Response to Referee #1

We thank the referee for the careful and constructive evaluation, and for the positive assessment of the scientific motivation, results, figures, and presentation. We particularly appreciate the emphasis on the discussion of observational controllability, which we have sharpened throughout the revision. Our revisions cluster around four themes: (1) the secondary role of assimilated Chl-a relative to CDOM in controlling light attenuation; (ii) a clear separation of assimilation from validation observations; (iii) the distinction between dynamical coupling and observational controllability across the water column; and (iv) the physical controls and observational limitations governing the deep-water oxygen state. We have considered all comments carefully and revised the manuscript accordingly. Below, we respond to each comment point by point.

This manuscript assesses how the weakly coupled assimilation of Chl-a and SST into a coupled physical-biogeochemical model of the Baltic Sea, contributes to improve the representation of temperature, chlorophyll and dissolved oxygen, both in surface and in depth. The representation of the assimilated variables is improved in surface (spatially and temporarily). Whereas assimilating SST contributed to improve the biological state by improving the physical context, the assimilation of CHL impacts only minorly but noticeably the physical state. The transferring to depth, on the other hand, is limited for the observed variables given the vertical structure of the water column, and absent for the non-assimilated oxygen, which reveals different controls on shallow and deep water of oxygen content.

Main comment

I think the findings of the manuscript are interesting for the potential readers of Biogeosciences. It is timely, as the coupled assimilation of physical and biological variables is becoming more popular. Therefore, assessing to which extent the physical and/or biological state is controllable by the available observations is worthwhile. I find particularly interesting the discussion on the potential missing model constraints that could be explaining the non-controllability by DA. The ms is well composed and written, the figures are good-quality and informative. Below, I include some comments of specific aspects that I think would benefit from some clarification. Other than that, I think the manuscript reads well.

We thank the referee for this encouraging overall assessment. We have retained and strengthened the discussion of observational controllability, which the referee identified as a key contribution, and we address each specific point below. The line numbers in the response refer to the revised manuscript.

Specific comments

ABSTRACT

L20. “optically complex marginal seas” usually accounts for waters where the optical signal is not dominated solely by Chl-a. In waters defined like that, I do expect that the bio-optical feedback is driven by other optical constituents, as it is explained by the authors in the introduction (L64). In that case, I do not get why the hypothesis is that Chla assimilation could propagate to temperature though bio-optical feedback. In fact, other works have shown that, in CDOM-rich waters, this mechanism is driven by the high CDOM attenuation and not Chl-a. Therefore, the influence on thermal structure of Chl-a-induced variations in light absorption should be second order. I understand the authors justify using only Chl-a as the attenuating component because they validate extinction coefficients, am I right? Maybe that is something the authors could elaborate on a bit in the Discussion.

Response:

We fully agree, and we realize our original use of "optically complex" may have implied a dominant Chl-a control we did not intend. Our hypothesis is not that Chl-a assimilation dominates the thermal structure; rather, we test whether increments to the modelled Chl-a field produce a secondary, spatially heterogeneous modification of light attenuation that redistributes solar heating in the upper water column. Our results support precisely this reading: CHL_SAT and CHL_3D generate localized subsurface temperature changes (<0.4 °C; Fig. 10) while leaving basin-scale SST statistics essentially unchanged — the expected signature of a second-order effect in a CDOM-rich, salinity-stratified basin. The referee is also correct about our methodological scope: because we validate the extinction coefficient through the model's existing optical formulation, our experiments isolate the incremental optical effect of changes in modelled phytoplankton biomass, with CDOM and other constituents remaining important, unaltered absorbers.

We have therefore revised the Abstract to explicitly characterize the Chl-a-induced thermal response as a secondary bio-optical effect in the CDOM-rich Baltic Sea.

