Interactive comment by Ian Brooks and Dominic Salisbury:

We thank our colleagues Ian Brooks and Dominic Salisbury for the thorough review of the manuscript and the constructive comments, which contributed to the improvement of this manuscript.

In response, the manuscript is substantially revised with the following:
1) Updated analysis of global W data to develop W(U10) parameterization.
2) Extended analysis of regional W data to develop W(U10,T) parameterization with SST explicitly included; this was done for both quadratic and cubic wind exponents.
3) Analysis for statistical significance (with Student's T-statistics and ANOVA) of new and previous W parameterizations.
4) Extended 'Methods' section to justify and clarify approach, data, and implementations.
5) Revised and extended 'Results and Discussion' section to clearly describe results and give substantive and quantitative interpretations and conclusions.
The table of contents of the revised manuscript is added after the responses for reference.
Manuscript revisions with track changes are provided in a separate pdf file.

Several comments and questions are similar in all 3 reviews (e.g., uncertainty not reduced, quadratic wind speed exponent, embedded secondary forcing, intercept interpretation). To avoid repetitions, we attempted combining responses to these common points in one file. We found, however, that one-fits-all responses do not always address the reviewers' comments and questions fully. Thus, risking some repetitions, we proceeded with a specific response to each comment.

Responses are presented below. The original comments are in bold italic; we enumerated those (23 comments) for easy reference.

General comments:

1. This paper aims to improve the accuracy of sea spray source function defined via the whitecap method – where the source flux is defined as the product of whitecap fraction, W, and the aerosol produced per unit area whitecap over the lifetime of the whitecap. It aims to improve the accuracy of this approach by reducing the uncertainty in the parameterization of W "by better accounting for its natural variability". We feel it fails to demonstrate such a reduction in uncertainty.

We acknowledge that as formulated, our objective was not met. We revised Sect. 1 to introduce magnitude and shape factors comprising the SSSF and how uncertainties from each factor contribute to the uncertainty of the SSSF. This allows us to clearly define our objective as "a study investigating the second of these two routes, namely—how using *W* values carrying information for secondary factors would influence the SSA production flux."

We use comparisons between our and Grythe et al. (2014) results for SSA fluxes to examine and quantify variations of SSA emissions attributed to magnitude and/or shape factors. The results are in new Sect. 3.4. These results are summarized in the Conclusions as follows: With or without the SST effect included in the SSSF, SSA emissions obtained with the new $W(U_{10}, T)$ parameterization vary by ~50%. Different approaches to account for SST effect yield ~67% variations. Different models for the size distribution applied to different size ranges lead to 13%-42% variations in SSA emissions.

We conclude Sect. 3.4 with the following:

On the basis of these assessments, we can state that the inclusion of the SST effect in the magnitude factor and/or the choice of the shape factor (size range and model for the size distribution) in the SSSF can explain 13%-67% of the variations in the predictions of SSA emissions. The spread in SSA emission can thus be constrained by more than 100% when improvements of both the magnitude and the shape factor are pursued. Our results on the *W* parameterization (Fig. 13a) suggest that accounting for more secondary forcing in the magnitude factor would explain more fully the spread among SSA emissions. Because, after wind speed, the most important secondary factor that accounts for variability in *W* is the wave field (SAL13), efforts to include wave parameters in *W* parameterizations are well justified.

2. While the paper focuses on the issue of parameterizing W, it is worth noting that this is not the only source of uncertainty in the parameterization of the sea spray source function by this method; there is also uncertainty in the aerosol produced per unit area whitecap – this is inherently assumed here to be a constant, but is almost certainly not. A study on which one of the co-authors here is also a coauthor (Norris et al. (2013)) has demonstrated that the aerosol flux per unit area whitecap varies with the wind/wave conditions.

