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Interactive comment on "Tuning and assessment of the HYCOM-NORWECOM V2.1 modeling system" *by* A. Samuelsen et al.

Anonymous Referee #1

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This paper presents a sensitivity study of the coupled model HYCOM-NORWECOM v2.1 applied to the Arctic system and as a consequence a new parameterisation has been derived and applied in two model implementations with increasing spatial resolution. The work surely fits into GMD scope and it sufficiently contributes to the biogeochemical modelling science, particularly in a challenging environment like the Arctic. Methods are all well-known and established, although I appreciate not always (and enough) applied. Authors clearly point to a public svn repository to download the code, and clearly separate their contribution from previous work.

Some more details regarding the model set-up would be needed, in order to better understand the work presented and to allow reproducibility (even though being a complex 3D model the chance for this is very limited, but this is not authors' fault). In particular:

- What are the sources for the forcings, the boundary and the initial conditions (both physics and biogeochemistry? (page 8404)

- Model has been spun up for three years. Is there any reference to show that this time is enough, particularly for the deeper part of the domain? Or did the authors make some test to check this? (page 8405)

- After equation 3 (page 8404) authors write the value for "g" for Meso-zooplankton. Although the equivalent reference value for microzooplankton is reported in table 2, I would recommend the authors to report the standard value here as well for clarity

- Authors did not explain the meaning of "mu_z" in equation 5. I assume this is the maximum mortality rate. If this is the case, I suggest authors to use the latin letter "m" instead of the greek letter "mu" because authors (in agreement with most literature) already use the greek letter "mu" for growth of the phytoplankton (equation 1 and 2) therefore it is misleading having the same symbol representing growth in one equation and mortality in another one.

It is unfortunate that authors do not take full advantage of the spatial resolution of the model and the data and presents results lumped for the whole domain or at most for just 2 areas. I acknowledge that spatial coverage of the data can be limited particularly in winter, but figure 3 highlights that spatial pattern of uncertainty could be investigated in a more detailed way than the one authors already discussed contrasting two subdomains (NWS and BAS). Furthermore authors describe different performance of the model in the deeper domain (below 500m) compared to the upper domain (page 8407): nevertheless figures 4 and 5 show only the synthesis of the model-data comparison across the whole domain. I suggest authors to show also the outputs from upper and bottom domain separately, particularly because authors state "[Upper] Silicate has no skill in the years 1999 and 2000." while figure 4 shows good to very good skill for silicate in those years. This leads to think that the good results for nutrient simulation highlighted in figures 4 and 5 could be biased by good initialisation of the model in the deep basins where the dynamics are limited in a 10 years period. I would also suggest authors to mention the residence time of the basin, in order to give the opportunity to readers that are not expert of the area (like myself) an idea of the relative importance of endogenous dynamics versus boundary forcings. Similarly, authors show vertical profiles for Chl and nitrate, but they discuss also phosphate and silicate. I suggest authors to add similar plot to figures 9 and 10 for P and Si (perhaps as supplementary information or removing the June panel from figure 9 and 10 to limit the number of figures).

In the discussion section, the main items are discussed, but I would suggest authors some changes in order to make the section more clear:

- The introductory paragraph is redundant, as it simply summarizes the entire workflow and this is already well clear from the previous sections. This section looks more like a conclusion than an opening of discussion.

- The title of section 4.1 is misleading. In the paper the model is not validated, as this would require to compare the model output with a completely independent dataset from the one used for calibration/tuning. From my understanding of section 2.3 all data have been used for tuning therefore the model has not been validated. I would suggest to title this section "uncertainty connected to observation" that is an appropriate title for the discussion in this section. I would also suggest authors to refer to Stow et al., 2009 (http://dx.doi.org/10.1016/j.jmarsys.2008.03.011) for comprehensive analysis of this topic.

