General comments:

This article is discussing the development and validation of an ocean-bio-geochemistry model customized for the north Indian Ocean. The authors make a good effort to get the simulations done and for the validation, and is publishable. Modelling ocean bio-geochemistry is very difficult and many models still struggle to get the bio-geochemistry right in those simulations. However, I have some concerns, which need to be addressed before it can be accepted.

We sincerely thank the reviewer for the constructive and thoughtful comments, which helped us improve the quality and clarity of the manuscript. We have prepared revisions to address all of these comments. The main reviewer's concern is the bias in surface chlorophyll compared to satellite observations. In our response, we show that our chlorophyll biases are within the range of previously published models, and argue that our model biases in primary production – which is a more robust indicator of biological nutrient and carbon uptake than chlorophyll – are much lower than the chlorophyll biases. The other comments are requests for additional points of evaluation or clarification of the model configuration. We have prepared all additional analysis required to address these comments, and the model performs well on these evaluations. Specifically, we propose to:

- Include an assessment of wind forcing using ERA5 and validate it against CCMP winds and RAMA moorings, along with an expanded discussion and an additional figure (major comment #1).
- Expand the discussion of surface and subsurface chlorophyll biases, comparing model performance with other regional studies (e.g., Sunanda et al., Chakraborty et al.), and presenting depth-dependent validation using bio-Argo data (major comment #2, 3, 8, 9).
- Clarify the oxygen minimum zones evaluation, supported by volumetric and concentration-based diagnostics (major comment #4).
- Clarify the simulation setup, including spin-up duration and equilibration, boundary conditions, atmospheric forcing, as well as references and criteria for rescaling river discharge and assigning riverine lithogenic concentrations (major comment #6; minor comment #9, 12, 14, 16).
- Clarify what we developed vs. what we customized in the model configuration, and add model performance metrics (major comment #6).
- Implement minor edits and clarifications are proposed to enhance precision and consistency throughout the text, as detailed in the point-by-point response (major comment #5, 7; minor comment #1-28).

Please see the detailed response below.

Major comments:

1. I do not see any wind simulation and its validation in the model. Since the monsoonal currents dictate the dynamics and associated processes in NIO, the wind simulations and their assessment are very important and must be presented in the main text.

   Our model is a physical-biogeochemical coupled, ocean-forced model. The atmospheric forcing — including wind, precipitation, and solar radiation — is derived from the 1/4° horizontal resolution European Centre for Medium-Range Weather Forecasts (ECMWF) Reanalysis 5th Generation (ERA5) at 1-hour frequency (Hersbach et al., 2020). ERA5 is widely recognized as one of the highest-quality atmospheric reanalysis products. Numerous studies have employed ERA5 to drive ocean models in the Indian Ocean, capturing key features such as the monsoonal seasonality of ocean waves (Sreelakshmi and Bhaskaran, 2020) and ocean circulation (Vogt-Vincent and Johnson, 2023). See details in section 4.1 in Screelakshmi and Bhaskaran (2020) and sections 4.2-4.3 in Vogt-Vincent and Johnson (2023).

   We evaluated ERA5 winds in the Indian Ocean by comparing them with the Cross-Calibrated Multi-Platform (CCMP) wind product, an observationally constrained dataset derived from satellite measurements, to ensure the robustness of the atmospheric forcing used in our model (new Figure R1). The comparison indicates that the ERA5 wind forcing reproduces the seasonal cycle and spatial distribution of summer and winter monsoons. To assess interannual variability, we further propose to compare ERA5 wind data with in situ observations from two RAMA mooring stations. The root-mean-square errors (RMSE) at these stations are 0.25 m/s and 1.39 m/s, respectively. These relatively small errors indicate that ERA5 forcing captures not only the seasonal dynamics but also the interannual variability of wind fields over the Indian Ocean.

   In addition, Section 4 of the current manuscript assesses the seasonal dynamics of key oceanic and biogeochemical features, including sea surface temperature, upper ocean circulation, coastal upwelling and downwelling, sea surface salinity, river plumes, and plankton bloom. These features are well reproduced in comparison with available data products, providing further evidence that the ERA5 forcing captures the seasonal dynamics of the Indian monsoon system. The details of atmospheric forcing setup was described in Lines 109-110 in the manuscript. We propose to add the wind evaluation using the new Figure R1 in the revised manuscript.

   The proposed clarified text reads: "**In addition, a comparison between ERA5 and CCMP wind products demonstrates that ERA5 wind forcing effectively captures the seasonal cycle and spatial distribution of the summer and winter monsoons (Appendix Figure A6).**"