The Abstract states now: "*Although chlorophyll-a increases can modify subsurface heating through a secondary bio-optical feedback, the thermal response remains weak relative to the dominant physical controls in the halocline-dominated Baltic Sea. Deep-water oxygen profiles at monitoring stations remain insensitive to surface-focused constraints and are governed instead by physical ventilation and remineralization. These results demonstrate that, in optically complex marginal seas, joint physical–biogeochemical assimilation can improve the consistency of the upper-ocean biological and physical state, while surface observations alone remain insufficient to constrain the full water column.*"

In addition, the Discussion has been revised to clarify that the experiments isolate the incremental contribution of assimilated Chl-a to the modeled optical and thermal response in Section 5.1(lines 635–637):

"the Chl-a-related thermal response represents the incremental optical effect of changes in modelled phytoplankton biomass within the existing optical formulation, while other optically active constituents, particularly CDOM, remain important contributors to light attenuation."

INTRODUCTION

L53. Some of the references provided indeed assimilate oxygen and nutrients, but on a broader view, is not Chl-a more commonly assimilated than nutrients and dissolved oxygen? Could the authors clarify if they refer to Baltic Sea applications?

Response:

We agree; the original wording was too broad. Our intended meaning concerned the relative scarcity of studies examining joint physical–biogeochemical assimilation in the Baltic Sea. The sentence now acknowledges that Chl-a assimilation is widely used in regional and coastal applications while emphasizing the Baltic-specific gap (lines 52–56):

"Assimilation of biogeochemical variables, including nutrients, dissolved oxygen, and Chl-a, has increasingly been applied in regional and coastal ocean applications (Liu et al., 2014; Ciavatta et al., 2018; Teruzzi et al., 2021). However, the relative availability and use of these observations differ among regions,

and the effectiveness of jointly constraining physical and biogeochemical states remains less systematically assessed in the Baltic Sea (Goodliff et al., 2019; Skákala et al., 2021)."

L79. This is a continuation of the previous comment. Some of the references provided are not Baltic-specific studies, but the sentence seems to apply only to studies that have assessed the effectivity of joint assimilation in the Baltic Sea. Maybe the authors could clarify.

Response:
We agree and have rewritten the sentence to distinguish broader regional applications from Baltic-specific evidence (lines 78–82):

"Although joint physical–biogeochemical assimilation has been explored in several regional ocean applications (Ciavatta et al., 2018; Goodliff et al., 2019; Teruzzi et al., 2021), its effectiveness in the Baltic Sea has not been systematically assessed for the simultaneous assimilation of satellite and in situ Chl-a together with sea surface temperature (SST) within a weakly coupled framework."

METHODS

L114 The bgc model is less detailed described than the physical model. I think how tracers “interact through local biogeochemical transformations” should be elaborated a bit more, especially those tracers and processes that are explored in this study. Maybe, the authors could include few words to clarify, for instance: is Chl-a a single tracer, or does it account for several plankton groups? how do Chl-a impact light propagation? which proceses produce/consume oxygen? Also, which is the origin of the “monthly climatological fields” prescribed for bgc in the open boundaries is not clear.

Response: We thank the referee for this suggestion and agree that the description of the biogeochemical component required additional detail. We now specify that phytoplankton biomass is represented by 3 functional groups and clarify its links to chlorophyll-a, light attenuation, and vertical solar heating. We also identify photosynthesis, respiration, and remineralization as the main biological processes affecting oxygen, while noting the role of physical transport and ventilation. In addition, we have specified that the monthly climatological biogeochemical fields prescribed at the open boundaries follow Ruvalcaba Baroni et al. (2024).

We have expanded Sect. 2.1 (lines 120–124):

"Phytoplankton biomass is represented by three functional groups and is regulated by nutrient and light availability, grazing, and other loss processes. Changes in phytoplankton biomass affect the modeled chlorophyll-a concentration and light attenuation, thereby influencing the vertical distribution of shortwave radiation and solar heating. Oxygen is produced through photosynthesis and consumed through respiration and remineralization, while its distribution is also controlled by physical transport and ventilation."

We also specify that the monthly climatological biogeochemical boundary fields follow Ruvalcaba Baroni et al. (2024).

Ruvalcaba Baroni, I., Almroth-Rosell, E., Axell, L., Fredriksson, S. T., Hieronymus, J., Hieronymus, M., Brunnabend, S.-E., Gröger, M., Kuznetsov, I., Fransner, F., Hordoir, R., Falahat, S., and Arneborg, L.: Validation of the coupled physical–biogeochemical ocean model NEMO–SCOBI for the North Sea–Baltic Sea system, Biogeosciences, 21, 2087–2132, https://doi.org/10.5194/bg-21-2087-2024, 2024.

