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Interactive Comment

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 4

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The manuscript " Global ocean biomes: mean and temporal variability " by A. R. Fay and G. A. McKinley proposes a methodology to define seventeen large biomes in the global open ocean, using set of biogeochemical and physical parameters such as sea surface temperature, sea-ice fraction, mixed-layer depth and satellite-derived chlorophyll. The authors propose these new biomes as useful framework for intercomparison studies such as RECCAP. I read the article with high interest. The manuscript is clearly structured and reads well. Nevertheless, I think this paper needs some clarification that

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Discussion Paper

have to be addressed first, and which prevent me of accepting this paper in its present form. Therefore, I recommend acceptance of this manuscript after major revisions. While I am convinced by the use of such a framework, my largest concern is in the lack of quantitative information proving the robustness of such biomes definition. A extended quantitative analysis performed on

(1) the sensitivity to the observational datasets (using alternatively other data or re-analysis such as SST from (Reynolds et al., 2002), GlobColour for surface chlorophyll or MODIS/MERIS),

>We have done this and included mention in the Discussion.

(2) the sensitivity to threshold/characteristics for each biome (How small changes in characteristics would changes biomes coverage?).

>We have included some discussion, particularly with respect to the ICE biome.

(3) Finally, a proof of concept is needed to my point of view. For example, comparing biomes surface areas in various models (across the CMIP5 database or RECCAP) and the surface areas defined by latitudinal boundaries would further support the main point of the study than looking at the gyres expansion/ contraction.

>We have included Figure 6 to compare directly to other regional definitions, and Figure 7 to illustrate better coherence in seasonality within the biomes.

Specific comments: L 21 p108: Definition of alternative "physical" boundaries has a long story. For example, physical oceanographers use watermasses as a physical framework since a longtime (e.g., Walin, 1982). Therefore consider citing related references:

- Iudicone, D., Rodgers, K. B., Stendardo, I., Aumont, O., Madec, G., Bopp, L., Mangoni, O. and Ribera d'Alcala, M.: Water masses as a unifying framework for understanding the Southern Ocean Carbon Cycle, Biogeosciences, 8(5), 1031–1052, doi:10.5194/bg-8-1031-2011, 2011.

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- 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.

- Sallée, J. B., Shuckburgh, E., Bruneau, N., Meijers, A. J. S., Bracegirdle, T. J., Wang, Z. and Roy, T.: Assessment of Southern Ocean water mass circulation and characteristics in CMIP5 models: Historical bias and forcing response, Journal of Geophysical Research-Oceans, 118(4), 1830–1844, doi:10.1002/jgrc.20135, 2013.

- Séférian, R., Iudicone, D., Bopp, L., Roy, T. and Madec, G.: Water Mass Analysis of Effect of Climate Change on Air–Sea CO2 Fluxes: The Southern Ocean, J. Climate, 25(11), 3894–3908, doi:10.1175/JCLI-D-11-00291.1, 2012.

- Walin, G.: On the relation between sea-surface heat flow and thermal circulation in the ocean, Tellus, 1982.

Other definition related to satellite-derived chlorophyll clustering should be also included in this manuscript:

- 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.

Finally, definition related stressors of marine ecosystem should also be discussed here:

- Bopp, L., Resplandy, L., Orr, J. C., Doney, S. C., Dunne, J. P., Gehlen, M., Halloran, P., Heinze, C., Ilyina, T., Séférian, R., Tjiputra, J. and Vichi, M.: Multiple stressors of ocean ecosystems in the 21st century: projections with CMIP5 models, Biogeosciences, 10(10), 6225–6245, doi:10.5194/bg-10-6225-2013, 2013.

>Thank you for the references provided. We have included many in the revised text. We concur that there are other approaches that can be used to group observational and/or model data to study the large-scale biogeochemical responses to climate variability and change. The approach we propose certainly does not attempt to invalidate these other approaches.

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Water masses are a relatively new and promising method of studying surface-to-deep connections in the ocean for the modeling community, and have been particularly useful for global-scale comparison with respect to heat uptake and biogeochemical change (Bopp et al., 2013) and in the Southern Ocean (Séférian et al., 2012; Sallée et al., 2013). Iudicone et al. (2008, 2011) discuss partitioning the domains using specific neutral density ranges to classify the water masses following Sloyan and Rintoul (2001). There is also a need for the capacity to partition the surface ocean for studies of surface data and for surface processes such as air-sea CO2 fluxes and primary productivity. Also note that water masses have not been used in studies such as ocean inversions for air-sea CO2 fluxes based on interior data, even though one might expect this to be a very good place to use such an approach as it explicitly deals with the surface-to-deep connection (Gruber et al. 2009). Our goal is to offer another approach for regional discretization to the community. We have added in the Discussion a paragraph on these issues.

L 2 p109: please mention which observations were used. Note that satellite-derived chlorophyll are not direct observations.

>We include a very detailed explanation of which products we use in Section 2.2-2.4 of our manuscript. We understand that satellite-derived chlorophyll is not observations and indicate in the manuscript that these are estimates from ocean color data.