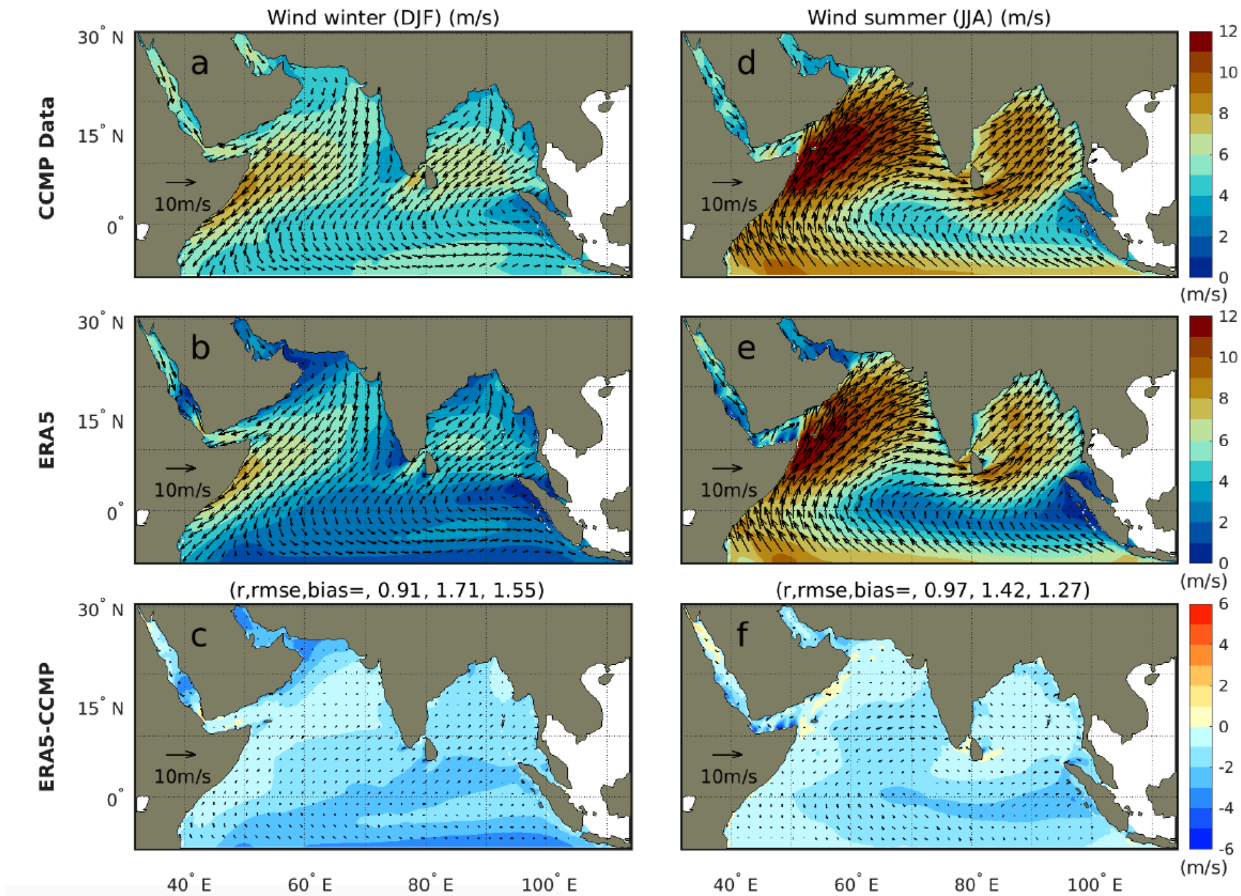

Figure R1. Surface wind (10m) during (a-c) winter (December-February) and (d-f) summer (June-August) monsoons. (a,d) CCMP satellite data, (b,e) ERA5 data product and (c,f) differences between ERA5 and CCMP. Correlation coefficients r, root-mean squared error (rmse) and bias between ERA5 and CCMP seasonal means are indicated. Wind data is from CCMP satellite (see details in Table 2). ERA5 results are averaged over the 1980-2020 period.

2. I thought, generally the models have relatively good simulations for surface Chl-a and compare well with the satellite and Argo data, which is not the case here in your model simulations. It is good to have a discussion based on other model simulations for NIO and other oceanic regions for Chl-a comparisons. Please see this and mention such model simulations and validation: https://doi.org/10.1016/j.ocemod.2024.102419

We emphasize in our manuscript that primary productivity is a better metric of biological production, nutrient and carbon uptake. In this regard, our model performs very well in comparison with observations (Section 4.5 and Figure 10). The seasonal patterns of PP are consistent with satellite-derived estimates and in-situ observations (351 stations). The model captures the magnitude of the double bloom productivity in the central and western Arabian Sea (about 1000-1500 mg C m^{-2} d^{-1} in CAS and WAS), as well as the lower productivity observed in the Bay of Bengal (<1000 mg C m^{-2} d^{-1}, Figure 10 a,b,e,f).

Regarding the chlorophyll bias in our and other models. We carefully reviewed other biogeochemical modeling studies in the northern Indian Ocean (e.g., Sunanda et al., 2024; Chakraborty et al. 2023; Gutknecht et al. 2016). The biases in our model fall within the range of these other studies. Chakraborty et al. (2023) reported a RMSE of ~0.7 mg m^{-3} in the western and eastern Indian Ocean coast, similar to our model which yields a range from 0.62 (winter) to 0.93 (summer) mg m^{-3}. Gutknecht et al. (2016), using the NEMO–PISCES model, reported a domain-mean chlorophyll concentration of 0.53 mg m^{-3} for the Indonesian Throughflow (ITF) region, compared with a MODIS-derived mean of 0.30 mg m^{-3}. Although our model covers a different domain (north Indian Ocean), its mean chlorophyll concentration is 0.44 mg m^{-3}, versus an OC-CCI observational mean of 0.34 mg m^{-3}.

As suggested, we also made a comparison to Argo Chlorophyll showing depth-dependent statistical metrics, including RMSE, bias, and correlation coefficient (see new Figure R2 below). The RMSE between our model and Argo ranges from 0.03 to 0.37 mg m^{-3} ni upper ocean in the Arabian Sea and the Bay of Bengal, which is lower than the RMSE between our model and satellite products listed above, but higher than the RMSE reported for the model of Sunanda et al. 2024 (0–0.1 mg m^{-3}).

The potential sources of the chlorophyll bias were discussed in section 4.5 of our manuscript. Notably, large phytoplankton are characterized by a higher chlorophyll-to-carbon ratio, whereas small phytoplankton have a lower ratio. Given that the model's carbon-based estimates (primary production) align well with the observational data, we propose that the overestimation of chlorophyll arises from an overrepresentation of large phytoplankton, which contribute disproportionately to the chlorophyll signal. However, the strong agreement of our model with observations of PP gives us confidence in the model's representation of biogeochemical dynamics despite moderate biases in chlorophyll.

