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Interactive comment on "Biogeochemical variability in the equatorial Indian Ocean during the monsoon transition" *by* P. G. Strutton et al.

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*** Below we have reproduced the review verbatim and responded in blocks of text bounded by three asterisks. ***

Recommendation: Major revisions.

While knowledge of the physical variability of the Indian Ocean has very quickly advanced over the past decade, the progress has been slower for its biogeochemical variability. This paper is one that attempts to start filling this gap by examining a set of newly-acquired near surface chlorophyll proxy time series at the equator and 80.5°E, and resituating it in its large-scale context using climatological, satellite and coupled physical-biogeochemical modelling datasets. The paper describes an increase in back-

ground chlorophyll during the second part of 2010 that is consistent with available climatological observations, as well as a series of "spikes" in near-surface chlorophyll at ~2 weeks interval. The paper proposes that two processes contribute to these spikes: increased mixing resulting from wind stirring and meridional advection resulting from mixed Rossby-Gravity wave variability current perturbations at biweekly timescales. The paper is well written. The data that is presented here is very nice, and it is very exciting to try to understand those spikes in Chl. I thus have little doubt that this paper can eventually be published in Biogeoscience, and be a very nice contribution to our understanding of equatorial Indian Ocean biogeochemistry. But it seems to me that major issues first need to be solved, namely: 1) the eastward propagation of the Chl signal in satellite data is not consistent with previous observations of a westward phase propagation of Yanai waves at bi-weekly timescales; 2) more background on the biogeochemistry of the mooring region is needed in the introduction and 3) a lot of relevant references are missing from the text. These 3 points are developed in my major comments below and in the detailed comments. *** We thank the reviewer for the encouraging tone and the effort put into creating a thorough review. ***

General comments 1) My major issue is that previous studies that have discussed Yanai Waves at biweekly frequencies in the central and eastern Indian Ocean (e.g. Sengupta et al. 2004, Chatterjee et al. 2013) found westward phase propagation, while satellite data here seems to indicate an eastward propagation of the Chl signal. I think that further analyses of the satellite data and model are needed to sort out the various possibilities: a) the propagation shown by the satellite data is spurious; b) Mixed-Rossby gravity waves at a shorter timescale (10-12 days) – which have eastward phase propagation – are responsible; c) another mechanisms (the 1.8 eastward phase propagation mentioned by the authors is curiously close to the second baroclinic mode Kelvin wave speed). *** This is an excellent point. It is not clear how we have mistaken the direction of propagation. Rechecking these references (Sengupta, Chatterjee, Miyama) does indeed reveal that they all discuss westward propagation. We can closely re-examine the results and discussion and evaluate the possibilities suggested above by the reviewer.

***

2) I feel that a more general overview of previous literature describing processes contributing to the biogeochemical mean state and variability (seasonal, interannual) at the mooring site is needed in the introduction. *** Reviewer 1 also asked for an expanded introduction and suggested some appropriate references, which we will incorporate. Some of the deficiencies in linking our work to the existing literature have occurred because this is first author Strutton's first paper on the Indian Ocean, so we appreciate the suggested references. ***

3) I found that in several instances, a bit more effort should have been made to dig existing bibliography and link it to statements made in the text. I point a few examples in my detailed comments, but I recommend to the authors to carefully check the text for statements that are not supported by a reference and do an extensive bibliographical search. *** As above, we appreciate the suggestions for links to the literature and we can incorporate the specific suggestions below. ***

4) The bi-weekly mixed Rossby gravity waves in the Indian Ocean are themselves forced by quasi-biweekly variations of meridional wind stress across the equator in the atmosphere, that corresponds to atmospheric equatorial Rossby waves forced by convective variability in the western Pacific (see, e.g. Chatterjee and Goswami Q.J.R. Meteorol. Soc. 2004, doi: 10.1256/qj.03.133 and references therein). This maybe contributes to somewhat align peak in southward wind and southward current, and to phase the two processes that are proposed to contribute to the “spikes”. *** This is an excellent point and we appreciate the suggested reference. ***

Detailed comments *** Almost all of these comments can be quite easily addressed. Below we’ve responded specifically only to a few that require elaboration. ***

P6186, L5: I’d say “at approximately 2 weeks interval” rather than mentioning a “periodicity” here (and everywhere else), since your autocorrelation analysis (Fig. 4a) does not display a significant negative correlation at two-weeks lead/lag.

P6187, L9: I would remove the word “physical” since our understanding of the Indian Ocean physical variability has made huge progresses over the last decade.

