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# *Interactive comment on* "Influence of physical and biological processes on the seasonal cycle of biogenic flux in the equatorial Indian Ocean" *by* P. J. Vidya et al.

**P. J. Vidya et al.**

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Reply to the comments of Referee #2

We wish to thank the referee for the constructive criticism that helped us to improve the manuscript. In the light of the comments, we modified the manuscript and point-wise modification is detailed below.

Referee's comment: Review of Vidya et al. 'Influence of physical and biological processes on the seasonal cycle of biogenic flux in the equatorial Indian Ocean' This manuscript examines the seasonal cycle of biogenic fluxes at 2 sites in the equatorial Indian Ocean and attempts to elucidate the mechanisms that result in the observed



differences. The dataset of fluxes and in situ data is potentially a very interesting one, but I don't think the authors make the best of it. The authors show a lot of data, but don't synthesise it into a coherent story particularly well. I recommend major revisions before publication.

Response: We thank the referee for the comments. Based on the comments major part of the introduction has been re-written to make it more coherent. We also have modified the discussion providing better synthesis as suggested.

Specific comments:

Referee's comment: Define SBBT and EIOT in the abstract.

Response: We have now provided an expansion of the abbreviation SBBT and EIOT (see pp.1, line 11).

Referee's comment: The introduction jumps around a lot and is unfocussed. In particular, the paragraph spanning pages 2892 and 2893 is just a list of previous studies without bringing out the salient conclusions relevant to this study.

Response: We have largely modified this part by removing the portion of the manuscript which is not directly relevant to the present study and including conclusion from the previous studies that are relevant to present study.

Referee's comment: On page 2894 the authors describe 3 components of biogenic flux that were measured at the study sites: organic carbon, calcium carbonate and silica. Which of these is plotted in Figure 3 which is labelled only 'biogenic flux'? Examining all 3 components of the flux separately may bring some additional information/insight.

Response: Figure 3 describes the total biogenic flux, which is the sum of organic carbon, calcium carbonate and biogenic silica. This is now designated as Figure 3a. Following the suggestion of the referee, we have now added 2 more panels Figure 3b and 3c (see below) which depict all the 3 components of flux at SBBT and EIOT respectively.

From the Figure 3b, it is clear that at SBBT all the 3 fluxes showed strong seasonality during summer monsoon, but no such seasonality observed at EIOT (Fig.3c). This helped us to understand the phytoplankton community structure in the upper water column. The elevation opal flux at SBBT during summer monsoon supports the dominance of Diatom bloom which further substantiates our conclusion (pp. 12, lines 273-278). Referee's comment: Line 4-6, page 2896: I don't follow the logic here of why consistent patterns of flux 'prove' that particle flux can be used to characterise deep water fluxes.

Response: We agree with the referee that there is some ambiguity and hence we have removed this portion in the revised manuscript.

Referee's comment: Line 15, page 2896: The two sites are _ 600 km apart so it's unlikely that they are affected 'by the same processes' – in fact, I thought this was the core of the authors' arguments that these are 2 distinct sites.

Response: The referee is right. The core of our argument is that the sites SBBT and EIOT are distinct. As the sentence is ambiguous we have removed it from the manuscript in the revised version.

Referee's comment: Lines 19-24, page 2900: Define SD.

Response: 'SD' indicates 'Standard deviation', which has been added in the revised manuscript. (pp. 11, line 257).

Referee's comment: Lines 19-24, page 2900: Also the 1SD of the wind speed in June is not different at the 2 locations.

Response: Yes, we agree. Though the magnitude of 1SD of wind at SBBT and EIOT is approximately same, their mean is different (SBBT 9.1$\pm$0.7m/; EIOT 7.1$\pm$0.9m/s). Referee's comment: Line 2-5, page 2901: 'Since both follow similar patterns: ' Not clear what 'both' is here. This whole paragraph needs some more precise writing to make the meaning clear to the reader.



Response: By "both" we meant climatology and the year 1996. We have modified the entire paragraph.

Referee's comment: Parts of section 3.4 and the whole of sections 3.5 and 3.6 could be omitted without losing the story. Ekman pumping, static stability and horizontal advection are unimportant to explain the observed differences and so are a 'null result'. The authors could mention that these factors were examined and found to have no influence and so reduce the number of figures and length of text.

Response: We would like to clarify the following points in the context of local Ekman-pumping at the trap site versus that at the southern part of Sri Lanka; and advection of chlorophyll to the trap location:

From the analysis of the Ekman-pumping and advection what we conclude is that at SBBT, the observed chlorophyll enhancement is not brought about by the local Ekman-pumping, rather it was advected from the high chlorophyll region in the southern tip of Sri Lanka where the Ekman-pumping played a role in enhancing the chlorophyll. This advection of chlorophyll was facilitated by the southwest monsoon current (SMC), which passes through the trap site of SBBT.

The above point may not have been made clear in the earlier version of the text and hence we have modified the text to make it more explicit. In the light of the above we would retain the figures of Ekman pumping and horizontal advection (pp.16 line 360-362; pp. 16, lines 371-375).