Figure 1. The figure legend does not mention that the satellite and in situ observations are Chl-a. It is clear in the main text, but at first read this was a bit confusing.

Response:
Agreed; The caption now reads: "The Baltic Sea sub-basins and the corresponding relative errors between satellite-derived Chl-a and in situ Chl-a observations are shown in (b)…"

L168. Regarding the vertical coverage of the in-situ Chl-a. Since it is fluorometric Chl-a retrieved from CTD casts, I believe those are profiles taken every 1m in the first 50m. Maybe the authors could mention the 1m vertical resolution. Later, when reading the results of assimilating CHL_SAT and CHL_3D, the doubt came to my mind on how representative was the 3D coverage.

Response: We agree the vertical sampling characteristics are important for interpreting the satellite-only vs. 3D comparison. We now distinguish the original CTD sampling from the subsequent quality-control/interpolation and avoid implying uniform sampling (Sect. 2.2.1, lines 175–177):

"The Chl-a profiles typically include multiple depths within the upper 50 metres, but their vertical sampling interval varies among casts. Thus, these three-dimensional observational constraints should not be interpreted as uniformly sampled 1 m profiles for every station and date."

L215. The previous validation for the Baltic Sea of this solution is described in the references cited in the previous sentence? This is not clear.

Response:
We agree that the original wording was ambiguous and have clarified (sec 3.2, lines 242-245):

“The horizontal and vertical localization treatments follow the distance-based methods used in previous applications of data assimilation in the Baltic Sea (e.g., Liu et al., 2013; Liu et al., 2021); in this study, uniform horizontal localization radius of 150 km and 100 km are adopted for physical and biogeochemical variables, respectively, while a uniform vertical localization radius of 30 m is applied to all variables.”

Thus, the cited Baltic Sea studies provide the methodological basis for the localization approach, whereas the specific localization radii used here are choices made for the present assimilation experiments.

L216. In this paragraph the authors discuss the uncertainty associated to the sensitivity of results to the choice of the localization radius. This is the first of several sentences through Methods discussing limitations related to pre-, pro or post-processing choices. I feel all these considerations belong to Discussion. Another example in L286 regarding the specification of observation errors.

Response:
We agree with this suggestion. The Methods section should primarily describe the configuration actually used, whereas the implications and limitations of that choice are more appropriately discussed later.

We therefore retain the numerical values and methodological justification required for reproducibility in the Methods section but move the broader discussion of uncertainty, sensitivity, and limitations associated with the localization radius to the Discussion.

The same principle will be applied to the discussion of observation-error specification around L286.

Section 3.2 now retains the numerical configuration:

“a uniform vertical localization radius of 30 m is applied to all variables.”

The broader uncertainty discussion has been moved to Section 5.2, where we now state:

“The fixed 30 m vertical localization radius does not adapt to seasonal changes in stratification and may reduce vertical controllability during rapid mixed-layer deepening.”

The analogous discussion of observation-error uncertainty has also been moved from Section 3.4 to the Section 5.2:

“Similarly, prescribing a uniform 10% in situ observation error provides a pragmatic and consistent weighting across the domain but omits method-dependent uncertainties and point-to-grid representativeness errors at the ~3.7 km model resolution.In optically complex environments with high CDOM absorption and spatial patchiness, static observation errors may over- or under-constrain the analysis increments. Developing adaptive vertical localization and state-dependent observation-error specifications therefore remains an important direction for refining coupled assimilation frameworks. ”

L276. This “relative error coefficient” is the one displayed in Figure 1 panel b, right? Maybe the authors could point to this figure.

Response:
Yes. Sect. 3.4 now states (lines 311–313): "The sub-basin-specific relative error coefficient (ε_{rel}), estimated from statistical comparisons between collocated satellite and in situ Chl-a observations, is shown spatially in Fig. 1b."

L292. Here the authors mention initial and restart fields taken from a previous reanalysis. I am assuming, therefore, that the authors have run a spinup period and later restart the proper runs? This is not clear from the simulations period stated earlier. If thats not the case, where those restart files are used is not clear from the text.

