Dear Editor, Dear Reviewers,

Thank you for taking the time to review our manuscript and for providing comments, we appreciate this very much. Your comments have been very constructive and have made a significant contribution towards improving our manuscript.

Please find below our response to the individual reviewer's comments. The original comments are numbered (e.g. R1C1 – Reviewer 1, Comment 1 and R2C1 – Reviewer 2, Comment 1) and shown in black italic text. Our response is shown in blue normal text. We have included two versions of the updated manuscript, one version which shows the changes and one clean version. Please note that when we refer to line numbers in our responses below, we refer to the new line numbers in the clean version of the manuscript. We have also included 14 additional references in the revised manuscript, as follows:

Additional References:

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3. Fennel, W. and Sturm, M.: Dynamics of the western Baltic, Journal of Marine Systems, 3, 183-205, https://doi.org/10.1016/0924-7963(92)90038-A, 1992.

4. Fournier, G.R. and Forand, J.L., Analytic phase function for ocean water, Proc. SPIE 2258, Ocean Optics XII, https://doi.org/10.1117/12.190063, 1994.

5. Freda, W. and Piskozub, J., Improved method of Fournier-Forand marine phase function parameterization, Optics Express, 15(20), 12763-12768, https://doi.org/10.1364/OE.15.012763, 2007.

6. Gallegos, C.L., Werdell, P.J and McClain, C.R.: Long-term changes in light scattering in Chesapeake Bay inferred from Secchi depth, light attenuation and remote sensing measurements, Journal of Geophysical Research, Oceans, 116, C7, https://doi.org/10.1029/2011JC007160, 2011.

7. Gregg, W.W. and Rousseaux, C.S.: Directional and spectral irradiance in ocean models: effects on simulated global phytoplankton, nutrients and primary production, Front. Mar. Sci., 22, 3, https://doi.org/10.3389/fmars.2016.00240, 2016.

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10. Kirk, J.T.O.: Light and Photosynthesis in Aquatic Systems, 3rd Edition, University Press, Cambridge, 649pp, 2011.

11. Meier, H.E.M., Modeling the pathways and ages of inflowing salt- and freshwater in the Baltic Sea, Estuarine Coastal Shelf Science, 74(4), 717-734, https://doi.org/10.1016/j.ecss.2007.05.019, 2007.

12. Omstedt, A., Pettersen, C., Rodhe, J. and Winsor, P., Baltic Sea climate: 200 yr of data on air temperature, sea level variation, ice cover, and atmospheric circulation, Clim. Res., 25(3), 205–216, https://www.jstor.org/stable/24868400, 2004.

13. Rozwadowska, A. and Isemer, H.J.: Solar irradiation fluxes at the surface of the Baltic Proper. Part 1. Mean annual cycle and influencing factors, Oceanologia, 40(4), 307-330, 1998.

14. Zielinski, O., Llinas, O., Oschlies, A. and Reuter, R.: Underwater light field and its effect on a one-dimensional ecosystem model at station ESTOC, north of the Canary Islands, Deep Sea Research II; 49, 17, https://doi.org/10.1016/S0967-0645(02)00096-6, 2002.

Reviewer 1:

The introduction section 1 – 1.1:

R1C1:

I would suggest to restructure this section to make it much more systematic, making sure the narrative flows coherently from the beginning until the end of the section. Perhaps the discussion can be simplified by presenting a process diagram showing the main optically active tracers, how they attenuate underwater light at different wave bands, how this feeds into biology (primary production), impacts the temperature gradients, which loop back into biology through reduced mixing and so on. A single Figure could replace here many lines of text. I would then start with describing the properties of the incoming irradiance in the different wavebands, how these are attenuated by clear sea water (invisible band) and OSCs in the visible band (essentially what is the paragraph on the lines 72 – 84), then I would list the main OSCs (phytoplankton, POM, CDOM, sediments..) and say something on how and where each of those OSCs impacts the light. Then I would stress the particular importance of CDOM in the western Baltic Sea and discuss its seasonal dynamics.. After describing the impact of OSCs on the underwater light field I would discuss their impact on the heating and stratification, and how that feeds back into the primary production. In all those instances I would refer to the schematic Figure.

