Spectral correction factors for the removal of glint perturbations in above-water radiometry

Replies to the Comments from an Anonymous Reviewer

This paper presents updated estimates of the ρ look-up-table (LUT) for the glint removal from above-water measurements using Mobley's approach, yet for an azimuth Δφ=90°, which is not what recommended by Mobley, but the operational choice in the AERONET-OC network.

There are a number of facts from this paper that I find positive, and a number that I find negative. The editor may decide whether these latter ones compromise publication or not.

Comment 1 On the positive side I acknowledge the quality of the work. This is definitely the state of the art of radiative transfer, and both the finite element code and Monte Carlo simulator are unique tools that can provide the deepest insight on optics, maintaining the highest accuracy. The care on the atmospheric modelling is excellent, and hence, this whole scientific approach has high potential.

> ***Reply 1*** *The acknowledgment is appreciated. For what concerns the relative azimuth Δϕ=90°, this is what is recommended by the IOCCG protocols to minimize the impact of superstructure perturbations in above water radiometry: an issue often overlooked by field scientists.*

Comment 2 The motivation for a new ρ LUT beyond those that already exist is not clearly stated. I understand that the authors improved their modelling of the physical setup, but they also must explain what are the current physical or numerical inconsistencies that motivate the creation of yet another LUT. But even if the authors explained it, such a need is not very clear to me. There appears to be a proliferation of ρ's in the recent years that is only adding entropy. I anticipate that, at the end, all but the small group of expert users will get confused and simply apply Mobley's coefficients.

> ***Reply 2*** *As stated in the introduction (lines 120-125) the ρ-factors accounting for the sole diffuse component of non-polarized sky-radiance were neither available nor discussed in the literature. Aside any general investigation, the factors described in the work are those that best support AERONET-OC processing (see Zibordi et al., JTECH, 2025). Because of this, they are applied in the AERONET-OC Version 4 processing, which will be released soon. This strongly recommends that the relevant rho-factors be published and made accessible.*
> *The above elements will be further detailed in the introduction.*

Comment 3 I downloaded the data. I cannot understand why the authors are releasing ρ values for Δφ=90° only. I understand this is the azimuth of AERONET-OC and that the authors may not care much about other setups, but unfortunately that azimuth is only a value across a continuum of possible values. The authors are shooting themselves on the foot if they self-impose that limitation, given that the calculations were made for all, as it can be seen in the plots. Most users, especially those operating on smaller platforms, will choose Δφ=135°, whereas automated sensors on ships face a different range of constraints, and have variable Δφ. This definitely diminishes the impact of the paper.

> ***Reply 3*** *The ρ-factors were definitively computed for the sole Δϕ=90°measurement configuration. Consistently, the plots always show ρ-factors for Δϕ=90°, which is the*

measurement configuration adopted in AERONET-OC above-water acquisitions and supported by the IOCCG community protocols. So, still appreciating the point raised by the Reviewer and far from any self-harm, the manuscript is intended to solely support the presentation of factors computed for measurements collected with $\Delta\phi$=90°.
The above elements will be further stressed in the manuscript.

Comment 4 The other big issue has to do with a basic requirement in science, which is reproducibility. The calculations come from proprietary software that cannot even be purchased. Hence, a reviewer or an independent scientist cannot reproduce the results. Yes, textual explanations on the simulation setup are provided, but that is largely insufficient. To me that would be a sufficient case for rejection, but it is the editor who has to make the decision after revising the journal policies.

Reply 4 *The unavailability of the FEM and MOX source code does not, by itself, prevent independent reproduction of the results. The manuscript specifies the full physical and numerical setup needed to reproduce the simulations with an equivalent radiative transfer implementation: the atmospheric model (AFGL gas profiles, OPAC aerosol optical properties, Sect. 2.1.1), the water-surface model (Cox and Munk (1954) slope distribution, Eq. 4), the sky-dome discretization scheme (Eq. 3), and the complete parameter space (Table 1). Both FEM and MOX are, in addition, separately published and peer-reviewed methods (Bulgarelli et al., 1999; Bulgarelli and Doyle, 2004; D'Alimonte et al., 2010). We therefore consider the results scientifically reproducible— i.e., independently obtainable by applying the documented physics and parameters with an equivalent radiative transfer code — even though the specific software used here is not distributed.*
Aside from this, there were already two peer-reviewed publications presenting comparisons between equivalent factors obtained with the proposed codes and alternative ones (see D'Alimonte et al., Optics Express, 2021, & Zibordi et al. JTECH, 2025). This will be clearly stated in the manuscript.

Comment 5 On a smaller level of importance is coining the name "glint correction factor" for ρ. Now we don't only have yet another set of ρ's. We even have a new name. As a whole community, are we not even able to agree on a common nomenclature after 30 years that we are using these terms?

Reply 5. *As indirectly recognized by the Reviewer, any definition proposed for the ρ values is matter of discussion and of expected improvement. An attempt was also made in this work to propose a better terminology for the ρ-factors. More text will be added to explain the reason for the proposed name, still sure that any knowledgeable reader will not be confused by the terminology.*

Comment 6 The comments on other authors' results and on their own results are scientifically sound but excessively focused on the numerical point of view. I would prefer a more didactic tone, with more insight into physics, so that young researchers could use this document to learn about above-water radiometry. As an example, in lines 95-98, it is said that ρ tends to increase spectrally with increasing wind speed, driven by the spectral distribution of skylight. But why is that? Because the model is trying to explain reflected light with a significant contribution of sunlight using a reference measurement that is largely devoid of sunlight.

