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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 #2

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Overview

The manuscript describes an update of the MESMO 1 model, published previously by the same lead author. The manuscript presents two new model versions: MESMO 2 and MESMO 2E. The former expands the marine biogeochemistry scheme to include the ocean's iron and silicon cycles and adds "large phytoplankton" (synonymous with diatoms). The silicon cycle in MESMO 2 also includes silicon isotopes, used in observational studies to constrain the role of ocean biology in this major elemental cycle. MESMO 2E includes the same oceanic upgrades from MESMO 1 to MESMO 2 and additionally adds the ENTS terrestrial biota submodel. This gives MESMO 2 an additional suite of earth system processes with the potential to influence simulated climate. As well as biological additions, the new models have a number of changes made to

the setup and parameterisation of their physical climate components, including numerous changes to parameters and forcings. The manuscript includes comparisons with observations and between the various model versions.

Summary

My primary comment about the manuscript would be "disappointing". From the results presented, MESMOs 2 and 2E appear to be valuable additions to the GENIE stable. However, the manuscript does a fairly patchy job of describing the model, with both a number of significant missing details and a series of lingering questions.

Considering that GMD is a forum for accurately and completely describing models, the description of MESMO, or rather its lack, is particularly difficult to justify. For instance, there's little reference to the physical framework used, a cursory treatment of the marine biogeochemistry that makes its modelled components difficult to ascertain or understand, practically nothing on the simulation design, and only vague allusions concerning how the various versions of the models were developed.

A particular bugbear is that although MESMO 1 has been "upgraded" via a number of discrete changes to ocean physics, the marine biogeochemistry and the land scheme, the authors have tended to lump these increments together such that it is difficult to understand (and, apparently, to explain) which performance improvements can be traced to which upgrades. Probably naively, it would seem to me that a step-by-step upgrade and description would have been preferable, but here each step is more of a jump.

Finally, while the manuscript does try to put MESMO into context by comparisons with observational data, it doesn't give much of an idea about where MESMO sits in relation to other models – even ones associated with the GENIE framework such as BioGEM.

I've listed a detailed series of comments and criticisms below. While I don't believe that the model presents any serious flaws (from what I can judge from the results), I find that the manuscript requires significant expansion – largely just description – to make it a useful overview of the MESMO models. My recommendation is publication after major revision.

Review criteria

1. Does the paper address relevant scientific modelling questions within the scope of GMD? Does the paper present a model, advances in modelling science or a modelling protocol that is suitable for addressing relevant scientific questions within the scope of EGU?

Generally, yes.

2. Does the paper present novel concepts, ideas, tools, or data?

Not especially, though it does not claim to.

3. Does the paper represent a sufficiently substantial advance in modelling science?

Yes; a useful tool has been developed.

4. Are the methods and assumptions valid and clearly outlined?

Mixed; there are large gaps in the description of the model.

5. Are the results sufficient to support the interpretations and conclusions?

Generally, yes.

6. Is the description sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? In the case of model description papers, it should in theory be possible for an independent scientist to construct a model that, while not necessarily numerically identical, will produce scientifically equivalent results. Model development papers should be similarly reproducible. For MIP and benchmarking papers it should be possible for the protocol to be precisely reproduced for an independent model. Descriptions of numerical advances should be precisely reproducible.

No. The presumption of the authors would appear to be that earlier papers should

also be consulted. This is acceptable for the underpinning climate model, but the developments in marine biogeochemistry here need to be better described.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes.

8. Does the title clearly reflect the contents of the paper? The model name and number should be included in papers that deal with only one model.

Yes.

9. Does the abstract provide a concise and complete summary?

Yes.

10. Is the overall presentation well structured and clear?

Generally, yes.

11. Is the language fluent and precise?

Yes.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Some are. Where they are, they are used correctly.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

I have suggested an extra table and some figure modifications.

14. Are the number and quality of references appropriate?

Yes.

15. Is the amount and quality of supplementary material appropriate? For model description papers, authors are strongly encouraged to submit supplementary material containing the model code and a user manual. For development, technical and benchmarking papers, the submission of code to perform calculations described in the text is strongly encouraged.

Unfortunately I have been unable to access the supplementary material. Given my concerns with the manuscript, I judge this a secondary consideration at this point.

Specific comments

- Pg. 3000, ln. 17-23: while readers familiar with GENIE may understand the version used by the authors, there should at least be some mention of Earth system components, spatial representation (horizontal and vertical), time-step, etc.

- Pg. 3001, ln. 25: the text states that diatoms are the most important biological group when it comes to export production, and cites Armstrong et al. (2002) in support; this and subsequent papers also emphasise the role of CaCO3, and even near-contemporaneous ones, such as Klaas & Archer (2002), put the emphasis on this latter biomineral ahead of opal; as MESMO includes a parameterisation of CaCO3, the relationship of the two biominerals should be made clear

- Pg. 3002, ln. 16: the text would be much clearer on albedo if the authors added a sentence explaining why it varies with latitude; at present readers not versed in climate might suspect everything is simply more white at higher latitudes (which, unhelpfully, is not entirely wrong)

- Pg. 3002, ln. 16: I would be grateful for some explanation for why the authors improve MESMO 1 in several directions (physical climate, marine biogeochemistry) simultaneously rather than incrementally; not least so that the later intercomparisons allow separation of the contributions of the new components; at present, it is difficult to ascertain which change to MESMO 1 now present in MESMO 2 is most significant

- Pg. 3002: more generally, it would be helpful if the authors included figures here like those in the MESMO 1 description that give a flow chart overview of the development process

- Pg. 3003, ln. 16: could the C-14 numbers here be converted to ventilation timescales using C-14 half-life?

