Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-300-RC1, 2018

Biogeosciences Discussions
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Interactive comment on "Vertical distribution of chlorophyll in dynamically distinct regions of the southern Bay of Bengal" *by* Venugopal Thushara et al.

Anonymous Referee #1

Received and published: 13 August 2018

Review of "Vertical distribution of chlorophyll in dynamically distinct regions of the southern Bay of Bengal" by Thushara et al.

Based on an observational campaign in the southern Bay of Bengal authors have tried to document the bio-physical interactions, particularly for the evolution of surface/subsurface chlorophyll blooms, in this region during the summer monsoon. They have also used an OGCM to explain the dynamical processes relating to the nitrate limitations for the chl concentration. Considering the data sparsity in the Bay of Bangal, particularly for the biogeochemical data, this manuscript certainly contributes to enhance the existing literature of this region. However, I often find statements made

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in this manuscript are not well supported by the figures. Below, I have listed some of them.

Also, I have serious doubt about the application of the model, particularly because spin-up time for the biogeochemical model is only 10 years, which is way too small for the nutrient levels to be stabilized. I believe, for such a basin scale TOPAZ, a minimum 30-50 years of spin-up is needed to stabilize the climatological nutrient levels, which will ultimately determine the surface chl concentration. Authors may plot the climatological simulation for the subsurface nitrate to see if that is stabilized. However, as this manuscript described the processes for a month long only and therefore, the results presented here might be unaffected by the slow drift in the nutrient levels of the model during the initial spin-up. But even then, a proper spin-up would be a good choice. Further, what about using open boundary conditions for the biogeochemical variables?

Page15, line 7-10: "The hydrodynamics of the region suggests that the triggering mechanism for bloom generation is open ocean Ekman pumping forced by positive wind stress curl Vinayachandran et al., 2004; Wijesekera et al., 2016a), favouring vertical transport of nutrients to the surface sunlit layers."

The authors relied too much on referencing. It is not difficult to calculate Ekman Pumping for the specific period. Authors are encouraged show that indeed the Ekman pumping is the primary driver. What about instability? This region exhibits one of the strongest barotropic/baroclinic instability of the north Indian Ocean.

Page 15, line no. 15: "The decay of surface bloom after 02 July (Fig. 5) followed the weakening of the dome (Fig. 3)." Not vary clear.

Page 15, lines 17-21: "CTD observations within the dome until 29 June, when the ship was at TSW, show that the subsurface chlorophyll concentrations were weak (< 0.5 mg m -3) just before the surface bloom event (Fig. 4e). This indicates that the vertical redistribution of subsurface phytoplankton does not have significant contribution in en-

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hancing the surface chlorophyll. The generation of surface blooms is presumed to be dominantly controlled by the vertical transport of subsurface nutrients to the euphotic zone."

The mixed layer in SG579 does not seems shallowed considerably during the initial phase, but the chl concentration enhanced significantly in the mixed layer. The clear sky might be the major factor for this surface bloom as the authors said that the monsoon was active and therefore had considerable cloud cover in the previous week. It is possible that as the sky became clear it enhance the available light and thus marked by enhanced Chl. However, as the surface nutrients get consumed in few days the Chl concentration decreases again in spite of the persistence clear sky. How, authors can discard this possibility?

Page 16, lines 13-15: "Subsequent deepening of the mixed layer (âĹij70 m, Fig. 4d) suggests the role of mixing and entrainment in triggering the surface blooms."

What happens after 7th July when the MLD shallowed again in spite of increased wind speed? This does not explain authors hypothesis that the MLD deepens due to increased winds.

"The decay period of the bloom (08–10 July) coincided with the development of a freshening event. Surface salinity decreased by about 0.8 psu from 06 July to 10 July (Fig. 6) and the corresponding decrease in surface chlorophyll was about 0.27 mg m -3 (Fig. 5)."

The decrease of salinity during 6-10 July is of same order as seen during 4-5 July. This is only due to the fact that MLD shallows again and thereby inhibits the subsurface mixing of salinity. It may not be linked with lateral advection of fresh water and more to do with dynamics behind deepening of MLD during 5-7 July, which is not quite explained by the authors.

Later, in Figure 7 authors nicely explained the formation of barrier layer which inhibits

surface Chl. However, yet to convincingly explain why MLD deepens during 6-7 July. It may also help to extend the Figure 7 from 3rd July to see the barrier layer evolution.

Page 29, line 11: " Hence NO 3 was preferred over PO 4 and Fe (SiO4 does not limit growth in TOPAZ)"

I think this statement is not true. In TOPAZv1 large phytoplankton limitation term is dependent on Silicate. Please verify.

Figure 14: This figure is very confusing. It would help to overlay the weekly mean currents over the tendency terms. Many a times statements are made on vortices, SMC and its consequences on the NO3 budget, but without showing the mean currents it is very difficult to follow as a reader.

For example, authors said "Along the path of SMC, a clear patch of increased nitrate levels was evident (Fig. 14i), which extended from the southern tip of India up to about 85E. This indicates horizontal advection of coastally upwelled nutrients from the southern coasts of India and Sri Lanka (Fig. 14k) into the southern BoB by the SMC"

To me the NO3 show a negative tendency in the core of the SMC (east of SriLanka) and the positive patch may be along the edges. However, I can not make a concrete conclusions without any information of currents.

Further, authors claimed that Ekman pumping is the primary mechanism of surface Chl bloom, which I think is not well supported. Also, what about entrainment? At least SG620 show a clear signature of entrainment during 6-7 July.

Finally, what will be the effect of Rossby wave radiations from the eastern boundary of the Bay Bengal. Since, 8N is very close to the equatorial region, Rossby waves can travel pretty fast ($\sim$20-25 cm/s ?) which means a Rossby wave front can cover about 2 degrees during the observation period and therefore, can implicate the east-west contrast between TSW and TSE.

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