P6187, L21: please replace by “intraseasonal to interannual”

P6187, L22-29: I’d expand a bit these statements for readers unfamiliar with those concepts. The Wyrtki jet is an oceanic equatorial current with marked semi-annual periodicity and intraseasonal modulation; the MJO is an atmospheric phenomenon that is energetic at intraseasonal timescale and whose surface flux perturbations induce a clear dynamical and thermodynamical upper Indian Ocean response, etc.

P6188, L16-25: I think that previous papers that describe those waves in the eastern Indian Ocean need to be summarized here: Sengupta et al. (2004) that you cite, but also Ogata et al. J. Geophys. Res. 2008 and maybe others. I’d also mention the atmospheric source and the Chatterjee and Goswami (2004) paper here.

P6188, L23-25: You may be right, but it’s worth checking again! I remember there’s a brief discussion of that at the end of Sengupta et al. (2004). *** Correct, we have re-checked Sengupta and there is a paragraph discussing biological variability with a few references (Murtugudde et al., 1999 and 2000) that we will incorporate into the introduction. ***

P6191, L1-2: a lot of readers (including myself) are not familiar with this “counts” and “dark counts”, so a little bit of explanation is required here. *** Reviewer 1 wanted more details on the measurements which we are happy to provide, including a better explanation of what is meant by counts and dark counts. ***

P6192, L18-21: Why use this SST product which is largely based on infrared data and thus susceptible to cloud masking? Microwave SST products such as TMI are much more reliable in those cloud-ridden regions. *** The choice of satellite products is a trade-off between spatial resolution and cloud contamination. This comment from the reviewer is regarding seasonal climatologies, which are averaged over months and years, thus removing the worst of the cloud contamination. The spatial resolution of the infrared data is at least 5 times better than the microwave data so we have used this for the climatologies to make a nicer looking map, but for the Hovmuller plots we prefer the microwave data because of the better spatial and temporal coverage. ***

P6194-6195, section 3.1: It would have been useful to superpose Oscar currents on some panels (see the detailed suggestions on figures below). In general, in this section, I find that a little bibliographic work would have been useful to comment about the main processes responsible for the SST, SSS and Chl seasonal changes that are described here. I think that papers such as Rao and Sivakumar (JGR 2000 and 2003), de Boyer Montégut et al. (JGR 2007), Levy et al. (JGR 2007) – and probably several others – could help you for that.

P6196, L13: As I mentioned earlier, other papers have discussed that. Please cite them.

P6196, L26: negative, not positive.

P6197, section 3.3: in order to have an effect on Chl, the Mixed-ROssby gravity wave meridional currents need to be associated with a strong meridional gradient of Chl. It may be nice to add a last panel with the meridional currents at the equator and meridional Chl gradient superposed to see if th spikes occur when there are both large oscillations in V and (relatively) high Chl water north of the equator. *** This is a great suggestion and can be easily accommodated. ***

P6197, L15: how do this fit with the theoretical phase speed of mixed Rossby gravity waves with bi-weekly frequency? Easy to work out from the dispersion diagram.

P6197, L26: mark them on the figure.

P6198, L15-17: You need to explain better here. The increase in subsurface salinity cannot be due to 1D processes, so it must be lateral advection. The fact that this increase does not happen below 125m is most likely due to vertical shear in zonal current

(compare panels b and f of figures 3 or 9). It would actually have been interesting to have a more quantitative approach (you can work out all the terms of the salt budget in the ocean model). The statement at line 17 is quite far-fetched.

P6198, L18: here again, you probably need to remind that a westward flow is associated with downwelling, and that the increase in surface salinity is thus probably linked with vertical mixing (you probably also need to rule out evaporation because rainfall probably domintes at this time of the year). Again, a quantitative budget in the model would have been helpful to support some of the discussion here.

P6199, L4: I would say that the main effect is more linked with strong vertical velocities induced by the divergence/convergence of the background flow near the island, than to mixing. There are also other papers that have proposed similar hyoptheses to explain intraseasonal variations of Chl in the Indian Ocean that should be quoted: Vinayachandran and Saji (GRL 2008) and Resplandy et al. (JGR 2009) (in fact there are probably other papers that I don't know on this topic).