Response:
We agree that the distinction between model initialization/restart and the formal experiment period needs to be clarified.

The three experiments were integrated over October 2015–November 2018, while the initial and restart fields were obtained from the specified reanalysis product. These fields provide the initial state for the experiments; they should not be interpreted as an additional spin-up period unless explicitly stated otherwise.

We revised the description to clearly distinguish the source of the initial/restart fields from the actual simulation period used for the assimilation experiments.

Section 3.5 now states:

“All experiments were integrated over October 2015–November 2018. Initial and restart fields were taken from the ocean reanalysis described by von Schuckmann et al. (2019) and were used to initialize the experiments; no additional model spin-up period is included in the analyzed experiment period."

L303. It is not clear which observations are used for validation. I understand the variables are Chl-a, SST and dissolved oxygen, but Chl-a and SST are assimilated. Are Chl-a and SST not used for assimilation being used for validation? This is not clear form Section 3.6

Response:
We agree and have made the separation explicit. A new Table 1 lists, for each variable and experiment, the assimilation dataset, the validation dataset, and whether the validation is independent. Sect. 3.6 now states (lines 360–367):

"To distinguish assimilation constraints from independent model evaluation, the use of each observational dataset is summarized in Table 1. Chl-a and SST observations used for assimilation are not considered independent validation data for the corresponding experiments. In situ Chl-a profiles therefore provide an independent observational reference for REF and CHL_SAT, but comparisons in CHL_3D and CHL_SST assess consistency with the assimilated profiles. Oxygen observations remain independent for all experiments because oxygen is not assimilated. SCM depth is treated as a diagnostic derived from Chl-a profiles, and its independence therefore depends on whether the underlying profiles are assimilated. Similarly, comparisons with satellite products used in assimilation are interpreted as consistency with the assimilated observations rather than as independent validation."

Table 1. Observational datasets used for assimilation and evaluation, and their experiment-specific assimilation status.

Observations	Assimilated in	Used for Validation	Independent validation
OSISAF SST	CHL_SST	Yes	CHL_SAT and CHL_3D: Yes CHL_SST: No
Multi-sensor and OLCI Satellite Chl-a	CHL_SAT, CHL_3D, and CHL_SST	Yes	No
In-Situ Chl-a	CHL_3D and CHL_SST	Yes	CHL_SAT:Yes CHL_3D and CHL_SST: No
Oxygen	No	Yes	Yes

RESULTS

L338. I am not understanding this sentence. The thermal response to biology is observable in CHL_SAT and in CHL_3D. In CHL_SST is mixed with the SST assimilation reorganization. The use of “subsequent” suggest to me that the authors are referring here to CHL_SST, which is confusing. What I am understanding from this Section 4.1 is that assimilating SST improves SST, assimilating Chl-a doesn’t, at least in the grand scheme of things, but it is expected to have a residual effect. Maybe the authors could help the reader (me) by clarifying here how they are going to explore this secondary adjustment, by comparing which specific experiments?

Response:
We appreciate this comment and agree that the original wording is potentially confusing.

The purpose of this part of the analysis is to distinguish the direct effect of SST assimilation from the indirect physical response associated with Chl-a assimilation. In particular:

- REF provides the baseline without assimilation.
- CHL_SAT isolates the indirect physical response associated with satellite Chl-a assimilation.
- CHL_3D isolates the effect of adding subsurface/in situ Chl-a information.
- CHL_SST represents the combined effect of Chl-a and SST constraints.

Thus, the Chl-a-induced thermal response is primarily diagnosed by comparing **CHL_SAT against REF**, and where relevant **CHL_3D against REF**, rather than interpreting CHL_SST alone as evidence of a biological effect.

The end of Section 4.1 now states:

"…The thermal response associated with biological corrections is therefore diagnosed primarily from the differences between CHL_SAT or CHL_3D and REF, where SST is not directly assimilated. CHL_SST is used to assess the combined effect of physical and biological constraints rather than as an isolated diagnostic of the Chl-a-induced thermal response. The thermal response associated with biological corrections is therefore interpreted as a secondary model-mediated adjustment rather than a basin-scale physical reorganization.."

