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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "On direct passive microwave remote sensing of sea spray aerosol production" *by* I. B. Savelyev et al.

Anonymous Referee #1

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This is an interesting paper that presents results from a field experiment where sea-salt aerosol (SSA) populations and microwave brightness temperatures were measured. The data are used to develop a proposed relationship for predicting the SSA flux across the air-sea interface from microwave brightness temperature. Interestingly, the obvious reason for the two quantities to be related, the foam coverage due to breaking waves, was found to be of less importance than sea surface roughness. One possible explanation for this are discussed in the paper, although the point is not resolved.

Overall I think this is a nice piece of work, and the questions it raises will stimulate further work in this area, even if further measurements determine that the link between brightness temperature and SSA flux is not straightforward as presented. I recommend it be published after the points raised below are addressed.

In Figure 7, it is interesting that there appears to be a series of outlier measurements where U10 was relatively high yet Fpms was low. Were these data recorded in a contiguous time frame so that they represent a particular meteorological event? Would it be possible to color code them in some way so the equivalent data points could be shown in Figure 7b, 7c, and 7d? My suspicion is that there is some reason why these particular points have anomalously low fluxes (for example, perhaps there was swell running with the wind direction so that breaking was suppressed, or perhaps there were atmospheric stability issues?). Since the processes that drive Tb as function of wind speed are the same, mostly, as the processes that drive Fpms, and the correlation of U10 and delta-TB is so high (see Figure 9), I do not see why there should a series of outliers in 7a that are not also shown in 7d. Furthermore, if you exclude those outliers (the line of points to the lower right of panel 7a), a chi-by-eye suggests that the fit of Fpms to U10 would look almost identical to the fit of Fpms to delta-TB in panel 7d. It seems to me that the data in 7a and 7d warrant a bit deeper discussion into the sources of the variability shown in 7a.

The authors provide an interesting hypothesis for why the active breaking fraction is more relevant to SSA production than total foam fraction. However, they conclude that not enough is known about spray droplet production to draw any conclusions. Oddly, they do not cite Fairall et al. (Fairall, C. W., M. L. Banner, W. L. Peirson, W. Asher, and R. P. Morison (2009), Investigation of the physical scaling of sea spray spume droplet production, J. Geophys. Res., 114, C10001, doi:10.1029/2008JC004918.) which is likely the most detailed laboratory study of this process. It is possible that Fairall et al. might shed light on this situation. Additionally, there have been some studies relating the air-sea gas flux to brightness temperature, and although my recollection is they did not try to separate the foam impact from roughness, the reasoning used by the authors to justify roughness as the primary driver for the SSA flux might also apply to the gas flux. This is especially true in light of the work by Chris Zappa, who demonstrated that the roughness generated by microscale breaking waves (i.e., small scale breaking waves that do not visibly entrain air) correlates with the gas flux.

Minor Issues:

P15386, Line 4: Perhaps a more accurate way to state this is that the overall shape of the Smith et al. parameterization (SP) agrees with the empirical fluxes calculated using the dry deposition method (DDM). At the highest wind speed for large particles, the fluxes from the DDM are an order of magnitude larger than the SP fluxes. I would not necessarily call that agreeing "fairly well." Perhaps the agreement would be clearer if some estimate of the uncertainty in the DDM fluxes were shown?

Figure 7: Labeling the panels in a counterclockwise manner is confusing. Suggest relabeling as a(top left), b(top right), c(bottom left), d(bottom right).

P15386, Line 14: Suggest substituting "decimating" for "rarefying." Decimation is the standard term for the procedure I think the authors are describing.

P15386, line 25: "greater linearity" It is not clear to me that the fit of 7d is in fact more linear than the fit in 7a (in the sense the fit is closer to a straight line. I think it might be better to say "more correlated" and provide an estimate of the coefficient of determination.

P15389, L1-L4: I must be missing something in Figure 9. My experience suggests that the increase in Tb at h-pol due to foam and breaking waves is larger than the increase in Tb at v-pol. This is supported by the measurements of Padmanabhan et al. (2005, TGRS, Figure 13), showing that the increase in emissivity is larger at h-pol than at v-pol. Yet Figure 9 shows that the increase is larger for v-pol than h-pol. There should be some discussion in the text as to why the result shown is at odds with previous measurements. Furthermore, discussions such as found in Pandey and Kakar (1982, IEEE JOE) suggest that the effect of roughness on Tb at v-pol is relatively small. I feel I am missing something in interpreting Figure 9 with respect to the discussion in the text. Finally, the authors state that the predicted functionality matches the data. However, the experimental Tb values look to be almost linear with respect to U10, whereas the model results predict an increase that will be nearly cubic. Perhaps the model curves

could be shifter by the constant offset to more clearly show the observed dependence is the same as the model.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 15363, 2014.