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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 #3

Received and published: 18 April 2013

General Comments:

The manuscript describes the MEDUSA-2.0 global biogeochemical model, which is an upgrade to MEDUSA-1.0 that seeks to explore perturbations of the marine carbon cycle due to climate change and oceanic carbon addition. This model, as compared to its earlier version, includes several new tracers that represent the marine inorganic and organic carbon cycles, dissolved oxygen, and benthic nutrient cycling. The paper describes 1) core features of the previous version of the model as well as modifications, 2) the addition of new state variables and parameters, and 3) the steady-state results of a spin-up of the model from 1860 to 2005. The model description is good and its

subject is very appropriate for publication in a specialized model description journal such as Geoscientific Model Development. The impact of carbon addition to the ocean and atmosphere on the marine carbon cycle, and its role in ocean acidification, is an incredibly important subject and a variety of models and experimental work will be required to gain a better grasp of the magnitude of this complicated problem. This model has the potential to do just that. However, as it has been presented, it is difficult to identify what MEDUSA-2.0 will add to this scientific question that other comparable models could not. This issue was not sufficiently argued by the authors and should be included in the abstract. Furthermore, essential figures and results describing the state of the MEDUSA-2.0 marine carbon cycle (DIC and alkalinity) and comparison to available data, were lacking. I expect that the model will be publishable; however, I view the problems mentioned above as important omissions and recommend that the manuscript be returned to the authors for major revisions.

Specific Comments:

I have read and agree with the general and specific comments of Reviewer #1 and will not repeat them here unnecessarily.

1) Important variables were not presented, such as DIC and alkalinity, along with comparison to available data (GLODAP, for example). Given that the goal of the manuscript is to describe a model that will be used to study the marine carbon cycle, these are absolutely necessary and will go a long way to justifying the value and novelty of MEDUSA-2.0.

2) Discussion of many of the results was not more than a simple description of the most evident features of the associated figure. The authors must provide (for pCO2, DIC, and alkalinity, among others) more interpretation and insight, and in particular a more profound discussion of the sources of model bias.

3) The authors do not sufficiently discuss how this model compares to others of similar structure and complexity. More importantly, the authors do not describe what are the

most positive features of the model and why one should use it over other models. This should be a prominent feature of the abstract.

Page 1261 Line 6-11: A core result presented in Behrenfeld (2006) was that net primary production had been decreasing from 1999 to 2006. However, this was following a strong increase in net primary production during the 1997 to 1999 transition from El Nino to La Nina. To say that the conclusion of Behrenfeld (2006) is that primary production is decreasing is a much stronger conclusion than that drawn and detailed in the article, and is moreover an incorrect one. Please rewrite this sentence to better reflect the limitations of Behrenfeld (2006). Moreover, since it is by no means clear how carbon dioxide emissions will impact net primary production in the future, please phrase this as something that is being investigated and not something that is certain (see, for example, Taucher, J., and A. Oschlies (2011), Can we predict the direction of marine primary production change under global warming? Geophys. Res. Lett., 38, L02603).

P 1262 L 22-23: I do not agree that a multi-decadal hindcast simulation for the years 1860-2005 was performed, since no data throughout that time period was used to validate the model. Instead, I would term this simulation as a spin-up starting from a pre-industrial state for certain variables (DIC and alkalinity, results not shown) and a present-day state for others (DIN, silicic acid, and oxygen).

P 1277 L 17 (Eq. 53): On L 6-8 the authors note that excess carbon is respired and excess nitrogen excreted. Therefore, in the case where cabon is limiting, there should be no excess carbon, and therefore no excess carbon respired. I wonder therefore why equation 53, describing R_Z,mu, is not simply zero, as equation 49? Another way of asking this question is, why are the respiration equations (53) and (50) identical to one another under the different regimes of nitrogen and carbon limitation?

P 1278 L 7-8: Since there are no exact equivalents in the microzooplankton equations, equations 55 and 56 should be explained in more detail.