We fully agree with Brooks and Salisbury comment and are well aware of the limitation of the whitecap method, specifically its basic assumptions. We included new Sect. 2.4.1 to more fully discuss the uncertainties coming from the whitecap method. However, the whitecap method (in the form of Monahan et etl., 1986, or M86) has been widely used in many models for SSA flux (e.g., Table 3 in Textor et al., 2006). Therefore, to those who have worked with M86 until now, a meaningful way to demonstrate how the new satellite-based W data and new parameterizations W(U10) or W(U10,T) based on them would affect estimates of SSA flux is to held constant the shape factor and clearly show differences caused solely by the use of the new expressions.

3. Much of the material in the paper is very similar to that presented in Salisbury et al. (2013, 2014 –both widely cite throughout). The authors could use this to their advantage by removing repeated background material, most notably in section 2.

We mentioned this fact in Line 25 on p. 21225 and consciously proceeded to "briefly" describe the W database (as said in Line 1 on p. 21226). The comment here suggests that we should shorten Sects. 2.1 and 2.2 even more. We agree: Sects. 2.1 and 2.2 (72 lines) have been combined and revised to a shorter new Sect. 2.2.1 (41 lines).

4. The recent paper by Paget et al, (2015) needs to be considered too given that it uses the same data set and one of its main focuses is parameterisation of satellite W. In particular, Paget et al. address the use of equivalent neutral winds in the satellite W database. Here, the inherent difference between QuikSCAT winds and ECMWF winds is an important point, and warrants more than a passing comment (section 4.2.1).

Paget et al. (2015) didn't derive W(U10) parameterization from the satellite-based W data. Paget et al. investigated and quantified variations of W values when different wind speed sources are employed to derive W(U10) parameterizations. Paget et al. did that by coupling in situ W data with in situ (thus stability-dependent) and satellite (thus stability-corrected) wind speed values, then analyzing how the coefficients in W(U10) expressions change. The satellite-based W database was used to assess differences between W(U10) expressions obtained from in situ W and different wind sources.

In contrast, we used both satellite-based W data and U10 from the W database to derive W(U10) expression. For the revised manuscript, we extended our regional analysis to derive also W(U10,T) parameterization. In the revised manuscript, we cite Paget et al. (2015) in Sect. 2.2.3 regarding stability effects on U10 data sources.

Use of independent wind speed:

5. A novel aspect of the paper, and a key difference from the Salisbury et al. studies, is the aim to assess the impact of intrinsic correlation between W and the QuikSCAT-derived U_{10} values used in the Salisbury et al papers, because the same U_{10} data is used in part of the W retrieval. However, the approach adopted fails to properly address the issue.
To avoid the potential self-correlation of W and $U_{QuikSCAT}$ the simple approach would be to fit W to the independent measure of U_{10}. Here the ECMWF model values, U_{ECMWF}, are adopted; however, instead of this, the authors fit W to $U_{QuikSCAT}$ (eqn 7), then fit U_{ECMWF} to $U_{QuikSCAT}$ (eqn 8), rearrange (8) and substitute U_{ECMWF} for $U_{QuickSCAT}$ in (7) to give (9). There are multiple problems here, both conceptual, and in implementation.

We plot U_{ECMWF} vs. $U_{QuikSCAT}$ to assess how the U10 values from the two sources differ. We find this necessary as we comment that it is not easy to find truly independent U10 data (Lines 27-28 on p. 21229). The small difference of 5% between U_{ECMWF} and $U_{QuikSCAT}$ prove this point to some extent. The fit between the U_{ECMWF} and $U_{QuikSCAT}$ (made over approximately 700 000 data points) is useful because a reader might have either QSCAT or ECMWF data and this fit offers an easy and reliable conversion between the two wind speed sources.

