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

Anonymous Referee #2

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General description

This paper describes MEDUSA, a simple marine ecosystem model designed to be embedded and run in a global scale model of the physical ocean (in particular NEMO, though it should be straightforward to adapt it for other physical models). MEDUSA uses nitrogen as the model currency and features three nutrients (dissolved inorganic nitrogen, silicic acid and dissolved iron), two types of phytoplankton (diatoms and non-diatoms), two phytoplankton chlorophyll variables (one for each phytoplankton), diatom silicate, two types of zooplankton (micro- and meso-zooplankton) and slow-sinking detritus. However much of the particulate export to the deep ocean is via the fast-sinking detritus, which is not modelled as a tracer, but which instead is modelled

implicitly using a version of the ballast models of Armstrong et al., Klaas et al. and Dunne et al.

The model equations are presented in full and discussed, with special emphasis placed on the model for fast-sinking detritus. Simple process parameterisations have mainly been used for nutrient and light limitations of production, for chlorophyll:carbon ratios and for feeding by zooplankton, but the model has been intelligently constructed. A multi-decadal hindcast simulation (the forty years up to 2005) is described, and results from the last ten years of that simulation are presented. Where the data are available, the model results are compared.

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 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.

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

(b) ξ 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 (g C) to nitrogen content (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 ξ 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 ξ should also be allowed to be different; but maybe ξ needn't be used at all, just θ_{Pn} (or θ_{Pd}) and M$_{org}$.

(c) In the fourth term on the rhs of eqn 8, I presume that M$_{Zm}$ should be M2$_{Zm}$?

(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$_{frac}$ and D2$_{frac}$ (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$_{frac}$ 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$_{frac}$ 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.

(e) On the subject of D2$_{frac}$, I presume the second appearance of D1$_{frac}$ in Table 6 should refer to D2$_{frac}$?

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

(g) Eqn 15 is dimensionally incorrect; the $\hat{\alpha}$ and I in the numerator should not be squared; ditto in Eqn 22.

(h) Eqn 18 shows, and the two following lines report, that the light and nutrient limitation factors determining PP_{Pn} 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 PP_{Pd}: should there be one?

(i) In eqns 34 and 35 the θ^{Chl}_{max} term should be $\theta^{Chl}_{max,Pn}$ and $\theta^{Chl}_{max,Pd}$ respectively.

(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!

(k) On page 1952 (and subsequent pages) a number of parameters (e.g. ϕ, β_N, β_C, k_C) 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.

(l) In eqn 49, similar comments to those made about eqn 36 apply to use of p_{m1}, etc.; using p_{mPn}, etc., would be less confusing.

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

(n) In Table 4, parameter k_{FeL} should have units of (μmol Fe m^{-3})$^{-1}$

(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.

(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 M_D which earlier eqn 72 relates to parameter μ_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.

(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 remineralise

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).

(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 '+ ($D2_{frac}$. $M2_{Zm}$)' 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).

(s) In lines 20 to 23 of page 1960 you mention the function fc(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 $CaCO_3$; and Dunne et al. (2007) compare, in their Table 2, $CaCO_3$ 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 $CaCO_3$ formed will be exported than of the organic carbon produced (since the latter is actively 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 fc(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 $CaCO_3$ 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 fc(latitude) asked for previously). Finally, it would be good to confirm around eqn 86 the units of T_{CaCO_3} (which are mol $CaCO_3$ d^{-1} ?).

(t) In Section 3.2.2 eqn 88 presents the depth-variation of the flux of sinking $CaCO_3$. 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 CO_2 which could make an acidic micro-environment where dissolution occurs despite the 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).

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

(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 pat-

terns. 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.

(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.

(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.

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