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Interactive comment on "Observing and modelling phytoplankton community structure in the North Sea: can ERSEM-type models simulate biodiversity?" *by* David A. Ford et al.

Anonymous Referee #3

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1 Summary and overview

In this work, HPLC data collected on two cruises in the North Sea is compared against two variants of the ERSEM model, run in different physical model environments, GETM and NEMO. After validating the SST, Chlorophyll, particulate matter, nitrate, phosphate and silicate in the two models, the size-categorised HPLC data was compared against sub-sampled model data, and discussed. The authors compared the community structure using distribution plots of total chlorophyll vs PSC fraction, then introduce a unique ternary plots style to highlight the differences between the three data sources.

The authors do not thoroughly investigate the differences in the physics or the nutritive

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environment and instead focus on the functional types chlorophyll, where as these aspects of the model are crucial to understanding the origins of the divergences between these two similar models. The two versions of ERSEM are structurally very similar, and so any diverges must be due to the physical environment, the river inputs, the SPM and light models or the parameterisations. The authors have not convinced me that the differences in community structure aren't due to the physical environment or the river influx, yet they conclude that the differences are due to the sensitivity to light and nutrients parameterisation.

For instance, the GETM-ERSEM-BFM has the productivity set much higher than NEMO-ERSEM, but this appears to be compensated by much higher river nutrient fluxes than NEMO-ERSEM. This extra riverine source of nutrients leads to the stable diatom growth in the river-influenced regions. In addition, the central parts of the North Sea that do not feel the influence of the rivers have extremely low values for total chlorophyll, as the nitrate has been completely depleted there due to the high affinity of picophytoplankton. This suggests that the GETM-ERSEM-BFM community structure may be right, but for the wrong reasons. A thorough investigation of the river input, and the relationships between nutrients, chlorophyll and community structure for the two models may be needed to identify compensating errors.

Otherwise, I find that this paper to be an adequate comparison of the data and the model, but I was hoping to see more depth in the discussion and conclusions section. For instance, this paper points out that the NEMO-ERSEM simulation struggles to reproduce the August diatom concentrations seen in both the data and the other model, but does not further investigate the root cause of the differences. After providing such an in-depth description of the divergences in between the model parametrisations in section 3.3 and Tables 1 and 2 , the authors are letting themselves down by not even speculating on how specific model features could bring about the observed differences in model output. For instance, what features are in one model but not the other that could cause this? If these questions are not answerable with the current set up, per-

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haps the authors could propose an extension to the work that would highlight the major cause of the differences, be it nutrient affinity, nutrient stress,river influx, stratification, sinking, lysis or light susceptibility. A sensitivity study in a 1D column may be a possible extension that could answer the remaining questions about the ERSEM parametrisations.

2 General Comments:

The title should be revised, removing Biodiversity and "ERSEM-type models". As the authors mention in the introduction, phytoplankton community structure is an important consideration when assessing marine biodiversity, but it certainly not the only indicator. Furthermore, ERSEM-type models is not strictly defined. Please consider something like: "Observing and modelling phytoplankton community structure in the North Sea: a comparison of two biogeochemical models", but I'll let the authors come up with something better.

There is only a shallow validation of the underlying physic model validation is shown. A single figure demonstrating that the SST matches is not particularly convincing by itself, especially as one of the models already assimilates SST. In the North Sea, it is important to demonstrate that the physical model that are capable of reproducing natural vertical mixing with appropriate sources of river influx (and river nutrients). This can be done by introducing a validation of the models stratification (perhaps Mixed layer depth) and surface salinity (as a proxy for influx of fresh water). The focus of the paper should remain on the PFT's, but a demonstration of the modelled physics would allow readers to gauge how realistic of an environment the two ERSEMs live in.

The overall tone of the writing sometimes slips into a colloquial spoken-style instead of formal written tone, but this is up to the author's discretion.

I find the figure and table captions to be too brief. I would prefer having more informa-

tion in them. Typically the rule of thumb is that it needs to be enough to describe the figure/table, were it taken out of context.

