RC2: 'Comment on egusphere-2025-4973', Anonymous Referee #2, 28 Nov 2025

Author's response:

We thank the reviewer for the careful review of our paper and for the remarks and suggestions that will help us improve its clarity. Please find below our replies to specific comments and updates we brought to the text in blue.

This work presents development and implementation of a radiative transfer model providing high spectral resolution (33 bands) and separate diffuse and direct downwelling irradiance for the underwater light-field. Furthermore, the model used here also simulates the upwelling stream and enables one to estimate the sea surface reflectance. Such development follows the ongoing improvements in ocean optics in the current generation of marine biogeochemistry models, as evidenced by the papers cited by the authors. An extra interesting feature that is presented by the authors is a stochastic perturbation scheme for the IOPs, enabling the authors to analyze the often large uncertainty in this domain. Finally the model was tested and validated with NEMO-BAHMBI in the Black Sea environment.

I think the paper is a nice contribution to the growing literature, but I have a series of comments, mostly minor, that I would like the authors to address, before I can recommend the paper for publication.

Specific comments:

1. In the Introduction section, the authors list references evidencing the 3D configurations using RT models (around line 35). I would add few other relevant references to the ones cited by the authors:
- Gregg, W. W., & Rousseaux, C. S. (2016). Directional and spectral irradiance in ocean models: Effects on simulated global phytoplankton, nutrients, and primary production. Frontiers in Marine Science, 3, 240
- Mobley, C., Sundman, L., Bissett, W., & Cahill, B. (2009). Fast and accurate irradiance calculations for ecosystem models. Biogeosciences Discussions, 6(6), 10,625–10,662.
- Gregg, W. W., & Casey, N. W. (2007). Modeling coccolithophores in the global oceans. Deep Sea Research Part II: Topical Studies in Oceanography, 54(5-7), 447–477.
- also the Skakala et al. 2020, 2022 references cited in the paper can be also mentioned in this context.

The suggested references were added to the text.

2. Line 65 "RT module ready to be coupled with NEMO" is a little bit confusing, is the module stand-alone? In such case how hard is it to couple it to a range of physics models and how necessarily it has to be NEMO? I would either rephrase, or explain better how much the module is tied specifically to NEMO.

Rephrased to "integrated within the NEMO framework". The code that is shared with this paper is meant to be used in NEMO configurations. While the choice of physical model is constrained, there is no limitation on the choice of biogeochemical model.

3. Line 71 "This model was used in ...." – it is a bit confusing which model you say was used in those references, as the two-band model gets easily mixed up with the PISCES model, and

even more confusingly, the paragraph seems to be specifically talking about the NEMO physics model. In this regard note that e.g. Ciavatta et al. 2014 used POLCOMS-ERSEM model, so neither NEMO for physics, nor PISCES for biogeochemistry was used.

This section has been rephrased to clarify the differences between configurations. The reference to Ciavatta et al. (2014) has been moved further down the text.

4. A minor thing: in Eq (7), if you are defining R_below, I would mark z=0 on the right-hand side of the equation as z=0_{-}, or z -> 0_{-}

Indices were added to the equation.

5. Eq (8-9), I know this is meant to be a general section and then for specific implementation the Q, T, gamma parameter values are listed in Tab.1, but can you link the Table.1 to the text already here, e.g. saying that perhaps representative values of these Q, T, gamma parameters can be found in the Table 1? By doing that, the reader gets a basic quantitative understanding for what are at least the right orders of magnitude for those parameter values…

A sentence has been added to refer to Table 1.

6. Eq. (10) I expect that “T” is temperature, but this needs to be stated. Also please consider that although from the context this might be hopefully clear enough, “T” is the same symbol than used for transmittance in Eq (9), which is never a very good practice…

The symbol for transmittance has been changed for τ.

7. Line 200: “POC” often refers to all organic carbon above certain particle size, I suspect you mean here the non-living part of POC, i.e. detritus?

The sentence was changed to make clear that we indeed refer to non-living organic matter: “...are derived from the concentration of detritus, or non-living particulate organic carbon (POC)...”

8. It is interesting that your attenuation model seems to omit the SPM/sediment, can you comment on this? E.g. in Black Sea environment you don’t need to include it near the river mouths/coastlines?

