Review of the manuscript "Coastal Atmosphere & Sea Time Series (CoASTS) 1 and Bio-Optical mapping of Marine optical Properties (BiOMaP): the CoASTS-BiOMaP dataset", by Giuseppe Zibordi and Jean François Berthon, Revision 1

Reviewer's comments and Replies

Comment

There were a high number of comments to be addressed, and I appreciate their consideration. I appreciate the clarification of some doubts also. Below I report a few comments to this version. I appreciate the inclusion of the uncorrected absorption by the ac-9 at 715 nm for the investigation of possible alternative correction methods. This is a contribution to the field that did not require extra work by the authors.

Reply

The inclusion of the uncorrected absorption at 715 nm required the full re-submission of the data set and of the related metadata. When dealing with PANGAEA, this should be considered some relevant effort. Never mind, it was a relevant and necessary action.

Comment

The statement "the values of Lw determined with Eq. 3 exhibit differences well within ±1% with respect to the values computed accounting for the spectral dependence of the water refractive index in the spectral range of interest (Voss and Flora 2017)" is not true. In the Figure 1 of my previous review, based on Hydrolight simulations, I documented differences in $\tau w{-}a$ much higher than that. For instance, at around 412 nm, for moderate wind speeds of 5 m/s or less, there are values found for $\tau w{-}a$ ~0.53 or less instead of the used value 0.544. The relative difference between them is ~2.5 %. This is a big part of the total uncertainty budget. Therefore, that ±1% does not hold. Then, the authors argue "Thus introducing a wind speed dependence on the water-air transmission factor would add such a dependence to Lw. This is not a desirable dependence for data envisaged to support bio-optical modelling." This dependence is unavoidable. If the sea surface is involved, knowledge of the wind speed or any parameter reduction of the wave field is necessary.

Reply

The overall radiometry data included in the dataset are from in-water profilers: they include the subsurface upwelling radiance Lu and the normalized water leaving radiance LWN.

The transfer of subsurface Lu values from below to above water to produce Lw values (input for the determination of LWN) must be done without accounting for any dependence on the wind speed to avoid introducing perturbing effects in LWN. In fact, LWN must be independent of any observation geometry (i.e., viewing angle, sun zenith angle, and relative azimuth angles) as well as from environmental conditions (atmospheric and water inherent optical properties, and also surface perturbations). It is thus firmly restated that the transmission factor of the water-air interface applied for the determination of LWN, must be computed without accounting for the wind speed dependence. Because of this the uncertainties provided in the manuscript due to the neglected spectral dependence of the water refractive index are appropriate!

Action

To avoid any further comment on uncertainties, it is now specified that the water-air transmission factor is determined for a flat sea surface.

Comment

The reply on the uncertainties affecting the IOPs is fine but it is not properly transferred to the revised manuscript. I expect to have actual estimates of the real uncertainties for each water type, not sentences like "but it is expected to be much larger". I somehow regret that all the work that was identified as to be done, such as the mention to the uncertainties in bb uncertainties back to 2008, has not actually been done. In fact, the authors added the new sentence to the manuscript (lines 362-363 of the author's tracked changes manuscript) "In the absence of any advanced and consolidated processing for HydroScat-6 measurements". This is highly disturbing, knowing that the Hydroscat-6 has been available for purchase since the late 90s. Is this really the current state of things? In fact, one here can see the huge gap in processing protocols and uncertainty assessment between the radiometry and the IOPs since both types of measurements started.

Reply

*On this comment we need to recall wat already stated by the other Reviewer on the original manuscript: '*As JP states, the 0.4 factor for the Hydroscat correction is problematic, **but any value is guessing really**. There is also no separation of a constant water background in Eq 4, which has always been inherently problematic. **The only thing we can really do however is acknowledge what the realistic errors for this sensor are**.'

If the 0.4 is a guess and any other value is a guess, the comment from the JP (the current Reviewer) is speculative. We can only apply what recommended. In addition, it cannot be expected the Authors can do the work on uncertainties that nobody else was able to produce in the former years. Or alternatively, when it was done, it was referred to very specific measurement conditions, which could not be generalized. Because of this, in the revised manuscript we simply state that the uncertainties are much larger than those specifically provided in a cited work.

Action

Still, in view of attempting to satisfy the Reviewer's comment the following text has added: "The accuracy of the applied equation 4 was questioned by Doxaran et al. (2016). However, their newly derived relationship was determined from b_b (550) values comprised in the 0-2.5 m^{-1} range while the CoASTS and BiOMaP b_b(555) values are lower than 0.1 m^{-1} range with a_{t-w}(555) not exceeding 1.0 m^{-1}. In this interval, the equation proposed by Doxaran et al. (2016) does not appear to closely fit the plotted data (see their Fig. 5b). Because of this, still acknowledging their work, the processing equations originally proposed by Maffione and Dana (1997) were applied for the ydroScat-6 data."

Doxaran D., E. Leymarie, B. Nechad, A. Dogliotti, K. Ruddick, P. Gernez, and E. Knaeps, "Improved correction methods for field measurements of particulate light backscattering in turbid waters," Opt. Express 24, 3615-3637 (2016).

Comment

Line 470 on the author's tracked changes manuscript: "Assuming CDOM does not absorb in the red". I would like to see comments on the limitations of this assumption in highly CDOM waters such as the Baltic Sea, especially the northern sector.

Reply

We certainly appreciate the curiosity of the Reviewer. However, we are providing details on how the measurements were performed and reduced. The ays spectral values were obtained applying a bias correction determined from the average of the ays values in the 670-680 nm spectral region closely following community recommendations (see Section III in IOCCG 2019).

Action

Mention and reference to IOCCG (2019) has been added into the manuscript.

IOCCG (2019). Measurement protocol of absorption by chromophoric dissolved organic matter (CDOM) and other dissolved materials (DRAFT), In Inherent Optical Property Measurements and Protocols: Absorption Coefficient, Mannino, A. and Novak, M. G. (eds.), IOCCG Ocean Optics and Biogeochemistry Protocols for Satellite Ocean Colour Sensor Validation, Volume 5.0, IOCCG, Dartmouth, NS, Canada.

Comment

Line 727 on the author's tracked changes manuscript: there is a "6171 nm" that I believe is a typo.

Reply

Corrected. Thanks