RESPONSE TO REVIEWER 1

I thank the reviewer, Dr. Anh Pham, for the insightful comments and suggestions, which have helped me to improve both the present and the previous version of the manuscript. I have provided responses to the comments; the reviewer comments **(RC)** appear as normal font, my response (**AR)** in *italics* below the respective comments and I have used *blue italics* to quote the changes in the revised manuscript.

RC: In this article, Dr. Banerjee performed a suite of computer simulations in a relatively complex ocean biogeochemistry model, which includes a state-of-the-art ocean iron (Fe) cycling scheme, to quantify for the relative roles of different sources of dissolved Fe (dFe) on controlling the dFe budget, primary productivity, phytoplankton composition, and nutrient limitation in the northern Indian Ocean (IO). By comparing results of different simulations in which a certain external source of dFe is removed with results of a simulation in which all dFe sources are considered, the author showed that atmospheric deposition is the most important source of dFe to the dFe budget and phytoplankton growth in the upper northern IO. Sedimentary dFe release plays a secondary role and is locally important near the continental shelves and in the southern tropical IO, while the impact of dFe fluxes from hydrothermal vents and river discharges on the upper northern IO biogeochemistry is negligible. More importantly, by analyzing the nutrient limitation status in the northern IO through these model simulations, the author suggested that phytoplankton growth is most sensitive to external sources of dFe in regions where the background dFe concentration is low and the nitrate-to-Fe ratio is high. In those regions, the increase in phytoplankton growth when additional source of dFe is considered is driven mostly by an increase in diatoms. Finally, by analyzing the dFe budget over five biophysical regimes in the northern IO (the western Arabian Sea, the northern Arabian Sea, the southern Bay of Bengal, the central Equatorial IO, and the central southern tropical IO), the author demonstrated that in the surface ocean, vertical mixing is the most important physical mechanism supplying dFe throughout the year. At the subsurface levels, the dFe budget is balanced through scavenging and remineralization processes.

In my opinion, these results while not surprising are still important for understanding ocean Fe cycling in an important ocean region where primary production is high, biogeochemical cycles of various chemical elements are linked, the ocean circulation is highly dynamic, and both biogeochemical and physical processes are sensitive to climate variabilities and global warming. I also find the analysis of the dFe budget over different biophysical regimes insightful. Besides, the manuscript is well-written and easy to follow.

As a reviewer of the previous version of this manuscript, I am also happy that my suggestions are taken into consideration and thoroughly addressed by the author in this version. I am happy to endorse its publication with a few questions/comments for clarification below:

AR: *I greatly appreciate the positive evaluation of the manuscript. My responses to the specific comments follow.*

RC: Lines 127-129: How are iron and other tracers initialized in the model?

AR: *Ecosystem tracers, including iron, chlorophyll, dissolved organic/inorganic carbon are initialized from a previous simulation using CESM1. Temperature and salinity are initialized from January-mean* *values obtained from the Polar Science Center Hydrographic Climatology (Levitus et al., 1998). This information is included in the revised version of the manuscript.*

RC: Lines 147-148: I am surprised that the Fe fluxes from river are quite small even though the author assumed a high constant concentration of dFe in rivers (10nM - line 166)

AR: *This is an interesting point indeed. The impact of dissolved Fe (DFe) from river is mostly concentrated in the fresher water within the upper 30 m of the water column to the north of 21°N over the Bay of Bengal. Within this limited region surface concentration of DFe from river exceeds 1 nM throughout the year. The contribution of river-DFe to total surface DFe is ~60% near 23°N, but quickly reduces to ~10% in the vicinity of 20°N latitude. This is probably related to quick scavenging loss of DFe from river. Additionally, there is a strong seasonality to river discharge, leading to river-derived DFe peaking during March and November. Together, this leads to an overall low contribution of river DFe to the open ocean. I have added the following sentence in the revised version of the manuscript to make this clear: "River sources contribute negligibly to total DFe concentrations (Fig. 4d), except in the immediate vicinity of the mouths of large river systems in the northeast BoB: the Ganges-Brahmaputra and the Irrawady-Sittang-Salween. This can arise from the fact that DFe from river is mostly concentrated within the fresher upper 30 m of the water column to the north of 21°N over the BoB and also due to high scavenging losses of iron at the river mouth."*

For the reference of the reviewer, I am also providing a zoomed diagram of DFe concentration and scavenging from river source along 91°E longitude, which shows how DFe from river is quickly lost near the river mouth.

