

Interactive comment on “Calibration of a simple and a complex model of global marine biogeochemistry” by Iris Kriest

Anonymous Referee #3

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=Summary=

The manuscript presents a parameter optimisation study involving a conventional marine biogeochemistry model, MOPS, and a low complexity model, RetroMOPS. Both models are optimised to full 3D, global fields of nutrients and oxygen, and both perform equally well after this process. The work finds that, as the processes that govern N and O₂ distributions are decoupled from one another, both tracers represent valuable optimisation targets. In the case of the simple model, the work finds a relationship between DOP and phosphate, and suggests that observations of the former may help provide a useful model constraint.

=Review=

Overall, I found the manuscript an interesting, and relatively straightforward investiga-

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tion into model calibration. Almost all of the issues that I have identified below with the manuscript are minor, mostly requests for additional clarification.

However, my major comment on the manuscript is that - considering its title - the manuscript is weak on the intercomparison of the simple and complex models. This is in contrast to a much stronger examination of the role of DOP in RetroMOPS.

Regarding the model intercomparison, in the first instance, this is hampered by limitations in the traceability between the models. For instance, both models have DOP (results of which are later shown to be interesting), but (a) MOPS remineralises this at a single rate while RetroMOPS has two rates for different depths, and (b) this rate is around double that in RetroMOPS compared to MOPS. Similarly, the models both differ in value for other shared parameters, and even whether parameters are optimised between the models. Given that there is a degree of overlap in the components of both models, I would expect, firstly, that these would be as aligned as much as possible, and, secondly, if unaligned, that it would be fully explained why not. A passing remark on parameters being “hand-tuned” is not enough. The result of all of this is that the intercomparison seems less systematic and more arbitrary than it otherwise could be. Additionally, the manuscript’s discussion does not contextualise itself in a way that would help readers understand how what has been learned from this work could be applied to other models (the complex model here is still relatively simple, for instance).

Since the comparison between the two models is a major focus of the paper, at a minimum, I would expect a revised manuscript to better explain the seeming discrepancies in the choices made here, as well as more effort put into the intercomparison. Ideally, additional optimisations might be done to narrow the gaps between the models and permit a more complete intercomparison that would be of greater value to the marine biogeochemistry community.

My recommendation is publication after revision.

=Minor comments=

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==Abstract==

Lines 6-7: The paper introduces two models of differing complexity, but beyond noting that the simpler model does almost as well as the complex one (which is not a given; e.g. Kwiatkowski et al., Biogeosciences, 2014), nothing else is said; more generally, this is more widely true of the manuscript – a better case / explanation should be made for presenting both the simple and complex models; especially as the two model structures are not traceably related to one another (though I'm sure that they share subroutines for specific processes)

==Introduction==

Lines 7-16: While I understand the need to keep manuscripts to a reasonable length, this paragraph is extremely dense and confusing; ideally, the concepts it describes should be clearly spelled out

Lines 7-10: In particular, this list of tools is mentioned in passing without any contextualising information; a sentence on each would be helpful to readers unfamiliar with them

Lines 17-18: This point about simulation time is slightly confusing here, since the preceding text is talking about accelerated models

Lines 27-31: As you latterly introduce oxidant dependent decay, it may be helpful to expand briefly on what parameters and processes Kriest et al. (2017) looked at

Line 31: This sentence could do with an example, for instance “(e.g. parameter X was found to play a role in vertical distributions of process Y)”

Pg. 3, line 2: “have been popular” – presumably this refers to use in CMIP, etc.?

==Methods==

Pg. 3, line 28: You say “A fraction” but don't give a value; other parameter values are listed; what's this one?

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Pg. 3, line 30: “Non-buried detritus is resuspended into the water column” – this sounds intriguing, care to expand? For instance, resuspension over how much of the water column? What about the benthic boundary layer?

Pg. 3-4: Simple flow schematics of the two models would probably be helpful

Pg. 4, lines 19-20: It is not immediately clear to me that the absence of the explicit plankton reservoirs in MOPS means that remineralisation would be too slow in Retro-MOPS; arguably, the storage of material within particulate reservoirs for a period of time might instead act to slow down remineralisation back to useable nutrient; in fact, is that not what Table 3 reports for its optimised value for this parameter?

Pg. 5, equation 4: Why not just expand on what $s_{O_2(j)}$ and $s_{DIN(j)}$ are here instead of sending the reader to another manuscript?

