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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. ***

General Comments: The equatorial Indian Ocean is dynamically very different from other equatorial regions of the world ocean. The dominant characteristic of this region is the strong monsoonal forcing and the associated semi-annual variability in the oceanic response. Though the semi-annual variability of the equatorial Indian Ocean is relatively well researched, the intra-seasonal and inter-annual variability is less explored. Even scantier is the information on the biogeochemical variability of the equatorial Indian Ocean. It is in this context that the present study assumes importance.

The present paper addresses an important topic namely the role of bi-weekly MRG waves and wind-stirring on ocean biogeochemistry and ecological response in the equatorial Indian Ocean. The authors relies up on 7 months long in situ time-series data, starting on 22 May 2010, from RAMA instrumented mooring which includes a combined chlorophyll fluorescence-backscatter sensor mounted at 25m below the surface to identify 6 peaks in chlorophyll. They further use surface meteorological parameters from the same mooring along with data on temperature and salinity in the upper 500m and 140m respectively, and currents at 10m and 40m below the surface respectively. To give context to the variability inferred from the single spot mooring, the authors use satellite derived chlorophyll pigment concentration, in situ climatology of CARS and Re-Argo and output from biogeochemical model OFAM3. The physical model of OFAM3 is MOM 4.1 version while the biogeochemical model is WOMBAT.

It is a well written manuscript and should be published, in my opinion, but only after consideration of some of the comments listed below.

*** We thanks the reviewer for the generally positive view of the manuscript's importance. ***

Specific Comments: 1. The occurrence of MRG waves in the central equatorial Indian Ocean is well documented, so also its role in entrainment across the base of the mixed layer through vertical shear. Apart from the references in the manuscript, the authors may also like to have a look at the following ones: Ogata et al. 2008; Horii et al., 2011; David et al., 2011. Similarly, the notion of bi-weekly MRG waves causing chlorophyll enhancements in the equatorial Indian Ocean is also a known phenomenon. In the light of the above, the authors may like to modify their introduction and discussion. *** We thank the reviewer for pointing out these references to us. We have found the papers and will incorporate their major findings into the introduction and discussion. ***

2. Chlorophyll is the center of the discussion. Combining the satellite derived chlorophyll pigment concentration from SeaWiFS and MODIS is an issue, especially when it

is simply averaged (section 2.2 satellite derived observations). There have been earlier attempts in this direction in the equatorial Atlantic and I would like to see a discussion about it. This is absolutely not trivial. *** We think that the reviewer is referring to a paper by Gregg and Casey (Remote sensing of Environment, 2007) that quantified the sampling biases of MODIS and SeaWiFS. We can definitely include a discussion of these issues, but we also note that the satellite- and mooring-based chlorophyll estimates agreed with each other very well. We are confident that both data types are capturing the biological variability well, with the usual caveats around missing satellite data due to clouds. ***

3. Another issue with remotely sensed chlorophyll is the cloud cover in this part of the ocean (section 4.1 Evaluation of hypothesis based on observation). Being a part of the Indo-pacific warm pool, this region is expected to experience cloud cover for good part of the year. Hence, it is important provide information on the number of cloud free pixels versus total pixels in the study area used for deriving the chlorophyll data produced in the manuscript. *** We can do this no problem and add a time series of % pixel coverage as a separate panel to figure 2. ***

4. Tropical regions are known for the rampant bio-fouling, especially in the upper ocean. It is surprising that the optical sensor could retain its integrity for 9 months without being colonized by the bio-fouling organisms. The authors also need to provide a discussion on the possibilities of spurious spikes in chlorophyll in the context of signal interference arising out of the suspended particles and/CDOM etc. Some details about frequency of sampling and data processing applied to raw data also would help. It is not clear how long did each of the spike last. It is important in the context of doubling time for the chlorophyll a in oligotrophic waters. *** The reviewer is correct that some details have been omitted. We will include these in the revised manuscript. For example, we did not mention the antifouling components of the fluorometer (a copper shutter with rubber 'wiper'), or the details of the sampling strategy (5 x 1-second measurements every 15 minutes that were averaged into a single data point). We did not see any evidence

of spikes in the higher resolution data and will explain this. We can also quantify the shape (duration and rate of increase) of each spike in chlorophyll. ***

5. Location of chlorophyll sensor is an issue. In the central part of the equatorial Indian Ocean the upper 40m water column is generally oligotrophic with very low chlorophyll a, typically 0.1mg/m3. A deeper positioning of the sensor could have efficiently picked up the magnitude of the enhancement correctly. *** There is not much that we can do about this comment. However, based on our experience in similar waters (tropical Pacific) the high chlorophyll concentrations observed in the deep chlorophyll maximum are not responsible for much of the column integrated primary productivity. If we are interested in the variability of primary productivity, shallow deployments are favourable. ***

6. I would like to see more robust comparison of model nutrient and chlorophyll profiles/ vertical sections with actual data (section 2.3 al Biogeochemical modeling). This could be accomplished in two steps – (1) the model nutrient climatology can be compared with CARS and (2) the model inter-annual simulation f chlorophyll and nutrients can be compared with available co-located in situ data (see for example, Sardesai et al., 2010). The is very crucial as the authors invoke MRG waves as a mechanism to explain the observed peaks in surface chlorophyll, which depends up on the subsurface nutrient supply through shoaling of the nutricline by the passage of MRG waves and subsequent entrainment to the upper ocean kick starting the primary production. *** We have located the Sardesai reference and can produce both types of comparisons described above by the reviewer. ***

7. Lack of SCM is another serious issue with the biogeochemical model. It is well known that SCM is the characteristics of the chlorophyll profile in the Equatorial Indian Ocean, where the top 40m is generally oligotrophic with extremely low chlorophyll. The SCM is located between 40-60/80m and is expected to be impacted by the upward propagating MRG waves, there by altering the chlorophyll concentrations in the upper ocean. Authors need to rationalize their results in the context of the above. *** We

have already acknowledged the problem of the model not accurately representing the vertical distribution of chlorophyll, but we will improve the discussion of what this means for the response of biology in the model to mixed Rossby gravity (MRG) waves. ***

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