Earth System
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Data

Interactive comment on "Global ocean biomes: mean and temporal variability" by A. R. Fay and G. A. McKinley

Anonymous Referee #3

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This manuscript presents a method to define large biomes in the global open ocean, using biogeochemical and physical parameters such as mixed layers and chlorophyll. This could provide a useful framework for inter-comparison studies and model evaluation in the future.

I suggest a major revision for several reasons:

- My main concern is that the manuscript lacks some convincing argument and illustration on how these biomes could be better than other methods (latitudinal bands, water masses, other biomes definition etc..). (see comment #2)

- The choice of the biomes and parameters is not motivated and the impact of such a choice not discussed. Is it really sensitive to the choice of the chlorophyll or SST

threshold for example? This is not obvious from the manuscript that mostly states the biomes parameters and describes the temporal evolution of their areas.

- I find very surprising that the equatorial biome is a latitudinal band. Specially if you motivate this framework by emphasizing on the shortcomings of using latitudinal bands in RECCAP. Isn't there a better option here? (comment # 3)

- I also suggest to add the references of previous works on frameworks using bioregions or water masses

- Finally, the authors should expand the discussion on the possible applications of this framework and its limitations.

Specific Comments:

1) In the introduction or discussion, I suggest to add a paragraph about the previous works that defined more refined biomes or bioregions but at the scale of one basin (Indian Ocean, Southern Ocean and Mediterranean Sea etc.). These bioregions are not suitable to address issues at the global scale such as is done in RECCAP but it would emphasize what has been done before in that field and give some background to your manuscript. In particular, the studies in the Arabian Sea and Med Sea show how complicated these regions are and why it is so difficult to include them in a global framework such as yours. Below are some examples of such bioregions at the basin scale. Also, you might consider mentioning works where other global frameworks where used to compare physical and biogeochemical fields. The list below is not exhaustive.

-D'Ortenzio F., and M.R. d'Alcala, (2009). On the trophic regimes of the Mediterranean Sea: a satellite analysis. Biogeosciences 6 (2), 139-148.

-Lévy, M., D. Shankar, J.-M. André, S. S. C. Shenoi, F. Durand, and C. de Boyer Montégut (2007). Basin-wide seasonal evolution of the Indian Ocean's phytoplankton blooms, , J. Geophys. Res. 112, C12014, doi:10.1029/2007JC004090

-Grant S, Constable A, Raymond B, Doust S. 2006. Bioregionalisation of the Southern

Ocean: Report of Experts Workshop, WWF- Australia and ACE CRC, Hobart, September 2006

-Bopp, L., Resplandy, L., Orr, J., C., Dunne, J., P., Gehlen, M., Halloran, P., Ilyina, T., R., S., Tjiputra, J. and Vichi, M. (2013) Multiple stressors of ocean ecosystems under global change : projections with CMIP5 models. Biogeosciences.

-Resplandy, L., Bopp, L. and Orr, J., C. and Dunne, J., P. Role of Mode and Intermediate waters in ocean acidification : analysis of CMIP5 models (2013). Geophysical Research Letters. doi: 10.1002/grl.50414.

2) I know that ESSD is about showing the method and data. However, I find it difficult to get an idea of why your framework is a good option. I think that the reader needs more convincing elements. Maybe a comparison of some biogeochemical fields in your biomes vs. some latitudinal boxes. See how the standard deviation within the two frameworks changes and how your method might be more appropriate to capture patterns. In addition, overlaying your biomes boundaries with the annual Chlorophyll, MLD, SST and sea ice fraction might be a good illustration for the reader.

3) As you illustrate in other biomes, the interannual variability modulates the boundaries. I expect ENSO to have a major impact on the equatorial boundary if it was not fixed at 5N and 5S. I think that the processes taking place there are important enough to try to find a better option or at least assess by a sensitivity test what is the impact of this fixed boundary.

4) In the conclusion: "Opportunities for use of these biomes in future studies are likely to be numerous...". Maybe. However, I would expand on these opportunities because this is the point of this paper. I would recommend that you insert this part in the discussion section. I also recommend you comment on the fact that your region boundaries could be used as such for data analysis. However, to provide a framework for model inter comparison, the biomes boundaries should be re-defined for each model. Otherwise the method would face the same problems as RECAPP latitudinal boundaries.

Indeed, structures such as currents, gyres etc.. are not located at the same place in models and the data. Defining your biomes in models, using these criteria, could provide a tool to address multimodel comparison (CMIP5 etc) and models evaluation against data.

5) Finally, you should mention some of the shortcomings of your approach. For example, the Arabian Sea is completely absent from your framework and I do not agree with the assertion that it is a coastal area. It is indeed a difficult region to categorize but it accounts for a significant amount of the tropical biological production.

6) At the end of the discussion: about the chl merged product. Indeed, I agree and strongly recommend you use the merged product GlobColour that merges SeaWiFS, MERIS and MODIS (http://www.globcolour.info/index.html) at various horizontal resolution.

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