This is a very helpful suggestion. We have restructured the introduction section as suggested and streamlined the text, which is now a little shorter. We have also added two new figures to support the text. Figure 1 (in section 1.1) shows the spectral absorption coefficients used in the study and illustrates how water constituents preferentially absorb light at different wavelengths. Figure 3 (in section 2.2) shows the model components and how they interact within the model system.

R1C2:

line 85: perhaps "characterized" is a little bit too strong word, maybe "influenced"?

Agreed, this has been resolved in the revised introduction.

R1C3:

lines 89-91: I would be more careful with stating that the increased stratification has automatically positive impact on phytoplankton growth. This would be indeed true for specific times and locations, when/where phytoplankton is light limited. However, whenever phytoplankton becomes nutrient-limited, increased stratification will have the opposite effect and reduce its growth. Indeed it is widely expected that increased stratification due to global warming will lower primary production and not the other way round.

We agree. It was not our intention to suggest that increased stratification leads automatically to a positive impact on phytoplankton growth. The wording in this paragraph has been modified to address the reviewer's comment, as follows (lines 96 - 103):

"Enhanced near-surface stratification can have a positive feedback on phytoplankton growth by restricting phytoplankton within shallower mixed layers with more available light, which in turn increases near surface local heating (Dickey and Falkowski, 2002). A 10 Wm^{-3} change in the solar radiation absorbed within a 10 m layer can represent a temperature change within that layer of more than 0.6°C month-1 (Simpson and Dickey, 1981). However, as light limitation is replaced by nutrient limitation, increased stratification will inhibit the exchange of deeper nutrient rich water with the surface and limit phytoplankton growth. Ohlmann et al. (2000) demonstrated that an increase in chlorophyll concentration from 0.03 mg m^{-3} to 3 mg m^{-3} in the upper 10 m of the water column can decrease the solar flux in the waters below by as much as 35 Wm^{-2}."

section 1.2

R1C4:

lines 128-130: why there is lack of mentioning detritus and its impact on the light attenuation?

This was an oversight; we have added the word "detritus" to line 164.

R1C5:

lines 109-148: there is some discussion of spectral resolution here, but why there isn't discussion of directional resolution of incoming irradiance? E.g resolving light in two streams diffuse/direct is quite common, e.g Dutkiewitz et al, 2015, or the OASIM model in Gregg & Rousseaux (2016). It has been shown that resolving diffuse light has particularly important impact on biogeochemistry in the higher latitudes (Gregg & Rousseaux, 2016). Also why the section doesn't discuss finer spectral resolution than VIS/IR, or R/G/B within VIS? E.g OASIM model of Gregg & Casey (2009) resolves irradiance in 33 wavebands … Some words on how the incoming surface irradiance is usually calculated for the biogeochemistry model (using atmospheric models) would be valuable here as well...

Agreed, as part of streamlining the introduction, we have added the following paragraph (lines 174 - 177):

"Including directional and spectral light in coupled biogeochemical-circulation-radiative models has been shown to be important for ocean biology, especially for studies of community structure and succession (Gregg and Rousseaux, 2016). It is also important for regional studies which examine the role of other optical constituents such as CDOM and detritus in carbon cycling (Bissett et al., 1999a,b)."

In the methods section, we have also clarified how the incoming surface irradiance is calculated as follows (lines 280 - 290):

"Light energy just beneath the sea surface is calculated using a derivative of the RADTRAN code described in Gregg and Carder (1990) as a function of the model's meteorological forcing (i.e. wind speed, relative humidity, air temperature and pressure), and cloud cover, atmospheric gases (i.e. water vapour, ozone, oxygen), marine aerosols and the surface roughness and reflectance at the ocean-atmosphere interface. A constant percentage of 0.3 % cloud cover is assumed for clouds, while 1.5 cm precipitable water is assumed for water vapour. The underlying algorithms used to compute ozone, water vapour and oxygen absorption coefficients are described in detail in Gregg and Carder (1990). Marine aerosols are computed according to the simplified version of the Navy marine aerosol model, also described in detail in Gregg and Carder (1990). The surface solar downwelling spectral irradiance, $E_d(\lambda,0\text{-})$ (which is the sum of the direct and diffuse irradiance) and the average cosine zenith angle, $\mu_0(\lambda,0-)$ are provided at 5 nm wavelength intervals between 400 and 700 nm and are used as inputs to Ecosim's daylight module."

