Review of egusphere-2023-1943: "HSRL-2 Retrievals of Ocean Surface Wind Speeds" by Dmitrovic et al.

In this work the authors describe an algorithm for deriving ocean surface wind speed estimates from measurements of ocean surface backscatter acquired by the NASA-Langley high spectral resolution lidar (HSRL). The NASA HSRL can accurately characterize the signal attenuation above the ocean surface and reliably partition the surface signal pulse into pure surface and ocean subsurface components, and hence can deliver high quality measurements of surface integrated attenuated backscatter (β_{surf}). The accuracy of the wind speed retrieval thus depends on the equation relating β_{surf} to wave slope variances and the fidelity of the model used to convert wave slope variances to wind speeds. The authors derive wind speeds using two different models – the classic Cox-Munk (1954) and a lidar-specific model developed by Hu et al. (2008) – and compare these results to near-simultaneous dropsonde measurements of wind speeds. The paper is well organized and well written and its subject matter is entirely appropriate for Atmospheric Measurements Techniques. There are, however, several issues that should be addressed prior to publication.

My primary concern is that the authors' equation (2) fails to acknowledge the possible presence of whitecaps and/or sea foam. While the authors cite Josset et al., 2010b as their source for equation (2), that work explicitly includes reflection from the whitecap fraction within any footprint; see section 2.2 and equations (2) and (21) therein. Furthermore, comparing the upper and lower panels of figure (2) in Hu et al., 2008 suggests that omitting whitecap contributions could have a significant impact on the results derived in this paper. On the other hand, Lancaster et al. (2005) suggest that the "contribution of whitecaps to the nadir lidar measurements is seen in Figure 2 to be negligible".

I note that the authors' equation (2) also omits the atmospheric two-way transmittance term give in equation (21) in Josset et al., 2010b. Given the discussion in the introduction about calibration transfer and the assertion on line 116, this omission seems a bit surprising.

When considering the authors' wind speed difference statistics, I kept wondering about wind speed variations over time at a fixed point. HSRL biases relative to the dropsondes are given as either 0.15 ± 1.80 m/s or 0.62 ± 1.70 m/s, depending on the model used. But the temporal offset between matched HSRL and dropsonde wind speed estimates can be as large as 15 minutes. How do these bias magnitudes compare to the natural variations in wind speed that would be measured at a fixed point over a 15-minute time interval? Perhaps wind speed variability information is readily available from the NOAA's National Data Buoy Center? A box and whisker plot showing wind speed differences as a function of temporal offset between the two data sets might also shed some light on this issue.

On lines 51–52, immediately after describing the rudiments of Hu et al., 2008 derivation, the authors introduce equation (1) by say, "The wind speed (U) was then approximated from the wave-slope variance (σ^2) through this linear relationship". What I was expecting to see were the Hu equations subsequently given as equations (3.1) through (3.3). Instead, equation (1) is Cox-Munk. This section of the text (lines 44–52) should be rewritten to clearly distinguish between the original Cox-Munk equation and the subsequent CALIPSO derivation by Hu.

From a quick glance at the studies cited on line 54, I do not find any support for the assertion that "CALIPSO retrievals of surface wind speeds have been used in many studies". Josset et al., 2010a used AMSR winds to investigate "the normalized scattering cross section" of the CALIPSO lidar and the CloudSat radar. Kiliyanpilakkil and Meskhidze used AMSR winds and aerosol optical properties derived from CALIPSO. Nair & Rajeev used QuickScat winds and CALIPSO cloud heights. Sun et al. uses "numerical weather prediction wind vector assimilated with observed wind component" obtained from ALADIN.

Figure 1 and its supporting description are all very nicely done. I commend the authors for their clear and informative presentation of this material.

Please provide an overview of the primary sources of uncertainty associated with calculating β_{surf} using equation (15). How is $\Delta\beta_{surf}$ estimated? In practice, is there some maximum $\Delta\beta_{surf} / \beta_{surf}$ above which the retrieval is deemed too unreliable for subsequent wind speed estimation?

Figures 4–7: ordinary least squares problems can be extremely sensitive to large outliers. Did the authors consider applying an outlier rejection scheme (e.g., Tukey fencing) before computing the regression lines shown in these figures?

Lines 470–477: I am totally bewildered by the authors' data screening criteria; i.e., "if a wind speed retrieval is taken in an area with a high cloud fraction [...] the retrieval is deemed a missing value". Please explain what is meant by "cloud fraction" in this context. Is this vertical cloud fraction within an individual HSRL profile? Perhaps naively, I would think that (a) wind speed retrievals would be possible any time the ocean surface was reliably detected and (b) a much better QA metric could be derived from the quality of the surface backscatter signal.

Minor Remarks

The formatting of equation (1) is ambiguous. Decimal notation would be much, much better I think (e.g., 0.003 instead of 3.0E – 3).

Line 105: what value did the authors use for the Fresnel coefficient? Note: Hu et al., 2009 use 0.0209, Josset et al., 2010a use 0.0213, and Venkata and Reagan 2015 use 0.0205.

Line 105: what is the typical off-nadir angle for the HSRL measurements? Should readers assume nadir pointing, so that $\theta = 0$?

Line 110: "Eq. 3.3 is ~~similar~~ identical to the log-linear relationship proposed by Wu (1990)."

Line 111: change "to be" to "being"

Line 192: practically speaking, is there some maximum AOD above which surface wind speeds are not considered reliable? Or are there perhaps some meteorological conditions in which the method is not applicable (e.g., exceptionally dense surface-hugging fogs)?

Figure 3: use different line colors and/or line types to plot the two different sets of HSRL wind speed retrievals.

Lines 299–300: In the figure caption, the authors say, "A few collocated Hu08 and CM54 wind speed data points are on top of each other owing to similar values." They could (and should) eliminate any ambiguity by specifying UTC for these pairs of points.

Lines 319–325: I would have appreciated a bit more detail here. The authors' description does not provide sufficient information to distinguish the bisector method from other 'errors in variables' techniques (e.g., Deming regression and orthogonal distance regression). Is the bisector method especially effective for problems of this sort? Or will any errors in variables method do equally well?

One Reviewer's Opinion

A scatter plot of dropsonde wind speeds (U) versus matching values of σ^2 (derived using equation (3) and β_{surf} computed using equation (15)) would have made this paper enormously more interesting. Both Cox-Munk and Hu et al., 2008 are approximations of the true relationship between wave slope variance and surface wind speeds. The collocated measurements reported in this manuscript offer a superb opportunity to evaluate the relative merits of both models. Perhaps this tantalizing topic can be briefly explored in an appendix included in a revision to the current manuscript.

References

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