Implementation issues:

6. 1) A potentially minor issue, but in fitting U_{ECMWF} to $U_{QuikSCAT}$ the authors adopt a fit forced through zero, rather than an unconstrained fit. No justification is given for doing so.
We did not need to give a justification because we did both unconstrained and zero-forced fits of $U_{QuikSCAT}$ to U_{ECMWF}. Both were shown in (old) Fig. 8a with dashed and solid lines, respectively. It is seen in the figure that the two fits are very close (almost overlap) with corr. coef. almost identical. The comment suggests that the closeness of the two fits should be clearly pointed out in order to be noticed. We do that in the new Sect. 2.2.3 and in the figure caption (new Fig. 4).

7. 2) When substituting U_{ECMWF} for $U_{QuickSCAT}$ in (7), the authors completely neglect the scaling coefficient with the result that (9) is identically equal to (7) – the authors even note this themselves, and that it is a result of rounding the coefficients, and that the error introduced is up to 10%! There is no justification for doing this. In effect the authors are using the parameterization of W in terms of UQuikSCAT, and claiming it is in terms of an independent UECMWF.

We acknowledge that this was not the best way to pursue the W(U10) parameterization. Updated and extended analysis of the data now provides W(U10) on a global scale and W(U10,T) derived from the regional analysis. New Sect. 2.3 describes the implementation of the parameterizations. New Sect. 3.1.1 present the updated W(U10) expression. New Sect. 3.2 shows the derivation of W(U10,T). Revised Sect. 3.3 compares both W(U10) and W(U10,T) to parameterized W values and to W data.

8. As an aside, equation (8) essentially states "ax=y implies x = y/a" – this is so trivial that it really shouldn't need stating.

We agree. We revised Eq. (8) (new Eq. (7)).

Conceptual issues:

9. A serious problem here is that even if the substitution of U_{ECMWF} for $U_{QuickSCAT}$ was correctly done (no rounding of coefficients), this approach would not give an estimate of W unbiased by any inherent correlation with $U_{QuickSCAT}$, it would simply scale the value of $W^{0.5}$ by the coefficient relating U_{ECMWF} and $U_{QuickSCAT}$. In order to achieve what the authors claim to do, W must be fitted to U_{ECMWF} directly. Note that the is considerable scatter between U_{ECMWF} and $U_{QuickSCAT}$, thus any given estimate of W is likely to be paired with a different value of U_{ECMWF} than $U_{QuickSCAT}$ and the functional form of the fit may be different.
This point essentially invalidates one of the stated aims/conclusions of the paper.

The comment suggests that we did not convey clearly what we have done. So, to clarify:
We made time-space matchups between the WindSat W data and wind speed from ECMWF. For each W—$\boldsymbol{U_{QuickSCAT}}$ pair from the original W database, we have a corresponding W—U_{ECMWF} pair of data. These data are used to make the scatter plots in (old) Fig. 8.
We did make direct fit between the √W values and the ECMWF wind speed values (it was shown in Fig. 8b) and used it to obtain W(U10ECMWF). We thus have direct W(U10) parameterizations for the two wind speed sources.
To address the comment, we revised the text to more clearly present the formation of "independent" data set (new Sect.2.2.3) and the results (new Sect. 3.1.2).

Functional form of W(U_{10}) parameterization

10. When fitting W as a function of U_{10}, the authors adopt an assumed quadratic relationship. No justification is given for this assumption, and it is largely unsupported by previous studies. As the authors themselves noted, Salisbury et al. (2013) found different power laws for W_{10} and W_{37} ($U^{2.26}$ and $U^{1.59}$) respectively for the same data set used here.

We agree that we could have given a better justification of the approach that yielded quadratic wind speed exponent. See below.

11. Cubic or quadratic forms have been forced in previous studies based on theoretical arguments. But these arguments are based on idealised conditions such as a wind input – wave dissipation energy balance. If anything, secondary factors could be expected to lead to a deviation from a strict quadratic or cubic dependence on U10 alone.

