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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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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.

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. 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.

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 5^{o}N, 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 apparently chose (my guess, from the latitude given!) the flux data from the upper trap GBN3 at ~1.5^{o}N 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).

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.

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 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.

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?]. 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. 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?

[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).

p. 2090, 13^{th} 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?

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