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Interactive Comment

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

Anonymous Referee #3

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

General Comments:

This is a well-organized paper treated an interesting and important subject in Earth observations. The authors have, in this investigation, made good use of a very special opportunity to compare sea surface wind speed using the CALIPSO lidar and the AMSR-E microwave radiometer, both in the same orbit within seconds of each other. The level of detail in this intercomparison of lidar and radiometer surface wind speed measurements is unprecedented. The results that are presented very strongly support the idea that Earth-orbiting lidars can be used in backscatter mode to accurately measure sea surface wind speed.

The paper does have some issues that need clarification. Below are a few specific comments that hopefully will stimulate clarification and refinement of the presentation and the assumptions underlying the data comparison.

Specific Comments:

Section 1: Introduction:

It is stated that the CALIPSO lidar measures sea surface backscatter at only 0.3 degrees off-nadir. Since the footprint is only 70 m, the long-wavelength swell should introduce additional slopes. This may not seem to be important at first glance, since these slopes are small. However they can be significant compared with 0.3 degrees, and at very low wind speeds this effect should modulate the amplitude of the very large backscatter that one would expect so close to the specular reflectance angle, resulting in an average that should be somewhat different.

(p2774, line 17) The light reflected from the surface is co-linearly polarized only when there is no multiple scattering from the slopes. The near-nadir angle helps to avoid this circumstance when the surface roughens at high wind speeds; however there will be an upper limit to the range of wind speed for which this is applicable.

(p2774, lines 20-24 and 2775, lines 1-5) The statement “The lidar backscattering coefficient in the CALIPSO data product is instead normalized to 4*pi solid angle in order to account for scattering in the atmosphere. By definition then, the sea surface lidar integrated backscatter coefficient for a lidar in units of sr-1 is half of the total backscatter cross section of opaque objects such as dense clouds and surfaces….” is opaque to the reader. Why the normalization by 4*pi? Why is the sea surface backscatter treated differently from an opaque surface backscatter? Please clarify. We need to know how gamma appears in your version of the lidar equation in order to understand how to interpret your quantitative results. In any case, the

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normalization in the exponent of eqn. (3) appears to be incorrect, and this is an issue that certainly should be resolved. You use the two dimensional (2D) wave slope variance here, which can be considered as the sum of the alongwind variance and the crosswind variance. Barrick (1968) uses the same 2D wave slope variance in his expression (eqn. 13b) but does not have the multiplicative factor of 2. Menzies et al (1998) is consistent with Barrick. Tratt et al (2002) have a multiplicative factor of 2 in their eqn. (1) exponent, but they use a different definition of wave slope variance in their eqn (1), which is explained in eqn (2). The exponential factor has no bearing on your results with your pointing geometry, but it should be corrected.

(Same paragraph) You introduce the term “sea surface lidar integrated backscatter” here, without defining it. You should provide an explicit definition of what you mean by “integrated backscatter” in your analysis of CALIPSO lidar backscatter signals from the ocean surface. Obviously the surface backscatter signal is smeared in time due to the pulse duration and the finite receiver bandwidth. What is your effective instrumental line-of-sight spatial average? What is the extent of your integral when computing the integrated backscatter? At such a small off-nadir angle, your signals should be dominated by the surface scattering, even though the 532 nm radiation will penetrate through a subsurface volume.

The ordering of some of the material here and in the following Section 2 is somewhat bothersome. The introduction of the “alternate” relation between wave slope variance and surface wind speed (Eqn. (5)) is provided in the Introduction without an accompanying rationale. The reader wonders at this point why the departure from the historical Cox and Munk and Wu relationships. It is stated here that Eqn. (5) is based on the comparison between the CALIPSO lidar data and the AMSR-E data; however it is not until later in Section 2 that we read that Eqn. (5) is the “best fit” relation based on the CALIPSO/AMSR-E data, as illustrated in Figure 2. (It would be informative to explicitly state what your “best fit” algorithm is.) I would prefer to see the wave slope variance vs. wind speed relationships introduced

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in Section 2, after the individual instrument data products and the intercomparison are described in more detail.

I would prefer to see the x-axis of Figure 1 extend all the way to zero slope variance. As it is drawn, one must estimate at what value of slope variance the intersection with the y-axis occurs.

Section 2: Wind speed – wave variance relation….

Third paragraph: Elimination of the effects of white-caps and sub-surface contributions using polarization discrimination at 532 nm is a clever technique. It should be stated that use of the 1064 nm wavelength eliminates the sub-surface scattering contribution due to the very small water penetration depth at this wavelength. Although at your near-nadir look angle, the sub-surface volume scattering component should be a very small percentage of the surface backscatter component. Fourth paragraph: “For the ocean surface lidar backscatter measurements with the lowest atmospheric backscatter,….” Be quantitative here. What is meant by the “lowest atmospheric backscatter”? Figure 2 and accompanying text: This is a very informative figure – an impressive data comparison. Now it becomes clear that the purpose of your introduction of an “alternate” relation between wave slope variance and wind speed is based on the motivation to improve the intercomparison with AMSR-E, using the AMSR-E retrievals as the yardstick. Is there any additional physical reason for why one should use the relationship expressed in Eqn. (5)? The Cox and Munk relationship is clearly based on experimental observations in the visible. Wu provided fluid dynamical arguments to support his relationship. The Wu slope discontinuity at 7 m/s, and the departures at low wind speeds are the most significant “misfits” in the CALIPSO/AMSR-E data comparison. The lidar and the passive microwave radiometer (as well as the scatterometer) signals derive from different classes of wave spectra, with much different footprint sizes. The CALIPSO lidar observations may be biased at the very low wind speeds due to the restriction to a near-nadir look angle, in part due to the influence of the swell in producing residual

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slopes. As you state later in this section, further measurements using multi-angle lidar and collocated wind speed measurements would be very helpful in validation of your assumptions at low wind speeds. What is the uncertainty in AMSR-E wind speed retrievals at the low wind speeds when the microwave radiometers see the surface as nearly specular? Can you cite results of intercomparisons of AMSR-E retrievals with in situ surface wind data at wind speeds between 0-5 m/s? Paragraph 6: Any reason for choosing 15% lidar depolarization for backscatter from whitecaps and sub-surface scatterers, other than the data fitting? What source did you use for the percent surface whitecap coverage vs. wind speed? Please cite that source.

Section 3: Improving Calibration with Ocean Surface Lidar Backscatter

Your calibration methodology is very clearly described, and you have made good use of this approach. Figure 4 is good evidence of the utility of this methodology. It would be appropriate to point out that this approach to calibrating the 1064 nm channel was first applied to the LITE shuttle lidar data obtained in 1994. A brief description is found in the Menzies et al. (1998) paper. You have greatly expanded the use of this methodology with the CALIPSO lidar data.

Section 4:

“Considered on a global scale, there is no systematic bias between the CALIPSO wind speeds and the AMSR-E wind speeds.” Isn’t this simply due to the fact that you chose to use Eqn (5) – the “best fit” to the AMSR-E data – to derive the CALIPSO wind speeds?

”Technical corrections”: Section 4: Figure 6

Upper: The units for the color code are not explicitly stated.

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