We have the same understanding on this and fully agree with this statement.
The presentation of our approach to parameterize secondary forcing is now extended and clarified in new Sect. 2.1. In it, we show that previous experience strongly suggests that the influence of secondary factors is expressed as a change of the wind speed exponent. This has guided our analysis. We didn't choose the quadratic relationship upfront. It was suggested by: (1) the data (e.g., old Fig. 3), to which we tried to fit different functional forms (including cubic); and (2) our aim to apply the same approach to W data at both 10 and 37 GHz. So the quadratic wind speed exponent is, in fact, the adjustment which we expect from whatever idealized wind speed dependence there is (we usually assume cubic) to that dictated by the satellite-based W data. And, in accord with the previous experience mentioned above, this adjustment does represent some implicit account of secondary influences.

The finding of weaker (quadratic) wind speed dependence here is not a precedent. The first reported $W(U_{10})$ relationship of Blanchard (1963) was quadratic. With careful statistical considerations, Bondur and Sharkov (1982) derived a quadratic $W(U_{10})$ relationship for residual W (strip-like structures, in their terminology). Parameterizations of W in waters with different SST have also resulted in wind speed exponents around 2 (see Table 1 in Anguelova and Webster, 2006). Quadratic wind speed dependence is also consistent with the wind speed exponents of Salisbury et al. (2013).

To address this comment, we included justification for using wind speed exponent adjusted by the data in new Sects. 2.1 and 2.3. We also extended the data analysis to include parameterization using cubic wind speed dependence and compare it to the empirical quadratic expression. We report the results in new sects. 3.1.1 and 3.2.2.

12. In general making an a priori assumption about the exponent in such relationships is likely to lead to biases over at least part of the wind speed range. Here it is evident from figure 4 and figure 5(a,b) that the adopted function does not fit the data at either very low or very high wind speeds. There is no reason why the exponent should be an integer value, and it seems likely that many of the results and conclusions in this paper (e.g. Section 3.1.2) are a direct result of this unjustified choice.

Quadratic W(U10) fits well W data for wind speeds from 3 m/s (whitecap inception) to 20 m/s (chosen to minimize uncertainty of satellite-based W data at higher winds). In the updated analysis all fits are done for this range (new Fig. 8).

The quadratic wind exponent represents well the weaker wind speed dependence of the satellite-based W data. We show this in new Fig. 13a described in new Sect. 3.3.2. This confirms that the quadratic wind exponent **is** the deviation we expect due to secondary factors. We have checked with Student's T-statistics and ANOVA tests that indeed quadric W(U10) parameterization is not statistically different from the SAL13 W(U10) parameterizations with more specific wind exponents.

13. The authors state (p21232, line 5) that "The √W(U10) values at 10GHz for wind speeds below 3 m s^{-1} were discarded in the analysis because, as shown in Fig. 4, the linear relationship breaks up at about this wind speed" – the fact that a portion of the data doesn't fit a functional form that has been chosen without justification is not a good reason for discarding it. This is tantamount to cherry picking data that fits a pre-conceived idea. The fact that the data doesn't follow the chosen function is evidence that the function is not appropriate.

Yes, we state this in Line 5 p. 21232. And we continue in the next sentence to state that either discarding or taking into account these data points, does not significantly influence the position of the linear fit.

Discarding W data for wind speeds below 3 m s^{-1} is something we all usually do because we all recognize that this is the wind speed threshold for whitecap formation in most conditions (of course, the threshold wind speed vary). Moreover, in Line 10 on p. 21243, we give justice to SAL13 that they more carefully evaluated the W data to be used in their study by discarding those with large std. deviations. Coincidently, most of these discarded W data were for wind speed below 3 m s^{-1}.

More generally, it is well known that W data, whether in situ or satellite-based, have the largest uncertainty at both low and high winds. Following faithfully their trends at these wind speed regimes is not always productive. We thus introduce the range of wind speed from 3 to 20 m/s used for all fits (new Sect. 2.3). So there is no cherry picking of the data here to fit pre-conceived idea, rather we follow a reasonable and well established practice of quality control of W data.

