**Anonymous referee #2**

***General impression: The paper is presenting an extensive effort of data treatment, but the results and conclusions are rather limited. The authors need to better articulate conceptual aspects of the methods used, some of which I found misinterpreted. Overall, the paper has its potential and may become publishable, but needs additional work.***

We thank the Referee 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 in sequence: (1) the original comment from the Referee (in bold italic), there are 8 comments; (2) our response; (3) changes in manuscript.

***1.1 The main advantage over other similar W parameterisations is a quadratic form of a new parameterisation. Regardless of the well correlated linear fits of sqrt(W) there is little justification why it should be quadratic. The resulting good correlation cannot justify it. Perhaps it can be reduced to quadratic form after careful consideration of the uncertainties, but choosing it upfront is a thing of the past when analytical approaches were limited due to computing power.***

1.2 We agree with the Referee that we could have given a better justification of the approach that yielded quadratic wind speed exponent. To clarify, 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; and (2) our aim to apply the same approach to W data at both 10 and 37 GHz.

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 SAL13.

1.3 To address the Referee's concern, we have 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.

***2.1 Following the above the progress over the extensively referenced Salisbury et al. papers is poorly documented or highlighted.***

2.2 We have stated how this work relates to the work of SAL13 in two places. In Lines 17-19 on page 21242, we state that we see the current work as complimenting the work of SAL13. In Lines 3-7 on page 21243, we point out a difference.
To recap, besides using different analyses (e.g., regional analysis), we also added analysis and quantification of the possible intrinsic correlation in the W data and how this could affect W predictions with the new W(U10) expressions. We also assessed the utility of using the satellite-based W data to estimate SSA production rate.
Yet, we agree with the Referee that we could have distinguished the two studies more clearly.

2.3 As noted at the beginning, we extended our analysis. The results on new W(U10,T) parameterization at both quadratic and cubic wind exponents (revised Sect. 3.2) and the investigation of significant differences (revised Sect. 3.3) add to the results listed above and clearly set this study apart from the analysis done in SAL13.

***3.1 The main advantage of the paper might be exploration of regional differences, but the regions of extreme variability in global map (Fig.9) are poorly represented, namely, high latitude S. Atlantic, high latitude N. Pacific, high latitude North Atlantic, S. Indian Ocean. Five out of seven regions were in subtropical 60deg band. Was it due to limited clear skies? If so, that was a significant limitation of the exploratory effort.***

3.2 We appreciate that the Referee acknowledges the advantage of performing regional analysis. The comment suggests that we have not presented our reasoning for the choice of the regions well. Here are some clarifications.

The cloudiness doesn't play role in the choice of the regions because radiometric measurements at microwave frequencies, used to obtain W estimates, penetrate most clouds. Radiometric observations at the ocean surface could be limited by very thick clouds (with a lot of liquid water content) and by precipitation. Such cases are flagged in the WindSat algorithm and are not used to obtain W values.

The number of samples was one of the criteria we had when choosing the regions (Line 28 on page 21227 and Line 1 on page 21228). By this criterion, there are fewer samples for latitudes above 60°S or N (see Fig. 3), mostly because WindSat and QuikSCAT have fewer matching points there (Sect. 2.1).

The latitudes between 40°S and 50°S are known as "The Roaring Forties" for the strong westerly winds there. Our region 5 is chosen in these latitudes. And because the conditions in the Southern Ocean are relatively uniform (due to lack of land masses), region 5 represents the Roaring Forties well. The regions at subtropical latitudes are placed within the Trade winds zone. These are persistent easterly winds blowing over different fetches in different oceans with different salinity and surfactants. So regions 2, 3, and 7 are representative of different cases.

Still, to address the Referee's comment, we analyzed W data in more regions.

3.3 Additional regions were chosen (updated Fig. 2); climatology for different conditions is given (new Fig. 3); extended text to justify the region choices is included (new sect. 2.2.2); and results from the extended regional analysis are given (new sect. 3.2).

***4.1 The use of a chosen coarse mode SSA tool to prove usefulness of a new W parameterization is quite useless considering that available SS source functions range several orders of magnitude and would likely swamp any variability between different W parameterisations or, certainly, the impacts of secondary factors. That part is redundant in the paper as it adds very little useful knowledge. Fig. 12 is sufficient for the purpose.***

4.2 We respectfully disagree with the Referee's comment because, while our modified SSSF predicts SSA production which falls within the range of variability of previously used SSSFs, 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.
We updated our previous comparisons (old sect. 4.3) with additional comparisons between our and Grythe et al. (2014) results for SSA fluxes (new sect. 3.4). This gave us the possibility to examine and quantify variations of SSA emissions attributed to magnitude and/or shape factors of the SSSF.

4.3 The new 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.

***5.1 I disagree with the author's interpretation of the intercepts arising from 10 and 37GHz datasets. Negative intercept of 10GHz dataset is physically meaningful (contrary to what authors say) as it is pointing at onset of white-capping. Contrary to what authors say, positive intercept of 37GHz dataset is meaningless, suggesting white cap at negative wind speed. Reference to residual foam is wrong as residual foam does not produce SSA as it lingers for hours, does not relate to wind speed (no bubble plume can be produced at 2m/s) and, therefore, has nothing in common with actively generated foam by bubble plumes only occurring above 3-4 m/s wind speed. A surfactant related foam while lasting a little longer is forming (and dissipating thereafter within seconds, not hours) at significant wind speeds. While data below 3m/s have little impact on W it should at least be correctly discussed.***

5.2 We agree with the Referee that we didn't convey well our interpretation of the y-intercept.

5.3 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.

