

## ***Interactive comment on “MEDUSA: a new intermediate complexity plankton ecosystem model for the global domain” by A. Yool et al.***

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Referee 2

In the following, referee comments are *italicised*. Note: this response was not originally formatted using and some formatting errors occur – the reader is encouraged to consult the associated supplementary file which contains fewer such errors.

### *General comments*

*The model seems to have been thoughtfully constructed for the purpose of running multi-decadal global ocean biogeochemical simulations. I strongly agree that models with this level of complexity have a important role to play in earth system modelling (alongside more complex plankton functional type models). The parameterisations of*

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*growth and grazing are tried and tested rather than the most up-to-date ones – possibly that is a missed opportunity - but such concerns certainly do not invalidate the model, and its overall structure is suitable. The inclusion of the fast-sinking detritus and the related ballast model is a good development and allows for interesting feedbacks to be explored.*

*This is very much a paper that sets out to be the definitive presentation of MEDUSA, its equations and the results of a standard simulation; this seems to be a perfectly valid approach for a paper submitted to this journal, and both the equations and the results are presented clearly. The paper gives (nearly) all the details that you would need to implement the model from scratch. Most of the detailed comments below relate to places where I think needed details are missing, or where the equations have clearly been presented incorrectly (a number of errors slipped through the proof-reading net).*

*My biggest general criticism is that I would like to see more justification for why some of the modelling decisions have been made, and in the detailed comments below I ask for brief discussions at a number of points. I realise that in constructing a model such as this some decisions are made to avoid making the model as a whole over-complicated, or to avoid the model being ‘unbalanced’ because the level of complexity is very variable across the model; however it is good to consider the consequences of such choices, and acknowledge places where a possible slight reduction in the accuracy of the simulation has been judged a price worth paying for increased simplicity in the model structure. Some of the choices made look somewhat arbitrary, when I’m sure they were in fact well-reasoned!*

### *Detailed and technical comments*

*There are lots of them: if this paper is intended to be the definitive description of MEDUSA it needs to be accurate and free from confusion. Not everyone who wants to run it will do so in NEMO, and so they may have to code it up from scratch.*

We agree with the referee and are extremely grateful for making what was clearly a

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particularly painstaking effort to uncover errors and omissions in our model description. Regarding coding up the model, we hope that the model code that accompanies our manuscript will spare additional effort on this point.

*(a) On page 1944 the units of Iron nutrient  $F$  are given as  $\text{mmol Fe m}^{-3}$ . I would expect them to be  $\mu\text{mol Fe m}^{-3}$ , and the units for  $k_{\text{Fe,Pn}}$  (which adds to  $F$  in eqn 17) are given as  $\mu\text{mol Fe m}^{-3}$ , so is this an error?*

While this is not an error, it is liable to cause confusion. To this end we have amended description of all iron parameters to be in  $\text{mmol Fe m}^{-3}$ . This is particularly important since the model code itself runs in  $\text{mmol Fe m}^{-3}$  rather than  $\mu\text{mol m}^{-3}$  (though the code does still convert dissolved iron to  $\mu\text{mol m}^{-3}$  for the scavenging submodel).

*(b)  $\theta$  appears in eqns 3, 4, 12 and 19, and is described and given a value in Table 1. However it seems to me that its description and value are incorrect: it has nothing to do with chlorophyll, and in fact is the conversion from biomass ( $\text{g C}$ ) to nitrogen content ( $\text{mol N}$ ); and its value should surely be  $1/(6.625 * 12.011) = 0.01256$ . This would make eqn 12 (and eqn 19) compatible with the corresponding one in Taylor et al. (1997), and would make the units correct in those equations. Also, if  $\theta$  is the reciprocal of the product of the C:N ratio and the molecular weight of carbon then since you allow for there to be different diatom and non-diatom C:N ratios then  $\theta$  should also be allowed to be different; but maybe  $\theta$  needn't be used at all, just  $P_n$  (or  $P_d$ ) and  $M_{\text{org}}$ .*

We are grateful to the referee for spotting this, since it was a mistake in the original run rather than a manuscript error. The values of parameters  $\theta_{P_n}$  and  $\theta_{P_d}$  were also incorrectly specified. To resolve this problem, the parameter values were corrected and the entire simulation was repeated. This change required simple compensating changes to parameters  $\alpha_{P_n}$  and  $\alpha_{P_d}$ .

