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Discussions

Interactive comment on "Parameterization of oceanic whitecap fraction based on satellite observations" *by* M. F. M. A. Albert et al.

Anonymous Referee #2

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The authors tried to derive a new empirical parameterisation to predict W as a function of wind speed for the use in SSSF. 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.

General comments

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.

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

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.

The use of a chosen coarse mode SSA tool to prove usefulness of a new W parameterisation 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.

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.

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.

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).

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 21219, 2015.