Similarly we would like to retain the figure on static stability to argue the point that the similar mixed layer depth at both SBBT and EIOT was due to the similar water column stability at these locations (see our response below as well) (pp. 15, lines 341-347).

Referee's comment: Line 21, page 2903: a difference of 9m in MLD is surely within the 'noise', bearing in mind that estimating MLD from profile data has large uncertainty (as it strongly depends on how one defines the MLD).

Response: We do agree with the referee that 9m difference in MLD among SBBT and EIOT may well be within the noise. This is also substantiated by the fact that the static stability of the water column at these 2 locations is not very different as mentioned above. Therefore, to account for the large difference upper ocean production at both the traps locations we have to invoke advection as explained previously. We have drawn the following figure (time-longitude variation of chlorophyll a) just to elucidate our point on advection. It is evident from the figure that the high chlorophyll during summer monsoon (driven by the upward Ekman pumping at the southern part of Sri Lanka) gets advected in to the trap location at SBBT. Thus, in the revised manuscript, in addition to wind mixing we included the role of horizontal advection towards the enhancement of biogenic flux at SBBT (pp. 16, lines 371-373).

Referee's comment: Line 2, page 2908: I didn't understand what the authors meant by 'an anomaly' here.

Response: What we meant by "anomaly" is that the result is "unexpected". We have removed this sentence in the revised manuscript.

Referee's comment: Nutrients: Are any silica measurements available? That might make an interesting comparison with the nitrate data.

Response: Yes we do have silicate data. Both nitrate and silicate profiles are given below for comparison at both the locations. We have incorporated the silicate profile in the modified Fig. 11 and also included details in the text (pp. 16-17, lines 379-389). Referee's comment: Comparison with equatorial Atlantic and Pacific: I don't see why it's 'important' to compare these results with Atlantic and Pacific, and the authors don't make a particularly convincing job of it. They claim 'striking similarities' with the equatorial Atlantic/Pacific, but at the same time acknowledge that there are substantial differences, e.g. lines 7-8, page 2911. In short, I don't think this is a particularly insightful comparison. Perhaps if it was better justified why this comparison is made and the similarities and differences were more eloquently explained:



Response: We have removed this portion in the revised manuscript.

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Interactive comment on Biogeosciences Discuss., 10, 2889, 2013.

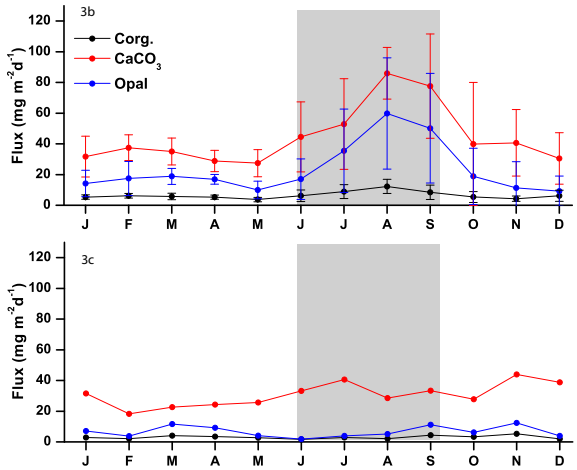


**Fig. 1.** Figure 3b & c represent component fluxes (red line indicates calcium carbonate, blue line indicates Opal and black line indicates organic carbon) at SBBT and EIOT respectively.



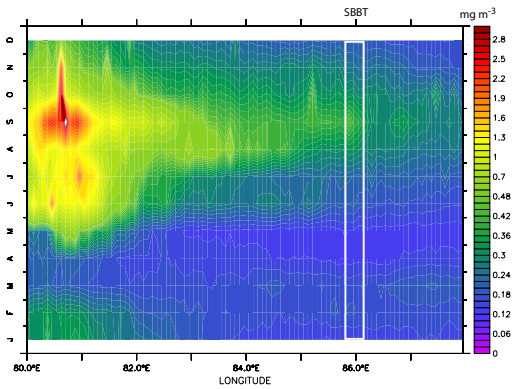


**Fig. 2.** Figure caption: Longitude vs. time diagram of monthly mean climatology of SeaWiFS chlorophyll a along the SBBT site (5-6oN averaged). White rectangle indicates the location of SBBT trap.

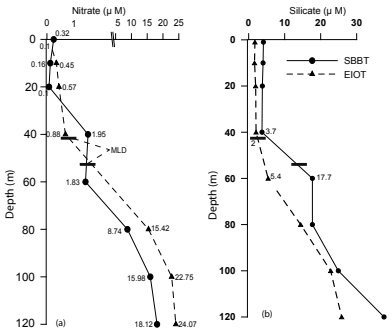


**Fig. 3.** Vertical profiles of (a) nitrate (ïA■M) (b) silicate (ïA■M) in the vicinity of SBBT (solid line) and EIOT (broken line) during August 2006.