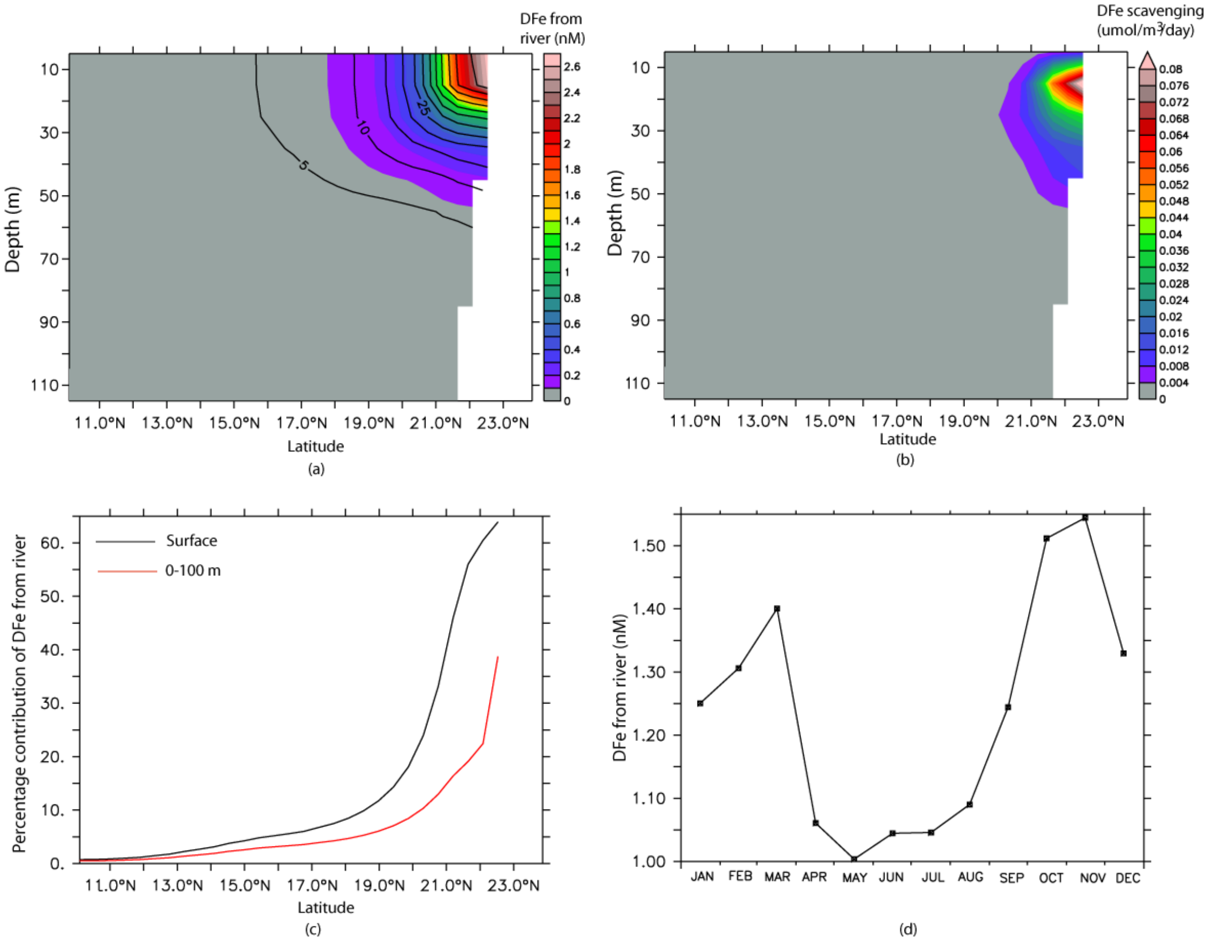

Fig. 1. (a) *Shading shows DFe concentration only from river source along 91°E longitude averaged for last 10 years of CESM simulations. The black contours are the percentage contribution of DFe from rivers to total DFe concentration.* ***(b)*** *Scavenging loss of DFe from river source along 91°E longitude.* ***(c)*** *Percentage contribution of DFe from river to surface (black curve) and 0-100m averaged (red curve) DFe concentration along 91°E.* ***(d)*** *Seasonal cycle of DFe concentration from river averaged over 21-23°N latitude and 90-93°E longitude.*

RC: Lines 161-163: What are the constant low background fluxes here? What is its value?

AR: *The constant low background flux can be obtained from CESM-MARBL Github code repository (https://github.com/marbl-ecosys/marbl-forcing/) as:*

fesedflux_oxic (μmol m^{-2} d^{-1}*) = coef_fesedflux_current_speed2 X sedfrac_mod X* $current\_speed^2$

*where, coef_fesedflux_current_speed2 = 0.0006568 * 1.2*
sed_frac_mod= fraction of each cell that is ocean bottom at each depth
current speed = bottom current speed

However, I would like refrain from placing this in the main manuscript until I come across publications from the model developers giving more information on how this coefficient has been derived.

RC: Line 177: What is the value for the constant desorption rate?

AR: *The value for the constant desorption rate for scavenged Fe from particles is 1.0 X* 10^{-6} cm^{-1}*. This is now included in the revised version of the manuscript.*

RC: Lines 259-260: Is this underestimation an implication that iron limitation here is not strong enough in the model?

