situations, this artifact can result in the spurious regression presented.

(3) What percentage of the AOT fraction is fine versus coarse mode? Given that remote ocean optical depth is typically evenly split between fine and coarse mode, does a correlation with the fine mode make sense? The assumption here is that AOT is all from sea salt-a very poor assumption particularly around Hawaii when sulfate is often present due to Kilauea.

(4) This study area was chosen because of amenable meteorology, but there is little in the way of extrapolation to other areas. What about the 20 other coastal AERONET sites? What evidence is there that this regression holds elsewhere? In Smirnov's paper regarding the correlation of AERONET AOT to wind speed at Midway (which was statistically significant), they also mentioned that such regressions did not hold in other sites around the world. How applicable are these results? This goes back to not only a clear AERONET validation of the products, but also the nature of marine meteorology as well.

Minor concerns: (1) ECMWFs wind speed is used in this study. The uncertainty for the model wind speed should also be included. How does this compare to observation in the study area?

(2) Page 11628, line 27, "The figures show a striking resemblance, which suggests that the retrieved AOT was sensitive to the surface wind speed over large areas of the North Pacific, although weak or no correlations occur between the two quantities over some areas." It is not proper to credit the similarity to an AOT - wind speed relationship without exploring other possibilities.

(3) Do MampSS and McmpSS represent BLH averaged values? Need to be clearly stated.

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(4) Page 11631, line 10, “Furthermore, Furthermore” Need to remove one “furthermore”

(5) Figure 4b and Figure 5. It would beneficial to readers if the distribution of data density is also superimposed.

(6) I am surprised to see a R2 of 0.92 in Figure 5. Does this R2 come from all data points or from the averaged data points? If it the latter one, what would be the R2 if all data points are included in the analysis? Same question to Figure 6.

(7) What is the averaged AOTobsNH4SO4 value used in this study?

(8) Page 11633, line 14, “For the latter two stations the AOT shown in the figure have been averaged around the time when the Sea WiFS satellite passed over Hawaii and the error bars correspond to one standard deviation.” What are the criteria used for the averaging process?

(9) Figure 8, what are the 10-m wind speed values for the shown Sea WiFS observations? It would be more convincing if a longer-term study (with more stations, hopefully) of the differences between AERONET and Sea WiFS AOT as a function of near surface wind speed is shown.

(10) Page 11633, line 27 “and actually much better on most of the days.” Need to rephrase this phrase in a scientific way.

(11) Page 11634, line 4, “From empirically derived parameterizations, we know that the emissions of both sea salt and water vapor over the oceans are dependent on the surface wind speed, see Fig. 9.” Figure 9 doesn’t provide hard evidence for the comment. Need to reference some published papers. Besides, higher wind speeds probably result in higher boundary layer heights, which may offset hygroscopicity effects.

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

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