L372 & Figure 5. The differences in Chl-a retrieval from the multi-sensor product and the olci-sensor product are intriguing. OLCI Chl-a shows a completely different (and a bit weird for a temperate to cold sea in the northern hemisphere…) seasonal cycle. Have the authors any idea on the reason for the discrepancy between the multi-sensor and the OLCI Chl-a's?

Response:

We agree this deserves fuller treatment. The two products differ in sensor composition, resolution, temporal coverage, and sampling; in the optically complex Baltic Sea, retrieval algorithms, atmospheric correction, and sensitivity to CDOM and suspended matter may further contribute. We cannot attribute the difference to a single mechanism. To add independent context, we introduce a comparison with station-sampled CMEMS in situ surface Chl-a (new Fig. S1): the in situ record shows a pronounced spring maximum peaking in April, OLCI shows a spring maximum of similar timing but lower magnitude, while the multi-sensor product and the model are lower still in this period. The early-season signal is therefore not unique to OLCI, but its magnitude is strongly product-dependent. Sect. 4.2.1 now retains the cautious interpretation that the mismatch reflects a combination of low-light seasonal-dynamics limitations, retrieval uncertainty, and sampling differences.

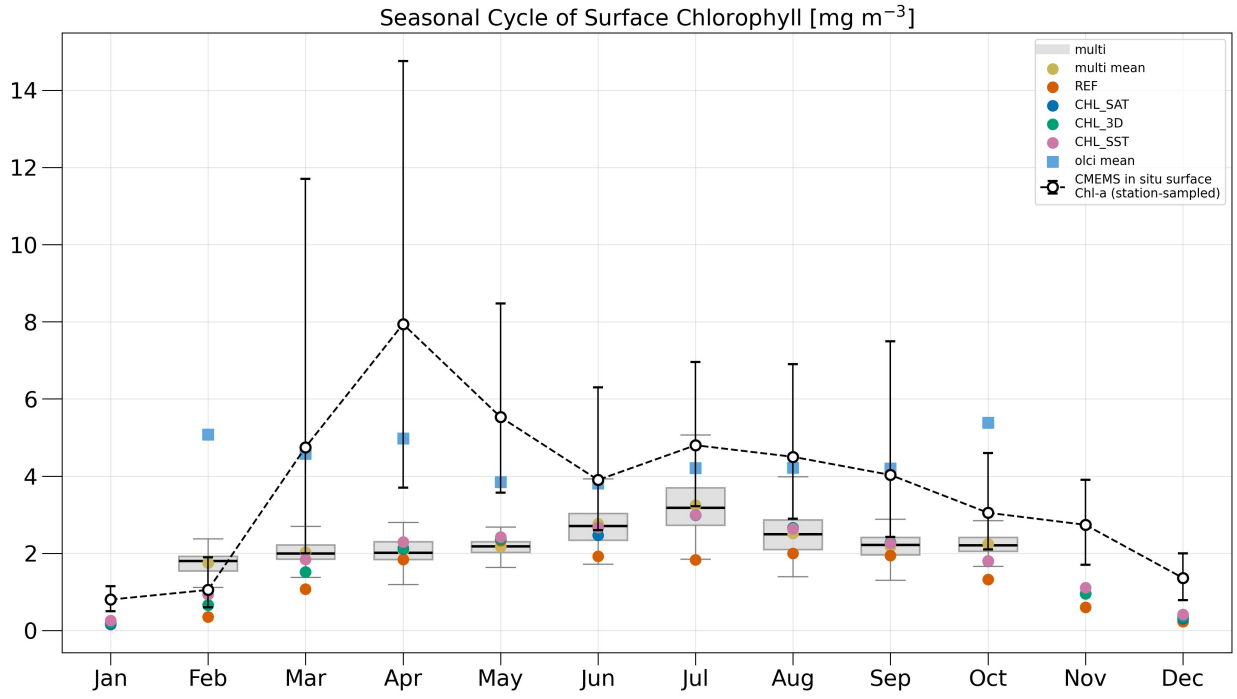


Figure S1. Seasonal cycle of surface chlorophyll-a (Chl-a) for model experiments and observational products. Grey boxplots show the distribution of daily spatial means from the multi-sensor satellite product for each month, and gold circles show corresponding monthly means. Blue squares show monthly OLCI means, while coloured symbols show monthly climatologies of the model experiments. Black open circles and vertical bars show the monthly median and 25th–75th percentile range, respectively, of station-sampled CMEMS in situ Chl-a observations from 0–2 m depth. Results using the 0–5 m depth range are similar. The satellite and model fields are spatially aggregated, whereas the in situ observations are station sampled; the in situ comparison therefore provides observational context rather than a spatially equivalent basin-wide reference.

