Interactive comment on "Biogeochemical protocols and diagnostics for the CMIP6 Ocean Model Intercomparison Project (OMIP)" *by* James C. Orr et al.

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Response to Comment from Anonymous Referee #1

Referee #1's detailed comments are much appreciated. They are repeated below in gray, while our responses are given in black.

This paper lays out a strategy for the biogeochemistry component (OMIP-BGC) of the Ocean Model Intercomparison Project under the umbrella of the 6th Coupled Model Intercomparison Project (CMIP6). The paper is well written and mostly

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does a good job of outlining the experimental design for potential participants in clear and unambiguous terms. The promised OMIP-BGC web page (bottom p. 19) does not appear to be operational yet (and rather more than 4 weeks have passed).

Thanks. It is now planned that the OMIP-BGC web page be made available at about the time that the revised manuscript is submitted.

Major comments:

Overall strategy and ordering of priorities

The weakest point of the strategy is that it is vague about the priority of experiments. In CMIP5, there were tiers of experiments (Core/Tier1/Tier2), and tiers of output fields (Priority 1/2/3). This document does not really separate the two, implicitly treating all experiments as Core. The list of Priority 1 output fields is expansive and probably unrealistic.

We agree that a weak point of the manuscript is that the priority of experiments is not as clear as it should be. In the revised manuscript, we will strive to improve this deficiency, following these comments and Short Comments from others.

Some tracers are referred to as "level=1" and "level=2" (5/25), but the term is not defined. It appears to refer to output fields in which case it is synonymous with "Priority" (Tables 4-14) and there is no real conceptual problem. But the most important thing the authors need to do is to separate the x (experiments) and y (output fields) axes in a fashion similar to the CMIP5 data request, and pare down the list of Priority 1 fields

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to a more realistic level.

Separating out the experiments and output fields in a clearer fashion is a good idea that we will try to implement in the revised manuscript. In the light of these comments, the coauthors will rediscuss the list for Priority 1 fields.

The list of 3D monthly fields is long. In CMIP5 no 3D ocean biogeochemistry fields were monthly. According to Moore's Law, computing power should have increased about 16-fold since CMIP5, but in practice the gain is probably much less. Making previously annual fields monthly requires a twelvefold increase just to break even in terms of the time and storage capacity it will take to access and download data, even assuming no increase in model resolution.

The lack of 3-D monthly fields below the surface in CMIP5 was an impediment to analysis. We do not wish to repeat that mistake during CMIP6. Monthly fields will be valuable to the community, and cutting back on requested fields will limit the science that can be done. See also the comments from Referee #2 along the same lines. Those that do not wish to analyze the monthly fields may choose to download and analyze only the annual mean fields.

For any model that includes feedbacks between saturation state and biology, the duplication of tracers is likely to make the 'natural CO2' experiments prohibitively expensive. Yet these are said in the Conclusion to be critical (19/14) and to be "required" on 6/19. Even if one only considers the effect of saturation state on dissolution of CaCO3, that is a minimum of three additional tracers (or maybe two if CaCO3 is parameterized). I agree with the authors that these experiments are important, but the rather superficial consideration given to the actual cost (bottom p. 6) simply underscores that the

strategy does not include a clear hierarchy of priorities for the different experiments proposed.

In defense of the original manuscript, adding three passive tracers in an online coupled physical-biogeochemical simulation does not typically increase costs greatly, far less than a factor of two. In such models, the computational cost of running the dynamical model typically dominates. If a separate simulation needs to be run for the natural component, then of course that would double the computational time. But the only required simulation (ocmip1) is initialized from data and run for 310 years. These forced ocean simulations being proposed for OMIP are much less computationally expensive than are the Earth System Model simulations at the same resolution. Nonetheless, we agree that a clearer strategy needs to be elaborated, an effort we will take on with the revised manuscript.

Alkalinity and speciation

I agree that using a truncated expression for alkalinity causes large errors, but the paper could spell out in detail exactly what they envision alkalinity as including (since the full formal definition includes lots of species that are not provided in a model simulation), rather than simply referring the reader to the provided codes.

The revised manuscript will include the equation for total alkalinity that is provided already in the common code for computing the carbonate chemistry (mocsy). This equation is also provided in the publication which describes that code (Orr and Epitalon, 2015, Equation 7).

Similarly, the authors could clarify exactly what they mean by N speciation (18/14). I

agree that the alkalinity sources and sinks associated with biological transformations (e.g., nitrification) of N species should be accounted for. But I think it is better if the word speciation is not used here.

