

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2020-408

Anonymous Referee #2

Referee comment on "MESMO 3: Flexible phytoplankton stoichiometry and refractory dissolved organic matter" by Katsumi Matsumoto et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-408-RC2>, 2021

I agree with Reviewer #1's analysis that this manuscript represents a meaningful contribution to the current and potential users of the MESMO model and is well-suited in scope for GMD. I too recommend publication after my comments below are addressed. Many of my comments ask for better documentation and/or clarity of the methods used while I also offer some recommendations on the future calibration of the optional DOM cycling module.

Equation 3, Line 132-135. It is not immediately apparent how this equation is analogous to the $Q_{10} = 2$ relationship. For example, the value of FT for a temperature of 288 K is 0.973 and for a 10 degree higher value of $T = 298$ K the value of FT is 0.974 or only 0.1% higher.

Line 141 – 144. It is unclear how Eq 1 varies as a function of z_{ml} from the statements as written. Does this mean that nutrient uptake can be confined to model level 1 if $z_{ml} < z_c$?

Table 2b typo: $[NO_3]_0$ is listed as $[PO_4]_0$

Equation 5: It is unclear how this equation produces an estimate of P:C (or C:P) that approaches the observed range of ocean C:P stoichiometry of marine phytoplankton and associated nutrient uptake to produce said plankton stoichiometry. For example: if I use Eq. 5 and the parameters from Table 2 to compute the P:C of uptake for the cyanobacteria in a low latitude oligotrophic setting with in situ $[PO_4] = 0.1 \mu M$, $[NO_3] = 0.1 \mu M$, $T = 298K$, and $I = 150 W/m^2$ I get: $6.3 \times 0.614 \times 0.411 \times 0.827 \times 0.962 = 1.266$

for P:C or $1/P:C = 0.79$ for the C:P ratio. Redfield C:P is 106 or 117 to 1 and cyanobacteria should likely exhibit a C:P > 117:1. What am I missing? Is P:C expressed in per mille (‰)? There isn't any mention of this in the text or Table 2. If that is the case I compute the same example P:C = 0.001266 for C:P = 790:1. Is there any evidence from the literature for such a P-depleted uptake stoichiometry for small phytoplankton? The maximum C:P for cyano that I am more familiar with maxes out around 350 or 400:1. Figure 4d shows the cyano C:P and its max is ~280:1. Better clarification of how Eq. 5 (and Eq. 6) in conjunction with the parameter values in Table 2 are used to compute PFT C:N:P stoichiometry is needed.

Equation 19 and the discussion on the variability of fDOM (Line 234-243). I refer the authors to Roshan & DeVries 2017 in Nature Communications who report on the latitudinal variability of this parameter from an artificial neural network model of DOC production and export. The variability is within the range ~0.01 – 0.7 which differs from the 0.28 – 0.96 values used in this study. I do not mean to infer that the one study's range is more correct than the other. I do think that the manner in which the authors vary fDOM as a function of temperature in this study to be a clever formulation and as it captures the likely environmental factors that cause fDOM to vary related to the differences in nutrient levels, light levels, and phytoplankton community structure which co-vary in the ocean.

Equation 31 and value of Kr in Table 2. CaCO₃ is more soluble at colder temperatures unlike most solids due to the positive Gibbs free energy change at standard conditions for the dissolution reaction. The parameter Kr is listed as positive 0.69 in Table 2. Thus when computing the temperature scaling term for CaCO₃ remin rate $\exp(Kr \cdot T)$ you will get a faster remin rate at higher temperatures for CaCO₃, not at colder temps. Should the value of Kr be negative 0.69? This would give a faster CaCO₃ remin rate at colder temps. CaCO₃ solubility is also pH dependent, through the changes of [CO₃²⁻] as a function of pH, but without a full carbonate system formulation in MESMO I think it is ok to simply vary CaCO₃ remin as a function of temp since pH and temp co-vary in the ocean. The handling of the opal remin rate appears to obey the correct temperature dependence for opal with a higher opal remin rate at warmer temperatures in Eq. 33.