We ackowledge our first objective with the implementation of this model is the simulation of optics in the central deep area of the Black Sea. The inclusion of optical contributions from constituents such as CDOM and SPM close to river mouths is still lacking and it shows directly in the poorer perforances of the model in coastal areas. CDOM and SPM simulation are among the future axes of improvement for the BAMHBI model. The method for simulation of SPM in BAMHBI, in particular in coastal areas such as the northwestern shelf, is currently being investigated and this component could be included in future developments based on this work. A comment in the discussion has been added to add the inclusion of SPM optics as a possible future improvement of this model.

9. In Fig.1 the origin of many of the plotted values seems to be undeclared, can you please reference them a bit better? Detritus seems to have some cited references (text around the line 200), but even in this case (and in other cases) it would be good to cite the real origin of these values, e.g. lab experiments, or field studies? Also what are the uncertainties of those values, e.g. I expect the CDOM value to be highly uncertain, can you comment on that?

For water, "We use the absorption and scattering spectra from the lab experiments described in Pope and Fry (1997)", a sentence was also added to reflect the fact that the optical properties of water are considered accurate. For phytoplankton, "Pythoplankton absorption and scattering spectra are taken from the outputs of the literature review presented in Álvarez et al. (2022), that includes field observations in the Mediterranean Sea. We may therefore expect uncertainties arising from the difference in the definition of PFTs across basins and models". For non-algal particles, the following sentence was added: "This model aggregates data from various field campaigns to provide a model of absorption and scattering by non-algal particles.". For CDOM, we added the following sentence: "A literature review on Scdom can be found in Terzic et al. (2021)". We also updated the text in the section describing the Black Sea test case as the uncertainty mostly arises here from the use of a forcing derived from BGC-Argo data (see answer to comment n°11).

10. Sec 2.4 – I know you give references including Mace et al (2025), but introducing perturbation scheme is always non-trivial, perhaps you can include a short paragraph justifying why the specific scheme was chosen (e.g. "first-order autoregressive process"), i.e. what thinking this is based on?

A sentence was added: "This approach has been used in Garnier et al. (2016) and Popov et al. (2024), showing its ability to generate dispersions that are consistent with uncertainties in biogeochemical variables through the perturbation of biogeochemical parameters.". The methods for implementing first order autoregressive processes in a NEMO framework have already been developed and tested in the context of perturbation of biogeochemical parameters. We decided to use the same methods to take advantage of the experience acquired by coauthors during those previous works.

11. Sec. 3.1 - CDOM – it has been derived from Bgc-Argo data as described on the lines 265-270. However, this description I find insufficient – is CDOM taken as horizontally spatially varying? If yes, with what effective resolution? It seems that there are some seasonal variations imposed into the CDOM values? With what temporal resolution? I also assume that we are talking about the CDOM forcing based on a seasonal climatology rather than flow-dependent values? Best, can you give a plot showing surface CDOM annual mean concentrations for the forcing and/or Hovmoller plot of horizontally averaged CDOM values (depth x time)? If you have too many Figures, can you put this in the Appendix, or Supporting Information?

The absorption by CDOM is taken from a lookup table that provides an absorption coefficient based on seawater density and the day of the year. As such, it is something in between climatology and a flow-dependent value. We have added a Hövmoller plot of this forcing (time vs density) in the paper. CDOM absorption is then computed for each model cell and has the same resolution in the model as the other absorption coefficients (15km horizontal, 59 progressive vertical levels). The description of the CDOM forcing has been extended both in the main text (section 3.3) and in appendix A.

12. Sec 3.2 – lines 280-295 dedicated to the atmospheric forcing of the light module. I would like to see more information on what the atmospheric MAR model exactly simulates when providing the desired outputs, e.g. does it simulate aerosol dynamics and therefore aerosol optical depth? I assume it provides separate outputs for the surface downwelling diffuse and direct irradiance? Also did you validate this model in the Black Sea against observations? Can there be a sentence, or two on how it validates?

