

Interactive comment on “The influence of tropical Indian Ocean warming and Indian Ocean Dipole on the surface chlorophyll concentration in the eastern Arabian Sea” by Syam Sankar et al.

Anonymous Referee #2

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General Comments:

The authors use remotely sensed chlorophyll pigment concentration data from OC-CCI for the period 1998 to 2014 to study the surface chlorophyll variability in 3 small boxes (very close to the coast in the latitude bound of 5-8°N & longitude bound of 73-78°E) in the eastern boundary of the Arabian Sea in the context of warming Indian Ocean and Indian Ocean Dipole. This subject was dealt with several earlier researchers and we do have some mechanistic understand of rapid warming of tropical Indian Ocean and its impact on chlorophyll. See for example, Goes et al., 2005; Prakash and Ramesh, 2007; Levy et al., 2007; Prasanna Kumar et al., 2010; Roxy et al., 2016, some of which the

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authors have cited in their ms. Role of IOD has also been explored by several authors to understand the interannual variability of chlorophyll in the Arabian Sea/Indian Ocean (see for example, Currie et al., 2009; Wiggert et al., 2013).

The only two results emanating from the study is that (1) in the 3 small boxes that the authors have chose to study chlorophyll during 1998 to 2014, a steady decreasing trend in the surface chlorophyll concentration was noticed which did not show any statistical significance, (2) under warming Indian Ocean and frequent occurrence of positive IOD the surface chlorophyll concentration is likely to decrease. The later is already reported by earlier authors giving reasons for such occurrence. However, in the present study authors do not provide any mechanistic relationship to explain why chlorophyll should decrease. Major concerns:

• My major concern is that I do not understand what the motivation and purpose of the present study is. Neither do I understand what the authors want to convey to the reader.

• In the 11 lines of abstract there is no substantial result that is worth reporting. As indicated under general comment the first result is inconsequential as the decreasing trend in surface chlorophyll is not statistically significant. The 2nd result is already known as previous authors have already reported declining trend in the chlorophyll concentration using long term data as well as model simulation (see for example Roxy et al., 2016; Currie et al., 2009 and Wiggert et al., 2013).

• Why the authors have chosen only 3 tiny boxes close to the coast along the eastern boundary to examine the role of IOD.

• October is a transition month from summer monsoon to winter monsoon. What is the rationale in selecting only the month of October for the study? Why not consider the coastal upwelling months of June to September?

• What is the role of EICC in bringing about the variability in chlorophyll concentra-

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tions?

• How IOD, surface chlorophyll and Kelvin wave is connected? It is not clear from the manuscript.

• Why study period was restricted to 2014 from 1998, when the chlorophyll data is available until 2018?

• In the study area (2 tiny boxes along the eastern boundary) data on D20 is mostly not available as could be seen from Figs. 8 & 9. In such case how reliable are the inferences that the authors draw based on these diagrams?

• How the asymmetric warming favours formation of more frequent formation of positive IOD?

• What is/are the reason/s for the higher warming of the eastern Arabian Sea compared to the western Arabian Sea? This is not explained anywhere in the ms.

1. If the steady declining trend in surface chlorophyll in all the 3 small boxes along the eastern Arabian Sea is statistically insignificant

Minor concerns:

• What is the purpose of lines 13 to 32 in page 3? It can be deleted as it do not contribute to the theme of the ms.

• What is “indigenous” IOD (line 19, page 2)

• What is the “marine biological activity”? (line 5, page 4)

• In equations 3, 4 & 5, the notations for curl, zonal and meridional component of geostrophic current respectively are incorrect.

• What is “standardized anomalies” (line 16, page 6)

• Lines 16-20, Page 6: The authors cannot brush aside this by simply saying that our interest is only in positive IOD. The authors need to address “Why Seas chlorophyll

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anomaly alone showed a negative value while others were positive”

• Lines 23-25, Page 6: What is the basis for this statement? If it is so why such drivers are not being considered during positive phase of IOD?

• Line 30, Page 7: How this is possible? During transition the winds over the Arabian Sea is weak and variable.

• Line 4, Page 8: What is “vertical component”? This is not Ekamann mass transport. Authors may like to consult standard Oceanographic text book.

• Lines 22-23, page 8: Upwelling is not the only process that brings about the up-sloping and down-sloping of isotherms. Modify.

• Lines 5-10, Page 9: I cannot understand what authors wish to communicate? This is well documented by previous authors. How this is relevant in the present context.

• Lines 12-14, page 9: This is ambiguous. Authors need to provide a robust mechanism that unambiguously addresses how WICC would impact change in chlorophyll.

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