We propose to expand the chlorophyll assessment and discussion by including Figure R2 in the revised manuscript.

- The proposed revised text in Line 362 reads: "The model also simulates the subsurface chlorophyll maximum captured by Argo floats in both the Arabian Sea and Bay of Bengal, with RMSE values over the vertical ranging from 0.03 to 0.37 mg m−3 (Appendix Figure A5). This suggests that the model effectively represents the vertical distribution of plankton and associated subsurface biological dynamics. Overall, comparison of our model's mean bias and RMSE with values reported in previous studies suggests that our chlorophyll simulation performance falls within the median range relative to other regional biogeochemical models (Chakraborty et al., 2023; Gutknecht et al., 2016; Sunanda et al., 2024)".
- The proposed revised text in Lines 370 reads (new in bolded): "The fact that the model simulates the magnitude of observed PP (in carbon units) but overestimates the surface chlorophyll content suggests that it might overestimate the contribution of large phytoplankton**, which is characterized by a high chlorophyll-to-carbon ratio,**

compared to small phytoplankton**, characterized by a lower chlorophyll-to-carbon ratio**. **This overestimation of the contribution of large phytoplankton to the assemblage would indeed explain the good match in primary productivity and high bias in chlorophyll**".

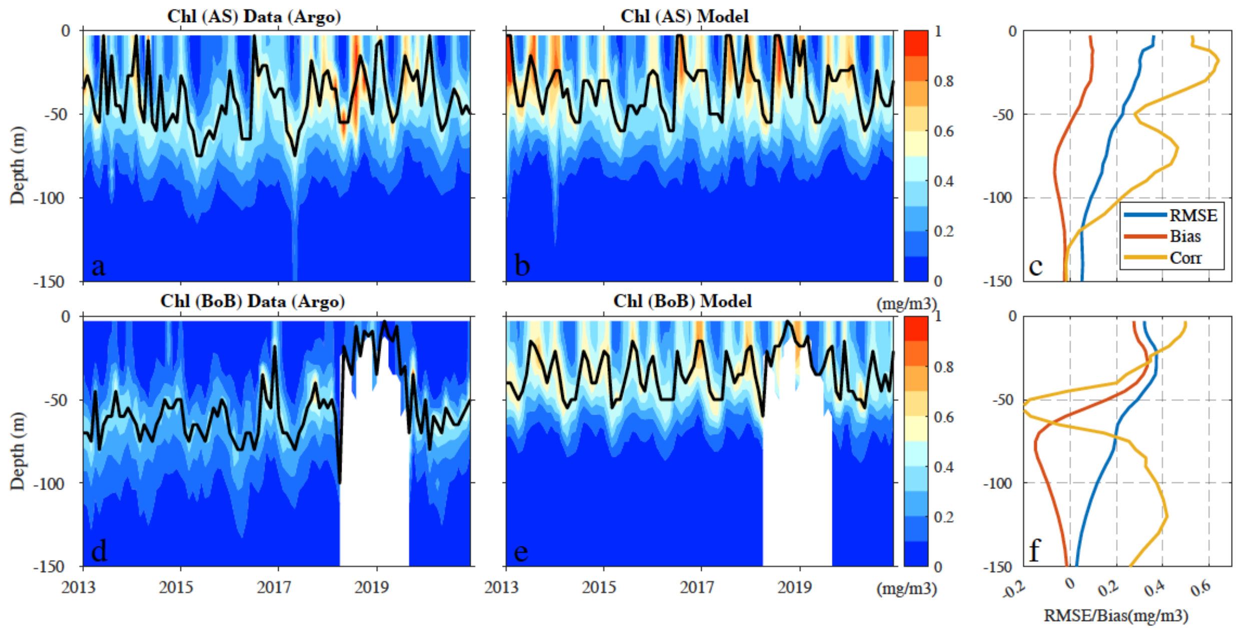

Figure R2. Comparison of observed and modeled vertical chlorophyll profiles in the Arabian Sea (AS, top row) and the Bay of Bengal (BoB, bottom row) using Argo float observations and model output. (a, d) Argo-derived chlorophyll concentrations; (b, e) model-simulated chlorophyll concentrations; (c, f) depth profiles of root mean square error (RMSE), bias, and correlation coefficient between model and Argo observations. In panels a-b and d-e, the black contour line indicates the depth of the subsurface chlorophyll maximum (SCM). Chlorophyll concentrations are shown in mg m^{-3}.

3. There is a subsurface maximum for Chl-a in the NIO. Please show the model simulations and comparison with Argo measurements.

   Thank you for the suggestion to compare the model to Argo. As detailed in comment #2, we propose to evaluate the modeled subsurface chlorophyll structure using Argo float observations in the revised manuscript (Figure R2). See the proposed revised text and figure in major comment #2.

4. L459: If the model overestimates, how that would affect the OMZ in AS and BoB? Which oceanic region has large OMZs? L463, you say that in AS, it is well represented, but BoB overestimated. Fig 16 provides no clue that any basin is better for this. Also, why the equatorial region has such a big difference?

   In observation-based products (WOA18 and Bianchi et al., 2012), the AS features an intense OMZ with subsurface oxygen concentrations around 5 µmol kg^{-1}, whereas the BoB exhibits a much less intense OMZ, with subsurface oxygen concentrations typically between 10–20 μmol kg^{-1}. Figure 16 of our manuscript does compare the OMZ in the AS and BoB in products and the model. Panel a shows maps of O2 in model, products and the model bias. Panel b compares the OMZ volume in products and model for the full basin, the AS and the BoB for different oxygen thresholds (e.g., suboxia is defined by 5 umol/kg but other thresholds such as hypoxia are relevant for organisms etc.).