Line 482. Global average fDOM of 0.71 seems problematic. Other estimates that have been constrained by the available DOC concentrations across the global ocean are much lower, e.g. 20% Hansell et al., 2009; 25% Letscher et al., 2015; 17% Romera-Castillo et al., 2016; 20% Roshan & DeVries, 2017. I recommend that the authors revisit the prescribed range on fDOM in future calibration efforts of the DOM module. To be clear, in my opinion I do not believe that a fully calibrated DOM module for MESMO 3 to be a prerequisite for publication of the current manuscript.

Line 486-489 and Figure S1. What happened to the EqPac HNLC in MESMO 3? It's gone. This suggests there is not enough Fe limitation in that HNLC to keep NO₃ and PO₄ in the surface or that the new variable P:C and N:C formulations allow for too much NO₃ and PO₄ drawdown in this HNLC, although this latter mechanism should operate in the Southern Ocean HNLC too which is retained in MESMO 3. This particular deficiency of MESMO 3, it's lack of an EqPac HNLC should be identified in the text with a possible explanation given by the authors. Also can the authors provide a more formal statistical comparison of MESMO NO₃ and PO₄ to World Ocean Atlas, e.g. point by point correlation

and overall mean bias.

Figure 2. Eukaryotes are limited by silica nearly everywhere in the global ocean except the Southern ocean and subarctic N Pac. How does this compare to the literature? My thoughts are that silica limitation is not as widespread in the real ocean. Perhaps the RNPG formulation for Euk growth is contributing to the high levels of Si limitation? Cyanobacteria are limited by nitrogen almost everywhere except the Southern Ocean. This is likely a result of the biased low surface NO₃ concentrations simulated by MESMO 3. Most field estimates suggest the Sargasso Sea is P-limited to all PFTs with this feature only simulated for the diazotrophs in MESMO 3. I recommend a discussion of the PFT nutrient limitation patterns in comparison to the broad patterns identified by the extant literature on the topic, pointing out where MESMO 3 does a better job and where it is deficient.

Figure 4 PFT C:P stoichiometry looks great and largely matches the global patterns and variability seen in field observations.

Figures 7, 8, and 9. Some comments about tuning MESMO DOC. MESMO3 DOC has a positive mean bias when the authors used literature values for the DOC production and remineralization fluxes. The authors then made adjustments to the production flux (from 1% of NPP produces DOC_r to 0.2%). I wish to point out to the authors that they can also make adjustments to the remineralization fluxes, i.e. increase the decay rate constants to attempt a better calibration of simulated DOC vs. observed. Neither the DOM_r production or consumption fluxes are well constrained observationally at present. The MESMO 3 deep ocean DOC_t gradient captures well the thermohaline circulation gradient. The simulation of DOC_{sl} in the upper ocean is reasonable as compared to the neural network data product of Roshan & DeVries. The authors choice of tying the DOM_{sl} production flux to temperature is a good first step to capture the behavior seen in the DOC observational data and the neural network product that DOC production peaks in the subtropical gyres where temperatures are also high. On line 647-650 the authors state that they may try to tie the fDOM parameter negatively to nutrient concentrations to try and achieve a higher DOM_{sl} production flux in the subtropical gyres. In my expert opinion I believe that tying fDOM to both temperature and inversely to nutrients will likely capture the behavior they wish to reproduce. I refer the authors to a review of this topic on 'extracellular release' by phytoplankton within Chapter 3 of "Biogeochemistry of Marine Dissolved Organic Matter" 2nd Edition. Edited by D.A. Hansell & C.A. Carlson published by Academic Press. Rather than temperature, to be more physiologically consistent, the authors may wish to tie fDOM to PAR rather than temperature (although both co-vary in the ocean). This could be left to a future study that focuses on calibrating the DOM module in MESMO 3.

Figure S6. Why not also show the water column depth integrated denitrification flux alongside the N₂ fixation flux?