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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.

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Reply to the comments of Referee #3

We thank Dr. Karl Banse for his exhaustive review which has helped us improve the manuscript to a substantial extent. Our response to his comments is as follows.

Referee's comment: The following review benefited from the two earlier reviews of the ms., which both urge re-submission. So, I restrict myself to a few points. In my view, the ms. at its core purports to study one year of sediment trap data from 912 m depth at the EIOT station in the equatorial Indian Ocean, to compare it with the ten-year time series (SBBT) in the same hydrographic regime, and to generalize the results by drawing parallels to the equatorial Atlantic and Pacific. The ms in its present form fails to do that because of several flaw or open issues.

Referee's comment: 1. Unger and Jennerjahn (as cited) in their fig. 6c show that six months preceded the period treated in the present ms. and, after a two-month break was eight months followed. Ramaswamy and Gaye (as cited) have treated the first 18 months. The temporal patterns of these additional data (omitted without a reason being given) differ greatly at EITO itself and in comparison with SBBT. Thus, "lack of seasonality" [or in my words, of marked variability) "characterized the flux at EIOT" (L. 6 of abstract) does not hold.

Response: We are aware that Unger and Jennerjahn (2009) used flux data for the period 1 July 1997 to 11 November 1997 (with gap of two moths) and Ramaswamy and Gaye (2006) used the flux data for the period 1 July 1995 to 27 December 1996. The reasons for not including the six months data prior to the period 1996 (July-December 1995) and during the period February-November 1997 in the present manuscript are the following.

(1) 1994 and 1997 are the two strongest Indian Ocean Dipole (IOD) years. During these periods thermocline in the eastern equatorial Indian Ocean rises and the easterly wind anomalies along the equator excite an upwelling Kelvin wave. These Kelvin wave after reaching the eastern boundary, lifts the thermocline and reflects as westward propagating upwelling Rossby wave (Vinayachandran et al., 2007). During the Indian Ocean Dipole (IOD) year of 1994, an upwelling Rossby wave was generated at 92oE in July/August and reached at EIOT (3°34'N, 77°46'E) region during the May/June of 1995 (see Figure 1). This upwelling Rossby wave presumably enhanced the chlorophyll biomass at the trap location of EIOT (ocean color data not available in this period) by shoaling nitracline, which in turn lead to the enhancement of biogenic flux at deeper depth with a temporal lag of one or two months (see Figure 4). Similar analysis confirms that SBBT is not influenced by the upwelling Rossby wave (see Figure 2; note that though there appears to be a west ward movement of low D20, computed speed

do not match with the Rossby wave speed).

(2) The year 1997 was the strongest IOD year as revealed from the DMI (see Figure 3 shaded region). It is well established that IOD plays a dominant role in altering the equatorial Indian Ocean dynamics and associated biogeochemical processes (for example, Wiggert et al., 2009). Hence we removed the flux data during the period 1997.

Based on the above two reasons we preferred not to use the biogenic flux data at EIOT during 1995 and 1997 (see Figure 4), as our objective is to delineate the processes that bring about the changes in the EIOT and SBBT on a seasonal time scale.Referee's comment: 1. cont., The same is true for the conclusion of section 2.2.1. [Methods] Biogenic flux data, "comparison with SSBT flux patterns reveals seasonal and episodic similarities between the two sites . . . .(p. 2896, L. 14) - it is not so at all (see also upper p. 2901). Also, in the flux treatment and elsewhere in the ms., too many decimal places are reported. Response: We have removed the entire paragraph in the revised manuscript. We also have removed the 2nd decimal point.

Referee's comment: 2. The attempt to compare the present data with the putative analogues in the equatorial Atlantic and Pacific is tricky to begin with because of relying on geographic points chosen for unstated reasons or/and short time series. Moreover, Table 2 (p. 2922), which is used in support, is afflicted by mistakes. While the caption states that a nominal 1,000 m depth is the basis, the EqPac data by Honjo et al. (as cited) reports on depths of >2,000 m (last Line of p. 837; 2,100 m in Table 7 for 5oN, p. 866). Also, the uncertainty of the export ratio of 0.9% in Table 2 for the EqPac in 1992 is large due the first half of the year being under El Nino influence, while the second half was normal. Figure 11, p. 865 in Honjo et al. shows that the export ratios were 0.4 and 1.4 for the two periods, respectively; hence, the average came out as 0.9. For the Atlantic in Table 2, the ms. at issue For the Atlantic in Table 2, the ms. at issue apparently chose (my guess, from the latitude given!) the flux data from the upper trap GBN3 at ∼1.5oN off Guinea (Wefer and Fischer, as cited, Table 1). I am unable, however, to find the flux values used on Table 2 of the ms. in the data Table 2 on pp. 1621-22 of Wefer and Fischer. Also it appears to me that primary production instead as 500 mg m−2 d−1 as in the ms. could as well have been <400 or >600 mg C m−2 d−1 (from Figs. 8 and 9 for one- and two months periods of two opposing seasons in Voituriez and Herbland, as cited, and using 10-hr days as in that paper). The usual summer upwelling was absent in 1963, the year used, when it could have been ∼1,300 mg C m−2 d−1 as in 1977 (p. 871 in Voituriez and Herbland). In addition, the carbon uptake figures in that paper were derived from in situ oxygen distribution (not rates).

