

Interactive comment on “Sea surface wind speed estimation from space-based lidar measurements” by Y. Hu et al.

Anonymous Referee #4

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General comments

This is an excellent, well-written paper that makes a major contribution to the field of space-based Earth observations. In particular, use is made of the CALIOP polarimetric channels to remove retroreflectance contributions from sea-surface foam and subsurface scatter, which have tended to confuse the results from similar past studies. Eliminating these effects has enabled the authors to home in on the better characterized aspects of wind-driven sea-surface phenomenology and has resulted in the first real refinement in our understanding of this topic in almost two decades. I have a number of comments and recommendations for the authors that I believe will enhance the paper.

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Specific comments

P2772, L8 P2773, L3: Both Wu (1972) and Wu (1990) are cited. In fact, the authors only make use of results from the 1990 reference, which should be regarded as Wu's updating of his 1972 paper. I believe it would be more correct and less confusing if reference to Wu (1972) were deleted.

P2774, L4: In the first embedded equation θ is lacking the 'x' subscript. Also, $\tan\theta_x$ is defined as the wave slope, whereas θ_x is actually the wave slope. This terminological error is repeated two lines later in the definition for $\tan\theta$. (I actually believe that the presentation would be clearer if most of the embedded equations on this page were given their own separate line and numbered, but that is between the authors and the copy editors.)

P2774, L11: Right after θ is used for the wave slope in Eq. 1 it gets redefined in the sentence immediately following as the lidar nadir viewing angle. This creates too much scope for confusion and should be rectified by choosing an alternative symbol and propagating the change through the rest of the paper.

P2774, L16: In the embedded equation defining the Fresnel reflectance, ρ is made a function of θ . In fact ρ is defined for normal incidence; the implied dependence on θ should be deleted.

P2775, L12-14: σ^2 coefficient definitions: Add the publication year for Cox/Munk and Wu.

P2776, L14-16: "The version-5 AMSR-E wind speed product, which has a spatial resolution of 20 km, agrees well with other satellite wind measurements." This is a very important finding. Two recent publications (Ebuchi and Kizu, 2002, doi:10.1023/A.1021213331788; Li *et al.*, 2007, doi:10.1117/12.732779) have suggested values for the σ^2 coefficients that are considerably at odds with Cox/Munk, Wu, and the present work and have attributed these observations to several potential

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factors, *including* disparities in sample area. Both of these studies took as their basis scatterometer winds on a 28-km grid. This is of similar order to the 20-km grid of the AMSR-E data used by the present authors. The fact that their study indicates no bias introduced by the sampling area differential between CALIOP and AMSR-E points to some other interpretation of the Ebuchi/Kizu and Li *et al.* observations. I believe it would be worthwhile (though not mandatory) for the authors to devote just a few sentences to this topic.

P2778, L3/5 P2790, Figure 3: The use of "cleanest" should be explicitly explained as referring to low aerosol loading.

P2778, L16 P2782, L24: 15% lidar depolarization is assumed. Why 15%? Some justification would be helpful.

P2778, L17-19: "For wind speed larger than 12 m/s, the correlation between AMSR-E wind speed and CALIPSO lidar backscatter increased significantly after this whitecap correction (lower panel)." This would be easier for the reader to judge if the model curve was included in the upper panel also.

P2778, L21: How was the "best fit" computed? What were the typical correlation coefficients and were any weighting factors applied?

P2779: Using ocean surface backscatter to improve space-based lidar radiometric calibration was first attempted by the ICESat project. The authors have built considerably on that pilot study, but a reference to that work (Lancaster *et al.*, 2005, doi:10.1029/2005GL023732) somewhere in this section would be appropriate.

P2781, L17-18: "This small bias is consistent with the magnitude of transient response anomalies." A little more explanation of this link is warranted.

P2784, L23-25: "...lidars can make sea surface wind measurements that could be calibrated, not requiring any empirical fitting of the data." Isn't this paper really doing just that (*i.e.*, empirical fitting)?

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P2785, L3: "The lidar can also make both day and night measurements." Some statement on the CALIOP experience with relative day/night SNR impacts might be useful to add.

P2788, Figure 1: If possible, it'd be nice to have some uncertainty bars added to the CALIPSO curve.

P2789, Figure 2, lower panel x-axis label: What does "1-7.66 δ " signify?

P2790, Figure 3: "Clean" presumably means low atmospheric aerosol loading and should be described as such.

Technical corrections

P2772, L26: Cox & Munk specified their results at 12.5 m ASL, not 10 m.

P2776, L18: "CALIOP" appears here for the first time without being defined.

P2781, L23-24: The final sentence of this same paragraph simply repeats what was said two sentences before. It should be deleted.

P2793, Figure 6, lower panel: Vertical axis needs labeling ("counts/bin"?). Upper panel: Color key should be labeled ("m/s"?).

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2771, 2008.

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