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Geoscientific Model Development Discussions

Interactive comment on "MESMO 2: a mechanistic marine silica cycle and coupling to a simple terrestrial scheme" *by* K. Matsumoto et al.

Anonymous Referee #1

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This manuscript describes recent developments that have led to two new versions of the Minnesota Earth System Model for Ocean biogeochemistry (MESMO 2 and MESMO 2E). The major model development is the addition of a new large phytoplankton class (diatoms) which requires a more complete representation of the marine silicate and iron cycles. MESMO 2E also includes the standard GENIE vegetation scheme (ENTS). Additional changes to the model include new seasonal wind forcing and an improved albedo representation of ice sheets. The paper is clearly written, interesting, and appropriate for Geoscientific Model Development.

Improvements to the representation of marine biochemistry in models is certainly an important research objective. Diatoms play an crucial role in in the carbon cycle and the response of ocean biology to climate change is likely to be key in determining the long term evolution of CO_2. Understanding diatom productivity and opal preservation will help in reconstructing paleo estimates of ocean productivity. This paper describes an advance in the representation of the marine biochemistry in this model and the ability to track delta 30 Si in a fully coupled model is unique.

As noted below, there are a few typographical errors in the paper and some text that needs clarification. I have also made a few suggestions for alternative or additional model modifications but changing these is not required - they are just for future consideration. Perhaps my greatest concern is that the silicate cycle (the novel development) is not compared well with observations and that the proposed mechanisms responsible for the distribution of Si limitation are not well explored. As a minimum a plot of observed silicic acid concentration should be provided in Figure 9. Differences from previous estimates of Si limitation (like Moore et al.,2002) should be discussed. A recent paper by Sousa et al. (2012) in GBC may be useful in comparing modelled Si isotopes to data - at least in the Atlantic. Although I do have a few other specific concerns, once these have been addressed, I feel that the paper could be published in GMD with only minor revisions.

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Specific Comments and Suggestions

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Page 3002, line 7: Maybe: "... allow for a land albedo feedback ..." or "allow for land albedo feedbacks ..." would be better.

Page 3002, line 16: Surely you mean the snow-free albedo ranges from 0.2 to 0.5, although 0.5 seems very high for a snow-free albedo. There must be some kind of a land snow feedback in MESMO 1 ... isn't there? Can you please clarify this.

Pages 3002-3003, lines 22-2: It seems to me that the planetary albedo should be a diagnostic quantity not something you specify. This is partly semantics but maybe you

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should think of this as a change in atmospheric albedo, which is then held fixed, while your planetary albedo is free to vary with changes in snow, ice or vegetation. It is not clear at the moment if your scaling effects these "feedbacks" on planetary albedo or not. Please clarify this. Since your atmosphere is two dimensional, another way to "tune" the surface air temperature would be to make a small, uniform, constant adjustment to the outgoing longwave parameterization.

Page 3003: Line 10: Missing "." after (1989).

It makes sense to try and make the wind forcing more consistent. It would appear that a side benefit of including seasonally varying winds is that you can reduce the use of an arbitrary scaling constant. Your comment that the ECMWF winds are 40% stronger than the observation-based winds (SOC) over the Southern Ocean, is curious. Both are observation based. They may just have different ways of "interpolating" missing data. Is this difference in strength before or after scaling the MESMO 1 wind stress? Please clarify. Perhaps some of the differences are from comparing monthly to annual mean strength? Monthly average winds may well be stronger than annual average winds (daily, stronger still) - especially in areas of high variability. In a similar vein, do you use the monthly mean of daily wind speed from ECMWF? Averaging daily or hourly wind speed would be preferable as it would be much stronger (more realistic) than obtaining wind speed from monthly averaged winds. It is not clear what you did here.

Page 3003, lines 14-18 and Table 1. You should define the domains over which you calculate NADW, CDW and NPDW delta 14C.

Page 3003, line 23: I think you should have a comma after "completely".