***6.1 I disagree with the concept of avoiding intrinsic correlation of W and U10 substituting QSCAT wind speed by ECMWF wind. In fairness, W should have been fitted directly to ECMWF data of whatever resolution because a large scatter (regardless of good overall correlation) between two wind speed datasets could have produced discernible differences in W. In conclusion the approach does not allow comparing statistical parameters of W fits.***

6.2 Please note that we had done what the Referee suggests should have been done. We did make direct fit between the WindSat W values and the ECMWF wind speed values; it was presented in Fig. 8b. We assessed the differences between U10 from QSCAT and ECMWF; it was presented in Fig. 8a. Also, we did assess how much W values from parameterizations using QSCAT or ECMWF winds differ (Sect. 4.2.1, Lines 13-29 on p. 21240 and Lines 1-14 on p. 21241). The Referee's comment shows that we didn't present these results clearly.

6.3 New Sect. 2.2.3 more clearly describes the independent data set. New sect. 3.1.2 with results for intrinsic correlation is revised for completeness and clarity.

***7.1 Another conceptual flaw was speculating over secondary factors influencing W quadratic relationship. The authors should have at least demonstrated that any two arbitrary chosen secondary factors were cancelling each other's influence before drawing any conclusion (or speculation in this case).***

7.2 We respectfully disagree that the concept of accounting for secondary factors via change of the wind speed exponent is flawed.

Our approach to parameterize secondary forcing is now extended and clearly presented in new Sect. 2.1. In it, we show the concept that the variability of W caused by secondary factors is expressed as a change of the wind speed exponent is not new. The Monahan and O'Muircheartaigh (1986) analysis of five data sets showed that the variability of W caused by SST (and the atmospheric stability) affect significantly the coefficients in the wind speed dependence W(U10), especially the wind speed exponent. The survey of W(U10) parameterizations by Anguelova and Webster (2006, their Tables 1 and 2) also clearly shows that each campaign conducted in different regions and conditions comes up with a specific wind speed exponent. This strongly suggests that the influence of secondary factors is expressed as a change of the wind speed exponent.

As said in the text (Lines 5-6 on p. 21234), the secondary effects could act in opposite ways. For instance, the low viscosity of cold waters (e.g., in the Southern ocean) acts to decrease the sea surface roughness, this delays the wave growth, leading to less frequency of wave breaking, and thus decreasing W. At the same time, the high productivity of cold waters yields higher surfactant concentrations, which stabilizes the submerged and surface bubbles, so though less often created, the whitecaps in such places persist thus increasing W. The net effect of these two processes could be nominal (i.e., no change), more, or fewer whitecaps. Monahan and O'Muircheartaigh (1986) and Scott (1986, The effect of organic films on water surface motions, in Oceanic Whitecaps, edited by E. Monahan and G. Niocaill, pp. 159–166) have presented this physical reasoning, and Anguelova and Webster (2006) have shown that such interplay of the secondary effects may explain the spatial distribution of satellite-based W values.

While we are quite interested in investigating and quantifying the net result of such interplay, it cannot be verified with the database we have. Data for seawater properties (including surfactants, which are difficult to measure), sea surface roughness, bubble lifetime in submerged plumes, and whitecap decay times are necessary for such an investigation. Still, being well aware that such interplay is physically probable, we used it to explain the small variations between W(U10) expressions derived for different regions. We, therefore, do not see this as a flaw of our approach, but more as a realization that there is much more to do to understand the natural whitecap variability and that the W database is only a start in this direction.

7.3 Because with our extended analysis we now clearly show that the effect of a secondary factor, such as SST, on W trend can be accounted for to a large extent by change of the wind speed exponent, we do not use the idea of the opposite action of the secondary factors.

Note that with our extended regional analysis, we have develop W(U10,T) parameterization using both empirical (adjusted quadratic) and cubic wind exponents. We used significance tests (Student's T-statistics and ANOVA) to establish similarity and differences between W(U10) and W(U10,T) with both empirical and cubic exponents. We found that the W(U10) trend predicted with a quadratic wind speed exponent does not differ significantly from the W(U10) trend predicted either with quadratic or cubic W(U10, T). This result clearly shows that to a large extent, the adjusted wind exponent accounts for the change in the trend caused by SST and other secondary influences. Our new sect. 3.3.2 shows that explicitly accounting of SST (and eventually other factors) helps to model the spread, not the trend, of the W data.

The changes in the manuscript to address this comment include: Description of the approach in Sect. 2.1, the significance test used in Sect. 2.3, and give the results regarding differences between parameterizations that account for variability implicitly or explicitly in Sect. 3.3. Through the text, with each new result presented, we drive the point that the adjustment from cubic to quadratic wind exponent accounts to a large extent for secondary influences on the trend of W with U10.

***8.1 I have additional comment regarding leveling of W relationship at very high wind speeds. While increasing wind energy is favoring more of air entrainment and consequently larger foams the wind is also blowing directly into the foam disrupting it in the process. Such process has not been quantified yet, but is obvious in even the simplest table top experiment.***

8.2 Fully agree with the Referee's comment—the leveling of W (and air-sea interaction processes associated with W) at high winds, while observed is not yet well understood and quantified. While appreciative of the comment, we decided to not speculate on the leveling off in the revised manuscript because we have a lot of new material.

The referee's suggestion, if we understand it correctly—that disruption of whitecap foam moving against the wind could explain the leveling of (at least partially)—is an interesting one and, frankly, new to us. Perhaps this is akin to spume droplets, just relates to the spume (synonymous of froth and foam) itself, not to the droplets formed from the spume. In any case, this is an idea which should be promoted by the Referee.

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