*(c) In the fourth term on the rhs of eqn 8, I presume that  $MZ_m$  should be  $M2Z_m$  ?*

Yes, this is correct. Manuscript amended.

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*(d) In eqn 10 the third and fifth terms on the rhs represent immediate dissolutions of biogenic diatom silicate due to non-linear diatom loss and to meso-zooplankton grazing on diatoms respectively. What is the rationale for using  $D1_{\text{frac}}$  and  $D2_{\text{frac}}$  (respectively) in these terms, since they are used elsewhere (eqn 8) in association with organic matter losses which will be caused by generally different processes than those which cause silicate dissolution? The use of  $D2_{\text{frac}}$  is particularly difficult to see the justification for as it is associated with meso-zoo losses rather than meso-zoo feeding (although since the value given to  $D2_{\text{frac}}$  in Table 6 is 1.00 this becomes somewhat academic). I would like to see a short justification given for the use of those parameters in the third and fifth terms of eqn 10.*

Yes, this is a good point, and we would not disagree with the referee's analysis. It would perhaps make sense to add a further parameter,  $D3_{\text{frac}}$ , which dealt specifically with this transfer of silicon. We have not implemented this at this time, but have drawn attention to the point made by the referee in an amendment to the text.

"Fast detrital silicon is similarly derived from loss processes though, since biogenic silica produced by diatoms is not utilised by zooplankton, one of the sources in MEDUSA is the egested remains of diatom cells rather than mesozooplankton mortality. Parameters  $D1_{\text{frac}}$  and  $D2_{\text{frac}}$  are again utilised to control the distribution of losses to fast detritus. However, since the fate of ingested diatom silicon is more associated with zooplankton egestion in faecal pellets than zooplankton losses such as mortality, the arrangement could arguably be altered such that biogenic silicon is directly channelled to fast detritus without reference to  $D2_{\text{frac}}$  (perhaps via  $D3_{\text{frac}}$  instead). However, since  $D2_{\text{frac}}$  already has value 1.00, for simplicity this embellishment has not currently been adopted. Note that, since there is no silicon component to small detritus, the small fraction of diatom losses that is not channelled to fast detritus is returned directed to dissolved silicic acid."

*(e) On the subject of  $D2_{\text{frac}}$ , I presume the second appearance of  $D1_{\text{frac}}$  in Table 6 should refer to  $D2_{\text{frac}}$  ?*

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Yes, this is correct. Manuscript amended.

*(f) On page 1948, it would be helpful (as a check) to confirm in the text the units of  $\_Chl Pn$  ( $(g\ Chl)(g\ C)^{-1}$  ?; line 1),  $E\_Pn$  ( $(Wm^{-2})^{-1}d^{-1}$  ?; line 1) and  $I$  ( $Wm^{-2}$  ?; line 10).*

We believe that these are correct. We have amended the manuscript to note the units of  $I$ . The other variables are assigned units in Table 1.

*(g) Eqn 15 is dimensionally incorrect; the  $E\_$  and  $I$  in the numerator should not be squared; ditto in Eqn 22.*

Yes, this is a mistake. Manuscript amended.

*(h) Eqn 18 shows, and the two following lines report, that the light and nutrient limitation factors determining  $PPPn$  are multiplied together. Not all models combine the limitations in this simple way, and (since iron is needed for the photosynthetic apparatus) it could certainly be argued that the light and iron limitations should be linked differently. I suspect that this choice was made to avoid over-complicating the model, which is fine if there are no major consequences. A brief discussion of this choice (a few sentences max) would be good. Also, there is no corresponding equation for  $PPPd$ : should there be one?*

This is a fair point. The decision to use a multiplicative form in the model was largely based on historical conventions. To examine its significance, the manuscript appendix now includes a sensitivity analysis in which the multiplicative relationship is replaced by a Liebig most-limiting relationship. This leads to only small changes in model behaviour.