Response: We agree with the referee that comparison of SBBT and EIOT with that of equatorial Atlantic and Pacific is a challenge. We also understand the short coming in our approach to make such comparison. In view of the comments made by the referee, we have removed this portion in the revised manuscript.

Referee's comment: For EIOT in the half year prior to 1996 (as used in the ms.) I estimate from Fig, 6 of Ramaswamy and Gaye (as cited) that the mass flux was ∼95 mg m−2 d−1 instead of ∼55 as in the same period of 1996.

Response: It appears that the referee got mixed up between Figure 6c in Ramasway and Gaye (2006) and the biogenic flux value that we reported in our manuscript. We would like clarify that in our manuscript we have used biogenic flux which is the sum of organic carbon, calcium carbonate and biogenic silica. In Figure.6c, Ramawamy and Gaye (2006) reported the total flux at EIOT from deep trap (trap depth is 2394 m) which is the sum of biogenic flux (∼ 98 mg m-2 d-1) and lithogenic flux (∼ 8 mg m-2 d-1). Please see Figure.6a of Ramawamy and Gaye (2006), which reports the trap data that have been used by us in the present study (trap depth 912m) (biogenic flux 52 mg m-2 d-1 and lithogenic flux 7 mg m-2d-1). This value of biogenic flux is consistent with our values in the manuscript. Referee's comment: There is nothing along these lines on p. 2910 of the ms. So, I conclude that the Concluding Remarks (p. 2912) of the ms. are no quite justified. 1. The ms. ends with Fig. 14, a schematic picture summarizing the physical and biological processes leading to different fluxes at EIOT and SBBT. It

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looks alright to me, but there is next to nothing said about the reasons for this or that depicted alternative, or the precedents in the literature. I do not find that acceptable.

Response: As stated earlier, we have removed the part dealing with compassion of SBBT and EIOT with Atlantic and Pacific. The manuscript has been modified accordingly.

From the Figure 14 also we have removed the comparison with equatorial Atlantic and Pacific part. Though in the equatorial Pacific and Atlantic there are several studies addressing the issues related to classical food web and microbial loop (for eg. Banse, 1992, 2013; Boyd and Trull, 2007), such studies are nonexistent in the equatorial Indian Ocean. We have not yet come across a reference in the equatorial Indian Ocean which puts together the processes (both physical and biological) starting from organic carbon production in the euphotic zone, its transformation and transportation to mesopelagic zone. Hence our objective in producing figure 14 is to schematically depict the distinctly different physical-biological processes that operate in two locations within the same geographical region.

Referee's comment: At the end I may mention a few specific points. [Methods] Phytoplankton cell numbers (p. 2898 and Table 1, p.2921): Regarding identification, it is SubrahmanyaN [not M; did he present a key?].

Response: Thanks for pointing out this one. It was an oversight. Appropriate change has been made in the revised manuscript. Yes, key is available.

Referee's comment: Also, Lebours's "The planktonic diatoms of Northern Seas" of 1930, reprinted in 1968, does not seem to be a good basis for species identification in the tropics.

Response: This book (reprinted in 1978 not 1968) was used basically for identification at the generic level.

Referee's comment: Striking in the counts in Table 1 is the recurrence of multiples of cell numbers (usually1, 2 and 3-fold). Were the reported numbers based on having counted 1, 2, and 3 cells each? If so, how much confidence do we have in those numbers?

Response: Negative. We have used, as mentioned in the methods, a known volume of concentrated phytoplankton samples (settling and siphoning procedure) in duplicates for cell count and only average values are presented in table 1. As per our computation the percentage error (IOC UNESCO, 2010) varies from 4-26.

Referee's comment: [Section 3.8] p. 2905, In situ chlorophyll a, , L. 2: . . . "characteristic subsurface maximum". I find it to be quite uncharacteristic by not being observed at the bottom of the mixed layer and the top of the nitracline (admittedly, the bottle-spacing may have camouflaged the actual nitrate distribution).

Response: We agree with the referee. One of the reasons could be choice of sampling interval as mentioned by the referee.

Referee's comment: p. 2090, 13th from bottom, Glover et al. (1985) is not relevant. Hasle and Syvertsen (1997) is not a good reference (aside from that I did not find their remark in the book); cite one of the many actual observations going back at least to the 1980s?

Response: We have removed both the references.

Reference:

Banse K.: Grazing, temporal changes of phytoplankton concentrations, and microbial loop in the open sea, Primary Productivity and Biogeochemical Cycles in the Sea, edited by Paul G. Falkowski, Avril D. Woodhead, 409pp., 1992.

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in oceanic waters: Is there consensus?, Prog. in Oceanogr., 72, 276-312, doi:10.1016/j.pocean.2006.10.007, 2007.

Ramaswamy, V. and Gaye. B.: Regional variations in the fluxes of foraminifera carbonate, coccolithophorid carbonate and biogenic opal in the northern Indian Ocean, Deep Sea Res. I, 53, 271–293, 2006.