P 1279 L 2: Same comment as for equation 53. Why is this not zero?

P 1279 L 12-13: I do not understand the sentence describing equations 67 to 71 as density-independent terms. These appear to all be linear density-dependent terms.

P 1280 L 1: Based on the response to the previous comment, this should be corrected so that it contrasts with the linear density dependent terms of equations 67-71.

P 1287 L 6-14: Since a core addition of this model is the inorganic carbon cycle, I think it would be useful to discuss the sources of the differences in Fig. 3 in more detail.

P 1283 L 5: The order of presentation in this section is different from the sections before it. Here, the authors describe equations and then present the equation, whereas earlier the authors presented equations and then described them. Please change this so that the entire text is consistent.

P 1295 L 10-21: The authors did not describe a possible cause of the most noticeable discrepancies between observational and simulated results for DIN and silicic acid.

P 1296 L 17-end: A few lines discussing how chlorophyll is better or worse represented than in MEDUSA-1.0 would be instructive.

P 1297 L 5-17: The authors should describe or suggest reasons why the global primary production has decreased relative to MEDUSA-1.0 and whether this is a positive or negative feature of MEDUSA-2.0.

P 1298 L 3-6 and Fig. 18: To be consistent with the observations as presented in Figures 13 and 14, I suggest that the observational data used for the Taylor diagram be the average of the three models (VGPM, Eppley-VGPM, and CbPM) instead of just those for the VGPM estimate.

P 1298: As mentioned for several other variables, it would be instructive to discuss a potential source (or sources) of the discrepancies between the model and observations.

P 1301 L 3-9: It would be a very useful validation to include a brief discussion comparing these results to databases of zooplankton (see COPEPOD, http://www.st.nmfs.noaa.gov/copepod/, for example).

Technical Corrections:

I suggest more clearly identifying the state variables by bolding or with an overbar, for example. The model consists of many equations and identifying the state variables clearly would greatly improve their readability.

P 1262 L 1: Archer reference not in parentheses

P 1262 L 4: I think “shadowing” should be “shallowing”

P 1268 L 9: The equation for time rate of change of DIC does not have a number.

P 1270-1271: Equations 15, 16, 17, 18 are missing the denominator d in the d/dt

P 1272: Equations 23, 24. The + N and + F terms in the denominator of the right-hand-side of the equations should not be subscripts.

P 1274 L 18: Reference to Martin-Jezequel et al. (2000) should be in parentheses

P 1275 L 14: “these” instead of “this”

P 1277 L10 and 14: The ellipses are not appropriate here

P 1277 L 23: As noted earlier, the equation for time rate of change of DIC is not numbered, and this causes an incorrect reference to the DIC equation, which is cited as equation 13 (which is the equation for time rate of change of alkalinity on page 1269).

P 1278 L 1: Although it is included in the equations, I think it would be instructive to mention in words the four prey items of the macrozooplankton.

P 1279 L 13 and 17: The ellipses are not appropriate here

P 1279 L 5: Should be “losses due to grazing”

P 1279 L 6: Should be “losses due to other processes”

P 1280 L 5: The comma at the end of this line should be a period.

P 1280 L 16: The S in the left hand side of equation 77 appears to be a different font (or italicized) than the rest of the text.

P 1284 L 6: in the fc(lat) term, please correct the fonts

P 1286 L 10: silic should be silica

P 1292 L 15: The reference to Jones et al. (2011) should be in parentheses.

P 1295 L 17-18: Correct “show very the same patterns of bias”

P 1297 L 6: “Simpled” should be “simple”

P 1298 L 2: “that” should be “than”

P 1298 L 12: Replace +ve with positive

P 1303 L 17: “size of supercomputers” should be “processing power of supercomputers”

P 1307 L 5: “foraminiferrns” should be “foraminiferans”

P 1307 L 16: “influence ambient” should be “influence of ambient”

Figure 1: There is an errant line to the right of the Benthic CaCO3 box.

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