__Reply 6__ A more didactic, tutorial-style discussion of the physical mechanisms behind published results falls outside the scope of ESSD (see ESSD's Aims and Scope and Manuscript Types). We further note that: i. lines 95–98 summarize a finding reported by Zhang et al. (2017) within a literature-review paragraph (lines 94–103); and ii. a step-by-step introduction of the physical equation at the base of above-water radiometry (Eq. 1 and 2) is carefully provided.

Comment 7 The authors claim on lines 84-85 that Mobley generated his ρ LUT using the Cox and Munk model without the 0.003 background term. I have checked the Hydrolight documentation, in particular the "Technical note 1" of Hydrolight 5, and the 0.003 term indeed appears. Authors shall clarify this.

__Reply 7__ We note that the Hydrolight Technical Note 1 ("How well does Hydrolight simulate wind-blown sea surfaces?" – September 2002) cites the Cox-Munk mean-square-slope formula $mss = 10^{-3}(3 + 5.12 \cdot U_{10}) \pm 0.004$ (its Eq. 2, attributed to Elfouhaily et al., 1997, Eq. 26) when addressing the comparison between the Cox-Munk and the ECKV formulation. However the same Technical Note does not discuss whether this constant is included or excluded in Hydrolight's own surface-generation routines.

The non-use of the the 0.003 background term is evident in the HYDROLIGHT code (see routine H42\data\surfaces\initial.f, lines 116 and 226).

This latter specific implementation of the Cox and Munk statistical model is consistent with what detailed in: Preisendorfer, Hydrology Optics, Vol. VI, pag. 145, 1976; Preisendorfer and Mobley, Journal of Physical Oceanography, vol 16, pag. 1298, 1986; Mobley, Light and Water, pag. 168, 1994.

Comment 8 The labels of Figures 5, 6, 7, 8 are hard to read due to the font size, and Figures 9, 10, 11, 12, 13, 14, 15, 16, 17 and 18 are totally unreadable. In addition, there appear to be rendering issues, so the resolution is poor.

__Reply 8__ The figures will be updated using a more appropriate character size

Comment 9 In some cases, where multiple panels are displayed on the same figure, the axis scale is kept fixed, at the cost of being unable to grasp the numbers that are actually displayed. Example: Figure 9.

__Reply 9__ The scale of multiple plots will be changed to improve readability.

Comment 10 The abstract must synthetise the paper as a whole. So far it lacks a summary on the numerical results and a comparison with former ρ estimates.

__Reply 10__ The abstract has been prepared following the Journal guidelines (i.e., " the abstract should be intelligible to the general reader without reference to the text. After a brief introduction to the topic, the summary recapitulates the key points of the article and mentions possible directions for prospective research. ..".)

Comment 11 I am missing information on the simulation of the optical proper of the water column. I understand that this aspect is of minor importance, given that the subject under examination is the reflection of the surface, but one still needs to make a simulation using specific water conditions. The authors are encouraged to disclose this.

> ***Reply 11*** *The simulations aim at reproducing the radiance L_R reflected by the water surface, as such the propagating system consists of an atmosphere bounded by a rough water surface. The water body can be thus assumed as completely absorbing. This will be specified in the text.*

Comment 12 The introduction states that the water leaving radiance is the primary product of aquatic remote sensing. This statement is not correct. The primary product is reflectance, as it is the quantity provided by satellite products, and the source of information that is extracted by remote sensing algorithms.

> ***Reply 12*** *We note that satellite distribution practice is not fully consistent in this regard: NASA's OC Level-2/3 products and Copernicus/EUMETSAT's Sentinel-3 OLCI Level-2 products are now both distributed as remote sensing reflectance (R_rs), while NOAA's operational VIIRS processing (MSL12) distributes normalized water-leaving radiance (nLw) as its standard product. We also note that the comment's use of "reflectance" is itself ambiguous, as it could refer to either remote sensing reflectance (R_rs) or water-leaving reflectance ($\rho_w = \pi \cdot R_{rs}$), which are formally distinct quantities. More fundamentally, we use "primary product" to mean the fundamental radiometric quantity from which the other reflectance-type products can be derived.*

Comment 13 I cannot understand the distinction between L_i and L_sky in equations 1 and 2. I believe they are the same. In fact, if one inspects analog equations by other authors such as Mobley and Harmel, one finds that they use the same variable for both. Hence, authors must unify the nomenclature. Indeed, equation 3 confirms that L_i is nothing more than L_sky for a fixed geometry. That does not justify a new variable name.

> ***Reply 13*** *L_i is the radiance measured by the sensor in sky-viewing mode (i.e., when pointing towards the sky: ϑ=40, $\Delta\phi$=90). L_sky is the simulated sky radiance. As such, L_sky(ϑ=40, $\Delta\phi$=90) is the simulated value of the sensor-measured L_i radiance. This conceptual difference will be better stated in the revised manuscript.*

Comment 14 Related to the previous comment, a table of mathematical symbols would highly help the reading.

> ***Reply 14.*** *A table of symbols will be added.*

Comment 15 Authors may revise the prose with the aid of a native speaker or a LLM. For instance, I find here several examples of what I call "JRC English", namely expressions such as "It is highlighted that…", "Still recognizing that…", "Acknowledging that…". Also, the division of the text in blocks might be improved. In particular, I find the Summary and Conclusions section rather disorganized.

> ***Reply 15*** *An attempt will be made to revise some statements, as well as the Summary and Conclusions.*