Regional W distributions

14. The analysis of W(U10) functions by geographical region is a potentially interesting and useful approach. Both this study and Salisbury et al. (2013, 2014) note the significant difference between global maps of W parameterized from this data set and by Monahan and O'Muircheartaigh (1980). The prime reason for that difference is that the Monahan and O'Muircheartaigh (1980) study used tropical data only, and thus represented a specific wind/wave/water-temperature regime, and further with a maximum wind speed of order 17 m s-1, much lower than common high wind speeds at high latitudes. Monahan has emphasised that this is a regionally specific function, but its widespread adoption in models means it commonly gets applied globally, and at wind speeds well above its range of validity.

We fully agree with this statement. We state similar understanding in Lines 9-12 on p. 21242. The revised manuscript has this information too—in Sect. 1 and the end of Sect. 3.3.2.

15. The different functions obtained here for different regions should similarly represent different wind/wave regimes, and the influence of other environmental factors such as sea surface temperature (SST), surfactant concentrations, etc. This point is touched on, but then the various functions are simply averaged to give a single 'globally applicable' function. In fact, as is demonstrated by the differing regional functions, this single function is not truly globally applicable at all – although the bias in any given region may be modest, it will be a mean bias, not random variability, and hence potentially significant in terms of global budgets.

We agree. With the extended significance analysis, we found that the slopes and intercepts of the regional √W to U10 fits are statistically significant; the seasonal variations are not. New Sect. 3.2.1 presents these results; we illustrate the results with Fig. 8 (old Fig. 5) and two additional new figures.

16. The analysis and discussion of the regional/seasonal relationships seems superficial, and perhaps misleading. The authors suggest that the smaller variability in fits with month of year in region 5 vs that between all the different regions for the month of march implies "extreme yet sporadic seasonal values of the major forcing factor such as U10 at a given location contribute less to the W variations than varying environmental conditions from different locations" – but the comparison is of dissimilar effects. The regional differences result from differences in mean conditions (wind/wave regime, SST, surfactant concentrations,...), whereas 'extreme yet sporadic' events will by their nature affect only a small fraction of the data points. Further, region 5 is not necessarily representative of other areas; figure 6 indicates that region 4 (North Atlantic) has a much larger seasonal cycle than other regions, while region 6 (tropical) has very little seasonal cycle. The statements cited above thus draw rather general conclusions from a small, and not necessarily representative, subset of the data.

The "extreme yet sporadic" text is now removed. Analysis is now extended for 12 regions in order to cover the full range of global oceanic conditions and represent diverse regional conditions. New Sect. 2.2.2, updated Fig. 2, and additional Fig. 3 describe the regional W data sets.

17. The analysis of regional/seasonal variations presented in figures 6 and 7 seems a curious approach.
Only the intercepts of the linear fits of $\sqrt{W_{37}}$ to U_{10} are examined – these are effectively the mean offsets in $\sqrt{W_{37}}$ between regions & month of year, the value of $\sqrt{W_{37}}$ at U_{10} = 0. As noted above, the fits do not represent the data well at low wind speeds, the intercepts thus greatly overestimate W at U_{10} = 0 – theoretically W should be zero here.
The justification given for examining the intercept only is that the intercepts show more variability than the gradients (according to the values given the standard deviation of the gradients is ~3%

and that of the intercepts about 20%). We would question the validity of this. Note that when the linear fits of √W are expanded to give W, the gradient scales U^2 while the intercept affects the mean offset and U. As an example we reproduce figure 5f below, with the two fits with extreme gradients highlighted in black and green. For reference the black line is copied as a dotted line with its intercept adjusted to match that of the green line, allowing the relative influence of intercept and gradient to be assessed – clearly they have a similar overall impact.