*(i) In eqns 34 and 35 the  $\_Chl\ max$  term should be  $\_Chl\ max, Pn$  and  $\_Chl\ max, Pd$  respectively.*

Yes, this is correct. Manuscript amended.

C920

*(j) In eqn 36 the use of  $p_{\mu}X$  in the numerator is fine, but the use of  $p_{\mu}1$  and  $p_{\mu}2$  in the denominator is confusing when the properly-defined  $p_{\mu}Pn$  and  $p_{\mu}D$  could be correctly used instead. Also, eqn 37 doesn't appear to exist!*

Yes, this is correct. Manuscript amended (similar changes have been affected for the corresponding portion of the mesozooplankton equations).

*(k) On page 1952 (and subsequent pages) a number of parameters (e.g.  $\_$ ,  $\_N$ ,  $\_C$ ,  $kC$ ) are used without being defined in the text. While they are defined in one or other of the tables it can make the passage easier to follow if they are also briefly defined in the text.*

This is a fair criticism. The manuscript has been amended by adding the following sections of text:

“Here, the separate quantities of nitrogen,  $INZ_{\mu}$ , and carbon,  $ICZ_{\mu}$ , ingested by microzooplankton are summed. Parameter  $\varphi$  relates to grazing inefficiency, so-called “messy feeding”, that returns a fraction of the grazed material back to dissolved nutrient. For the material actually ingested, the resulting C:N ratio,  $\theta F_{\mu}$ , can be calculated.”

“This makes use of the assimilation efficiencies of nitrogen,  $\beta N$ , and carbon,  $\beta C$ , as well as the carbon growth efficiency,  $kC$ , of microzooplankton.”

*(l) In eqn 49, similar comments to those made about eqn 36 apply to use of  $pm1$ , etc.; using  $pmPn$ , etc., would be less confusing.*

Yes, this is correct. Manuscript amended.

*(m) In Section 2.3.6 it would be good to have more explanation of which processes the density-independent losses cover. I would also like to see a brief justification of why the hyperbolic function of concentration was chosen for the density-dependent losses; while this has a density-dependent form at low concentrations it becomes density-independent at higher concentrations but it's not clear that the mentioned processes (e.g. viral infection) do. If the choice was influenced by model stability considerations*

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*that would be an acceptable reason, but I'd like to see the reasoning (briefly) discussed, or at least reported.*

These are fair points. We have expanded the text to be clearer about the processes and rationale for these terms.

Regarding our choice of density-dependent loss term, we chose a hyperbolic relationship for our default simulation on the basis of model stability and historical convention. However, as the referee notes, other forms could reasonably have been chosen. To address this, we have performed a series of sensitivity experiments in which the form of the closure term has been altered (including, for completeness, a linear term). These experiments are described and reported in the manuscript appendix.

Generally, the form of closure term does significantly alter model behaviour and performance (with the notable exception of linear closure). This points to model results being insensitive to the choice, i.e. other aspects of the model are more important.

*(n) In Table 4, parameter  $k_{FeL}$  should have units of  $(\mu\text{mol Fe m}^{-3})^{-1}$*

Yes, this is correct. Manuscript amended.

*(o) Section 2.3.9 is called 'Fast detritus remineralisation terms' but actually only gives equations for the production of fast detritus; and those equations are given again (differently!) in Section 3.2.1 The description of the remineralisation processes is actually given in Section 3.2.2, so this section needs to be cleaned up.*

Yes, this is rather confusing. We have changed the title of this section to simply "Fast detritus" and have amended the text to briefly explain what the terms are, and to direct the reader to a later section where they are more completely described.