Good point. The revised manuscript will clarify this issue, avoiding the word speciation.

For models that have N but not P it is recommended that the PO4 contribution to alkalinity be calculated as the P/N Redfield ratio times the total inorganic N concentration (16/29), which is appropriate. But then on 18/15-16 this is referred to as the effect of nitrate on alkalinity. But really what is being referred to here is the effect of phosphate on alkalinity, parameterized as DIN/16. How much N is present as e.g. NO3 vs NH4 is not relevant.

Thanks for pointing out this confusing passage. It will be clarified in the revised manuscript.

It might be useful to include a table of the net alkalinity change associated with biological transformations of N (phytoplankton uptake, remineralization, nitrification, denitrification, N2 fixation), to help ensure that this is done consistently across models. These numbers can be found in Wolf-Gladrow et al 2007 Mar Chem 106: 287. I think such a table would be more useful than Figure 3, which I do not think is necessary.

Thank you for this idea to include a table of net alkalinity changes due to biological transformations of nitrogen. We will consider this for the revised manuscript. As for Fig. 3, that presents the speciation of phosphoric acid and silicic acid species as a function of pH. It is not directly related to transformations of nitrogen. We think it adds value to the manuscript because it clarifies what species are important. There is a

common misconception among many ocean scientists who refer to phosphate (PO_4^{3-}) when what they really should refer to is the total dissolved inorganic phosphorus (P_T). Even the publications and web pages that describe the World Ocean Atlas incorrectly refer to phosphate when in fact the discrete and objectively mapped data is actually P_T. Eliminating this confusion seems to us to be valuable for the ocean modeling community as well as others.

I also note that Fe speciation is far more complex than implied in Footnote 2 to Table 5, so that it might be better to simply state that modelled dFe includes all dissolved species. Also Fe* (column 1) is not defined, assuming that the * is not just an erroneously placed footnote marker.

The revised manuscript will attempt to clarify these concerns about dissolved iron.

Minor comments

Is "online" rather than "in line" the proper terminology? In any case the authors should define it at first occurrence.

We may be confused about the meaning this comment. As we do not use the term "in line", is the Referee suggesting that we replace all occurrences of "online" with "in line"? This is certainly not something we are willing to do. Online has a particular meaning in the ocean modeling world. Perhaps though, the Referee is suggesting that we should define "online" when it is first used. Such will be done in the revised manuscript.

2/23 change "model-predicted" to "modelled"

This change will be made in the revised manuscript.

7/32 "Carbon-13 is typically included in ocean models as a biotic variable influenced by fractionation effects during photosynthesis that depend on growth rate and phytoplankton type." could use some literature references.

In the revised manuscript, one or more references will be added.

8/25-30 Might want to mention here that while the equilibration times for Fe and DOC are much longer than for e.g. phytoplankton biomass, they are much shorter than for DIC or alkalinity.

We will consider making such a statement in the revised manuscript.

9/18 not clear what the stray < means

This is a typo. It will be removed in the revised manuscript.

12/20 "polynomical"???

Another typo. It will be corrected to "polynomial".

12/27-28 change "pH2O is the water vapor pressure at saturation" to "pH2O is the saturation vapor pressure at sea surface temperature and salinity" (see 15/26)

We will clarify this phrase in the revised manuscript.

16/18-21 The authors recommend that carbon chemistry calculations follow the Best Practices Guide (Dickson et al 2007). They might also consider mentioning that the BPG also gives formulae for the coefficients in equation (26). Interestingly, the definition of R used here differs slightly (1e-6 level) from that in the BPG.

The original reference is Weiss (1974). We may add a reference to the BPG in the revised manuscript. The value of R has been updated since the best-practices guide was published 10 years ago. A reference for that will be provided in the revised manuscript.

17/5 delete the '*' in equation 27

This sign is indeed unnecessary and will be removed in the revised manuscript.

Table 5 "Mole concentration of phytoplankton expressed as chlorophyll" I think just "Concentration of chlorophyll" is more accurate (see Table 9). I also don't think the second sentence of the footnote is necessary.

In the revised manuscript, Table 5 will list the name of this variable exactly as it is given in the CMIP6 data request.

References

Orr, J. C. and Epitalon, J.-M.: Improved routines to model the ocean carbonate system: mocsy 2.0, Geosci. Model Dev., 8, 485–499, doi:10.5194/gmd–8–485–2015, 2015.

Weiss, R. F.: Carbon dioxide in water and seawater: the solubility of a non-ideal gas, Mar. Chem., 2, 203–215, 1974.

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