L374. In Methods, the authors mentioned that both products are assimilated (L150), but somehow the model follows pretty closely the multi-sensor Chl-a. In these lines, the authors explain that this has to do with the strict limitations the model imposes to phytoplankton growth. Given the large difference between the multi-sensor and the OLCI Chl-a's I do wonder which is the one that the model is supposed to capture if it wants to be realistic. Maybe it will be useful to add to Figure 5 the superficial in situ Chl-a. Looking at Figure 7, it looks like the bloom happens in late-spring, close to summer in the stations showed, which seems to agree more with the multi-sensor Chl-a product.

Response:

We agree that the large difference between the two satellite products raises an important question about which observational signal the model should reproduce. The assimilation is not designed to reproduce either product exclusively; the result reflects the observational constraints, their prescribed uncertainties, and the model dynamics..

Following the referee's suggestion, we have added the station-sampled CMEMS in situ surface Chl-a to Fig. 5. Because the in situ data do not uniquely identify either satellite product as the correct seasonal reference, we now interpret the mismatch as reflecting a combination of low-light seasonal-dynamics limitations, retrieval uncertainty, and sampling differences.

We have revised the discussion accordingly and now interpret the mismatch as potentially reflecting a combination of limitations in low-light seasonal dynamics, satellite retrieval uncertainty, and differences in the spatial and temporal sampling of the available observations.

L382. Section 4.2.2 describes Figure 6 but the individual panels of the figure are not pointed thought the paragraph, which makes the explanations a bit difficult to follow.

Response:
We agree. Explicit panel references have been added throughout Section 4.2.2.

The revised text identifies the observational panels, REF bias panels, and differences for each assimilation experiment. Specifically:

"The multi-sensor observations show the seasonal spatial distribution of surface Chl-a in Fig. 6a–d. The corresponding REF biases are shown in Fig. 6e–h. The differences between CHL_SAT and REF, CHL_3D and REF, and CHL_SST and REF are shown in Fig. 6i–l, Fig. 6m–p, and Fig. 6q–t, respectively."

Subsequent statements refer to the relevant panel groups directly.

L421. The same in Section 4.3.1, it seems to describe Figure 7 but where exactly in the figure we can see the facts the authors are describing is not clear.

Response:
We agree. The revised Section 4.3.1 now identifies the relevant panels when describing the observed and simulated vertical Chl-a structures.

The revised text states:

"Assimilating satellite-derived Chl-a (CHL_SAT) markedly improves the simulation of surface bloom dynamics, better constraining bloom timing and magnitude within the mixed layer (Figs. 7i-l). However, the impact of surface observations on subsurface layers is limited by vertical localization. The evolution of the SCM center depth provides a complementary indication of this limitation (Fig. 8). "

L435. There are some figure legends (Figure 8 in L436, and Figure 9 in L466) that include description of results. I would suggest keeping results in the text and leaving in the Figure legend only the description of the Figure.

Response:
Thanks for pointing this out. The captions have been shortened to describe only the figures, with interpretation retained in the Results text..

The Figure 8 caption now reads:

"Figure 8. Simulated chlorophyll-a (Chl-a, mg m^{-3}) temporal evolution of Subsurface Chlorophyll Maxima (SCM) from 2016 to 2019 at stations BY1, BY5, BY15, and BY29. Columns correspond to the monitoring sites and rows to the REF, CHL_SAT, CHL_3D and CHL_SST experiments."

The Figure 9 caption now reads:

"Figure 9. Seasonal mean vertical profiles of Chl-a at monitoring stations BY1, BY5, BY15 and BY29. Shaded regions indicate the variability of the observations, and lines show the corresponding simulation results for the four experiments."

The interpretation previously contained in the captions has been retained in the main text.