As described in section 5.2, the oxygen bias is higher in the BoB (approximately –10 to –20 μmol kg^{-1}) than in the AS (approximately –10 to +10 μmol kg^{-1}, Figure 16a). Additionally, we find that at the basin scale the model reproduces the volume of hypoxic (<60 μmol kg^{-1}) and low-oxygen (<100 μmol kg^{-1}) waters (Figure 16b). However, it overestimates suboxic waters (<5 μmol kg^{-1}), mainly due to the overprediction in the Bay of Bengal. This is visible in the lower right panel of Figure 16 when comparing the products (blue and red lines) to the model (black lines) at oxygen values of 5 umol/kg. We propose to clarify the description of Figure 16 in section 5.2 as follows: "In contrast, the model overestimates the volume of suboxic waters delimited by 5 umol kg-1 (0.17×1016 m3 vs. 0.06×1016 m3 in Bianchi et al. (2012) observations), mostly because of the large suboxic volume simulated in the Bay of Bengal (0.10×1016 m3 vs. 0.00×1016 m3 in observations, Figure 16b)".

We also propose to expand the results to include the western equatorial bias in section 5.2. It reads as: "In this region, the model shows a high oxygen bias near the base of the thermocline (500–1000 m), coinciding with a low nitrate bias (not shown). This pattern points to either a misrepresentation of biological remineralization at depth or an inaccurate representation of the relative contribution of the water masses forming the Central Waters supplying oxygen to this region. These waters originate from a blend of the Indonesian Throughflow (ITF) and southern-sourced Mode Waters. Previous studies have demonstrated that oxygen distribution in this region is highly sensitive to the relative contribution between these two sources (DItkovsky et al 2023). However, the scarcity of direct observations in this region limits our ability to conclusively attribute the model bias to either mechanism."

However, recent studies indicate an uncertainty regarding the true extent and severity of hypoxia in this region (Bhaskar et al., 2021, Bristow et al., 2017). We therefore also propose to expand the discussion of the model oxygen bias in section 8 (Discussion and Conclusions) by adding the following text: "Despite these model limitations, it is important to note that uncertainties remain regarding the strength of suboxia in the Bay of Bengal. Recent observations from Argo floats and ship-based in situ measurements have reported lower oxygen concentrations in the BoB than those presented in the WOA dataset, including nanomolar-level oxygen conditions (Bristow et al., 2017; Bhaskar et al., 2021). These findings suggest that the true extent and intensity of hypoxia in the BoB remain uncertain, making it difficult to definitively assess the magnitude of the model bias in this region."

5. Write the validation information with bias values in the abstract

We propose to revise the abstract (Lines 12-13) as follows: "**Quantitatively, the model exhibits relatively small biases, as reflected by root mean square error (RMSE) values in key variables: surface temperature (0.25–0.30 °C), mixed layer depth (7–8.09 m), sea level anomaly (0.02 m), sea surface salinity (0.53–0.71 psu), vertical chlorophyll (0.03–0.3 mg m^{-3}), subsurface temperature (0.33 °C), and subsurface salinity (0.07 psu)**".

6. Developed or customized the model, please make sure that you use a correct word for this

   The global MOM6-COBALTv2 model was originally developed by GFDL scientists including coauthors in this paper. However, regional capabilities are new. We have developed the regional Indian Ocean configuration and customized the MOM6-COBALTv2 model by modifying several parameters (e.g., detritus sinking velocity, burial fraction, oxygen half-saturation for nitrification, oxygen constraint on water column denitrification), as well as rewriting portions of the input and output Fortran routines within regional MOM6-COBALTv2 to better suit the specific needs of our regional configuration and biogeochemical applications. This is not merely a simple customization; rather, we are deeply engaged in developing and refining the model to address bugs and enhance its functionality as a regional biogeochemical modeling tool.

   - The proposed revised text in Lines 63-66 reads: "It is with these applications in mind that we **configured, customised and validated** the regional Indian Ocean simulation presented here based on the Modular Ocean Model 6 (MOM6, Adcroft et al., 2019) coupled with the Carbon, Ocean, Biogeochemistry, and Lower Trophics module version 2.0 (COBALTv2, Stock et al., 2014, 2020)."
   - The proposed revised text in Line 569 reads: "We **configured, customised and validated** a regional ocean biogeochemical model at 1/12◦ horizontal resolution (MOM6-COBALT-IND12 v1.0) that captures most key features of the northern Indian Ocean dynamics."
   - The proposed revised text in Line 589 reads: "During the **setup and customization** of the MOM6-COBALT-IND12 v1.0 model, we identified a series of physical and biogeochemical parameters and forcings that influenced the model simulation and led to a significant improvement of the results (see details in section 2)."

7. L336: reproduced is a "lighter" word; how good is the comparison? Please write some numbers.

   We propose to revise as suggested. The comparison is already quantified in the text, with a regional correlation coefficient ($r > 0.95$) and RMSE < 0.7, as also shown in Figure 8. We propose to revise the text to present these metrics more clearly: "Performance metrics indicate that the simulation achieves **a strong spatial correlation ($r > 0.95$) and a small regional RMSE (0.53-0.7)**".

8. LK353: Why the Chl-a simulation is not good in the Somali coast or western Indian Ocean? Summer Chl-a is even worse?

9. L371-372: How did you arrive into this conclusion; is this about the size of the plankton?

These two comments are related to Comments #2 and #3 and we answer them together here. The potential sources of chlorophyll bias are discussed in detail in our responses to comments #2 and #3 and were also discussed in Section 4.5 of the manuscript. Large phytoplankton has a higher chlorophyll-to-carbon ratio and the model likely overestimates the contribution of this group to the assemblage. Again we want to emphasize that the model simulates primary production comparable with observations (Figure 10), which is a more important metric than chlorophyll for constraining nutrient, carbon and oxygen cycling.