R1C6:

line 186: maybe the text below can be put in a separate section describing what has been done in the paper?

Agreed, we have added a sub section at the end of the Introduction to cater for this paragraph, as follows (line 178):

"1.3 Estimating the impact of optically significant water constituents on surface heating in the Western Baltic Sea"

The methods section:

R1C7:

Figure 1: a really minor comment, but I find the colorscale a little non-intuitive (blue where it's shallow and green where it is deep), maybe you can consider changing it, but really up to you..

Agreed, we have updated the figure (now Figure 2) with the colour scale reversed.

R1C8:

section 2.3.1: maybe you can consider to put some of the information on the atmospheric model/OSCs/spectral resolution in a Table? Just like the schematic diagram, it always makes life easier for the reader... Also can you please provide information on where the data on clouds, aerosols and water vapour (lines 303-304) are taken from? I assume you use spectrally resolved (up to 5nm) absorption, backscattering coefficients, where are their values taken from? It would be maybe good to get some extra detail on how the surface E_d is calculated from the atmospheric data, not just the Gregg & Carder (1990) reference. Some more information on all this is needed.

Agreed, we have included more information on the atmospheric model (see response to R1C5 above) and created Appendix A where the details of the model configuration are given in a Table A1.

R1C9:

section 2.3.2, lines 324-325: maybe you want to explicitly say already from the start that MOMO is used to validate the more approximate model? It makes the reader start to wonder why you are describing MOMO here..

Agreed, we have added the following text (lines 324 – 325):

"For this purpose, we use the vector radiative transfer model, MOMO (described below) to evaluate the more approximate solution provided by ROMS-Bio-Optic."

R1C10:

Table 1: in the model grid section I believe the "1nm" should be "1.8km"?

Agreed, the model resolution has been changed to kilometres in Table A1 (which has been moved to Appendix A).

R1C11:

Sections 2.5.1 are there no observations on other important OCSs, such as phytoplankton chlorophyll/even carbon? Why you did not try to validate phytoplankton (concentration/attenuation), only CDOM absorption?

We have clarified our model evaluation strategy (section 2.4, lines 375 - 393) and updated the results section with a more comprehensive evaluation of our model output using the Sentinel 3 OLCI 300m Level 3 chlorophyll, phytoplankton and non-algal particle absorption, and diffuse attenuation coefficient, K_d490 products on two consecutive days in May 2018 when a bloom event took place in the Arkona Sea (section 3.2, Figure 5, Figure 6, Table 2).

Section 3:

R1C12:

Figure 3: there are missing labels on the x-axes marking the time of the simulation. What is the white rectangle in the Arkona Sea temperature plot? Also can you explain the dip in the temperature at Arkona Sea at about 20m depth? It's quite unusual that temperature grows with depth (i.e in the stratified period?), which is what happens at certain times in the 20-40m range...

Agreed, the labels on the x-axis have been updated. The white rectangle in the Arkona Sea temperature plot refers to gaps in the time series. This has been clarified in the text (lines 402 - 403). We have also clarified the instability in summer between 20 and 40 m at Arkona in the text as follows (lines 407 - 410).

"At Arkona Sea, the model captures observed summertime baroclinic inflows between 15 and 30m depth. These inflows are intrusions of deep, saltier, cool water which are pushed over the Drogen and Darß Sills into the deeper Arkona Sea. Due to the estuarine nature of Baltic Sea circulation, these inflows not unusual in the Western Baltic Sea (Fennel and Sturm, 1992)."