The current MAR model uses the ecRad radiative transfer scheme (Hogan et al., 2018), which can be configured to use the CAMS aerosol specification (Bozzo et al., 2017) and ecCKD-based gas-optics models (Hogan et al., 2022). It is capable of finely simulating aerosol optical depth as a result. By default, MAR v3.14 is configured to run ecRad with both the CAMS specification and the ecCKD models, and it prepares aerosol mixing ratios for ecRad using climatological data, allowing it notably to produce fine spectral data in the UV area (Grailet et al., 2025). The ecRad radiation scheme is also capable of separating diffuse from direct irradiance, both in the broadband and the spectral data. It's important to note, however, that MAR is not currently able to simulate daily (or finer) variations of aerosol mixing ratios. This notably prevents it from catching peaks of UV radiation, which could be used in turn to predict peak UV indices. A few notes are added in the paper regarding the aerosol configuration of ecRad in MAR, with references.

We did compare the MAR v3.14 radiative fluxes against ERA5 reanalyses, to ensure average yearly biases remained close to reference data products, and compared its PAR spectral fluxes against AERONET PAR ground observations in Kichinev (Moldova), MAR achieving high correlation (> 0.9) and low bias in the latter case. Both results will be provided in another upcoming paper assessing MAR over the Black Sea region and presenting future projections for the same area. To avoid adding too many details about MAR evaluation, which is not the subject of the present paper, and since Kichinev is located quite far from the Black Sea coasts, we will not provide the Kichinev PAR evaluation in the present paper. It's also worth reminding that a thorough evaluation of MAR spectral fluxes over a European location is already provided by Grailet et al. (2025).

13. Paragraph around the line 305 – I wouldn't say that a forcing bias would not impact reflectance, i.e. reflectance is an AOP and depends on the overall direction of light, so if you had a bias towards e.g. diffuse light, it would influence the reflectance properties..

This was rephrased to incorporate the comment.

14. Sec 4.1 – the paragraph 360-365 makes it sound like PAR is directly compared with the Bgc-Argo, instead what is compared is PAR normalized by its surface value, right? Given the large short-time-scale (sub-daily) PAR variability I wouldn't expect it would be reasonable to compare PAR directly with observations, at least not if standard model (e.g. daily) outputs are used…

We added "normalised" in from of irradiance and E_0 terms to make it clear that we are comparing normalised variables. The sentence: "We use hourly model outputs in order to consistently match in situ data despite the sub-daily variability of irradiance" has also been added to highlight the use of hourly model outputs, allowing us to be consistent in the comparison of irradiances.

15. Does Fig.3, the left-hand panel (lambda=380nm) imply that the Argo sees overall higher levels of attenuation than the model? Can this be due to the missing SPM in the model? Can you comment? Curiously why do the data for the shorter wavelengths (the first two panels of Fig.3) near the surface (I guess near the "0" value) start slightly below the black line? It looks like there is some rapid attenuation near the surface in the Bgc Argo data that isn't present in the model? Can you comment on this?

It is actually the opposite: normalised irradiances are higher in the BGC-Argo profiles than in the simulations. Shorter wavelengths are attenuated faster in the model than what is observed with BGC-Argo floats. Although the SPM contribution is indeed missing from the model, we have too much absorption here. This could mean that some contributions that are important near the surface are overestimated in the model. Since CDOM dominates absorption in short wavebands, it is likely that the forcing we impose does not exactly fit the reality we are trying to reproduce. We added this discussion in the Discussion section of the paper.

16. Small comment to Fig.3-4, can you label the x and y axis differently, i.e. assuming the logarithm has base 10, I would label it as the ratio rather than the log-ratio and use 10^0, 10^-1, 10^-2 (…) on the axes… This means instead of using linear scale for the log, I would use log-scale for the ratios - this is generally easier to visually interpret..

Figures were remade with the axis updated.

17. Fig.5: how coarse is the model vertical resolution near the surface? I understand the model has 59 vertical layers unevenly distributed (I assume the model is using z coordinates with fixed depths?), but from the few histogram bins in this Figure it looks that near the surface the water column is vertically not very well resolved. Can you comment on that? Or is the coarse histogram resolution (the x-axis) due to Argo's not measuring across sufficient number of depths? Furthermore, why there are three colors in the Figure when only two runs are compared? You say "blue" and "orange", but I see also a brown color? Also, can you find a way how to make the colors transparent, or show only contours of the histogram bars, so the bars don't get in each other way? Btw. why are only distributions compared? What about pointwise comparison using e,g. RMSE metric?