AR: *Yes, I agree that underestimation of nitrate along with overestimation of DFe likely leads to, in general, weaker iron limitation in the model than in observations. I am including a line in the revised manuscript to indicate this: "To summarize, the ocean component of CESM has deeper MLD than observations, underestimates nitrate and chlorophyll, and overestimates DFe concentrations. Together, this can result in weaker iron-limitation in the simulations compared to observations."*

RC: Lines 831-834: I think Fe release from low-oxygen sediments is also vulnerable to global warming since the ocean oxygen level is a function of many biogeochemical and physical processes which are bound to change.

AR: *This is a good point and I agree with the reviewer. I am including the following sentences in the "Conclusion" section of the revised manuscript to indicate the importance of iron from low-oxygenated sediments:" Additionally, 59% of the continental shelves and bathyal sea floor over the northern IO experiences hypoxic conditions (Helly and Levin, 2004) and there are several lines of evidence pointing to reductions in oxygen content over this region during the last few decades due to enhanced upper ocean stratification (Schmidtko et al., 2017). This will possibly impact the flux of iron from reduced sediments. The present study thus provides foundations to explore how different future scenarios of atmospheric deposition and the extent of reducing sediments can impact biogeochemistry over the northern IO."*

Anh Pham

Additional references

Helly, J. J. and Levin, L. A.: Global distribution of naturally occurring marine hypoxia on continental margins, Deep-Sea Res. Pt. I, 51, 1159–1168, 2004.

Levitus, S., T. Boyer, M. Concright, D. Johnson, T. O'Brien, J. Antonov, C. Stephens, and R. Garfield: Introduction, Vol. I, World Ocean Database 1998, NOAA Atlas NESDIS 18, 346 pp, 1998.

Schmidtko, S., Stramma, L., and Visbeck, M.: Decline in global oceanic oxygen content during the past five decades, Nature, 542, 335–339, https://doi.org/10.1038/nature21399, 2017.