Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-155-RC1, 2016

Geoscientific Model Development Discussions
Open Access
EGU

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

Anonymous Referee #1

Received and published: 9 September 2016

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 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).

Major comments:

Overall strategy and ordering of priorities

The weakest point of the strategy is that it is vague about the priority of experiments.

Printer-friendly version

Discussion paper

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.

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 to a more realistic level.

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.

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.

Alkalinity and speciation

I agree that using a truncated expression for alkalinity causes large errors, but the pa-

Printer-friendly version

Discussion paper

per 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.

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.

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.

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.

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.

Minor comments

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

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

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.

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.

9/18 not clear what the stray < means

12/20 "polynomical"???

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)

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.

17/5 delete the '*' in equation 27

Table 2 is the second footnote really necessary?

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.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-155, 2016.

Printer-friendly version

Discussion paper