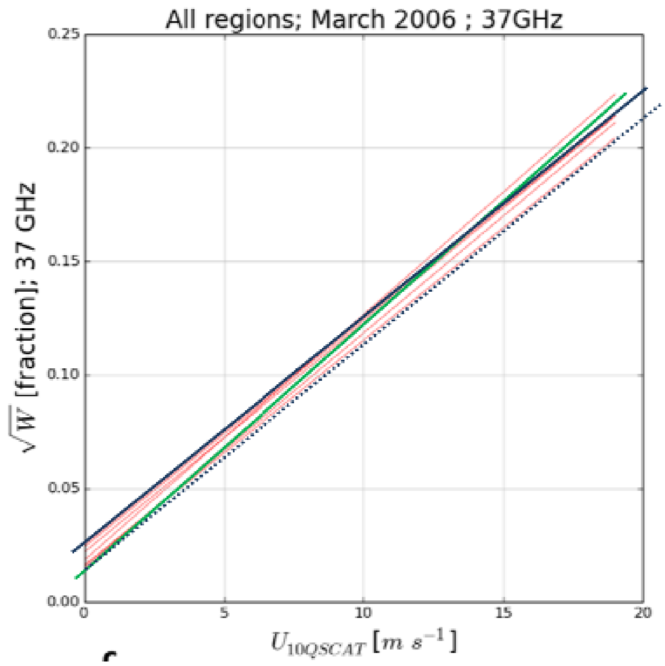
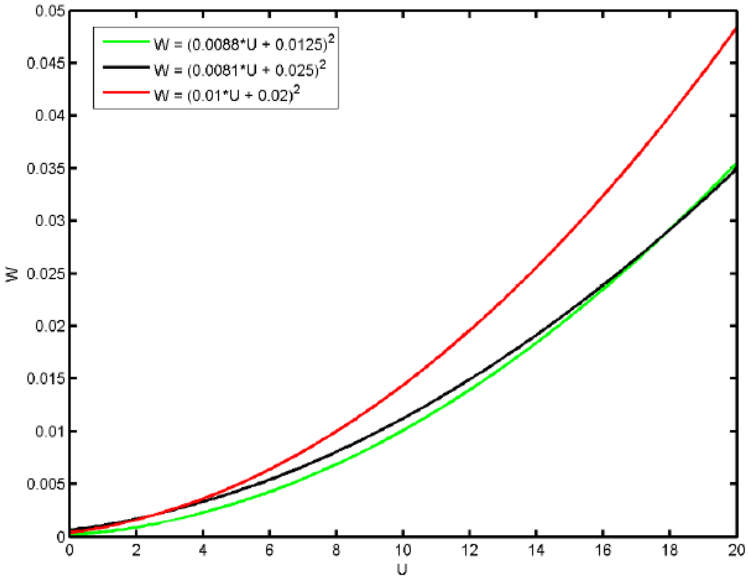


Figure 5f, Green line gradient = 0.0088, black line gradient = 0.0081, a difference of approximately 8%.

We agree that the initial regional analysis was incomplete. The new analysis is on both slopes and intercepts, for both 10 and 37 GHz, applied to all 12 regions for all months with both the adjusted quadratic and the physical cubic relationships. New Sect. 2.3 describes the implementation of the analysis. New Sect. 3.2.2 gives results for quantifying the SST effect. Parameterization W(U10,T) is developed as a quadratic (or cubic) wind speed dependence $W(U_{10})$ whose coefficients vary with SST; this is justified in new Sect. 2.1.

18. It is easier to see the true impact if we plot W instead of √W.



The black and green curves are as in figure 5f above, the difference in gradient more than compensates for the difference in intercepts. More dramatic is the comparison with the red line- the 'global' function given as eqn 7: √W = 0.01U$_{10}$+ 0.02. It is clear here that this 'global' function is far from representative of some of the individual regions for specific seasons.