L 10 p109: explain how coastal region are defined

>We do not specifically define coastal regions in our analysis. We do mask out bays and small seas (e.g. Gulf of Mexico, Mediterranean Sea, Arabian Sea, Java Sea) prior to defining biomes. Coastal regions are naturally eliminated by our criteria thresholds as they are created with open-ocean processes in mind and therefore do not extend to include values that would be found in coastal areas (high chlorophyll values in coastal upwelling regions for example). We have clarified this in the manuscript.

L 24 p110: Why using latitudinal boundaries for EQU domain. Regarding the pattern of

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surface chlorophyll in the EQU region, I agree that 5S-5N box makes sense. But this is not the case for the variability of this variable and for the sea surface temperature (as well as the mixed layer depth).

>We have incorporated biogeochemically defined equatorial regions and allow them to shift from year to year. Thank you for this suggestion.

S 3.3 p116: Regarding the definition of the EQU biome, I am not surprised that only high-latitude biomes do core biomes. I think the inclusion of physical/biogeochemical characteristics for the EQU biome would substantially change this view. EQU is the only biome defined by latitudinal boundaries (Table 1). Consequently, this biome does not vary in term of surface area in core location.

>Indeed, we have found that the equatorial biomes vary the most year to year and therefore have the highest loss of area when using core biomes versus the time-varying biomes (Figure 4). Thank you for this comment.

L 26 p 117: limitations of the framework. I think here further discussion is needed. The definition of the EQU is clearly one limitation. The use of more quantitative clustering methods to create an objective definition of biomes characteristics is one other (even if such methodology requires alternative dataset to cross-validate definition of biomes and is therefore preclude considering the lack of some data).

Reynolds, R. W., Rayner, N. A., Smith, T. M., Stokes, D. C. and Wang, W.: An Improved In Situ and Satellite SST Analysis for Climate, J. Climate, 15(13), 1609–1625, 2002.

>Thank you for this comment. We have expanded on our discussion of the limitations of our approach. We chose to use HADISST because of its complete coverage in the 1980s, which is important for previous work by the authors. As a comparison, we have recomputed the biomes using the OIv2 SST and ice product referenced above. Below is a map of the difference between mean biome maps when using OIv2 ice and SST product versus the HADISST. The main shifting occurs in the high latitudes. The ice

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concentration extends further away from both poles when using the Reynolds product (a shift of one biome assignment as shown by the blue and orange colors in the figure below), causing an expansion of the ICE biomes if using OIv2. Changes in biome assignment over the rest of the global ocean are very minimal. Time-varying maps of this comparison are similar. The core biomes do not change at all when using the OIv2 product. We maintain our choice to use the HADISST product but hope that this comparison helps to show how the choice of SST product has a minimal impact on the global biomes.

Interactive comment on Earth Syst. Sci. Data Discuss., 7, 107, 2014.

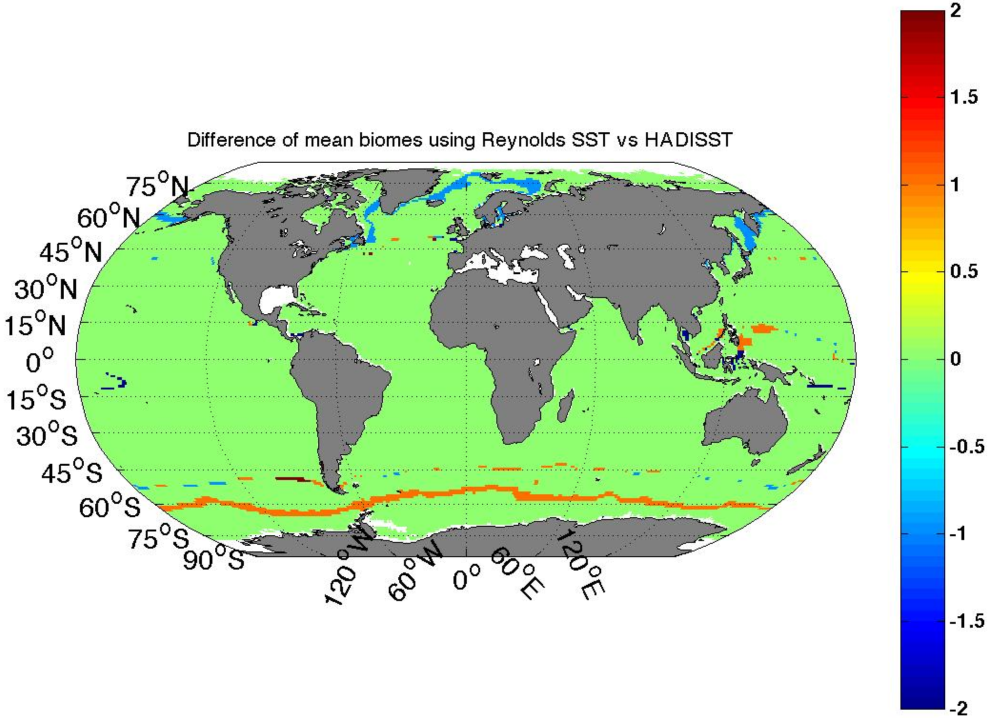
Difference of mean biomes using Reynolds SST vs HADISST
75°N
60°N
45°N
30°N
15°N
0°
15°S
30°S
45°S
60°S
75°S
90°S
120°W
60°W
0°
60°E
120°E
2
1.5
1
0.5
0
-0.5
-1
-1.5
-2

Fig. 1.