The larger bias in the Somali Current, and summer specifically, is likely related to enhanced upwelling during this season, which brings nutrients to the surface. In addition, the strengthened coastal current driven by the southwest monsoon may further transport nutrients into the western Indian Ocean. This elevated nutrient availability facilitates larger phytoplankton growth and higher Chl content in the model. See the proposed revised text and figure in major comment #2.

Minor comments:

1. L4: north of 8S? It can be anywhere north of that latitude. Please be specific

   We propose to revise as suggested: "The model covers the northern Indian Ocean (**from 8°S to the northern continental boundaries**), central to the livelihoods and economies of countries that comprise about one-third of the world's population."

2. L22: and is missing

   We propose to revise as suggested: "The northern Indian Ocean is central to the livelihood and economy of about one third of the Earth's population which live in its littoral countries (e.g., India, Indonesia, Pakistan, Bangladesh, Tanzania, Myanmar, Malaysia, Kenya**, and** Yemen) ".

3. L23; separate the Roy citation from the bracket

   We propose to revise as suggested: "... provides valuable resources via the "blue economy", such as fishery, aquaculture, **marine tourism (Roy, 2019)**".

4. L40: about the NIO stressors: https://doi.org/10.1016/j.pocean.2023.103164

   We propose to add further discussion on stressors affecting the northern Indian Ocean under climate change, and cite two relevant studies: Sunanda and Chakraborty (2023) and Sunanda et al. (2021).

   The revised text in Line 45 will read: "Projections from Coupled Model Intercomparison Project (CMIP) models suggest substantial shifts in net primary production (NPP) and sharp declines in pH in the coming decades, **highlighting the North Indian Ocean's particular vulnerability to climate change (Sunanda et al., 2021; Sunanda and Chakraborty, 2023)**."

5. L53: models are "tools" for studying

   We propose to revise as suggested: "Models are powerful **tools** for exploring the Indian Ocean".

6. L55: this is another model validation for this region: https://doi.org/10.1016/j.ocemod.2024.102419

   We propose to revise as suggested and cite one more model work in the revised manuscript. The proposed revised text will read: "... assess the impacts on biogeochemistry and ecosystems (e.g., Sengupta et al., 2001; Rahaman et al.,2014; Lachkar et al., 2018, 2019; Resplandy et 55 al., 2011, 2012; Schmidt et al., 2021; Ditkovsky et al., 2023; **Sunanda et al., 2024**)."

7. L85: coordinates of the region

   We propose to revise as suggested: "MOM6-COBALT-IND12 is therefore considered an 'eddy resolving' model for the region **with a rectilinear and orthogonal grid (32°E to 114°E and 8.6° S to 30.3°N)**".

8. L113: salinity from 1998 data, any updated version?

   The salinity restoring data used in this study is PHC2.1, released in 2002, which remains the version currently used by the Geophysical Fluid Dynamics Laboratory (GFDL), NOAA. A newer version, PHC3.0, became available in 2005. We plan to update the salinity restoring data to PHC3.0 or a similar newer product, such as WOA 2023, in future simulations.

9. L115: How long was the spin up and when did the model stabilize? Which year onward you analyse the model results for science?

   We propose to clarify the spin-up stabilization and hindcast simulation by rewriting section 2.2.1 of the revised manuscript (modified text in bold):

   "**The ocean model was initialized using temperature and salinity from annual mean fields from the World Ocean Atlas version 2013 (WOA13, Locarnini et al., 2014; Zweng et al., 2014). Our simulations were run using the atmospheric forcing from the 1/4° horizontal resolution European Center for Medium-range Weather Forecasts reanalysis 5th generation (ERA5) at 1-hour frequency (Hersbach et al., 2020).** The sea surface salinity (SSS) was restored to the polar science center hydrographic climatology (PHC2.1), which is based on the World Ocean Atlas 98 with data replenishment in the Arctic Ocean (Steele et al., 2001), with a piston velocity of 0.1667 m d−1. **We conducted a 32-year spin-up, which was achieved by looping four consecutive 8-year loops of the 1980 to 1987 forcing field and reached a well-equilibrated state with minimal linear trends of physical and biogeochemical variables (e.g., drift in sea surface temperature, SSS, oxygen, nitrate, primary production and ocean surface partial pressure of carbon dioxide pCO_2 < ~0.1% for model years 17-32). Using outputs from the end of the spinup simulation as initial conditions, the hindcast simulation was started on January 1 1980 and was run from 1980 to 2020 for our analysis in this study**."

   The minimal model drift in sea surface temperature, SSS, oxygen, nitrate, primary production and pCO_2 for model years 17-32 of the spin-up is shown in Figure R3 below for your reference.