These equations are now updated (new Eqs. (11-12)). The two new parameterizations, W(U10) from the global data set and the W(U10,T) from the regional analysis, are much closer, almost overlapping. Student's T-statistics and ANOVA tests show them to be statistically not different. Note that this is so for the trend of W with U10 shown in figures like the one above. The new W(U10) and W(U10,T) parameterizations give statistically different results when used with real U10 and T data because W(U10,T) is capable to model the spread of the W data while W(U10) only the trend.

19. In their discussion of the variations in gradients the authors give a rather vague description of why they believe the gradients vary little between regions, suggesting first that the use of a quadratic fit somehow accounts for the influence of secondary environmental forcing factors, which is clearly not possible, then suggesting that maybe multiple environmental factors cancel each other out, which is plausible but pure speculation without any evidence provided. In the discussion of the intercepts of the fits the authors then contradict the earlier claims by suggesting that the gradient accounts for the wind-speed dependence and the other environmental factors are accounted for by the intercept. Again, it is plausible that environmental factors such as SST or surfactant concentration would affect the mean offset in W$_{37}$ but no evidence is presented to support the claim here.

Though the action of secondary factors in opposite directions, and thus cancelling out effects, is viable (Monahan and O'Muircheartaigh, 1986; Scott, 1986, The effect of organic films on water surface motions, in Oceanic Whitecaps, edited by E. Monahan and G. Niocaill, pp. 159–166), we do not use this idea anymore because we cannot show this with our data.

As said above (comments 11 and 12), the quadratic wind exponent is the adjusted (empirical) wind exponent dictated by the satellite-based W data, so it represents a deviation from physical cubic due to secondary factors. We now prove that quadratic W(U10,T) replicates the satellite-based data well, while cubic W(U10,T) cannot. We present extensive discussion on this with two new figs. 12 and 13 in new sects. 3.3.1 and 3.3.2.

As for the intercept, we revised the manuscript to introduce the currently accepted interpretation of negative y-intercept (Sect. 2.1). Then in Sect. 3.1.1, we propose broader interpretation of the y-intercept in W(U10) expressions, be it negative or positive. Briefly, we promote the hypothesis that positive y-intercept could be interpreted as a measure of the capacity of seawater with specific characteristics, such as SST (thus viscosity), salinity, and surfactant concentration, to affect the extent of W. These secondary factors do not create whitecaps per se. Rather, they prolong the lifetime of the whitecaps thus contribute to W by altering the characteristics of submerged and surface bubbles such as stabilization and persistence by surfactants or rise velocity variations that replenishing the foam on the surface at different rates. These processes ultimately augment or decrease W and the y-intercept can be thought of as a mathematical expression of this static forcing (as opposed to dynamic forcing from the wind). In this light, our data showing negative y-intercept for W values at 10 GHz is consistent with our and SAL13 analysis that active whitecaps are less affected by secondary factors. However, secondary factors do affect strongly residual whitecaps and the positive y-intercept for our W values at 37 GHz can be interpreted and used to quantify this static influences. This is a hypothesis which is worth promoting for consideration, debate, and further verification by the community.

20. A relationship with SST is claimed from figure 7, where time series of the intercepts of monthly mean fits of $\sqrt{W_{37}}$ to U_{10} are plotted by region, along with similar time series of monthly mean SSTs. The authors claim an inverse relationship between the intercept and SST. This is (we presume) inferred by the progression of increasing SST from regions 5 → 4 → 6 and the corresponding decrease in intercept between the same regions (in a mean sense, there are individual points that do not follow the trend). However, this assumes all the differences between regions are a result of SST, and does not allow for the co-variation of, for example, SST and biology, and hence surfactant concentration, or of SST with latitude and hence wind/wave regime. Also, it is hard to determine anything but the most general relationship from a plot of overlaid time series. If you want to determine the relationship between the intercepts and SST, plot a scatterplot of intercept (y axis) against SST (x axis) and look for a functional relationship.

We now plot the slopes and intercepts of the W(U10) relationships in all regions and for all months as a function of SST (new Fig. 11). From these plots we derive expressions for the SST variations of the coefficients in the W(U10) dependence. The figure shows the inverse relationship between the intercept and SST.