Pg. 5, equations 4-5: These seem to imply that you calculate potential remineralisation, then calculate the possible remineralisation given O₂ and DIN, then apply the latter at level k but apply the remainder at level k+1; how does this deal with the situation where level k+1 has insufficient oxidant?; that doesn't seem all that unlikely in OMZs (though with low vertical resolution as here, this may be less of a concern); more obviously, why mess around with where the remineralisation flux occurs and not just stop the sinking flux from remineralising?; for instance, couldn't the calculation of $D(j)$ not use O₂ and DIN to affect how the export flux is attenuated?; i.e. when there's no oxidant, remineralisation cannot occur vs. remineralisation occurs, where the required oxidant is taken from is dealt with afterwards

Pg. 6, line 2: In saying “nitrogen fixation balances the simulated loss”, this implies a direct connection which does not appear to be the case in equation 6; instead, the model losses and gains inevitably come into a balance, but they are not directly linked (some other models do make this connection)

Pg. 6, equations 7-10: With unwieldy equations like these, underbraces can be helpful

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in providing a quick reference for the reader as to the identity of the terms

Pg. 6, section 2.3: This overlooks any statement as to the performance of the physical model, even one that simply cites a source on this; given that the whole ocean misfit is used as an optimisation target, letting readers know that there's not a strong ventilation bias in the model ocean might be useful; this has a relationship with the next point . . .

Pg. 6, line 26: Is the Marshall et al. (1997) reference is the source of the circulation state used here?; or is it based on a more recent simulation?

Pg. 7, line 1: "After 3000 years most tracers have approached steady state" – This is an oddly loose definition of equilibrium; you could instead refer to the stabilisation of misfit J (e.g. that it fixes to N decimal places)

Pg. 7, lines 24-25: While the normalisation to global concentration should help with N and P (since they are related linearly through the ocean), does this overplay or underplay O₂?; this doesn't show the same sort of relationships (for obvious reasons); also, you don't mention AOU at all – would this be a good alternative misfit target?

Pg. 8, line 15: I might not have waited until the last sentence of this section to explain about the default parameter set

Pg. 8, section 2.7: Why is R_{-O₂}:P optimised in MOPS but not in RetroMOPS?; I can't immediately see why this isn't an option

Pg. 8, section 2.7: MOPS is optimised with a reduced data set but RetroMOPS is not; this seems like a strange omission considering the same underlying issue affects both models; it is again symptomatic of the disparities between the models being intercompared

Pg. 8, lines 26-28: Is the numerical diffusion referred to here what's described in Section 3.3?; might be worth teeing the reader up for this; also does horizontal diffusivity play a role at all?; the model is pretty coarse in the horizontal as well (though the gradients may be weaker)

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==Results and Discussion==

Pg. 9, line 10: Missing "of", i.e. "Because of optimisation, MOPS^{oD} results . . ."

Pg. 9, line 17: Typo on "threshold"

Pg. 9, line 20: The statement "impose a threshold" is unclear; do you mean that denitrification could not occur below this concentration?

Pg. 9, lines 25-34: What does this omitting of the Equatorial Pacific to the misfit in this region?; is it better or worse than when it's included in the global misfit function?

Pg. 9, lines 25-34: Also, what about the reverse situation where only the Equatorial Pacific (and / or OMZs more generally) is used for tuning?; if you tried that, perhaps a passing remark on it would be interesting

Pg. 10, lines 8-17: You note in the manuscript that "nitrogen fixation counteracts denitrification" but, as mentioned above, there's no direct connection in the model (e.g. unlike some models that represent the former implicitly as a function of the explicit latter); in the context of (dis-)equilibrium, I don't have a feel for the relative rates of the two processes in the work here; I guess I'm wondering if certain combinations of parameter values promote or diminish equilibration time; I suspect this is unlikely, but optimisation can take models to strange places

Pg. 10, lines 20-22: Per previous remarks on circulation, how good is the ocean's ventilation?; reporting CFC or (especially) C-14 performance earlier would help (even if this reiterates previous work)

Pg. 10, line 23: One of the lambdas in the bracketed comment should be the surface DOP remineralisation rate; also, only one of them is given units

Pg. 11, line 15: Does "direct evaluation of steady state" mean that they calculated the steady state solution analytically?

Pg. 11, lines 15-16: "... may still exhibit some drift . . ."?; it would probably be helpful

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to make this clearer, or possibly quantify it (e.g. in terms of misfit J fit; see my earlier remark); by all means use a fixed simulation duration, but knowing what this means for the misfit measure would be useful (e.g. its drift rate at this point)

Pg. 12, line 4: separate this last part of Section 3.3 into Section 3.4?; as it's on the comparison of MOPS and RetroMOPS, it would make a clear section; it might also afford an location to delve a little deeper into the complexity issue that's currently rather glossed over in the draft manuscript

Pg. 12, line 27: remove spare comma to get "... (Table 4), and indicates that these tracers ..."

==Conclusions==

Pg. 13, line 12: Regarding the use of observational DOP, can you clarify somewhere in the text how homogeneous DOP is?; i.e. is a single remineralisation timescale likely to be representative?

==Acknowledgements==

Pg. 13: Is this paper part of a special issue or wider celebration of the life of Ernst Maier-Reimer?; if so, an earlier note in the introduction would seem to be in order; if not, it may be worth making the rationale for this tribute a little clearer (e.g. note Maier-Reimer's recent passing)

==Figures and Tables==

Table 1: Why is the ostensibly fixed parameter DINmin very slightly different in the two RetroMOPS runs?

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