Close to the surface, model layers are approximately 0.5 metres. The histogram was remade to match the vertical resolution of the model. The representation of distributions is therefore more consistent now with the model configuration. Bins are centered on model levels. In the representation, the third colour (brown) was actually the overlap between the blue and orange colours. Only contours are now shown to avoid any confusion when histograms overlap. RMSE and bias metrics results are shown in Table 2 in addition to the histograms in Figures 6 and 7.

18. Sec 4.2, the text between the lines 380-385: out of curiosity, sorry for my lack of knowledge, but how much can you vary the eta_h parameter – it looks to me like this parameter should be something reasonably constrained?

Ideally, this parameter should be close to 1. In reality, virtually all radiation is converted to heat (*Light and Water: Radiative Transfer in Natural Waters*, C. Mobley). This parameter is introduced here so that the temperature does not become biased in the

model, which is something we initially found in the Black Sea implementation. It is more of a numerical parameter than an actual physical parameter, and its existence remains a important question in the model. Why do irradiances match observations, but we observe a bias in temperature if we use them in the computation of physics? The text has been modified throughout the paper to highlight this idea that the parameter should be interpreted as a tuning parameter for the model.

19. When it comes to temperature, why only thermocline depth was compared? Btw. my non-expert understanding of Black Sea is that stratification is dominantly due to salinity and not so much due to temperature? Is thermocline directly associated with pycnocline, or is pycnocline more closely aligned with halocline in the Black Sea? If the latter, it could mean that thermocline is not so important in the Black Sea, or am I getting something wrong?

This observation is correct, the Black Sea is very stratified and its stratification is controlled by salinity rather than by temperature. For the comparison of the model, we have removed thermocline depth and replaced it by a comparison of SST, thus keeping our analysis on surface variables. In both cases, the differences between the use of simple optics and the RT model are small, but we notice a slight degradation of surface temperature using the temperature feedback of the model. Some discussion regarding the benefits of the model for chlorophyll vs this degradation in temperature has been added in the Discussion section of the paper. We have then chosen not to include a comparison of halocline depth to keep the focus of the paper on surface variables. Nonetheless, the differences are very small and comparable to that noticed for thermocline depth. The model therfore does not influence stratification in any significant way. Although it is not demonstrated in the paper, we added the following sentence: "The simulation of the cold intermediate layer cold content (CCC) and the mixed layer depth remain consistent over the four years of simulation, as well as thermocline and halocline depths.".

20. Again, as in Fig.3, why you don't show more detailed performance statistics for chlorophyll than the histogram comparison (Fig.6)? Furthermore, similarly to Fig.5, why there are 3 colors (?) and can you make the bars transparent?

RMSE, bias and correlation for surface chlorophyll can be found in Table 2. We decide to focus here on surface values given that the main focus on the paper is the simulation of sea surface reflectances. Bars on histograms have been made transparent to only show contours, thus removing the third colour that was actually the overlap between blue and orange.

21. Around the line 420, if you do explain the reflectances by a localized bloom underestimated by the model, please clearly state that you haven't shown this in the paper. Or does the bloom refer to the rChl comparison as per Fig. 10-12? Is it relative to Bgc-Argo? How did you come to the conclusion about the bloom and the reflectance?

The sentence was modified to highlight the fact that the location of high reflectances in the satellite data is not shown: "localised high reflectances on the northeastern coast of the basin that appear in the satellite data and are underestimated in the model (not shown in the figures)". On the other hand, the summer coccolithophore bloom is documented in the literature and the reference to Kubryakov et al. (2021) is given.

22. I find very interesting the RMSE difference between rChl and the BAHMBI Chl in Fig.12! I think it would be somehow interesting to show BAHMBI Chl also in Fig.10-11? Can you discuss better this discrepancy between rChl and BAHMBI Chl? Why exactly does the mismatch happen, is it discrepancy in the specific absorption, or scattering coefficients between the satellite and the ecosystem model? One conclusion one could make based on this discrepancy is the high uncertainty in chl (and perhaps also Rrs?) comparison when validating the model with the satellite!

