

Interactive comment on “Retrieved wind speed from the Orbiting Carbon Observatory-2” by Robert R. Nelson et al.

Anonymous Referee #1

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This paper presents several different methods for retrieving sea surface wind speed from the Orbiting Carbon Observatory-2 (OCO-2), and then assesses the retrieved wind quality using the AMSR2 radiometer wind speed as reference. It shows a single-band Cox-Munk-only model produces the best results as compared to AMSR2. Although there are many restrictions for the wind retrieval with infrared sensors, potential benefits of its high spatial resolution are drawing interest in ocean remote sensing applications. The manuscript is well-written and easy to follow.

My main concerns are as follows:

1. The spatial resolution of OCO-2 winds is not well addressed in the manuscript. Since OCO-2 and AMSR2 are actually resolving different size of surface wind characteristics, it is very relevant to discuss their spatial resolution and the associated inherent wind

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variability.

2. The number of samples in Figs. 1, 2, 4, and 6 are different, making the conclusions of the comparison be less convincing. Do you compare the winds over the same area and the same period? I understand that may be caused by different quality control methods, but the authors had better present a more fair comparison, or describe it clearly in the text.

3. Previous studies (e.g., Wang and Zhang, 2010) show that Sun glint models with wind direction dependence has better performance than those without wind direction dependence in terms of the correlation coefficient between model and satellite measurements. Do you think the wind direction could be also an important factor relating to the latitude-dependent bias in Fig. 7? The isotropic CM model may work well in case the wind variability within the sensor footprint is large, such that the overall slope statistics become isotropic, right? And vice versa?

4. Many details about the methods are missed as the authors assume that the readers can understand all the details involved in the retrieval scheme. I think it's necessary to present more details in Section 3.

5. It is well-know that the sea surface slopes depend on the local wind, fetch, and incoming swell as well. I think it is necessary to pay attention on the segregation of wind sea and swell in the verification, before concluding an algorithm as scientifically credible.

Minor Comments:

6. lines 21-24: the overview of sea surface wind measurements from active radars and passive radiometers is insufficient. Some other scatterometers, altimeters and SARs are not mentioned in the text.

7. Fig. 1 and among others, the acronyms (R, RMSD, Δ, σ) in the legend are not explained in the text or the caption.

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