It is unfortunate that MESMO must jump through such hoops in order to maintain a reasonable Atlantic meridional overturning circulation. Having to scale the winds in the North Atlantic and have different fresh water corrections in different basins and hemispheres makes me concerned that the response of the overturning to climate

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change is not going to be realistic. If you can not simulate the mechanism that creates the overturning correctly, how can you believe the response? Would it not just be best to remove all the wind scaling and just use a single fresh water adjustment? - it is simpler (more transparent) at least. Do you know how much more of a fresh water adjustment would be required if you removed wind scaling altogether? Would this have other consequences? Having said that, it is disturbing that this fresh water adjustment appears to be getting larger rather than smaller (in MESMO 2 compared to MESMO 1). Can you explain why the newer land surface scheme (in 2E) has a higher overturning? Is this heat (lower albedo => colder NA?) or fresh water related? Changing these flux corrections is perhaps beyond the scope of this paper but this is something that should be carefully considered in terms of future development.

Page 3004, line 20: It is not clear where the values for the new "Kx's (for diatoms) come from? The values seem a bit arbitrary. Any justification seems hand-wavy. How well can you constrain any "tuning" of these parameters?

Page 3004, line 22: "nutreint" should be "nutrient".

Page 3005, line 16-18. Not sure I see the justification for this in Sarmiento et al 2004. Please explain this more clearly.

Page 3006, line 11: Maybe "as the soluble" would be better.

In the discussion of how iron is implemented, it is not clear what is new and what is in the original GENIE "framework". Can you make this more explicit?

Page 3007, line 2: I think you mean "nM". "nm" is reserved for nanometre rather than nanomolar. Maybe using the equivalent nanomole per Litre [nmol L-1] would be less confusing. You could also use nmol kg-1 since most of your other concentrations are in mol kg-1 (although the SI unit for concentration is mol m-3).

Page 3008. line 2" "phytosynthesis" should be photosynthesis".

Page 3008; lines 4-5: Maybe provide a reference here.

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Page 3008, lines 12-13: Can you explain why these are so much lower than in Williamson et al. (2006)?

Page 3008, lines, 14-18: 5.5% is quite large. Again I am not sure adjusting planetary albedo is the best way to tune global average absolute SAT. If you continuously scale the overall planetary albedo you will also be modifying the effect of surface changes. Planetary albedo is also not very uniform and so changes in SAT will be concentrated in certain locations. Since the source of the absolute SAT error can not be easily identified, I still think a single, uniform constant added to the (relatively uniform) outgoing longwave would be a better way to adjust this. I suspect any adjustment would be very small compared to any uncertainty in the observed outgoing longwave.

The "equilibrium" simulations are not well described. What is the forcing year and how long is the spin-up?

The increase in overturning in MESMO 2E is clearly beneficial to the simulation of relatively young delta 14 C in NADW and this helps increase your NPDW and NADW contrast. It would be better yet if the overturning could be increased further. I am not sure I buy the argument that it is poorly resolved shelf processes that are the problem in not maintaining old NPDW. I do agree that AABW production and its transport may be to blame but, in general, your CDW is about the right age. It may be that your sea ice is too far north, causing AABW formation to form too far north (reducing its isolation). It is possible that your deep horizontal mixing is too high. Your ventilation of the NPDW may be too strong. You may also have inadequate topographic resolution to slow the invasion of younger AABW into the North Pacific. There are many possibilities but I think missing shelf processes is probably a minor one.

Page 3009, lines 8-12: Are the reanalysis winds really stronger - even after scaling the MESMO 1 winds by 2?

Page 3009, lines 20-22: Is your SAT being compared over the 1960-1990 average of Jones et al. or is your SAT at some pre-industrial equilibrium? You should be more

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specific as to the times in your comparisons.

Page 3010, lines 1-5: Perhaps you should say something more like "contributes" rather than "leads" here. Certainly some NADW is formed in the GIN seas but much of it is also formed in the Irminger and Labrador seas. "Iceland" is also one word rather than two.