R1C13:

Table 2: it is missing the significant details in the caption – it needs to explicitly say that what is shown is temperature and what are the units for RMSE, bias (I assume K/C)

The table caption (now Table 1) has been extended to clarify that the statistics are provided for modelled versus observed sea surface temperature °C as follows (line 425):

"Table 1: Model versus observed sea surface temperature (°C) statistics."

R1C14:

lines 469-481: I think it would be also worth to show Figures directly for the phytoplankton, CDOM, detritus concentrations, not just on their spectral absorption, e.g. on their seasonality at the surface and comparing it with in situ/satellite data.

We have included a comparison of modelled surface chlorophyll-a with satellite data (see response to R1C11). Modelled surface concentrations of phytoplankton, CDOM and detritus in 2018 are shown in Appendix C for each of the analysis locations (Figure C1).

R1C15:

Line 488: has the irradiance been validated with observations?

We have cited the paper by Dera and Woźniak (2010) who have summarized and used field observations from two other papers by Rozwadowska and Isemer (1998) and Isemer and Rozwadowska (1999). These authors used meteorological observations from Voluntary Observing Ships to derive monthly climatologies of solar irradiance intensity at the sea surface and later to derive simple parametrizations of the solar irradiance transmission through the atmosphere over the Baltic Sea. We have added a figure to Appendix D (Figure D1) which compares ROMS Ecosim/BioOptic monthly mean surface irradiance in the Western Baltic Sea with the climatology shown in Dera & Wozniak (2010; Table 2 – Western Baltic Proper). The two additional references, Rozwadowska and Isemer (1998) and Isemer and Rozwadowska (1999), have been added to the text (line 492). We have also added the following text (lines 497 - 500):

"Our monthly mean modelled surface irradiances converge with those reported in Dera and Wozniak (2010) (Appendix D, Figure D1). We applied a constant fraction of 0.3 cloud cover while in Dera and Wozniak (2010), the clear sky assumption was applied. This would explain why our irradiances are lower than Dera and Wozniak (2010), especially in May, June and July."

R1C16:

Line 540: should be section 3.2, not 3.3?

This has been fixed with the restructuring of the text and "3.3" is now correct (line 543).

R1C17:

Lines 575 – 583: this is nice and exactly what I would expect. However the storyline is not entirely clear to me. What is the exact role of light here vs the role of temperature in stimulating growth? Why I can't say that the increase of light in spring supports the growth, increasing the surface temperature (due to both water and phytoplankton absorption), stratifying the water column and preventing phytoplankton of being mixed into the deeper darker waters, which further stimulates growth... Btw to support your statements why don't you re-do the Fig.9 as Hovmoller diagrams, rather than showing different curves for different times? It would be a much better way how to package the information (!) Also, is there any change to phytoplankton seasonality patterns/phenology between biofeed and nonbiofeed? E.g to the timing of the bloom peak and it's magnitude?

Agreed, we have updated the figure (now Figure 13) to a Hovmöller diagram. We have also reworded lines 579 - 587 to be more consistent with the reviewer's comment as follows:

"The increase in light in spring, supports phytoplankton growth and increases the surface temperature (due to both water and phytoplankton absorption) in the surface layer. Thus, the availability of light below the algae layer is strongly reduced and phytoplankton are restricted within the shallow mixed layer with more availability of light, which will in turn increase surface heating. The net effect is more biomass production in the surface layer at the beginning of the spring bloom in biofeed compared to nobiofeed."

Changes in the seasonality/timing of phytoplankton growth have not been explicitly investigated here. The first author is preparing a separate paper on this subject.

R1C18:

Fig.10: again caption needs better description, what are the left-hand panels and what the right-hand panels? Also in the buildup to the Figure can you explain why you chose the Bornholm Basin?

Figure title has been expanded to clarify what the different panels represent as follows (lines 602 - 604):

"Figure 14: Surface heat fluxes for both biofeed and nobiofeed experiments during the entire productive period, April to September, (left panel) and zooming in on the period where the difference in surface heat fluxes between experiments is greatest (area shown in rectangular box shown in top left panel) at Bornholm Basin."

