RC1: ['Comment on egusphere-2025-4973'](), Anonymous Referee #1, 25 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.

The paper by Macé and co-authors describes an effort to improve the modelling of light propagation within the NEMO ocean modelling framework (NEMO stands for "Nucleus for European Modelling of the Ocean").

Improving modelling of light propagation without introducing excessively computationally expensive parameterisations is a long-standing question in the community of developers of global circulation models coupled with some biogeochemical (BGC) components. This is an important aspect that drives the profile of heat deposition in the top layers of the ocean and also determines how much energy is available for photosynthesis, as input to the BGC component.

The authors acknowledge that this modelling is often oversimplified. The work they detail in this manuscript intends to improve this.

The authors should be commended for such an attempt.

However, I did not see much novelty here. The 3-stream model that they incorporate, based on Aas 1987 in particular, was already selected and used by Dutkiewicz et al. (2015) (as indeed cited by the authors). I could not find a clear explanation of what is new here. But maybe this is not what this journal expects, so presenting the implementation of an existing method and how it modifies the outputs of the model could be enough. I leave this to the appreciation of the editor.

Also, there is a sense that this model is made of odds and ends, if I may use this expression, and the overall coherence of the various parameterisations is hard to fathom.

With this paper, our goal is to introduce an implementation of the 3-stream model in a NEMO framework, in a model that is used in operational mode within the Monitoring and Forecasting Centre of Copernicus for the Black Sea. Our NEMO implementation is publicly available and ready to be used by other users, on other domains and in conjunction with other biogeochemical models. To our knowledge, code of the implementation of a three-stream radiative transfer model into NEMO had not been made available before.

Besides this, I do not think that testing this new RT model implementation in the Black Sea is the most relevant choice. Most of that sea is made of "optically complex" waters, making the modelling of optical properties quite difficult (e.g., comments about coccolithophorids lines 420-425), and also making the uncertainties of the satellite products (used for validation) higher than in an open ocean setting. Definitely not the best framework. The Western Mediterranean Sea could have been better, as any other open ocean area (I understand that the choice of the Black Sea is essentially made for practical reasons because an implementation of the model already exists for this area). There is for instance an open ocean field site in the Western Mediterranean (BOUSSOLE site) where most of the optical properties and radiometric quantities are measured and could help validating a model run in that area.

Indeed, the Black Sea is an optically complex basin that does not create the easiest framework to work with when it comes to coupling optics to physics and biogeochemistry. Several reasons justify this work. First, large discrepancies have been consistently noticed in the Black Sea between remote-sensing chlorophyll products and in situ observations. For instance, the large coccolithophore blooms described in Kubryakov et al. (2021) are in most cases absent from the remote-sensing chlorophyll products. The OC algorithms used seem to systematically reduce its magnitude. However, peaks in reflectance are present in remote-sensing reflectances, which leads us to investigate the potential direct use of sea surface reflectance to investigate such blooms. This work has also benefited from the frame provided by the NECCTON project, in which the University of Liège was responsible for the Black Sea area. The objective of task 5.2.4 is the improvement of the simulation of bio-optics in several regional biogeochemical models across Europe, including the simulation of in-water irradiance. The lead institue for this task, OGS, worked on the Mediterranean and in particular with the BOUSSOLE site.

I also have some doubts about the robustness (validity) of the argument that modelling the reflectance gives a better opportunity for validating the model against satellite-derived reflectances than validating against the chlorophyll (Chl) (as alluded to on top of page 3).

When validating the modelled Chl with satellite-derived Chl, the BGC model uncertainties in the calculation of Chl come into play as well as the uncertainties of the satellite-derived Chl. When validating (comparing) the reflectances, the uncertainty of the reflectance-to-Chl calculation is removed on the satellite side, yet this uncertainty is “transferred” into the model calculation of reflectance from optical properties, themselves calculated from some of the model state variables. Therefore, the benefit (other than practical) is unclear to me in terms of reducing the level of uncertainty in the validation process. I have tried to summarise this in the attached sketch.