Page 3010, lines 7-9: How was this tuning done? It seems to me that optimal solutions might not be unique given the uncertainly in the parameters and the data you are comparing to. Was this objectively tuned? If so, to what?

Page 3010, lines 13-16: It seems like you have tuned the model to show Si limitation for LP over much of the globe (your figure 8), justified with Sarmiento et al. 2004. However, looking at Figure 8 from Moore et al. 2002 (see figure below) it would seem that they suggest silica is limiting over a very small area while iron is the dominant limitation for diatoms over much of the ocean (nitrogen being the other major limitation). I realize that this is just another model but can you reconcile your figure 8 with figure 8 from Moore et al.?

Page 3010: The model seems to have about 73% of total production from diatoms, which seems a bit high. Are estimates not closer to 50%? Do you have a good reference for this (maybe Nelson et al., 1995)? Your high values of opal production (upper limits of estimates) seem to support the idea that the model has excessive diatom production.

Page 3011, lines 20-23: Is the improved upwelling (compared to MESMO 1) from increased wind stress?

Page 3012, lines 1-9: It seems that Si(OH)4 in the North Atlantic is pretty similar to the North Pacific (Figure 9a). I think it should be lower, as in Figure 1 of Horn et al. (2011). Is there a reason for this? You should show the observed values in Figure 9. The contrast between the northern Atlantic and Pacific basins does not show up in

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modelled $Si(OH)_4$ (as observed) but it does in modelled Si:N uptake. Is the low Si:N uptake just due to high iron from dust in the North Atlantic? Perhaps a bit more detailed explanation in the text would be helpful.

Page 3013, lines 22-28: Considering that the modelled vegetation stocks do not include land use change, these seem a bit low. It looks as if the modelled boreal forest in Northern Asia is under represented. Is this due to a poor climate simulation there? Again, I am not suggesting the vegetation distribution needs to be changed - only that it should be considered when making further improvements,

Page 3015, lines 1-3: This sounds slightly awkward to me. Maybe "By implementing the existing Fe code, two classes of phytoplankton, and a dependence of the $Si(OH)_4$ utilization on Fe availability, the model is able to simulate key features of the marine silica cycle. These features include extensive $Si(OH)_4$ limitation . . ." would be better.

I think that you have not really shown that the mechanism behind Si depletion is due to low values being exported via AAIW. This still seems speculative, and while this might be something you could show in a model, I see no evidence that this is the case. Perhaps this is suggested by Figure 9c, but it is not very convincing. Consider revising the wording of the second last sentence in the summary.

Page 3015, lines 7-9: Maybe something like ", which have recently received attention," would be better. Even that sounds a bit odd - maybe also consider revising your last sentence.

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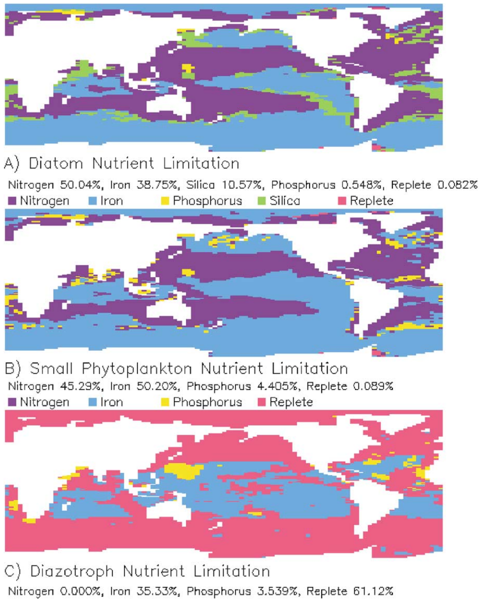

Fig. 8. Nutrient-limitation patterns for the diatoms (A), the small phytoplankton (B), and the diazotrophs (C) during summer months. Areas where all nutrient cell quotas are >97% of the maximum cell quota values are arbitrarily defined as nutrient-replete. Also shown is the percentage of total ocean area where each nutrient is limiting growth.

Fig. 1.