- Pg. 3004, ln. 2: given the apparent flexibility of the physical model's ventilation, why did the authors not just choose values for Atlantic-to-Pacific FW flux that give MESMO 2 and MESMO 2E MOC strengths that were the same?; instead, the authors seem happy to settle on different base physical climates in the models which (again) makes it difficult to establish the source of model improvement

- Pg. 3004, ln. 2: the authors focus here on the Atlantic MOC, but it would be helpful if they said more about the wider circulation; for instance, the strength of the ACC or the Pacific overturning cell in the various model versions

- Pg. 3004, ln. 11: while I understand the reflex to avoid repetition in papers, this manuscript would be a lot more helpful if it added a paragraph to explain how, exactly, SP and LP are modelled; are they standing stocks of biomass, or do they simply represent fluxes of material?; the manuscript – which is ostensibly about model description – is pretty opaque on this point

- Pg. 3004, ln. 12: the authors tie primary production to the availability of CO2 rather than DIC – this is quite an unusual formulation, and is not justified here; most biogeochemical models instead view DIC as an effectively unlimited resource that phytoplankton can draw on; as well as justifying the choice here, it would be helpful to know how significant it is for model behaviour – i.e. are phytoplankton limited by CO2 anywhere?

- Pg. 3005, ln. 6: I may be misunderstanding the description here, but does MESMO 2 have a fixed C:Si ratio?; this seems very much at odds with many observations of diatom productivity; conventionally, the ratio varies with growth rate such that diatoms limited by nutrient availability (e.g. in HNLC regions) accumulate Si and shift their C:Si ratio downwards; this stems from a degree of decoupling of silicon uptake from the cell division cycle

- Pg. 3005, ln. 20: the text implies that opal is supersaturated in some locations in the modern ocean; I do not believe this to be the case, but can the authors clarify here?; it's certainly true to CaCO3, but I was not aware that opal was anything other than undersaturated everywhere (something which is not true at earlier points of Earth history)

- Pg. 3006, ln. 2: I would expect a model description paper to be far less circumspect in describing components than here; the authors refer to a version of the GENIE model at a particular time, but neither give a published source for this specific model nor attempt to formally describe the iron cycle equations used here

- Pg. 3006, ln. 21: the relationship between remineralisation and scavenging would be much clearer if the authors simply wrote out the equations

- Pg. 3007, ln. 9-11: I'm struggling to grasp what the authors mean here; perhaps explaining what used to happen – and the effective consequences of this – would make it clearer

- Pg. 3008, ln. 9: "are adjusted" – how was this done?; for instance, with three parameters it's not at all obvious how an optimal solution was reached (assuming that it was!); did the authors just engage in separate and sequential tuning by eye?; or was there more to the process?

- Pg. 3008, ln. 20: the authors jump immediately into discussing model performance at equilibrium in this section without making it in the least clear how simulations were performed; what was the initial physical state?; what was the initial biogeochemical state?; do these matter?; how long were the simulations?; is this long enough to ensure "equilibrium"?; how is this "equilibrium" defined?; the authors mention OCMIP, but

are they using the sort of equilibrium used there?; is the carbon cycle closed, or is a component of it (atmospheric pCO2) held constant while the rest of the cycle comes into balance?; there are a long list of considerations here, but the authors seem to think they don't matter; again – this is inappropriate for a model description paper

- Pg. 3009, ln. 8-10: "The stronger reanalysis winds ..." – this is stated rather baldly without any explanation; could the authors expand a little to be clear why this is the case

- Pg. 3009, ln. 12: we segue from equilibrium simulations to transient simulations with no commentary from the authors here; I presume that OCMIP protocols are being followed, but there's nothing in the manuscript to explain

- Pg. 3009, ln. 19: how does penetration depth of anthro CO2 and CFC-11 compare with observations?

- Pg. 3009, ln. 20: just an idle observation: would the average air-sea gradient in temperature be an instructive metric?

- Pg. 3009, ln. 27: how about sea-ice volume?; or seasonal extent?

- Pg. 3010, ln. 7: "values ... are tuned" – how?; is this done formally via a objective technique, or informally by hunch and by eye?

- Pg. 3011, ln. 25: the authors attribute high North Atlantic Si to the deposition of Fe that decreases Si demand; however, Si reaches its lowest high latitude concentrations in this basin in observations; furthermore, in part this is due to stripping out of Si as water moves northward in the Atlantic basin – what's happening here?

- Pg. 3013, ln. 27: the authors state that MESMO 2E is simulating the pre-industrial vegetation state but given that the simulations include anthro CO2, is this true?; or are the authors showing vegetation at the end of the equilibrium phase?

- Table 0: perhaps a table listing all of the changes to create the models would be good?

- Figure 1: why no colour?; these are ugly and difficult to interpret

- Figure 4: is this really observed sea-surface temperature, or is it observed near-surface temperature averaged vertically to a depth appropriate for comparison with the MESMOs?; the latter would be more appropriate (and appear to be done for "surface" nutrient)

- Figure 5: why is this plotted so differently from the other model fields? I can see no good reason not to stick to the same format – not least because it would allow presentation of sea-ice cover or height, and would do so far more clearly than this plot

- Figure 7: any chance of a corresponding plot (except MESMO 1, obviously) for Si?; also, why use Levitus (1993) when there have been a succession of World Ocean Atlas fields since then? (1994, 1998, 2001, 2005, 2009)

- Figure 8: why is P-limitation in white?; I can't see any cells that are P-limited; also, would a parallel pair of plots (for SP and LP) showing Fn (on Pg. 3004, ln. 18) be instructive?

- Figure 11: why no comparison to observations?; I've seen comparable fields plotted before

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