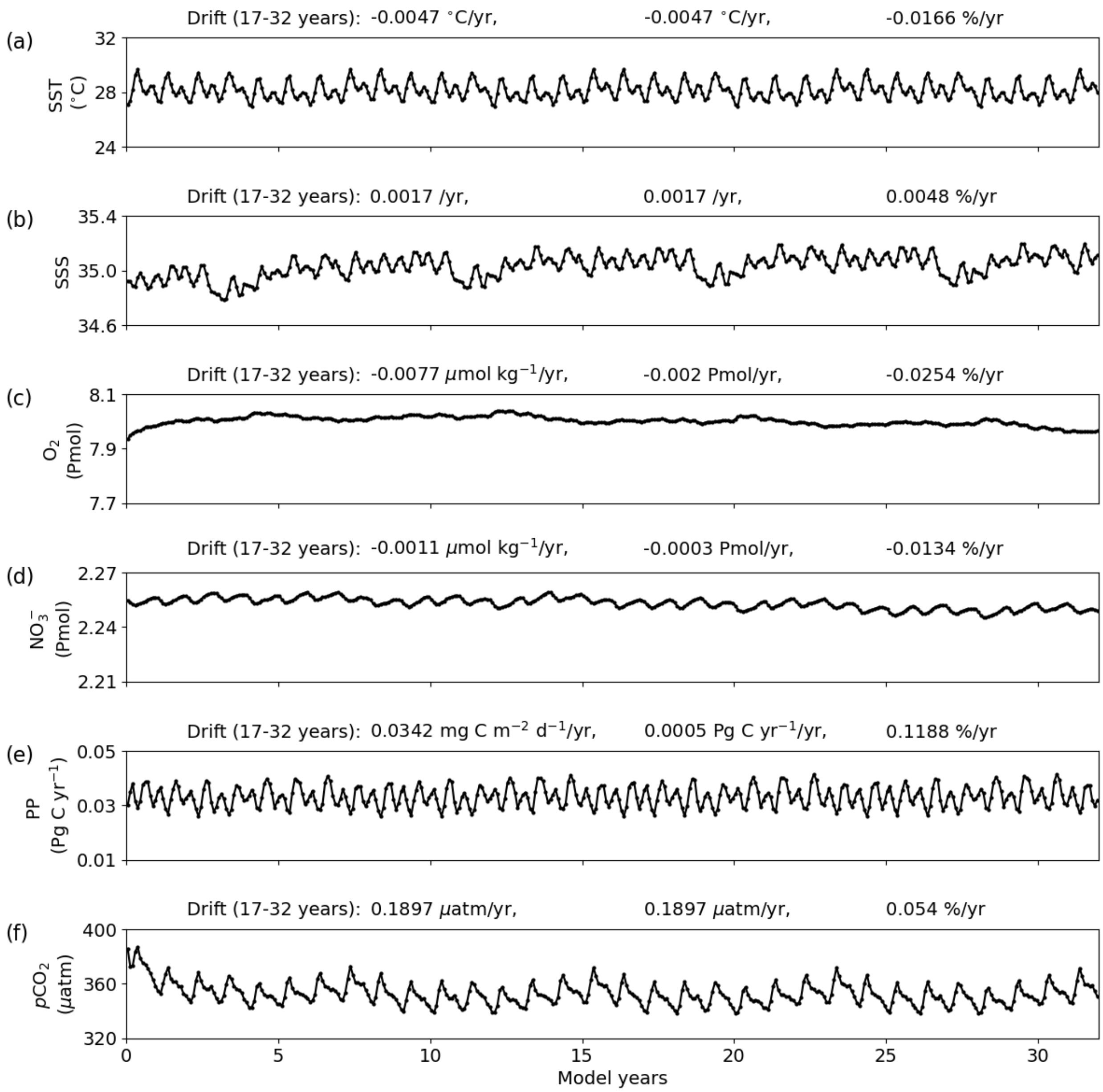

Figure R3. Time series of domain-mean model variables during the 32-year spin-up forced by repeated atmospheric and open boundary conditions looping over 1980–1987. Panels show: (a) sea surface temperature (SST), (b) sea surface salinity (SSS), (c) oxygen (O_2), (d) total nitrate (NO_3^-), (e) primary productivity (PP), and (f) ocean pCO_2. Drifts over model years 17-32 are indicated above each panel and are all < ~0.1%

10. L119: citation format is not correct

We will correct the format. The proposed revised text reads: “Open boundary conditions (OBC) are set using the Flather formulation for the tidal and sub-tidal sea level and barotropic velocity and the Orlanski formulation for the baroclinic velocity (**Flather, 1976**; Orlanski, 1976)”.

11. Figure 1: rivers can be in red color, to differentiate from the bathymetry blue color

    We propose to revise as suggested. The updated figure is shown below.

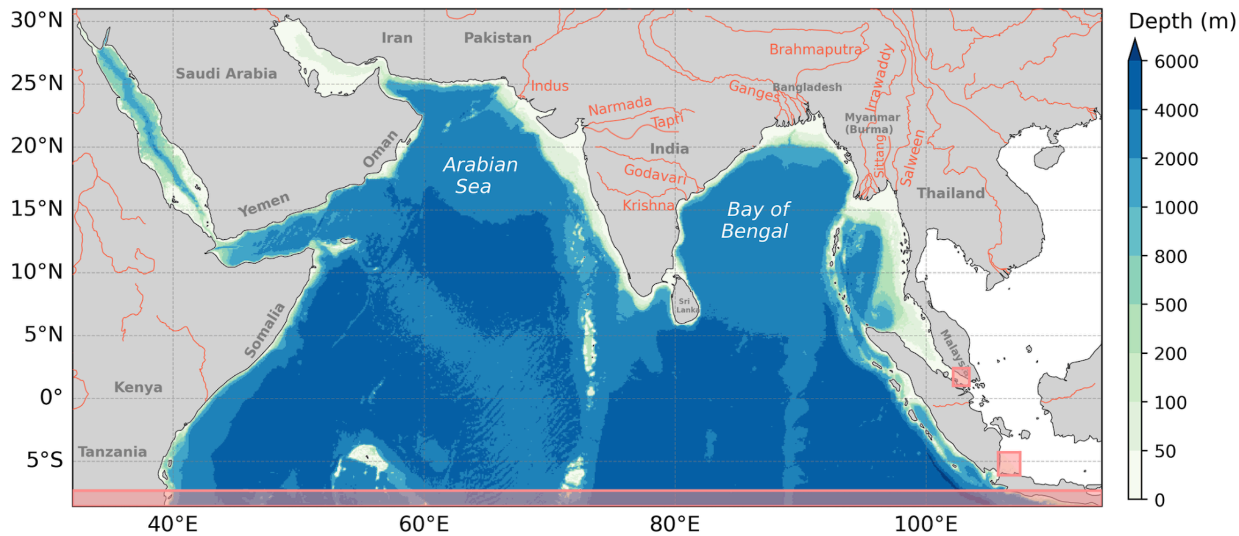

Figure R4. Domain and bathymetry of the regional Indian Ocean MOM6-COBALT-IND12. Pink shading indicates the extent of sponge layers (see methods). Major rivers are indicated in red.