The text in the figures can be a little on the small side and the figures are inconsistent in terms of style, legends, axes and plotting range.

3 Specific Comments:

L13-L16. “A comparison of the ...is key to capturing the observed biodiversity”. While I agree with this statement (after replacing “biodiversity” with “Phytoplanton Community Structure”), I’m not convinced that the work establishes it as a fact. Some changes to the discussions section are needed for it to be the case.

P7-L18: “PML-ERSEM” is used from this point onwards, but should be explicitly defined in section 3.2.

P8-L8-L9: “More explicit approach to sinking”. What does this mean? As the impact of sinking SPM underpins one of the important results of the paper, it would be good to have a more in-depth description of sinking in the two models. Also, the detrital sinking rates (if they are explicitly defined), should be presented here or in Table 2.

P8-L9-L11: The light susceptibility parameters in table 2 are not directly comparable, due the differences in model choice described here. Can they instead be converted into some other measure of light susceptibility in order to make them comparable? Alternatively, please include the equations to allow readers to draw their own conclusions.

P10-L14. Does using the WOA data as validation and as a boundary conditions have an impact on the statistical independence of the validation? Similarly, figures 4, 5 and 7 have what looks like an edge effect in the GETM-ERSEM-BFM plots along the Northern Edge. The high chlorophyll growth there in figure 5 suggests that there is an influx of

nutrients from the edge condition, especially as the central region of the North sea has been completely nitrogen depleted. Dp any of the IBTS measurements sit in a region influenced by edge effects?

P11-L12: ”both models were still able to capture the main observed features throughout the domain”. I disagree. NEMO-ERSEM does not capture the high chlorophyll values along the German coastline, and otherwise appears relatively okay. GETM-ERSEM-BFM also does not capture the high chlorophyll in the German coastline (but does a better job than NEMO-ERSEM), but has far too little chlorophyll by a factor 10x-20x over most of the North Sea. This model also has far too much chlorophyll at the mouth of the Great Ouse river, at least a factor of 10x, perhaps up to 100x, but it’s hard to tell because of the colour scale. The colour scale is hiding quite a lot of the variability in high chlorophyll values. The legend is logarithmically even between 0.1 and 5., but then adds what should be three colour bands into one (ie 5-10, 10-20,20-50 are all hidden in 5-50.) My suspicion is that using a logarithmically even colour scale for this figure will result in a change the description in the section of the paper.

P11-L25-L28 and figure 7. The WOA datasets seem a little coarse for this analysis, and are already used as boundaries conditions for both models. ICES have extensive nitrate, phosphate and silicate datasets available for the North Sea, which could be converted into a climatological dataset for future works.

P11-L30: “Both [nitrate plots] are in broad agreement with the climatology. Likewise for phosphate and silicate”. I do not agree with this statement. To me, it looks like GETM-ERSEM-BFM is entirely nitrate depleted over most of the model region, by a factor of between 10x-100x. In addition, the regions around the south-eastern North Sea have far too much nitrogen (factor 10x). These regions coincide with the largest diatom concentrations, and it looks like diatom growth is strictly governed by excess or deficiency of nitrate. This suggests the presence of compensating errors, and may imply that the diatom community structure is right for the wrong reasons. Similarly for phosphate, GETM-ERSEM-BFM has significantly lower by around 10x than WOA, and

NEMO-ERSEM fails to capture the depleted area around the Norwegian Trench. In terms of silicate, NEMO-ERSEM has significantly depleted the silicate in the northern North Sea, which is surprising, as there are not enough diatoms in the community structure.

P12-L14: "are likely to have arisen from the differences in parametrisations." Is this true? Could some of the differences in the models also arise from differences in the physical models? For instance, if one model's mixing or riverine input results in lower surface silicate, then a difference in diatom behaviour would be expected.

P12-L31: The ternary plots to describe the three population community structure are unique to this work, and may deserve their own subsection (up to the authors discretion).

