

**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**

**General comments**

I have read the revised version of the manuscript. 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.

The statement “*the values of  $L_w$  determined with Eq. 3 exhibit differences well within  $\pm 1\%$  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  $\tau_{w-a} \sim 0.53$  or less instead of the used value 0.544. The relative difference between them is  $\sim 2.5\%$ . This is a big part of the total uncertainty budget. Therefore, that  $\pm 1\%$  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  $L_w$ . 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.

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.

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.

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