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:

1. Bidigare, R.R., Ondrusek, M.E., Morrow, J.H. and Kiefer, D.A.: In vivo absorption properties of algal pigments. Proc. SPIE, 1302, 290-302, doi:10.1117/12.21451, 1990.

2. Fasham, M. J. R.; Ducklow, H. W.; McKelvie, S. M.: A nitrogen-based model of plankton dynamics in the oceanic mixed layer, Journal of Marine Research, 48 (3), 591–639, doi:10.1357/002224090784984678, 1990.

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.

8. Huang, B., C. Liu, V. Banzon, E. Freeman, G. Graham, B. Hankins, T. Smith, and H.-M. Zhang, 2021: Improvements of the Daily Optimum Interpolation Sea Surface Temperature (DOISST) Version 2.1, Journal of Climate, 34, 2923-2939. doi: 10.1175/JCLI-D-20-0166.1

9. Isemer, H.J. and Rozwadowska, A.: Solar radiation fluxes at the surface of the Baltic Proper. Part 2. Uncertainties and comparison with simple bulk parameterization, Oceanologia, 41(2), 147-185, 1999.

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, the reviewer's suggestion has been implemented.

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 281 - 291):

"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, $\mu_0(\lambda,0\text{-})$ 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.

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 325 – 326):

"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 424):

"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 496). We have also added the following text (lines 494 - 497):

"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 549).

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 576 - 584 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 596 - 598):

"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 453 – 455).