Agree, we cannot account for the interplay between the secondary factors in different regions with the data we use in this study. However, with new Fig. 13a (in new Sect. 3.3.2) we show that including SST in the W parameterization explains only part of the spread/variability of the satellite-based W data. This suggests that besides SST, other secondary factors have to be included explicitly to fully replicate the variability of the satellite-based W data.

Aerosol Flux

21. The whitecap method for parameterization of the sea spray source flux is built upon the premise that W can be used as a scaling factor. That is, for a given shape function (the size-resolved interfacial flux from a unit area whitecap), any change in the production flux is linearly related to the change in W. Though it has been noted that this premise is likely to be incorrect (Norris et al. 2013), given the need for relatively simple parameterisations of SSA production rates in global climate and aerosol models, the community is not yet at the stage where the whitecap method can be developed to reflect this fact.

Therefore in presenting new globally-averaged estimates (or global maps) of SSA emission rates calculated via the whitecap method (in its current form), little new information is gained.

We respectfully disagree with this comment because we consider as an important result the fact that our SSA estimates have quite a different spatial distribution thanks to the satellite-based W data. To demonstrate these differences, the widely used whitecap-based SSSF in this form is a useful baseline for comparison; we justify this in new Sect. 2.4.1 (see also comment 2). Also, with our and previous results, we were able to examine and quantify the variations of SSA emissions attributed to magnitude and/or shape factors in the whitecap-based SSSF (see comment 1).

22. One could argue that it is worthwhile comparing the resulting new estimates of globally-averaged SSA production rates with those of previous studies, but often these estimates simply lie somewhere within the large spread of previous estimates, and no further illuminating conclusions can be deduced.

That SSA emission inferred by our new parameterization is within the range of previous estimates of SSA emissions shows that our modified SSSF gives consistent estimates. Certainly, we do not want to be an outlier among SSA emission estimates, especially knowing their large spread. Again, what is more important is that the spatial distribution of this total SSA emission is significantly different from those of previous SSSF predictions. And, again, our estimates of the total SSA emission proved useful to evaluate variations due to magnitude and/or shape factors in the SSSF (see earlier comments 1, 2, and 21).

23. All the new and novel information is contained within the new W estimates and their spatial variation (Figure 9). Figure 10, therefore, adds little to the paper, especially when followed by the difference map [Figure 11]). We suggest that maps of the difference (bias) between W from the new parameterisation and those obtained from a previous parameterisation are more easily interpretable.

Salisbury et al. (2014) show global maps of the new satellite-based W data. Old Figs. 9 and 10 showed global maps from W parameterizations, not W data. In our view, it is informative for the readers to see global maps of W and SSA with the absolute values obtained with the new W(U10) parameterization.

Still, we agree that difference maps for W and SSA with reference values from MOM80 and M86, respectively, is a more informative and focused way to demonstrate differences. So Fig. 9 (new Fig. 13b) is revised to show difference between W from MOM80 and W from our quadratic W(U10,T). Old Figs. 10 and 11 are combined in a new Figure 14 with top panel showing SSA from the M86 SSSF using our quadratic W(U10,T), and lower panel showing difference map with M86 SSSF using MOM80 W(U10).

References

Norris, S. J., I. M. Brooks, B. I. Moat, M. J. Yelland, G. de Leeuw, R. W. Pascal , B. J. Brooks, 2013: Near-surface measurements of sea spray aerosol production over whitecaps in the open ocean. *Ocean Science*, 9, 133–145, doi: 10.5194/os-9-133-2013

Paget, A. C., M. A. Bourassa, and M. D. Anguelova (2015), Comparing in situ and satellite-based parameterizations of oceanic whitecaps, *J. Geophys. Res. Oceans*, 120, 2826–2843, doi:10.1002/2014JC010328