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Discussions

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

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Author Response to Anonymous Referee 3

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)

>We have included Figure 7 and discussion to make this case.

- 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.

>We motivate the use of these parameters at the beginning of Section 2. Sensitivity to input product is discussed in Discussion.

- 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)

>We have changed the definition of the equatorial biomes as suggested.

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

>We have included many of these references.

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

>We have expanded discussion and comparison to other studies.

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 biore-

gions at the basin scale. Also, you might consider mentioning works where other global frameworks were 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, Septem- ber 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 Interme- diate waters in ocean acidification : analysis of CMIP5 models (2013). Geophysical Research Letters. doi: 10.1002/grl.50414.

>We have added further discussion of previous work related to the effort of defining ocean bioregions in our Introduction. Thank you for the mentioned references.

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.

>Thank you for this suggestion. We have included a comparison of both pCO2 climato- logical variance and annual chlorophyll variance between select biome and RECCAP regions in the Discussion (Figure 7).

3) As you illustrate in other biomes, the interannual variability modulates the bound- aries. 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. >Thank you for this suggestion. We agree with your critique and have revised the biomes to allow for biogeochemically defined equatorial biomes. 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. >Thank you for this comment. We have elaborated on these opportunities for using the biomes in the Conclusion. We also added a paragraph in the Discussion to discuss the need to redefine biomes for models when conducting model inter-comparisons.

5) Finally, you should mention some of the shortcomings of your approach. For ex- ample, 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. >Thank you for this suggestion. Omission of coastal areas as well as smaller bays and seas around the world is a limitation. However, it is not a limitation that is inconsistent with typical "blue-water" oceanographic studies attempting to capture the processes and large-scale fluxes of the entire global ocean (e.g. Takahashi et al. 2009). Further, we argue that inclusion of these regions in with the open-ocean regions would be a mis- take as there are different processes controlling biology and mixing in these regions,

and these tend to have significant small-scale regional variations. We have included some resources for those looking at coastal and regional seas but restate that this partitioning is for surface processes in open-ocean regions only.

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.

>Thank you for this suggestion. After careful consideration and comparison we have opted to continue using the SeaWiFS chlorophyll product. While the GlobColour merged product would allow us to extend our time-varying biomes to 2013, we found that decreasing chlorophyll values from 2010-2013, especially in the equatorial regions, were suspect. Such large decreasing trends in chlorophyll during these years have not been supported by published literature and a similar trend is not found when looking at chlorophyll from the AQUA MODIS satellite product. We worry that the decrease is an artifact of the ending of the SeaWiFS satellite record as it is most pronounced at the exact year of shifting from merged product that contains SeaWiFS to one that does not.

We also considered a similar product produced by a US effort (Measures: http://wiki.icess.ucsb.edu/measures/MainPage), which merges the satellite products at the level of radiance which they find to be a more accurate method. However, this product is currently available for a shorter period than SeaWiFS (2002 – 2012).

We hope that improvements in these merged products will allow for extended biomes in the future but at this time we feel biomes produced with SeaWiFS-only chlorophyll is the most appropriate option.

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