Below is the itemized author response to both reviewers, as well as the list of relevant corrections in the manuscript.

Anonymous Referee #1

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 outlier population in Figure 7a was identified, highlighted, and shown on other panels in Fig. 7. It appears to correspond to the time frame (YD 118.5 – 119.2) of rapid wind growth, where the wave field is not fully developed and therefore does not produce as many whitecaps (and hence SSA Flux) as is expected from a mature wave field at the corresponding wind speed. Figure 7d does not show this population as an outlier because the corresponding controlling parameter is directly sensitive to the amount of breaking wave activity. A corresponding discussion emphasizing these points is now added to section 5.

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.

The concluding paragraph of section 6.1, specifically the part regarding the lack of the literature on the subject was meant to address specifically the lack of the quantitative evidence of difference in aerosol production rates between active and passive phases of breaking waves, not the overall lack of literature on the subject. We agree that the phrasing we used was not clear and made appropriate modifications to the paragraph. The paper by Fairall et al. 2009 is indeed relevant to the discussion of the choice of the input parameter and is now added to the appropriate section (2.1.3). The effect of microbreaking on the air-sea gas exchange is primarily due to the enhanced subsurface mixing. We hesitate to suggest a similar effect on the aerosol production, because microbreakers do no entrain air (by definition), and therefore lack the ability to produce any aerosol in the coarse mode.

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?

Correction made as suggested.

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).

Correction made as suggested.

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

Correction made as suggested.

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.

Incorrectly used term “linearity” replaced with term “smoothness”.

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.

The reviewer is correct to expect faster growth of H polarization, compared to V. Both panels of figure 9 actually support that expectation. Panel (a) shows H and V separately and it can be seen that both modeled and observed H values grow about twice faster than V. That is why H-V difference (defined by eq.10) shown in (b) has a positive slope, whereas it would have been negative if V grew faster than H.

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.

The text has been changed to state that in Figure 9a the model indeed does NOT match the observation. Potential reasons for this difference are listed in section 6.1 (3rd paragraph) and appear to cancel out when polarizations difference is used (H-V), resulting in a much better agreement between the model and the observations, shown in figure 9b.

Anonymous Referee #3

The authors of this paper present a new sea spray source function, based on brightness temperature, which incorporates several aspects of the sea surface conditions (roughness and foam). This function, derived from fitting curves through measurements, is compared to the most well-known sea spray source functions that were based on wind speed only. The approach is original and findings are discussed thoroughly in terms of physical meaning and in terms of the consequences of different approaches. The paper is well written and deserves publication. Still, I have a few questions and remarks.

P15388: I'm a bit confused on which data are used: FLIP or COAMPS meteorology.
It is mentioned that in Fig 7 the presence of foam only starts from wind speeds above 7 m/s, but in Fig 1 the FLIP data only show wind speeds >7 m/s for the episodes with TB measurements. The same holds for the curves for the different wind speeds.
COAMPS however has wind speeds around 5 m/s on day 118. Do you use the data for the emissivity model as well? In the meteorology section it is only explicitly mentioned that COAMPS was used for running HYSPLIT. Can you clarify this?

The values of wind speed used in Figure 7 were obtained directly from the Vaisala meteorological station located at 10 meters above mean water level. This point is now clarified is Section 5. Slightly higher wind values seen in Fig.1a (black curve) were registered by the Davis station located at 24 meters. COAMPS data was not used in any way to produce figures 5-10.

P15395: You suggest to replace U10 by TB in existing relationships. It would be interesting to show an example of the result. In addition, if you replace _T in eqns 13 or 14 by the proposed fit (eqn 15) you could comment on the general dependency of your sea spray source function on the wind speed (exponent of U10) and compare that to literature values based on wind speed only (U10ˆ3, U10ˆ3.41).

Both steps have been added to section 6.3, as suggested.

P15396: Sea state and resulting TB may vary considerably within a day. What kind of resolution in terms of space and time do these satellites provide? Would that be enough for an assessment of sea spray production for e.g. climate models, for which sea spray concentrations are highly relevant? These models would typically require meteorological/oceanographical fields as input every 3-6 hours.

Spatial and temporal resolutions vary from satellite to satellite, as well as among numerical models. Our rough estimate suggests that at the present time it is possible to obtain the desired product ~ 4 times per day at ~30km spatial resolution, which is similar to spatiotemporal grid spacing of a global aerosol model NAAPS. This estimate has been added to section 6.4.

Technical comment P15373: TBP is already used but only defined op P15374 (eq.7).

Corrected.