12. L147: any reference for this? Overestimation and scaling have got any criterion? Why 25%?

    The overestimation and subsequent scaling of river discharge are based on our evaluation of the GloFAS dataset, as shown in Appendix Figure A4 in the current manuscript and discussed in section 2.2.3. We compared the river discharge from GloFAS with observational estimates for the Ganges–Brahmaputra river system reported by Jian et al. (2009). To address this bias and improve the realism of the freshwater input, we applied a 25% reduction to the GloFAS-derived discharge for the Ganges–Brahmaputra river system (see Figure A4). We also corrected the Irrawaddy-Sittang river system using a similar approach and data from the Global Runoff Data Center (Recknagel et al., 2023). These references are already cited in the original manuscript (Line 145 and Table 2) but we will also add them in Line 147 to improve clarity in Section 2.2.3.

    The proposed revised text reads: "By comparing GloFAS to published discharge observations (Jian et al., 2009; Siswanto et al., 2023), we found that GloFAS overestimated discharge in the Ganges-Brahmaputra river system, and therefore scaled down the freshwater discharge by 25% to match observations in these two rivers (see Appendix Figure A4, Jian et al., 2009, Siswanto et al., 2023)".

13. L175: not from WOA 2023?

The nutrient and oxygen fields were initialized using data from the World Ocean Atlas (WOA) 2018, as the project began before the release of WOA 2023. We plan to incorporate the updated WOA 2023 fields in future model simulations.

14. L176: CO2 is increasing, so the old climatology values are good?

The initial DIC field used for the model spin-up is derived from the GLODAP climatology, which reflects approximately the year 2002 conditions, higher indeed than the 1980 conditions of our initial state. However, the 32-year spin-up with repeated forcing fields from 1980–1987 (air CO_2 levels and DIC concentration at open boundary conditions are also from 1980-1987) yielded a stabilized surface ocean pCO_2 lower than its initial values, toward the end of the spin-up period with a linear trend of ~0.05% (see Figure R3 above), indicating minimal drift. We propose to clarify this point in section 2.4.1 of the revised manuscript when describing the initialization of the model spin-up and model drift (added text in bold):

"**For the model spin-up**, nutrients (nitrate, phosphate, and silicate) and oxygen were initialized using annual means from the World Ocean Atlas 2018 (WOA18, Garcia et al., 2019). DIC and alkalinity were initialized using annual means from GLODAPv2 **which are representative of year 2002** (Olsen et al., 2016). … Model drift after the 32-year spin-up and over the 41 years of a control simulation with constant forcing is small, with linear trends < 0.05% for oxygen, nitrate, DIC, alkalinity, semi-refractory dissolved organic nitrogen pools and integrated primary productivity (see Appendix Figure A1). **The slight drift indicates that the hindcast simulation starts from a well-equilibrated initial state provided by the spin-up simulation**."

15. L192: SSP 5-8.5 is an extreme case. So how much that would influence your simulations?

The atmospheric deposition data were taken from the GFDL ESM4.1 historical simulation for the period 1980–2014 and from the SSP5-8.5 scenario of the same model for the period 2015–2020. We selected the SSP5-8.5 pathway because it is regarded as a "business-as-usual" scenario, and our current society did not largely reduce carbon emission in the past few years (i.e., 2015-2020). Therefore, it is reasonable to still consider the past few years as the "business-as-usual" scenario. This is the reason we select the SSP5-8.5 data for the period 2015-2020.

In addition, we assess the consistency between the historical and scenario-based datasets. As shown in Figure A2 in the current manuscript, there is no noticeable discontinuity between the two periods in either magnitude or trend of nitrate deposition. Therefore, the use of SSP5-8.5 for the 2015–2020 period does not introduce artificial discontinuities and is not expected to significantly influence the model simulation results.

16. L223: How the adjustments are made? Just random or any criterion followed?

The adjustments to riverine lithogenic concentrations are described in the manuscript (Lines 226-228) and the supporting observational dataset (Milliman and Farnsworth, 2011) is listed in Table 2 under the "Riverine lithogenic flux" entry. We will clarify this point in the revised manuscript at the beginning of the lithogenic input description by adding the following sentence (**added text in bold**): "**To reflect spatial differences in sediment supply, we specify riverine lithogenic concentrations based on observational data from Milliman and Farnsworth (2011):** the lithogenic input from rivers was adjusted to 200 g m^{-3} for major rivers (i.e., rivers with sediment loads exceeding 10 Mt y^{-1}, e.g., Godavari, Krishna, Ganges, Brahmaputra, Irrawaddy, Sittang, Salween, Indus, Tapti and Narmada rivers, see Figure 1 for rivers location) and 20 g m^{-3} for all other rivers, rather than applying a global constant of 13 g m^{-3} used for all rivers in Stock et al. (2020). These adjustments account for the significantly higher total suspended sediment loads in these rivers (Milliman and Farnsworth, 2011; Rixen et al., 2019b), and are supported by river observations from Milliman and Farnsworth (2011) showing a broad range from 10 g m^{-3} (Muvattupuzha River) to 1,061 g m^{-3} (Ganges River)".

17. L243: citation format is not correct

    We propose to edit as suggested: "temperature and salinity from the World Ocean Atlas 2018 (WOA18, **Garcia et al., 2019**)."