P6201, L13-16: this is not an accurate rendering of the Chatterjee et al. (2013) results. The lower frequency (20-30 days) mixed Rossby gravity waves in the western Indian Ocean (which have a clear SST signature and have been the subject of many papers in the 80s-90s, see refs in Chatterjee et al. 2013) are indeed largely energized by instabilities, but the quasi bi-weekly mixed Rossby gravity waves that dominate in the central and eastern Indian ocean are largely forced by the wind, more precisely by quasi bi-weekly oscillations in meridional winds that are attributed to atmospheric Rossby waves (Chatterjee and Goswami, 2004). *** We appreciate the input on how best to represent the results of Chatterjee. ***

P6201, L17: "is advected eastward by the Wyrtki jet" would be better than "propagates" (which one could attribute to a wave).

P6201, L19: there is however a problem with that explanation. Sengupta et al. (2004) (see e.g. their figure 6 or the space-time dispersion diagram of figure 7) or Chatterjee et al. (2013) (see e.g. their figure 5) describe westward-propagating Mixed Rossby Gravity waves with quasi biweekly frequencies (the dispersion diagram of equatorial waves indeed shows that eastward propagating Yanai waves are higher frequency and smaller scale, see a text book as, e.g. Gill's "Atmosphere-Ocean Dynamics"). This means that, at 2 weeks timescale, regions of strong southward (or northward) meridional current propagate westward, and once would thus expect the Chl signals to travel in the same direction. So I guess there are three possibilities there: 1) the eastward propagation seen on figure 5d is spurious (e.g. the result of aliasing by clouds?) and then the explanation linked to Yanai waves may remain valid; 2) there is indeed eastward phase propagation, and 2a), another mechanism than the one associated with mixed Rossby grvity waves must be invoked. 1.8 m/s is actually not so far from the speed of the second baroclinic mode Kelvin waves in the Indian Ocean (e.g. for instance, Nagura and McPhaden 2010). One could imagine that intraseasonal Kelvin wave modulate vertical shear along their path and hence input of nutrients (and of Chl) into the surface layer along their path. But this does not seem to be supported by the discussion you have P6202 at lines 13-22. 2b) Thoise signals are indeed associated with Yanai wave, but at higher frequency (at 7-10 days frequency, Yanai waves propagate eastward, see Figure 7 of Sengupta et al. 2004). I looked at the animation and found it difficult to see the direction of propagation of mixed Rossby gravity waves. You may want to plot Oscar and modelled meridional currents (or better their symmetric part with respect to the equator) as a longitude-time section to look into this. *** These suggestions relate to the reviewer's general comment number 1 which we are happy to accommodate. ***

P6201, L20: I guess that you mean Fig. 6.

P6202, L8: the figure caption says velocity but the main text says friction velocity. Please disambiguate (you should indeed use friction velocity).

P6202, L17: I guess you mean the Yanai waves. Instability waves in the tropical Pacific and Atlantic do indeed display significant projection on Yanai waves, but they should

not be confused.

P6203, L15: I'd say the Yanai waves are less obvious in the model than in observations.

P6204, L11-12: isn't there a subsurface Chl maximum in that region? Please clarify this somewhere in the text, maybe in the introduction section by providing an overview of the mean seasonal cycle of biogeochmical conditions in that region.

P6204, L15: Intraseasonal wind variability has a much wider meridional range than the one seen on figure 10. I think that the meridional scale of figure 10 is set by the equatorial waveguide (near the equator, currents are much more responsive to zonal wind, which generates large shear and strong mixed layer depth variations).

P6205, L1-2: It would be easy to verify. Plot a time-longitude diagram of bandpassed meridional velocity at the equator.

Figures Figure 1. The panels on figure 1 are tiny and difficult to read. A lot of space can be gained on this figure by removing duplicate information (color bars, latitude or longitude labels which are needed only once for each row/column since figures are lined up), etc: : : I think that the authors can thus obtain much larger panels, and thus potentially add Oscar climatological currents on those panels, which could be useful for some of the discussions. *** We'll do our best to accommodate this suggestion, which we agree will improve the look of the figure. ***

Figure 2: the grey shading is not very visible. Maybe make it darker.

Figure 4: maybe indicate which variable leads / lags in the x axis lavels. Mark lag 0 with a vertical line.

Figure 5: I'd mark the peaks seen on panel (e) by marks at 80.5_E on panels a-d.

Figure 8: the figure tile says "U*3". U* is usually a notation for friction velocity (square root of (wind stress divided by sea water density)), not wind speed, while the caption says "cube of wind speed". Please disambiguate because both quantities have the

same units. The cube of friction velocity is actually a better indicator of vertical mixing in the ocean.

Figure 9: mark “U”, “V”, “T”, and “S” on panels b, c, e and f, respectively.

Interactive comment on Biogeosciences Discuss., 11, 6185, 2014.