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Geoscientific
Model Development
Discussions

Interactive comment on “MEDUSA-2.0: an intermediate complexity biogeochemical model of the marine carbon cycle for climate change and ocean acidification studies” *by* A. Yool et al.

Anonymous Referee #2

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Yool, Popova and Anderson describe and present an updated version of the MEDUSA model. The predecessor, MEDUSA-1.0 has been extended by oxygen and components of the carbon cycle, including a variable stoichiometry in some of the pools, a simple formulation for pelagic-benthic exchanges and extended phytoplankton parameterisations. Model results are compared to those of its predecessor, and to observations of nutrients, oxygen, Chl and production, pCO_2 and CO_2 flux at the sea surface.

General comments:

The modified model presents important additions to the MEDUSA model and is a more appropriate tool to investigate recent questions such as ocean acidification. The paper

is very well written, concise and mostly comprehensive. I really appreciate the effort the authors have put into the model documentation, even if - having had a look at the MEDUSA-1.0 documentation - the paper in some parts repeats the previous description. However, this is stated clearly at the beginning, and I agree that a “stand alone” version of model description is really helpful. I would even suggest to extend this a bit more with respect to the large detritus description (see below).

The few concerns I have are related to the representation of oxygen, alkalinity, and the associated stoichiometry (see below). I would further appreciate a few sentences on two discrepancies between models and observations that have not been addressed in the - otherwise very open and thorough - discussion (see below). I recommend publication after this, and a few minor points, have been addressed.

Specific comments:

1) As already noted by Anonymous Referee 1, oxygen requires a more detailed description. I also suggest to comment more on the stoichiometric relations used by the model. In particular, my suggestions are as follows:

1.1 Eq. 14 and Table 4: As far as I understand, $\theta_{nit} = 2$ describes the oxygen demand for complete nitrification, i.e. it stems from the bulk equation $NH_3 + 2O_2 \rightarrow HNO_3 + H_2O$. If this is true, I suggest to mention it explicitly.

1.2 For remineralisation including nitrification the model seems to use the stoichiometry by Anderson (1995), with -O_2:P=150, or, alternatively, -O_2:N = 9.375 $\approx = \theta_{nit} + \theta_{rem}\,\theta_{Pn}$. If this is the case, I suggest to mention it explicitly.

1.3 Table 4 notes a value of $\theta_{Phy} = 9.4375$ for “phytoplankton O_2:N ratio”, which I find quite confusing. If this is meant to be the oxygen content of phytoplankton organic matter (i.e. about 19 oxygen atoms per nitrogen atom, or more that 300 oxygen atoms per phosphorous atom), this would imply an extreme aqueous phytoplankton (see e.g., Paulmier et al., 2009 for the implications of different organic matter constituents, and their relation to oxygen demand of remineralisation). I therefore suggest to either omit this parameter (which does not seem to be used anyway) from the description, or to be more explicit about the oxygen cycle and associated stoichiometry. The same holds for the zooplankton parameter, θ_{zoo}

1.4 There are Taylor plots for all sorts of model-observation comparisons - why not for oxygen?

1.5 A side note: What about denitrification? Currently denitrification seems to be built implicitly into that model (p. 1289, lines 9-10). On the other hand, the model already simulates - in contrast to phosphorous based models, e.g. by Najjar and Orr - another potential oxidant, namely nitrate. With the given parameterisation, however, simulated nitrate to me seems to be closer to phosphate times 16, than to "real" nitrate. The issue is discussed briefly at the end of the paper, but I suggest to perhaps comment on this also in the model description. I also would not consider this as an optional task for a "specialist" model (as stated in the discussion), but, given that the model is supposed to be "an efficiently-sized tool for realistically simulating the oceans major biogeochemical cycles" (p. 1309, lines 9-11) a more necessary future step than some other complications.

1.6 The only effects on alkalinity considered in the model seem to be those related to $CaCO_3$. However, this representation is not exhaustive, as production and remineralisation, and associated changes in nitrate (and phosphate) will also affect alkalinity. See e.g. Paulmier et al. (2009) for a brief overview on how the different processes affect this tracer, or Wolf-Gladrow et al. (2007) and citations therein for more details. Also, the effects of denitrification (i.e. the removal of nitrate) might have a large effect, if these were considered.

2. (p. 1282-1284) I found the description of fast detritus production (and remineralisation) a bit confusing. The "T"-terms are not explained, and suddenly there seems to be some reference to the vertical box index k (correct?). Does, for example, eqn. 88 mean

that the gain in detritus-N in box $k+1$ comes from detritus in the layer above, plus the local production from zooplankton grazing etc? In other words, does detritus sink one vertical box per day? If so, sinking speed is is coupled to the vertical discretization, which could be mentioned, in case other users want to apply this model to different grids.

3. Many of the model features in the equatorial Pacific and the upwelling off Peru and Chile do not appear in the observations, e.g. elevated DIN or production. Could this be due to an insufficient physical model? On the other hand, at least along the equator the model's ΔpCO2 seems match the observations quite well (but not in the coastal upwelling). A few sentences on this would be nice.

4. (Fig. 25 and p. 1299 ff) Astonishingly, the model shows a high rain rate of organic matter to the sediment south of $\approx 40°$S. I am not aware of this pattern in any observation or compiled data set (e.g., by Seiter et al., 2005, or Jahnke, 1996). Is it possible to comment on the reasons of this mismatch?

Technical/minor corrections and comments:

p. 1272, eqns. 23 and 24: I suppose +N and +F shouldn't be subscripts?

p. 1274, eq. 36: What is U_∞? Could it be explained below?

p. 1274, lines 18/19: brackets of reference

p. 1274, eqn. 38: If I am not mistaken, then units of $R_{Si:N}$ are mol Si/mol N? How can the inequality relates this term to its inverse?

p. 1277, eq. 50 and 53: These equations, i.e. respiration, seem to be the same, regardless of the food composition - so why write them two times? (Likewise for eqns. 63 and 66.)

p. 1280, eq. 79: I assume there is an index missing? (M_{Dc}?)

p. 1281, line 4: deposition

p. 1295, line 11 and elsewhere: what exactly is meant with "surface" - first layer only?

p. 1305, line 17: sensitivity –> seasonality?

p. 1307, line 5: foraminiferans

p. 1307, line 16: "influence OF ambient marine chemistry"?

p. 1309, line 6: hierarchy

References:

Anderson, L.A., 1995. On the hydrogen and oxygen content of marine phytoplankton. Deep-Sea Res. I, 42(9), 1675-1680

Jahnke, R.A., 1996. The global ocean flux of particulate organic carbon: Areal distribution and magnitude. Global Biogeochem. Cy., 10(1), 71-88

Paulmier, A., Kriest, I. and Oschlies, A., 2009. Stoichiometries of remineralisation and denitrification in global biogeochemical ocean model. Biogeosciences, 6, 923–935

Seiter, K., Hensen, C. and Zabel, M, 2005. Benthic carbon mineralization on a global scale. Global Biogeochem. Cy., 19(1), doi:10.1029/2004GB002225

Wolf-Gladrow, D. and Zeebe, R.E. and Klaas, C. and Körtzinger, A. and Dickson, A.G., 2007. Total alkalinity: The explicit conservative expression and its application to biogeochemical processes. Mar. Chem., 106, 287-300

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