18. L274: SST has been already defined

    We propose to edit as suggested: "Patterns of **SST** in the northern Indian Ocean follow the well described basin-scale features."

19. L276: particularly and especially, Please rephrase

    We propose to edit as suggested: "MOM6-COBALT-IND12 captures the seasonal SST patterns **well, notably** the contrast between the vast warm pool (SST >28°C) covering most of the basin and the colder SST regions that emerge in response to seasonal variations in atmospheric and oceanic circulation (Figure 3)."

20. Figure 4: Why summer MLD is bad in the model?

    We propose to expand the evaluation of winter and summer MLD in section 4.1 by adding the following paragraph: "The model captures the seasonal contrast in mixed layer depth (MLD) between the Arabian Sea and the Bay of Bengal, with deeper mixed layers in the Arabian Sea and shallower layers in the Bay of Bengal during both winter and summer (Figure 4). The MLD is generally deeper in summer than in winter. The spatial patterns, including the locations of local MLD maxima, are broadly consistent with observational data. Quantitatively, the basin-wide correlation values are similar between the two seasons, although the RMSE is larger in summer (8.09 m) than in winter (7.00 m). One possible contributor to the larger summer bias is the enhanced wind forcing during the monsoon season (see Figure A6), which intensifies turbulent mixing and deepens the mixed layer. At the same time, the MOM6 model includes the mixed layer eddy (MLE) parameterization of Fox-Kemper et al. (2011), which represents restratification driven by baroclinic eddies within the mixed layer. This restratification process may also be more active in summer, potentially leading to an overcorrection that offsets vertical mixing too strongly. The interaction between intensified wind-driven mixing and enhanced restratification may thus contribute to the larger MLD bias observed in summer compared to winter."

21. L337: SSS has been defined already

    We will remove and edit as suggested: "The model reproduces the main observed patterns of SSS, including the high **SSS** (>34) in the Arabian Sea".

22. L350: Narmada-Tapti

    We will correct as: "The GloFAS runoff product captures the discharge of one of the main river systems for which we have direct observations, i.e., the **Narmada-Tapti** rivers."

23. Fig 17: How that affects simulations of SLA?

    As shown in Figure 17, the model underestimates the amplitude of SSH variability in the 14–120 day/cycle range compared to AVISO observations. This frequency band corresponds to intraseasonal, including Kelvin and Rossby waves related to high-frequency atmospheric forcing responses and other ocean processes. As a result, the magnitude of sea level anomalies associated with these waves may be underestimated, and their influence on the biogeochemical system could be underrepresented in the simulations.

    Nevertheless, the model still performs well in reproducing most of the key variabilities in both physical and biogeochemical processes. As demonstrated in Section 4.3 and Figure 7, the model captures the seasonal cycle of sea level anomalies with high fidelity, yielding a strong correlation ($r = 0.93$) and low RMSE (0.02 m) relative to observations. Therefore, while the model underestimates SLA variability in the high-frequency band, this has only a minor impact on the overall accuracy of SLA simulation, which is largely governed by lower-frequency components.

24. L495-500: remarkably well? Not sure, if you look at the SLA figure.

    We propose to rephrase the text to ensure consistency between the descriptions of the SLA and chlorophyll simulations. Specifically, we propose to rephrase the sentence as the model "reproduces the fine-scale features structuring the winter and summer blooms," rather than describing the performance as "remarkably well". The revised text will read: "The model **reproduces the fine-scale features** structuring the winter and summer blooms".

25. L514: IOD has been defined, as for L528: RAMA, OISST

    We propose to remove and edit as suggested.

    - L514, the revised text will read: "The model reproduces the amplitude and zonal pattern of SST changes expected in response to the Interannual **IOD**". Line 530 in the revised manuscript.

- L528, the revised text will read: “We use observations from the in-situ **RAMA** that we complement with **OISST** data and Argo float-based thermocline depth.” Line 543 in the revised manuscript.

26. 579-580: model is good because of its good bio-geochemistry simulations? What about the model physics?

    The text here intends to emphasize that the model’s strong physical performance provides the foundation for accurately simulating ocean biogeochemical and biological responses. The physical component of the model has been thoroughly evaluated, as detailed in Sections 2–7, which assess circulation variability, meridional temperature and salinity profiles, seasonal and intraseasonal sea level variability, and interannual SST variability. The well physical model performance supports its capacity to accurately represent the biogeochemical and biological variabilities.

    We propose to rephrase the text for clarity: “The good agreement between observed and modeled physical features **provides a foundation for accurately simulating** the ocean biogeochemical and biological response”.

27. L584: a detailed account of winter blooms are here: https://doi.org/10.1016/j.jenvman.2023.117435

    We propose to revise as suggested. We plan to cite the suggested winter bloom work in the revised manuscript. The proposed revised text will read: “Specifically, the model reproduces the summer bloom associated with coastal upwelling systems and their extension offshore in mesoscale filaments, as well as the winter bloom associated with convective mixing and modulated by fine-scale eddies (Lévy et al., 2007; Resplandy et al., 2011, 2012; Mahadevan, 2016; Lachkar et al., 2016; Rixen et al., 2019a; Vinayachandran et al., 2021; **Anjaneyan et al., 2023**)”.

28. L588: different response? please be specific

    We propose to specify the detailed responses reported in Wiggert et al. (2009). The proposed revised text will read: “This includes the modulation of the production in the equatorial region, the Arabian Sea and around the tip of India, although we note these patterns are difficult to generalize to all IOD events. **As illustrated by Wiggert et al. (2009) who showed the chlorophyll responses vary with IOD intensity in most regions, such as the eastern and western Indian Ocean and the Bay of Bengal. Notably, chlorophyll concentration in the Arabian Sea decreased during the 1997 event but increased during the 2006 event**”.

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