As mentionned in the previous point, one advantage of working with reflectances is the ability to use data that contains more information than the remote-sensing surface chlorophyll, that sometimes misses important events. Macé et al. (2025) delves deeper into the analysis of uncertainties at play with the radiative transfer model and the IOPs, using ensemble methods to evaluate the influence of uncertainties in the IOPs on the simulation of sea surface reflectances.

Otherwise, when looking at the results in Figs. 3 to 6, the differences between the “simple optics” and the new “RT model” seem really minute. So, it is not obvious to me what improvements the new modelling brings. Then Figs 7-11 show quite poor results for the validation against satellite products. It is then hard to be convinced of the usefulness of the new model. There is no stated goal in terms of acceptable RMS or bias, which makes improvements, if any, hard to qualify as either significant or simply within the uncertainty of the modelling.

In figures 5 to 7, we ensure that the introduction of such additional complexity into the model does not generate significant biases in the computation of physics and biogeochemistry. We rephrased it so that it is clearly stated in the manuscript, we expect minor differences here. At this stage, our objective is not to directly improve the computation of temperature with this model but to use this tool to add a new capability. This is directly exhibited in figure 4 with the consistent computation of irradiances that was not possible before. We believe that the simulation of new quantites is already an improvement of the model. While figures 8 to 12 show contrasted results in the comparison with remote-sensing data, we also emphasise that figure 13 directly shows a reduction of the error in the computation of surface chlorophyll fields using sea surface reflectances. Furthermore, the comparison of reflectances in figures 8 to 10 is a relevant result to both identify ways in which the model needs improvement, but also when it produces distributions that are consistent with observations. We extended the discussion to highlight these aspects in a more detailed way.

In any case, the quality of the outputs of the best possible RT model entirely depends on the relevance of the inherent optical properties (IOPs, i.e., absorption and scattering) that are used as input. This aspect is barely addressed in the manuscript, when I think it should be the very first step to check. For instance, the Chl-specific absorption coefficients for phytoplankton displayed in Fig. 1 do not show much difference for the three phytoplankton groups, when the size should normally matter to determine this coefficient. It is also supposed to be depending on Chl as well. So, why these values, where do they come from?

We acknowledge that the relevance of IOPs is the main driver of the ability of the model to represent in a correct way the optical properties of the basin. Here, we have based the computation of IOPs on the previous attempts to couple RT models to biogeochemical models. The general method is in most part the method presented in Dutkiewicz et al. (2015). Specific coefficients for phytoplankton have been updated thanks to the data used in Alvarez et al. (2022) for the Mediterranean Sea. At this stage, we do not have a dataset for such coefficients in the Black Sea ready to be implemented into the model. We are also dependant of the PFTs simulated in BAMHBI, that contain their own uncertainties as phytoplankton is grouped in only 3 PFTs. We have updated the manuscript to highlight these important aspects in the discussion.

I would expect a revised version of the manuscript to discuss these points. Apart from this, I think this paper is publishable after some minor changes, however.

On a more general note, I do not understand why the modelling community does not try to work more closely with qualified RT and bio-optics specialists when trying to improve the modelling of light propagation. There might be some good justifications to use the parameterisations that have been chosen here but this is not made clear (neither it was in Dutkiewicz et al., 2015, by the way). For instance, the separation of the direct and diffuse components of the downward solar radiation is not really needed here. Modelling the diffuse attenuation coefficient for downward irradiance (K_d) does not require this separation (neither for heat deposition nor for photosynthesis).

Indeed, diffuse attenuation is not required to produce the results presented in this paper. It has been included as it only provides more information without influencing the simulation of the total downward stream (as measured by BGC-Argo floats), nor the backscattered stream (linked to sea surface reflectance). For this paper, most parameterisations have been kept from existing literature or adapted to better match Black Sea observations. As it is a new exercise for this basin and with the BAMHBI model, this work is only a first step into the simulation of bio-optics in the Black Sea and also serves to identify important future developments.