Bornholm basin was selected because the seasonal cycle of the heat balance there can be approximated as a 1-dimensional balance between the penetration of solar radiation and vertical mixing (Gnanadesikan et al., 2019) and advective and diffusive terms will be relatively small. This is clarified in lines 213 - 215 and supported by our model evaluation with satellite observations (lines 456 – 458).

Reviewer 2:

General comments

R2C1:

1) Very nicely and very much in details written general (published) and theoretical information but much less in the actual information. In particular, this novel bio-optical module the authors introduce, use and evaluate is currently described in words with references to equations in the Theory subsection. Even though the Theory section is nicely written, it would be more straightforward to explicitly formulate the model equations with proper citations in the subsection 2.3 (as a separate subsubsection) and remove then subsection 2.2 (Theory). Especially, if this "Bio-Optic" module is presented for the very first time. If it was already published and evaluated, please provide the related reference.

Agreed, we have streamlined the introduction section and combined the Theory subsection with the description of the Bio-Optic model (refer to section 2.2, 2.2.1 and 2.2.2). We also clarify how Bio-Optic is a new option within the ECOSIM model and provide appropriate references linking the two (see section 2.2.1., lines 291 - 313).

R2C2:

Looks like the ROMS-BioOptic model code used in this study is not easily accessible (is not easy to find) given the provided general link https://www.myroms.org (Is it within EcoSim?)

The generic version of the Ecosim/BioOptic code is accessible via the general link (https://www.myroms.org) within the Ecosim module. The update Ecosim module which includes the biofeedback options, is currently archived with at Zenodo (see link under data availability) but will be incorporated into the standard ROMS release in due course.

R2C3:

2) Some more details could be given w.r.t. setting the radiation model MOMO used for the evaluation of the heating rate estimates (see specific comments).

Agreed, more details on how MOMO was configured for our experiment are provided in the text (lines 359 - 373), as follows:

"MOMO simulations were performed at relatively high angular resolution (twenty-seven angles in the atmosphere between 0 and 88 degrees plus nine additional angles in the ocean to cover the angular domain of total internal reflection) to allow for an accurate calculation of the in-water light field. Up to 120 terms were used for the Fourier expansion of the azimuth dependence of the light field. The oceanic vertical structure in MOMO has been chosen identical to the ROMS-Bio-Optic vertical structure, i.e., the light field has been calculated at the thirty-one ROMS-Bio-Optic layer boundaries located between 0 and ca. 90 m. Absorption and scattering coefficients for phytoplankton, CDOM, and detritus are taken directly from ROMS-Bio-Optic output. Spectral resolution was done in steps of 5 nm between 400 nm and 700 nm. Two Fournier-Forand phase functions (Fournier and Forand, 1994; Freda and Piskozub, 2007) with differing backscattering to scattering ratios have been applied to phytoplankton (bb/b = 0.001) and detrital material (bb/b =

0.1), in line with phase functions measured by Siegel et al. (2005) for various Baltic Sea coastal waters. Seasonal heating rates were derived from MOMO simulations at the Bornholm Basin location and compared to the corresponding fluxes from ROMS-Bio-Optic in order to assess the suitability of the simplified treatment of radiative transfer in the latter and the implications of not resolving the full directionality of the light field therein. MOMO results are presented for the 38° solar incident zenith angle, representative of late spring to mid-summer in the Western Baltic Sea (Figure 12)."

R2C4:

3) Temperature observations at the "Oder Bank", "Darß Sill", "Arkona Sea" and "Bornholm Basin" stations provided by the German Maritime Agency (BSH) and Denmark Meteorological Institute (DMI) could be presented in the "in situ observations" subsection. Please provide the related references.

Agreed, we have clarified our model evaluation strategy (section 2.4, lines 375 - 393) and included relevant references for the in situ observations.

R2C5:

4) Are there actual satellite CDOM, phytoplankton total chlorophyll, Kd products available for the year 2018 (the year of your interest)? What about using Sentinel 3 observations?