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Unger, D. and Jennerjahn, T.: Impact of regional Indian Ocean characteristics on the biogeochemical variability of settling particles, Geophys. Monogr. Ser., 185, doi:10.1029/2008GM000703, 2009.

Vinayachandran P. N., Kurian, J and Neema, C, P.: Indian Ocean response to anomalous conditions during 2006, Geophys. Res. Letters, 34, doi:10.1029/2007GL03019, 2007.

Wiggert, J. D., Vialard, J., and Behrenfeld, M. J.: Basin-wide modification of dynamical and biogeochemical processes by the positive phase of the Indian Ocean dipole during the SeaWiFS era, Geophys. Monogr. Ser., 185, doi:10.1029/2008GM000776, 2009.

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

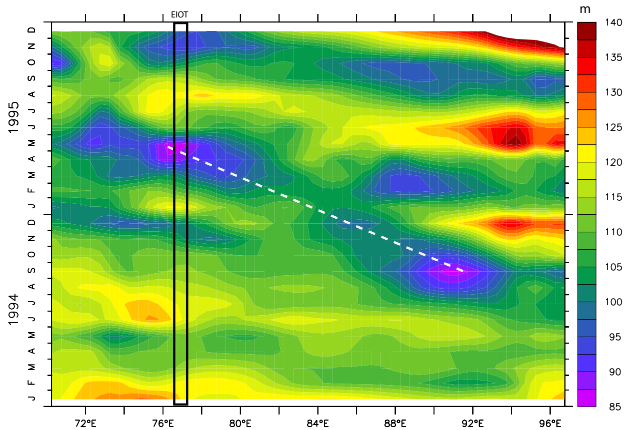

Fig. 1. Figure 1. Longitude - time plot of D20 (depth of 20oC isotherm) in meter at EIOT (averaged over 3oN - 4oN). White dashed line represents the Rossby wave signal and black rectangle represents EIOT regi

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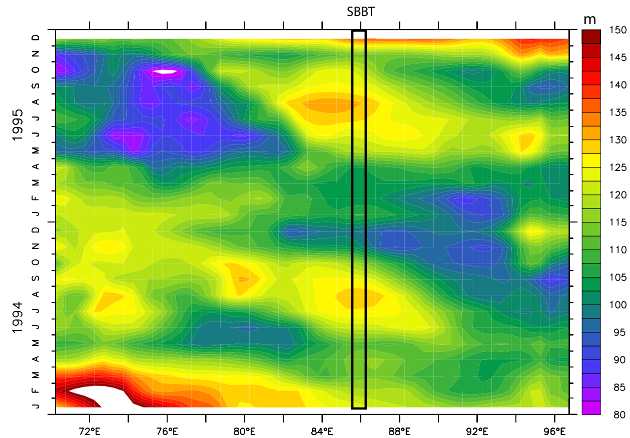

Fig. 2. Figure 2. Longitude - time plot of D20 (depth of 20oC isotherm) in meter at EIOT (averaged over 5oN - 6oN). White dashed line represents the Rossby wave signal and black rectangle represents SBBT regi

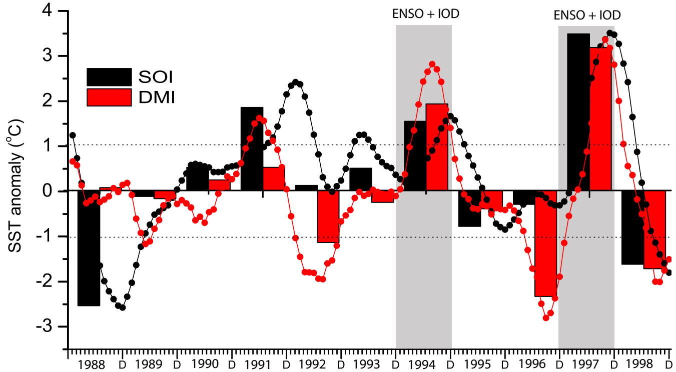

Fig. 3. Figure 3. Southern Oscillation Index (SOI) and Dipole mode index (DMI) for the period 1988-1998. Black line corresponds to monthly mean SOI and red line corresponds to monthly mean DMI. Red and black

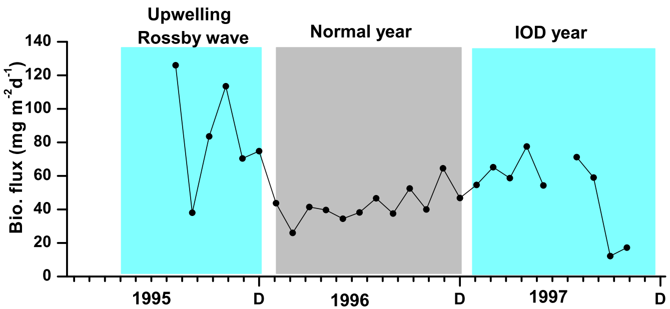

Fig. 4. Figure 4. Montlhy mean biogenic flux at EIOT during the period July 1995 to October 1997.

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