L461. It is not clear to me what the authors mean in: "This adaptive capability is further evident in their precise responsiveness to transient hydrodynamic forcing."

Response:
We agree that "precise responsiveness" overstates what is demonstrated by the analysis. The sentence has been replaced with:

"This response is also evident during periods of transient hydrodynamic forcing, when the assimilated profiles allow the simulated SCM to adjust following mixing events. During episodic mixing, such as the early-2018 event at station BY29, the profile-constrained experiments show a downward shift in SCM depth following the mixing event (Fig. 8)."

L481. I guess the sentence "The direct SST constraints in Section 4.1 provide the large-scale physical baseline for the analysis" connects to my comment in L338, where it was not clear how the authors were going to explore the Chl-a induced changes in temperature. Indeed, in this Section 4.4 they explore

CHL_SAT and CHL_3D which is reasonable, but still the meaning of the first sentence of the paragraph is unclear.

Response:
We agree. Section 4.4 has been rewritten to distinguish the direct SST assimilation effect from the indirect thermal response associated with biological assimilation.

The opening of Section 4.4 now states:

"Section 4.1 establishes the direct effect of SST assimilation on the physical state. Here, we instead examine the indirect thermal response associated with biological assimilation by comparing the Chl-a-only experiments (CHL_SAT and CHL_3D) with REF. Because SST is not directly assimilated in these comparisons, differences in light attenuation and temperature can be interpreted as the model-mediated response to changes in biological state. CHL_SST is considered separately as the combined physical–biological configuration and is not used as the primary diagnostic of the isolated Chl-a-induced thermal response."

Figure 10. It is not clear what the authors refer to with deep-water when explaining this figure, because the 0.4 degrees difference seems pretty near-surface to me.

Response:
We agree. The temperature anomaly shown in Fig. 10 occurs in the upper and subsurface water column and should not be described as a deep-water response.

"Deep-water warming" in the Figure 10 caption has been replaced by:

"approximately 0.4 °C subsurface warming relative to the REF run."

The associated discussion now consistently uses "subsurface waters" or "subsurface layer" and reserves "deep water" for the deeper oxygen environment discussed in Section 4.5.

Section 4.5. The description of layers is a bit confusing. The oxic layer is defined both as the 0-40m (L509) and the upper 20m (L527). Deeper waters are both defined as below 75m (L515) and 50–100 m (L528 and L530).

Response:
We agree. The current terminology is inconsistent and could lead to confusion about the vertical domains being analyzed.

The different depth intervals were originally intended to describe different diagnostics rather than to define a single universal boundary for the entire water column. However, this distinction was not sufficiently clear in the original manuscript.

We have therefore clarified the vertical domains at the beginning of Section 4.5 and now use consistent terminology throughout the section. In particular, we distinguish between:

1. **the upper mixed/oxic layer**, used to assess the well-ventilated surface waters;
2. **the intermediate/subsurface layer**, used to describe the transition and subsurface response; and
3. **the deep-water layer**, used to assess the oxygen environment below the stronger influence of seasonal surface processes.

The different numerical depth intervals are now presented explicitly as **diagnostic ranges associated with these different analyses**, rather than as competing definitions of a single oxic or deep-water boundary. The terminology has also been made consistent between the main text, figure caption, and discussion, while the specific numerical depth ranges are retained only where they correspond to the respective diagnostic.

We also add this text (lines 591-594) in the manuscript:

"For clarity, the depth intervals used in this section represent diagnostic ranges for different aspects of the oxygen environment rather than fixed universal boundaries between water-column layers. The upper mixed/oxic layer is considered when assessing the well-ventilated surface waters, while intermediate and deeper ranges are used to examine the subsurface transition and deep-water oxygen environment, respectively."

DISCUSSION

L606. Not quite sure, but the logical order of the two last sentences of this paragraph seems to be the inverse. As I see it: SST defines the stratified domain within which biogeochemical signals can be expressed, but it does not directly generate those signals. Therefore, the added value of assimilating SST lies in its restoration of a physically plausible upper-ocean background…

Response:
We agree with the referee's interpretation.