"Differential equations 9 and 10 (and, implicitly, 11) include terms for the remineralisation of fast-sinking detrital material,  $LDN(k)$  and  $LDSi(k)$ . These, together with the corresponding terms for the production of fast-sinking detritus (nitrogen, silicon, iron, organic carbon and calcium carbonate) are fully described in Section 3.2."

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*(p) Section 3.1: I like the science described, with a temperature-dependent remineralisation rate for slow-sinking detritus  $D$ , but I don't see it represented in the equations: all there is is the term  $MD$  which earlier eqn 72 relates to parameter  $\mu D$ , but the value of that parameter is given in Table 3 as a constant, with no temperature variation. The description of the science and the equations must be made consistent with each other, and with what was happening in the model simulation you describe. Also, eqn 82 is just a repeat of the relevant parts of eqn 8, and would be better labelled as such.*

Yes, this is correct. The following text has been added to the manuscript:

"Remineralisation of slow-sinking detrital particles to dissolved inorganic nitrogen occurs at a rate dependent on ambient temperature.  $MD = \mu D \exp(1.066T)$ "

Later mentions of slow detritus remineralisation now explicitly reference this equation by number.

*(q) I think the decision to model implicitly the sinking of the fast detritus is a sensible one. However the way it is implemented here, with the whole water-column integral of fast-detritus production being used to drive remineralisation at every depth, isn't the only way to do it, and this way runs the risk of having some material remineralised above the depth at which it is created (this would most likely happen where there was a pronounced deep chlorophyll – and production – maximum). I'm not asking you to change the scheme – I realise the amount of effort it would take to re-run the simulation! – but I would like to see a brief discussion of the possible flaws with this sub-model and the possible significance, if any (DCMs tend to occur where surface nutrients are low, so spurious remineralisation in the surface might have a proportionally large effect there; but then your model tends to over-emphasise the oligotrophic gyres so maybe this partly offsets that – not that correcting one error with another is necessarily good).*

The referee is correct to raise this concern. The original manuscript over-simplified the description of the fast detritus submodel and misleadingly implied that fast detritus was first vertically integrated and then remineralised. In fact, fast detritus production and

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rem mineralisation takes place level by level such that it cannot be aphysically translocated upwards in the manner identified by the referee. This is now clarified by the corrected equations in section 3.2.2, and in a new section 3.2.3 that describes the computation of ballast fluxes.

*(r) Section 3.2.1 presents, in eqns 83 and 84, the creation processes for fast sinking nitrogen and silicate detritus respectively, and I'm taking these as definitive rather than eqns 80 and 81 in Section 2.3.9. However eqn 83 should surely have an extra term '+ (D2frac . M2Zm)' representing meso-zooplankton losses which would make this equation compatible with eqns 8 and 85 and also with the text. Since the total will be calculated as a sum over layers in the code, rather than as an integral, it would be better to express it that way in the paper (remembering to weight by layer thickness); this would also make the significance of 'k=1' and 'k=64' clearer (not everyone knows NEMO usually has 64 layers). Ditto with eqn 85 (which has the missing term).*

Yes, these are mistakes in the original manuscript description – the model code was correct. Equations 80 and 81 have now been removed and section 3.2 on large detritus has been amended to correctly address the issue of mesozooplankton losses identified by the referee. The relationships between fast detritus production, rem mineralisation and model layer structure are also more accurately and clearly described now.

*(s) In lines 20 to 23 of page 1960 you mention the function  $f_c(\text{latitude})$ , and its general form, but do not present it explicitly: please do so, including whether the Atlantic/Pacific difference that you mention is seen in observations is included in your model formulation. Also, in eqn 86 you link calcium carbonate formation to total primary production, i.e. the sum of non-diatom and diatom production: but diatoms don't produce  $\text{CaCO}_3$ ; and Dunne et al. (2007) compare, in their Table 2,  $\text{CaCO}_3$  export to organic carbon export (not production) when coming up with the figures you give (and I accept that the organic export in Dunne et al. will include diatoms). So I have reservations about your formulation here, because it seems clear that a much higher proportion of the  $\text{CaCO}_3$  formed will be exported than of the organic carbon produced (since the latter is actively*