We added BAMHBI chlorophyll to the figures 11 and 12 (that used to be 10 and 11). Overall, we notice that BAMHBI chlorophyll "overshoots" more than rCHL. On areas such as the northwestern shelf during spring blooms, with high chlorophyll concentrations, BAMHBI chlorophyll can reach very high values compared to satellite data or rCHL. Conversely, in the central parts of the basin when the biological activity is low (especially in autumn), BAMHBI chlorophyll is near 0 and therefore underestimated, while rCHL is closer to remote-sensing data. Nonetheless, BAMHBI chlorophyll seems to perform better or similary against remote-sensing data in summer. Discrepencies arise from the inclusion of non-algal parrticles and CDOM optical properties in the computation of rCHL, that is not completely independant from BAMHBI chlorophyll. One interpretation could be that despite the fact that OC algorithms aim at deriving surface chlorophyll, the resulting quantity does not strictly match chlorophyll and is better compared with analog optical variables (that accounts for more than just chlroophyll). We have added more comments in the discussion to highlight this discrepancy.

23. In Section 4.6 how were the perturbations standard deviations selected? I hear that e.g. CDOM can have very high uncertainty in its specific absorption coefficient, is 50% standard deviation enough? Why?

A better description of the standard deviations was inserted in the text: "The perturbations are defined with a standard deviation of 50% for absorption and scattering by phytoplankton and non-algal particles, as in Garnier et al. (2016) for the perturbation of biogeochemical parameters. We use a standard deviation of 50% for the CDOM reference absorption profile a_{ref} based on the collection of CDOM profiles gathered from BGC-Argo floats described in Appendix A. The standard deviation for the slope Scdom is taken at 30% according to the range of values presented in Terzic et al. (2021)". Here, uncertainty on CDOM is higher due to the contributions of both the reference absorption profile and the spectral slope. For the Black Sea use case that we are presenting, the standard deviations used are enough to reproduce distributions of irradiance profiles comparable to that found in BGC-Argo data. We can suspect that standard deviations should be adapted to the use case.

24. Discussion section – I'd say it reads a lot like a Summary section, rather than Discussion. Maybe you want to change the title to reflect upon that?

The discussion has been largely updated in the revised version of the paper. We have therefore decided to keep the title.

25. Discussion on the lines 550-560 – I'd say that comparing Rrs has the advantage of capturing all the optically active tracers across its spectra. However, if there is large uncertainty in the specific absorption, or scattering coefficients (i.e. mismatch between the satellite and the

ecosystem model), the comparison with satellite can be a bit arbitrary, and I believe this is regardless of whether it is done through using inversion, or via model-derived Rrs, no?

The discussion is fleshed out in the revised version. In particular, the section related to the advantages of simulating Rrs now integrates the above comments. Indeed, high uncertainties in the modelling framework, whether in the optics or in the biogeochemistry, can reduce the relevance of comparing satellite data with model outputs. The simulation of reflectances nonetheless provides a greater amount of information that can be used. This can be seen in the Figures 8 to 11, where rCHL can exhibit a weaker match with satellite data than the reflectances at 490, 555 or 670 nm, or conversely. It shows that a weak agreement with the inversion algorithm does not mean that the comparison of reflectances will show a weak agreement too.

26. The mention of Rrs assimilation on the lines 565-570 is an interesting point. One thing that comes to my mind is that due to the relatively complex relationship between Rrs and the biogeochemistry model state variables (including inversion), the Rrs DA might be easier to do for DA approaches that attempt to directly represent cross-covariances between variables in the background covariance matrix (like EnKF and similar). For variational (3DVar) methods that often simplify the background covariance matrix (e.g. as part of parametrizing it), Rrs assimilation might pose a bigger challenge – do you have any comments on that?

The reviewer's comment about the choice of DA approach is correct. Approaches such as EnKF have become dominant in OC assimilation studies, such as in Jones et al. (2016) where they used a deterministic EnKF. With Rrs, we believe that such approaches are particularly relevant. In the case of the Black Sea, an ETKF approach has already been implemented with chlorophyll data and applied in the CMEMS Black Sea operational forecasting system (https://doi.org/10.25423/CMCC/BLKSEA_REANALYSIS_BIO_007_005_BAMHBI). A comment has been added to the text to reflect this idea: "Given the complex relationship between reflectances and other biogeochemical variables, approaches such as Ensemble Kalman Filter (EnKF) seem appropriate as they aim at directly representing cross-covariances ".