A few detailed comments

- Abstract: should in principle avoid using acronyms in the abstract or at least define them.

Acronyms have been removed from the abstract, except for model names that have been defined.

- Line 5: admittedly a bit subjective but 25 nm is not really a fine spectral resolution

The use of "fine" resolution has been decreased in the paper, in favour of terms such as "improved" compared to previous similar configurations.

- Line 9: typo (is can)

Fixed.

- Line 15: "more truthfully". Really? Not sure why this can be said

Changed to "more directly" which was the intended meaning.

- Line 22: I would rather say that solar radiation is primarily a driver of the evolution of sea temperature. And the much smaller fraction absorbed by phytoplankton contributes to photosynthesis.

This has been reformulated to explain it more clearly: "First, solar radiation is a driver of the evolution of sea temperature. As such, it influences the vertical profiles of the temperature and the depth of the thermocline. A fraction of radiation is also absorbed by phytoplankton communities through photosynthesis, which converts inorganic compounds into organic material"

- Line 22: not sure why Mobley is cited about photosynthesis. Should use another reference.

References have been checked in the introduction so they appear at the right position.

- Line 84: "work" rather than "paper"?
- Line 182: should be "seawater" instead of "water"

Both have been changed.

- Line 183-184: this sentence is quite vague. Sounds a bit like a truism "water highly absorbs except when it does not..".

Rephrased for clarity: "Outside of the visible range, high absorption by seawater does not allow radiation to propagates further than a few metres. In the visible range, absorption is lower and the contribution of other constituents becomes relevant."

- Line 184: molecular scattering is symmetrical with respect to the incident direction but it is not isotropic (slightly less scattering at 90° than 45° for instance).

This has been changed according to the comment.

- Page 10, eqs. (24) and (25) and corresponding text: it is unclear why this other light model is now used? This does not seem consistent with what was presented before.

We added an explicit mention that this is the light scheme initially integrated to BAMHBI, used as reference for the comparison that is developped in the paper. Both schemes are now listed to clearly indicate the differences.

- Lines 264-272: it seems that CDOM fluorescence, which is what some BGC-Argo floats measure, is here confounded with CDOM absorption. Should be clarified.

This paragraph has been modified to highlight the relationship that we established between CDOM fluorescence (fDOM) and CDOM absorption (a_{cdom}). For each BGC-Argo profile, we assumed that the shape of CDOM absorption profiles would be the same as fDOM profiles, and thus computed for each profile a coefficient A so that a_{cdom} = A x fDOM. The description of the CDOM absorption forcing has also been extended in Section 3.3.

- (26): PAR is normally expressed in units of Quanta, so the equation is incomplete.

There was a confusion in the paper where the scalar irradiance available to phytoplankton E_0 was called PAR in several instances. This has been corrected. Furthermore, we added the approximation for converting PAR (from BGC-Argo floats) to E_0 with its reference. The factor 4.6, taken for daylight conditions, is considered (Thimijan and Heins, 1983).

- Table 1: the average cosine for the downward irradiance cannot be greater than 1. Something to check here.

The number written was the inverse of the cosine. It has been corrected.

- Line 464: this reference is really wrongly placed. No need for a reference to say that 490 nm is in the blue part of the spectrum and 555 nm in the green.

Changed, the reference has been put somewhere more relevant.

- Line 471: not strictly true. Q spectrally varies.

A sentence was added to indicate that although Q spectrally varies, we assume that the difference between close wavebands does not significantly alter ratios between wavebands.

- Conclusions: I would not use so much “high spectral resolution” for this model.

Parts of the conclusion have been rephrased to reflect the fact that spectral resolution has been improved compared to previous versions of the coupled model.