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*respired and recycled), and therefore it's not correct to use the figures calculated from Dunne et al. for the production ratio. Of course, the exact form of  $f_c(\text{latitude})$  is not given, but it is implied that it uses directly the mentioned figures. I realise that, from a model mechanics point of view, it is much easier to link  $\text{CaCO}_3$  formation to primary production than to export, but I'd like to see a discussion of the issues raised by that choice (as well as the explicit form of  $f_c(\text{latitude})$  asked for previously). Finally, it would be good to confirm around eqn 86 the units of  $\text{TCaCO}_3$  (which are mol  $\text{CaCO}_3 \text{ d}^{-1}$  ?).*

The manuscript has been amended with a more complete description of the production of calcium carbonate in MEDUSA. An equation for  $f_c(\text{lat})$  has been added to make clear the basis of the geographical distribution of calcium carbonate production. Additionally, a figure has been added to show the seasonal production of calcium carbonate alongside the corresponding figure for biogenic opal.

Regarding the relationship between calcium carbonate and organic material, this was misstated in the original manuscript as being based upon primary production (as it had been in an early version of MEDUSA; as per Yool et al., 2010). In fact, calcium carbonate production is related to the production of organic fast detritus, and so closer to more conventional "rain ratio" descriptions than previously suggested. This formulation is more in keeping with that of the Dunne et al. (2007) model used as the basis for this submodel of MEDUSA.

*(t) In Section 3.2.2 eqn 88 presents the depth-variation of the flux of sinking  $\text{CaCO}_3$ . The equation does not suggest that it only applies below the lysocline, but line 13 of page 1963 says it does and that agrees with the right panel of Figure 3, and also with the formulation of Dunne et al. (2007). But I'd like to see that made explicit in the equation. I'd also like to see, in the page 1963 text, a brief discussion of why the model takes no account of the reported dissolution of calcium carbonate above the lysocline. In fact, respiration of organic carbon in a detrital particle will release  $\text{CO}_2$  which could make an acidic micro-environment where dissolution occurs despite the*

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*surrounding waters being over-saturated: though I accept such detailed considerations are beyond the scope of a modelling study such as this. But since the paper makes a lot of its representation of the export flux – and it is good – a bit more discussion of the processes left out or ignored is in order (even if you are following someone else's implementation, you have chosen that rather than another).*

This is a fair point. Though the text goes on to note that calcium carbonate is only attenuated below the lysocline, neither the text nor the equations here make this clear. To this end the equations have been adapted to explicitly make note of the local lysocline depth, and the text has been amended to note this as follows.

“By contrast, the sinking fluxes of both biogenic silica,  $TSi(k)$ , and calcium carbonate,  $TCaCO_3(k)$ , attenuate with depth independently of organic carbon. In the case of biogenic silica, since silicic acid occurs at undersaturating concentrations throughout the World Ocean, this attenuation occurs globally at all depths. Calcium carbonate, by contrast, is not generally soluble in surface waters because of the supersaturating concentrations of the carbonate ion. However, at depth, specifically below the lysocline, concentrations become undersaturating and dissolution can occur. In MEDUSA, the depth of the lysocline,  $lysocline(lat,lon)$ , is precalculated and used to regulate the spatial distribution of dissolution.”

Regarding the dissolution of calcium carbonate above the lysocline, it is our understanding that this is still a topic of ongoing research. While we have not modified MEDUSA to consider this aspect of calcium carbonate dynamics, we have added some text to introduce the concept as follows.

“Based on a survey of vertical fluxes of calcium carbonate, it has been suggested that biological activity may be able to cause the dissolution of calcium carbonate above the calcite saturation horizon (Milliman et al., 1999). Proposed mechanisms include dissolution in the acidic guts of zooplankton (Harris, 1994; Pond et al., 1995), and respiration-driven low pH conditions within sinking particles (Milliman et al., 1999).

