

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.

Anonymous Referee #2

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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.

Specific comments: Define SBBT and EIOT in the abstract.

C1651

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.

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.

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.

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.

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

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.

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.

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).

Line 2, page 2908: I didn’t understand what the authors meant by ‘an anomaly’ here.

C1652

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

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. . .

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