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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 1

In the following, referee comments are *italicised*.

Comments The manuscript describes clearly a well thought out model of this particular class. In my view there are no significant new advances in terms of the fundamental model structure or the parameterizations employed. However, collectively, the particular configuration presented is up to date, carefully considered and appropriate for many biogeochemical/climate modeling applications.

As a detailed technical description of this set of model algorithms, the manuscript is generally clear and allows the reader to find the sort of detailed information which

often goes unpublished. However, without any specific application, it is on the dry side and certainly for afficionados only.

We thank the referee for their careful review of our manuscript, and are pleased that its form and level of detail is clear and useful. Even if, as is noted by the referee, the manuscript does largely cater to a biogeochemical modelling audience.

Some specific points and questions: 1. Given that this is a technical description of the basic model algorithms I feel that some graphical depiction of the parameterizations used would be nice and helpful. For example, the relationship between growth and Si:N for diatoms would, I think, be very simple to absorb if also shown graphically but is rather stodgy in algorithmic form (though useful reference).

Our original manuscript did attempt this for several components of the model, including the status of dissolved iron and fast detritus ballasting. However, we omitted other aspects that could probably have benefitted from explanatory diagrams. To this end we have added two further diagrams to illustrate (1) the relationship of diatom growth to biomass Si:N ratio (as suggested by the referee), and (2) the relationship between prey C:N ratio and zooplankton growth. If there are other aspects of the model that the referee would like see framed with an explanatory diagram, we would be happy to add these.

2. Some of the key choices come into the values of the parameters and could be discussed in more detail. For example, why are Vpn and Vpd (maximum growth rate for non-diatoms and diatoms) 0.53 and 0.50 d-1 respectively? Presumably this is some balancing of the apparent taxonomic adaptation for fast growth rates (for given size class) by diatoms, traded off against their larger average size? How significant is the difference? What if it were reversed? Given the lower maximum growth rate and Si demand, why do diatoms persist in the solutions? (Presumably lower grazing pressure.). These somewhat subtle choices are very important but are not discussed.

Yes, the referee is correct. Parameter values such as maximum growth rate and nutrient half-saturation constants were chosen to divide the phytoplankton community into small and large classes. In general, maximum growth rates decrease and half-saturations increase with increasing cell size. Since the small class is assumed to largely represent prokaryotic picophytoplankton, the eukaryotic, silicon-requiring diatoms have been assigned to the large class. Obviously, there are many large phytoplankton that do not require silicon as well as some very small diatoms that could reasonably be assigned to the small class, and it could be argued that the model should represent these. However, one of the central aims of MEDUSA was to tractably increase complexity, hence this somewhat restricted framing.

Nonetheless, the criticism is fair and we have addressed this by adding text to expand on the points above. Additionally, our manuscript's appendix now includes results from sensitivity experiments in which growth rates were (1) equalised, and (2) reversed. While these are arguably at odds with the small/large distinction, they find limited changes in MEDUSA's output.

3. The decision to relax DIN and silicic acid concentrations towards WOA values in the global simulations seems rather curious and I wonder if it has any significant impact on model solutions at this resolution? If so, what?

As noted in the manuscript, the decision to restore macronutrient concentrations within 100 km of the coastline (approximately 1 grid cell's width) was taken to represent the influx of nutrients from riverine sources. These are of particular significance for the Arctic Ocean, one of the regions of most interest in climate change research (cf. Popova et al., 2010).

Since the relaxation of nutrients in simulations of MEDUSA only takes place within 100 km of the coastline (approximately 1 grid cell's width) it occurs largely within shallow water. While this doubtless improves model agreement with observational fields locally (cf. criticism 5 below), the model's weaker performance in the Southern Ocean (where nutrients are significantly elevated above initial fields; as noted in the manuscript) illus-

trates that relaxation does not strongly affect open ocean concentrations.

However, to address this concern more quantitatively, our manuscript's appendix now includes results from a sensitivity experiment in which nutrient relaxation was discontinued. This does lead to some localised changes in nutrient availability, but the model's global performance is broadly unaffected.

4. A measure of "skill" is presented in terms of the Taylor diagrams, but such evaluations are somewhat in a vacuum in the absence of a specific application: we dont have that context here. However, they might be useful for a future reference.

In this case, Taylor diagrams were used to simply present the quantitative performance of MEDUSA for a range of significant fields for which global-scale observational fields are available, primarily to illustrate which parts of the model are most successful (nutrients and primary production), and which are less successful and need further work (chlorophyll).

5. The relatively good correspondence of nutrient fields with WOA is rather unsurprising: the model was initialized with WOA fields, integrated for only 40 years, and relaxed towards WOA fields at the ocean margins.

Please see our response to criticism 3.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/3/C910/2011/gmdd-3-C910-2011-supplement.pdf

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