Comments on 'Paramterization of oceanic whitecap fraction based on satellite observations' by M. Albert et al., Atmos. Chem. Phys. Discuss., 15, 21219–21269, 2015

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September 29th 2015

General comments

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.
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 co-author (Norris et al. (2013)) has demonstrated that the aerosol flux per unit area whitecap varies with the wind/wave conditions.

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.

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).

Use of independent wind speed

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.

Implementation issues:
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.
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

$U_{QuikSCAT}$, and claiming it is in terms of an independent U_{ECMWF}.
As an aside, equation (8) essentially states “ax=y implies x = y/a” – this is so trivial that it really shouldn’t need stating.

Conceptual issues:
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.

Functional form of $W(U_{10})$ parameterization

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.

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 U_{10} alone. 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.

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.

Regional W distributions

The analysis of $W(U_{10})$ 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 wide-spread adoption in models means it commonly gets applied globally, and at wind speeds well above its range of validity.

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.

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 U_{10} 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 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 $\sqrt{W}$ 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.

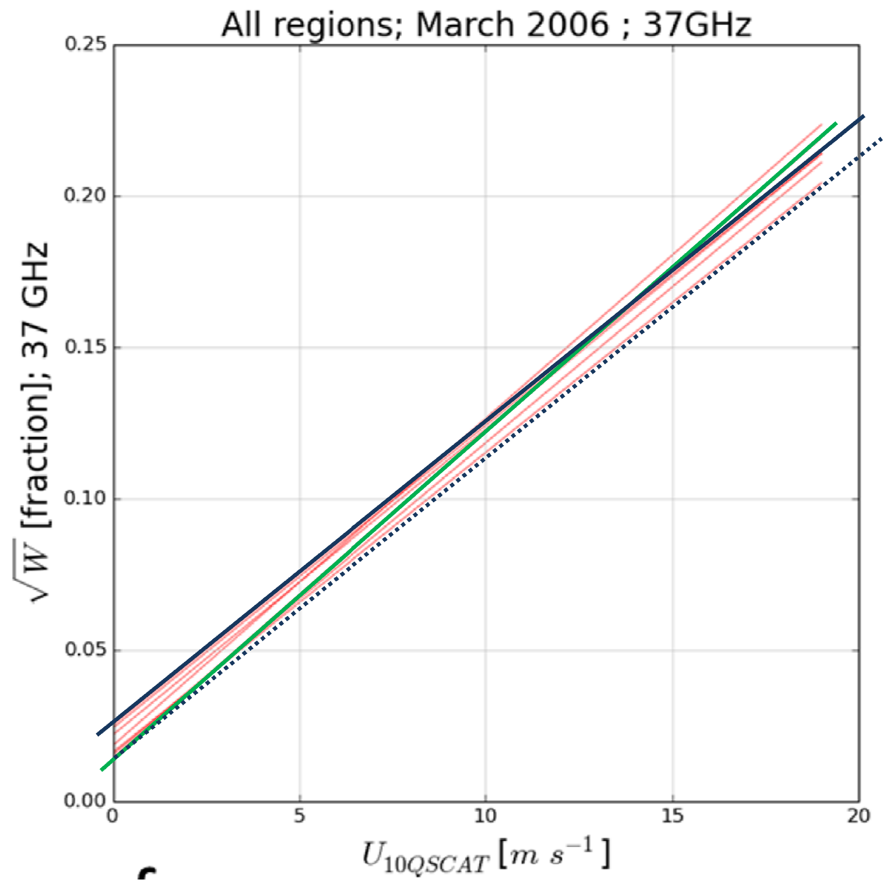
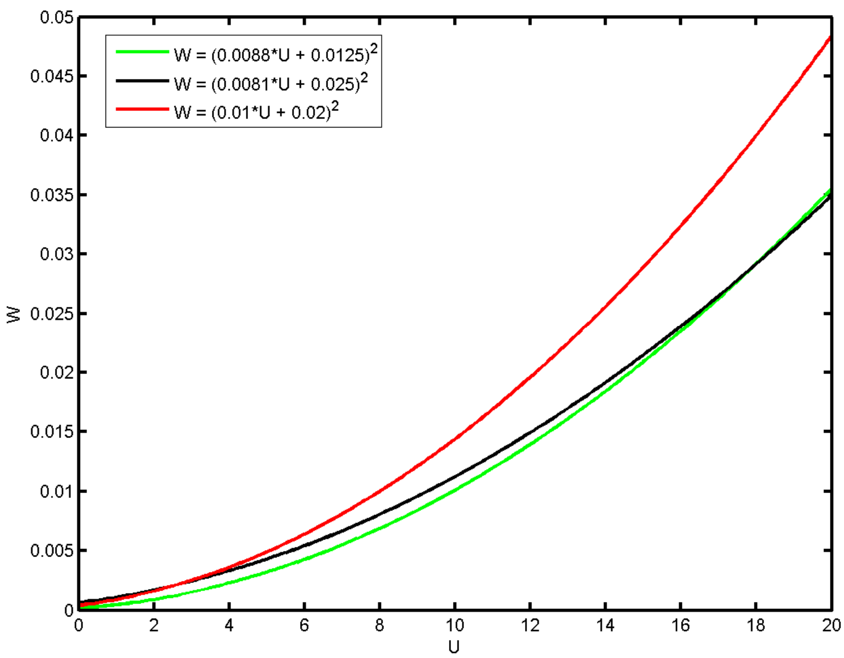


Figure 5f, Green line gradient = 0.0088, black line gradient = 0.0081, a difference of approximately 8%.

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: $\sqrt{W} = 0.01U_{10} + 0.02$. It is clear here that this ‘global’ function is far from representative of some of the individual regions for specific seasons.

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.

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.

Aerosol Flux
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. 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. 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.

References
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