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Geoscientific
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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 #1

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This model development manuscript describes the second version of the MEDUSA model. The modifications made to the model, which include the addition of state variables for DIC, alkalinity, oxygen, detritus C, and benthic pools of C, N, Si, and CaCO3, do indeed require a descriptive publication such as this. Overall, the manuscript is well written and tends to follow the general format of the first MEDUSA description. While some of the new biogeochemical properties require a better description and analyzes, there are no major issues with the manuscript that would prevent its publication after a moderate revision.

General comments:

1) Oxygen needs a better explanation and/or diagram. It's a new tracer but not well described or justified aside from the short paragraph on pg. 1289. The volume and spatial location of the oxygen minimum zones is also not shown in the results section. Can the model get the oxygen minimum zones in the right place? How would a plot of oxygen at ~300m compare to World Ocean Atlas data? Most models have a problem getting oxygen right, especially in the Indian Ocean, is MEDUSA any better?

2) Why do none of the results show simulated pH? The title mentions ocean acidification and the model can certainly calculate pH. Can the model reproduce the surface ocean pH decline from its preindustrial value?

3) Why are no results shown for the 2-D benthic tracers that have been added to the model? The authors state that these variables were initialized at a value of zero. What happens during the model runs? Perhaps supplemental figures could be shown that indicate what is happening to the simulated benthic pools of C, N, Si, and CaCO3.

4) Simulating the export of carbon to the deep ocean is one of the major reasons for the improvements made to the model, yet no serious attempts have been made to compare the model results to any observations. I suggest that the authors make some comparisons (both graphically and statistically) to the deep sediment trap data compiled by Honjo et al. (2008). Note that this data set also contains biogenic opal data that can be compared as well. Reference: Honjo, S., Manganini, S.J., Krishfield, R.A., Francois, R., 2008. Particulate organic carbon fluxes to the ocean interior and factors controlling the biological pump: A synthesis of global sediment trap programs since 1983. Progress in Oceanography 76, 217-285.

5) Why aren't more DIC and alkalinity comparisons made to the GLODAP database? It would be nice to see figures like Fig. 8, 9, and 10 comparing DIC and alkalinity. Annually averaged surface DIC and alkalinity comparisons (maps and statistics) could also be included.

Specific comments:

1) Why do zooplankton of the same size class not prey on each other (i.e.,self-predation)? How are these losses and biogeochemical cycling accounted for? Is it implicit in their mortality term?

2) Zooplankton grazing does not appear to be temperature dependant. Without temperature dependence there may be some instances in colder waters where strong top-down control occurs because the fixed zooplankton growth/grazing rate exceeds the temperature-dependent phytoplankton growth rate. Could the authors comment on this?

3) In equations 22 and 29 the "I" looks somewhat like a slashed division sign. In both equations this becomes confusing because the denominator is raised to the $\frac{1}{2}$ which looks very similar to the "I" just before it. Can the font of the "I" be changed?

4) In equations 22 and 29 how is irradiance calculated? What kind of light attenuation occurs? Is it dependant on phytoplankton biomass (i.e., self-shading)? If this is described in another paper could the authors please indicate which one?

5) Alkalinity paragraph on pg. 1289 (lines 12 – 17) is there any justification for this simplistic model and parameterization?

6) Pg. 1299 line 10 "localized observations" are mentioned. What regions or where are these localized observations from?

7) Pg. 1299 lines 12- 22 referring to Fig. 25. Do deep Chl/productivity maxima occur? I suspect that they do. This should be mentioned.

8) Pg 1304 line 9. The reference to Riebesell et al. is not clear. What did Riebesell et al. show?

9) Pg. 1308 lines 7 – 14. Nitrogen fixation and denitrification are discussed as potential future model improvements. This could be very important if the model is used in climate change studies. Right now the model assumes that the nitrogen budget is balanced. If future changes occur to throw off this balance, then the model won't work well since

it's a nitrogen-based model and the ocean losing or gaining nitrogen would change the biogeochemical dynamics considerably. Perhaps the authors should mention this caveat somewhere.

10) Fig. 26 right panels can the color bar be changed? There are really only 3 colors (limiting factors) so a range probably isn't needed and is confusing.

11) Fig. 29 and pg. 1301 why is only N detritus shown don't we have C detritus now. How does it compare especially give the varible C:N ratio detritus can now have. It's hard to figure this out even if looking at both Fig. 29 and 25.

Interactive comment on Geosci. Model Dev. Discuss., 6, 1259, 2013.