The sentences of Section 5.2 have been revised:

"In the Baltic Sea, SST assimilation modifies the thermal component of the upper-ocean background within a density structure that is also strongly controlled by salinity. It can thus influence the vertical expression of the assimilated biogeochemical state without directly generating the biogeochemical signal" and "The additional SST constraint therefore modifies not only the thermal field but also the physical background within which the vertical expression of biogeochemical corrections develops."

L611. The discussion about the lack of biological state update in the deeper water column is very interesting. However, it is not clear to me how the authors justify that this is a signal of layer decoupling or layer-specific unrelated processes. The assimilation of surface Chl does not translate so well to the subsurface Chl-a, and to my understanding this does not mean that the processes that lead to the dynamics of Chl-a at surface and subsurface are decoupled, but that subsurface Chl-a is not as controllable by surface Chl-a as it might seem. Maybe the authors could elaborate a bit on why if subsurface Chl-a is not controllable from surface Chl-a, it is expected the deep water dissolved oxygen to be so.

Response:

This is an important conceptual correction, and we agree our original term "decoupling" was too strong and inconsistent with the referee's (correct) framing. We have reframed the argument around observational controllability rather than dynamical independence, and the referee's logic is now the backbone of the revised section: surface and subsurface Chl-a remain physically and biologically connected (mixing, sinking, light, nutrients, stratification, remineralization), but surface observations carry insufficient information to constrain the subsurface state.

This applies *a fortiori* to deep-water oxygen, which is not a simple downstream product of surface Chl-a — its variability is additionally governed by ventilation, basin-scale exchange, benthic oxygen demand, and water-mass renewal history. Precisely because surface Chl-a cannot constrain even subsurface Chl-a, it cannot be expected to constrain the still more remote deep-oxygen state when those controlling processes are themselves unconstrained.

Section 5.3 is retitled "Limited Controllability of the Deep Redox Environment," and the opening paragraph now reads (lines 687–694):

"The nearly invariant mean oxygen profiles in our results (Fig. 11) indicate that the tested surface-focused assimilation configurations have limited ability to modify the deep-water oxygen state. This result should not be interpreted as evidence that surface and deep-water processes are dynamically independent. Surface and subsurface biogeochemical states remain physically connected through mixing, sinking, nutrient availability, stratification and remineralization, while the information supplied by surface Chl-a observations is insufficient to constrain the processes controlling deep-water oxygen. Below the pycnocline, oxygen variability is strongly influenced by ventilation, basin-scale exchange, benthic oxygen demand and the persistence of the halocline, in addition to biological processes."

All prior references to "complete insulation" and "dynamic decoupling" have been removed.

Data availability

According to the data availability policy for BG, all datasets used in a published work must be publicly available in repositories or been made public at the time of publication. I believe listing datasets as “available upon reasonable request” does not complain with the journal policy.

Response

We thank the referee for pointing out the need to clarify the accessibility of the underlying data. We agree that the original statement did not provide sufficient information about the publicly accessible data required to reproduce the reported analyses.

We have revised the Data Availability section to distinguish publicly available third-party datasets from model-derived data generated in this study. The derived model fields required to reproduce the figures and tables, together with the analysis scripts, have been deposited in a public repository and assigned a persistent identifier. The complete model output is not archived because of its large volume; however, the publicly archived derived fields contain the data required to reproduce the reported analyses.

The Data Availability section now states:

“The NEMO–SCOBI code as applied in this study is available from the SMHI GitLab repository upon registration and login: https://git.smhi.se/fouo/nemo3.6/. The data assimilation code is based on Liu and Fu (2018), developed using version 1.11.5 of the Parallel Data Assimilation Framework (PDAF, https://pdaf.awi.de) and was extended to assimilate chlorophyll-a. The river forcing used in this study is described in Ruvalcaba Baroni et al. (2024) and is freely available therein. The quality-controlled observational databases used in the present paper are publicly available from CMEMS, OSI SAF, and SHARK (https://shark.smhi.se). The complete simulation output is too large to be archived in a public repository. However, the model output and processed data required to reproduce the figures and main analyses presented in this study, together with the analysis scripts, have been deposited in Zenodo and are publicly available at https://doi.org/10.5281/zenodo.23435076. The complete simulation output is available from the corresponding author upon reasonable request.”