

The authors have without doubt clarified and improved the general focus of the paper, I welcome the omission of the somewhat pre-mature optimisation section and in particular the analysis involving a suite of biogeochemical models is a nice addition.

However, while I think the contents are generally adequate, the manuscript is still lacking significantly in terms of clarity and precision. I have the feeling that this is partly due to shortcomings in English language and grammar, which may be sorted by language editing support, but it is also due to a somewhat careless effort in elaborating and revisiting the text, which at this point of the submission process is a little concerning, so in its current form I cannot recommend the work for publication in Geoscientific Model Development. I can only re-iterate my final comments in this respect in the previous review step.

In the following I give some examples of my concerns (all line numbers refer to the manuscript version with track changes in the authors response):

I believe the title of the work is inadequate: even if optimisation is the ultimate goal, the work does not currently include it, so the title is misleading.

Throughout the main body of the text it appears that all states were treated equally in the analysis, while from some figures and the model descriptions in the appendix it appears that only or mainly inorganic phosphate was considered. This should be clarified.

Lines 29 following: State explicitly first that the tool has been tested with 6 biogeochemical models.

Line 87: I can see that the effort increases, but why would it get more complex?

Line 132-135: language

Line 144: versions of what?

Line 145: it's not clear to me what the latter refers to

Line 172: is->are

Section 2 is a brief mathematical description of the pdes of the coupled system, but not a description of marine ecosystem dynamics. Title of the section needs changing.

Line 265: While I accept that the overall application of the Neumann condition is good enough in the context of testing this software package, for a realistic implementation of a steady state solution of the annual cycle of marine biogeochemistry, I'm not sure how reasonable a general Neumann condition is. I would have thought that atmospheric deposition of nutrients and riverine discharges have a role here.

Line 274 Kappa is diffusivity, not diffusion, diffusion is the process described by the full term.

Lines 185 following: the 128 appears as a general rule here, while I'd expect it to depend on the number of grid points and the strategy of parallelization, which is restricted to horizontal domain decomposition. Also it's anticipating results and shouldn't be placed in the introduction.

Eq. 4: what is z?

Line 404: other

Line 406: "are equivalent with": I suspect what is meant is that all norms fulfill that condition? Equivalent is a different thing.

Line 520: which number?

Lines 555-556: Unclear what is meant by this sentence, I'd drop it.

Line 565: nx I suppose?

Line 744-756: I can't find any if the following represented in the figure it refers too up to Lin 754? E.g. what is the bottom layer, what is it's role within the software package?

Line 784 it's not true that it can't be split, but that would require message passing between processes.

Line 788-790 not clear what's meant by its mid in relation to the vector length and how that is used for balancing then?

Line 849 following: sounds like a lot of memory operations to reorganise the data structure in the memory space. Should be possible to avoid this using pointers.

Lines 877-879 I don't think there's much value in as adding the code fragment here, there no added information with respect to the equation.

Lines 890 following: The analogy to the treatment of interpolation remains unclear here as that section doesn't mention any of those routines.

Lines 919-921: This sounds more like the section would be a kind of step-through user guide, rather than a description of the software package as the rest of the text. In fact the rest of the section give a lot of details to enable reproduction of the results. This is great and very useful for interested readers, so I think it would be good to mention this in the introduction of the section rather than introducing it as a presentation of results. In fact there is no results in this section until pg 12. Might be worth splitting this into two sections to separate out the part with the actual results from the experiment description.

Line 925 "original implementation" is a bit misleading here as it may sound as it would be an original part of this work, while it was rather introduced in the paper cited shortly afterwards (Dutkiewicz 2005). I'd suggest to drop the "original"

Line 976 "filled in", does that mean it has been set to land?
If so it would be interesting to state the reasoning of this choice.

Lines 980-983: What is the relevance of these volumes?

Lines 1038-1040: Looking at the figure, I don't understand what the phrase "We observe that the solutions converge to the same difference in between consecutive iterations." means?

Table 16: What is the difference between the two columns, i.e. that does the V stand for?

Figures 110,111,... what happened to the figure numbering?

Figures 19 and similar: the states used in the formula of the norm are not normalised as far as I can see, so what are the states and units we are looking at in the norm? Is this just phosphate? Is it all

states? If it is all, shouldn't there be different weights between different states?

Line 790: Figures 117-115?

Lines 1132-1134: It is unclear to me how the Sievertsen work has impacted the profiling capacity in this work.

Lines 1143-1147: Does the TMM use the same boundary and initial conditions and time steps? I suppose so, but it might be worth mentioning it.

Lines 1165-1166: "Here, we use the given output, which is the timing for the whole run. Overall, for the calculation of the speed-up and efficiency results we use the minimum timings for a specific number of cores." Not clear to me.

Line 1185 How is the theoretical speed-up computed?

Lines 13002-1312 I don't understand the "On one hand ..., on the other hand..." here, isn't the point simply that the implementation of different biogeochemical models underlines the flexibility and generality of the interface?

Lines 1444-1447: meaning of "whose" is unclear.

Lines 1485: Not sure what is meant by the investment in the simulation itself.

A1.1 and A1.2: The formulation that phytoplankton is treated "implicitly" in these models is misleading, when it is actual a free model input parameter and should be treated as such (particularly with view on optimisation!).