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Such activity would effectively shoal the horizon shown in Figure 6 by more rapidly attenuating the “protection” offered by biomineral ballasting, with the result that the remineralisation of organic material would occur at shallower depths. However, subsequent work has instead suggested that such additional mechanisms may be unnecessary, and that conventional dissolution kinetics together with consideration of sinking velocity may explain the fluxes observed (Greenwood, 2009). Consequently, MEDUSA does not consider dissolution above the calculated saturation horizon at this stage.”

*(u) I like the discussion of alternative models in Section 3.2.3*

We appreciate the referee's comment. Our intention was simply to make clear to readers that there is still considerable uncertainty in our understanding of how organic material is remineralised down the water column, and that there are a number of distinct rival approaches to that used here. We would certainly welcome the use of alternative schemes by researchers who adopt MEDUSA.

*(v) In lines 1-6 of page 1970 several figures are (usefully) given regarding primary production and its limitation, and Figures 19 and 21 show the geographical patterns. I realise that insufficient data are available to validate these figures, but other models (e.g. NCAR's) have reported the geographical distributions of limitation by various nutrients, and it would be good to discuss briefly whether your model's figures/distributions are reasonable globally and in any particular area where data are available.*

This is a fair point. We have addressed it by adding new geographical figures that show both nutrient limitation of growth by all factors, and which of the nutrients considered is most responsible for limitation. The latter panels are comparable to those shown for the NCAR model. To this end we have also added text to the manuscript to discuss the congruence between the models.

“Figure 30 shows the overall scale of summertime nutrient limitation on both phytoplankton groups, together with maps that indicate the geographical distribution of “most limiting nutrient”. Although less factors are considered in this analysis, the factors most

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limiting phytoplankton correspond fairly well to those identified by the BEC model of Moore et al. (2004). In the case of non-diatom phytoplankton, both models identify nitrogen as the most limiting nutrient in oligotrophic gyre regions, and iron in the equatorial Pacific and high latitude regions. In the case of diatom phytoplankton, the picture is somewhat different. BEC's diatoms are stressed in a similar pattern to its non-diatoms, with additional silicon stress around the region of iron stress in the equatorial Pacific and in the high North Atlantic. While MEDUSA shows a similar pattern, its diatoms are also silicon-stressed in broad fringes around the oligotrophic gyre regions. Although these regions are not especially important for primary production, the extreme silicon stress in MEDUSA, at least relative to BEC, may point to excessively efficient export of silicon relative to nitrogen."

*(w) I like that the time-series are shown in Figure 27, and discussed in the last paragraph of Section 4. Getting the nutrients into a realistic steady-state is difficult task in a model that is expensive to run for decades/centuries, and your results look to be acceptable. The last sentence suggests 'that MEDUSA has a systematic deficiency that permits the accumulation of nutrients in the surface waters of (largely) the Southern Ocean'; what (if any) are the consequences for MEDUSA's intended use in climate change simulations, which are often run for several centuries? A brief discussion of this, probably in Section 5 where the deficiency is mentioned again, would be good.*

This is an important consideration. We have added the following text in the location indicated by the referee which we hope makes the restrictions imposed by spin-up clearer.

"This approach, or similar, will be of particular relevance in the case of investigations at centennial-scale duration (e.g. climate change forecast simulations) where drifts in biogeochemical properties may be comparable to, or even obscure, actual forced trends. In such cases either a parallel (and expensive) control, or a more completely spun-up model is essential."

C928

*(x) Finally, line 17 of page 1974 reports that 'the seasonal progression of plankton blooms and nutrient depletion are reproduced', which is good; but I was disappointed that there were no Hov-Muller plots showing this.*

We agree with the referee and have added Hovmoller diagrams to accompany each of the observation-model intercomparison figures.

Please also note the supplement to this comment:

<http://www.geosci-model-dev-discuss.net/3/C916/2011/gmdd-3-C916-2011-supplement.pdf>

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Interactive comment on Geosci. Model Dev. Discuss., 3, 1939, 2010.

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