Yes, we have updated the results section with a more comprehensive evaluation of our model output using the Sentinel 3 OLCI 300m Level 3 chlorophyll, phytoplankton and non-algal particle absorption, and diffuse attenuation coefficient, K_d490 products on two consecutive days in May 2018 when a bloom event took place in the Arkona Sea (section 3.2, Figure 5, Figure 6, Table 2).

R2C6:

5) I would recommend changing the format of the result visualization from 3D (as it currently Figures 4 -8) to 2D. The 3D representation does not add anything in comparison with 2D one but hides some information. Hovmöller diagram might suit better for the results depicted in Figure 9 (I agree with the first reviewer)

We have considered the reviewer's suggestion to update the Figures 4 – 8 to 2D (now Figures 8 - 12) but do not agree. We do not think information is being hidden, but actually find the 3D presentation provides a better view on the spectral response of inherent and apparent properties of the constituents. We prefer to keep these figures in 3D.

We have updated Figure 9 (now Figure 13) to a Hovmöller diagram.

Specific comments

R2C7:

L17-18: please double check and correct if required: currently it reads as both phytoplankton and CDOM effects dominate in summer.

We have re-worded this to clarify the statement, as follows (lines 17 - 19):

"… find that while phytoplankton and CDOM both contribute to surface heating in summer, phytoplankton dominates the OSC contribution to heating in spring, while CDOM dominates the OSC contribution to heating in autumn."

R2C8:

L120: you could cite also Fasham et al., 1990

Agreed, we have added the Fasham et al., 1990 reference to lines 157 and 898.

R2C9:

L126: I would suggest removing Equation 2 as not used in the current study.

Agreed, Equations 1, 2 and 3 have been removed as part of restructuring and streamlining the Introduction.

R2C10:

L137: Could Equation 3 be removed? Do you use this equation? A related equation used in the current study could be shown in the subsection dedicated to the "Bio-Optic" module.

Agreed, see response to R2C10 above.

R2C11:

Figure 1: Were the observations marked as red dots used for evaluation or only the observations from the four stations (marked green dots)?

The red dots were used to prepare the monthly CDOM absorption climatology, details provided in Appendix B1. The green (now blue) dots show the location of the sites we use in the model evaluation.

R2C12:

The material from Subsection 2.2 Theory could be adjusted in the subsection dedicated to the "Bio-Optic" module.

Agreed, see response to R2C1 above.

R2C13:

L287: reference is required if exists

This is clarified in response to R2C1 above.

R2C14:

L303-304: what is assumed/used as information on "clouds, water vapour and aerosols, the surface roughness"

We have clarified these details in section 2.2 as follows (lines 280 - 287):

"Light energy just beneath the sea surface is calculated using a derivative of the RADTRAN code described in Gregg and Carder (1990) as a function of the model's meteorological forcing (i.e. wind speed, relative humidity, air temperature and pressure), and cloud cover, atmospheric gases (i.e. water vapour, ozone, oxygen), marine aerosols and the surface roughness and reflectance at the ocean-atmosphere interface. A constant percentage of 0.3 % cloud cover is assumed for clouds, while 1.5 cm precipitable water is assumed for water vapour. The underlying algorithms used to compute ozone, water vapour and oxygen absorption coefficients are described in detail in Gregg and Carder (1990). Marine aerosols are computed according to the simplified version of the Navy marine aerosol model, also described in detail in Gregg and Carder (1990)."

R2C15:

L311: provide information on how a and b for phytoplankton, detritus and CDOM are explicitly calculated, show also the function used to calculate the average cosine (L309)

We have clarified how absorption and scattering are calculated, along with relevant references which include the function for the average cosine in section 2.2. The following text has been added (lines 291 - 302):