- The first sentence in section 4.1 is arguable. The quality of measures does not depend (only) on their abundance: a broken thermometer will always give the wrong temperature. I suggest authors to reformulate this sentence and in particular to be more clear with the meaning of "quality of measure" for them.

- Authors state (page 8412) that fluorometer Chl-a may vary with a factor of 3-4 compared to HPLC Chl-a. I may agree with this, but I recommend authors to clarify if they

refer to in situ fluorometer Chl-a or Chl-a measured with a fluorometer in the lab from extracted pigments. In the latter case the error is expected to be much lower than the one suggested by author. A reference to back up their estimate would be needed as well.

- In section 4.2 authors state that changes in mortality of zooplankton produced little effect, contrarily to the expectation but they did not provide any potential reason for this

- In the same section, authors rightly interpret the lack of effect of the change in N:Chl ratio on the model performance on simulating Chl with a compensatory mechanism. This mechanism should lead to a different distribution of phytoplankton along the water column. I suggest authors to bring this evidence to corroborate their hypotheses and to discuss the potential consequences

- I totally agree on the limitation due to computational constraint highlighted in the last paragraph of section 4.2. Authors could state while they chose to run the sensitivity test on using the entire 3D model instead of running those in faster 1D set-up (maybe in contrasting environment in the domain), particularly since authors do not show spatial pattern of sensitivity (by the way, this information could be really informative and would increase the impact of the paper)

- In few occasions (e.g. beginning of page 8415), errors in the simulation of physics have been used to explain errors in the biogeochemistry. The explanation given are perfectly reasonable, but the general performance of the physical model has not been shown, nor adequate reference has been given in support of authors' hypotheses.

- Similarly, bad simulation of bloom initiation has been suggested as potential error in Chl-a simulation, however comparisons between bloom initiation timing (model vs. data or model vs. model during the sensitivity test) have not been provided. Such a way, these statements remains quite speculative.

- From line 15 of page 8415 authors do not discuss regional differences in performance but they discuss the general performance of the model, therefore this part should go under a different header (either a 4.4 header or a generic Conclusion)

- Finally, but I appreciate that this is a personal opinion, I would remove any dubitative form when authors states that model could be improved in closer collaboration with empiricists. I believe that this is the way forward without any doubt if modellers want to build reliable model that describe the main ecological principle and pathway and are up-to-date to the more recent understanding of marine ecosystems.

Further minor corrections suggested:

- Page 8401, l 24-26: For clarity I suggest to write: "The HYCOM-NORWECOM model was tested against local in-situ data and derived gridded climatology of nutrients, as well as satellite data, however. . ."

- Page 8402, l10: add a comma between salinity and temperature

- Page 8402 l24: I'm not native English, however I believe that "provide" is a better word than "proved" in this context

- Page 8406, L20: the standard Taylor diagram show standard deviation, correlation coefficient and centered RMS not variance (see Taylor, 2001 figure 2)

- Page 8410, l21: "profiles in the upper 1000m of the water column IN THE NORWEGIAN BOX. . ."

- Page 8411, l22: I would rewrite the sentence starting with "It is howewver.." like this: "different requirements for geographical coverage, number of stations and frequency are needed depending on the diffrent issues addressed, parameters measured and the area complexity (e.g. Ottersen et al., 1998)"

- Table 2: I believe that there is a typo for case N12: it should be maximum microzooplankton grazing rate, and not grazing preferences for microzooplankton

- Table 3: this could be moved in the supplementary information

- Figures 4,5,6,7: I really like the colour coded system, as it is really communicative and easy to understand. However, at the same time it can be also misleading: e.g., in figure 4 a big improvement from 0.2 to 0.49 will not be highlighted at all, whilst a small improvement from 0.49 to 0.51 will stand out. I would suggest authors to write also the value of the different metrics inside the coloured box

- Figure 8: is the Taylor diagram showing relative SD (i.e. SDmodel/SDdata) or the absolute value? In any case I would plot the dotted circumference passing through the DATA point, to highlight when model and data have the same standard deviation

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