"The spectrally-resolved downward light stream, Ed(λ,z) is calculated according to Eq. (10) and is attenuated by absorption, a, and scattering, b (forward, b and backward, bb) of the OSCs. Phytoplankton and detritus both absorb and scatter light. Phytoplankton absorption is calculated for the four functional groups as a function of biomass, weight-specific pigment absorption coefficients (Figure 1b, Bidigare et al., 1990) and packaging effect (Bissett et al., 1999b; Kirk, 2011). Detrital absorption is calculated as an exponential function of wavelength (Gallegos et al., 2011). Phytoplantkon and detrital scattering and backscattering are accounted for as total particulate scattering and backscattering according to Morel (1991) and Morel (1988), respectively (see Equations 16 and 17 in Bissett et al., 1999b). CDOM only absorbs light and is calculated as a function of CDOM concentration and the weight-specific absorption coefficients adapted from Kowalczuk et al. (2005b) (Figure 1a). The average cosine is modified with depth as a function of absorption and backscattering. This is simplified as a linear function of the optical depth between two levels (see Equation 22 in Bissett et al., 1999b). The total scalar irradiance, E0(λ,z), which is the light available to phytoplankton, is calculated following Eq. (5) after Morel (1988)."

R2C16:

L347: you could list the observed characteristics here explicitly

We have added some text to summarize the important characteristics of the physical model (lines 349 - 351), as follows:

"It captures the annual cycle of temperature and salinity in the Western Baltic Sea and episodic inflows of saline, oxygen-rich North Sea water which control the salinity content and stratification in the Baltic Sea and are important for ventilating the deeper basins of the Baltic Sea (Omstedt et al., 2004; Meier, 2007)."

R2C17:

Table 1 reads somehow repetitive to the text. While you could still extend the table by information related to "Bio-Optic" module setup, including information on spectral resolution. Information used for evaluation can be summarized in a separate table (if required, it will support the subsection 2.5). There you could also provide the details on satellite data used.

Agreed, we have moved the details of the model configuration into Appendix A and clarified which in situ and satellite data products are used for the evaluation in section 2.4.

R2C18:

L357-359: please provide the setup details on the MOMO simulations

See response to R2C3.

R2C19:

L393: I suggest removing "using Eq. (11)" as the equation follows

Agreed, the text has been removed.

R2C20:

L402: I suggest removing "using Eq. (12)" as the equation follows

Agreed, the text has been removed.

R2C21:

Section 2.5.1 could be shortened. The sampling details (e.g. L384-391) if not used for the discussion can be moved into Supplementary material).

Agreed, we have moved these details into Appendix B1.

R2C22:

In this section, BSH and DMI observations could be presented.

The in situ observations used for the model evaluation have been clarified (see section 2.5).

R2C23:

What about data from Meler et al. 2016?

This is clarified in section 2.5.

Subsection 2.5.2 Remotely sensed data:

R2C24:

Are there products available from other satellite missions (e.g. Sentinel3) for the year of your interest 2018?

Yes, see our response to R2C5.

R2C25:

Could Figure 2 be moved to the Supplementary or Appendix since not presenting results of the study, although supporting the discussion? Instead, in the Result section the authors could show and discuss the CDOM (TChla, Kd) distribution simulated by the model.

Agreed, we have created Appendix B to describe the details of the climatologies used in the model evaluation. Meanwhile, we have updated the results section with a more comprehensive evaluation of the model output, see section 3.2.

R2C26:

L446-448: please provide references to the BSH and DMI observations

This is clarified in section 2.5, see our response to R2C22.

R2C27:

Figure 3 captions: detail the legend (abbreviation used in the Legend)

Figure 3 (now Figure 4) has been updated and legend abbreviations have been clarified in the figure caption.

R2C28:

Table 2: Please extend the title to clarify that the goodness of fit statistics is provided for the simulated sea surface temperature.

The table caption (now Table 1) has been extended to clarify that the statistics are provided for modelled versus observed sea surface temperature °C as follows (line 425):

"Table 1: Model versus observed sea surface temperature (°C) statistics."

R2C29:

Figure A1: please provide related labels (a, b, c, d) to be consistent with the figure caption.

Agreed, the figure has been updated with labels a, b, c, d.

R2C30:

Figure A1 could be moved to the main manuscript as model evaluation results and could be extended (or revised 2 upper panels) by comparing with more collocated with MERIS (or Sentinel 3) surface matchups (not only for the four stations).

This figure (now Figure 7) has been moved into the main results section as part of the update to the model evaluation.