P12-L32-33: "The observations form a distinct line in this space." Surely there are some interesting findings that are a direct result of this straight line? Can it be used to inform predictions? Could you fit a predictive function to this line and compare it with the three population model?

P13-L5: Knowing about the distinct straight line in the in situ data of figure 10, would it be sensible to revise the colour scheme. How about something like Green: along the line, red: too much pico; Blue: too much nano? This would highlight the regions where the model sits along the accepted line seen the the data, and where it is wrong elsewhere.

P14-L15: Why is August a challenging month to model the in North Sea?

P14-L19-L31: Please add a clear link and some discussion about the differences in the light parametrisations of the two models in Table 2. The differences in light susceptibility between the two models may be quite significant, but its hard to tell because of the incomparable parameters. This should help support one of the major points that you make in the abstract ("... changes in light and nutrients, is key to capturing the

observed biodiversity.")

P15-L3-L8: This is the most important part of the discussions section, and it would very informative to discuss how different model choices could have brought about the changes between these two similar simulations. (see general comments).

P15-L25: After this point, the conclusions risk becoming a little hand-wavey, non-specific and generic. For instance "close communication is needed between modellers and decision makers, so that the potential of biogeochemical models to support decision making is not lost in translation." While true, is not directly related to the conclusions of the study.

16-31 – p17-2. The closing paragraph will need to be changed to reflect the new title. "Can these two ERSEM variants simulate phytoplankton community structure? ..."

Regarding the figures, there is some inconsistency in the subplot titles and styles. Ie. Figure 2 has no sub plot titles, but figures 4,5,6,7,8 all do. The legends are in different places and have different styles. Ie Figure 5 uses discrete coloured circles, where as figure 2 uses rectangles. I prefer the larger rectangles or a color bar. (Congratulations are due for the consistent use of a single colour scheme.)

Please add a figure showing both model domains, the north sea domain used here and both cruise paths.

Figure 1: The axis labels, units, location markers are very small and hard to see. Please thicken the outline of the in situ data circles in a) and b) as in Figure 11. Also, can you please make the location makers (the capital letters) stand out more. You could do this by adding a white shadow to the letter edge or by adding an opaque text box. None of the subsequent maps include grid lines or latitude/longitude ticks, was there a reason why these plots do? If the goal is to introduce the readers to the domain, perhaps you would be better served by adding a map showing both model domains, the north sea domain used here and both cruise paths? The legend is above

the plot axes, where as in all other plots it sits on the right of the plot. Elsewhere, the chlorophyll is shown using logarithmic scale, but c) and d) use a linear scale. Parts c) and d) are described but not referenced again. What was the motivation of including two times series plots?

Figure 2: This plot has a very different style to the other plots. The sub-plots have no boarder, the coastlines are much higher resolution, they show rivers, the land is a different colour, and the coastline line is much thinner.

Figure 3: The “closest to the reference data” point of a Taylor plot is on the x-axis at avalue of 1. This point is usually marked with a dot, or the axis label “ref”. Please add that point or label to this figure.

Figure 5: This figure shows the satellite Chlorophyll and both models, but it may benefit from showing the in situ data as well in another two subplots. The colour scale hides quite a lot of variability here. The legend is logarithmically even between 0.1 and 5, but then adds what should be three colour regions into one (ie 5-10, 10-20,20-50 are all hidden in 5-50.)

Figure 6: This figure and the associated description in p10-L20-23 may benefit from a logarithmic colour scale.

Figure 8: I find myself flipping back and forth between this figure, figure 2 and figure 5. There are two gaps in the 4x3 grid. Would it be possible to put total chlorophyll for each model in there? Alternatively, would it make more sense to add a second version of this figure with the three populations instead of the 4 and 5 PFTs and also the in situ data from figure 2?

Figure 9: The subplot shape is not the best here. Tall and narrow subplots seems to mask the overall shape of the data. If they were short and wide, it may make it easier to inspect the data. As mentioned above, have you considered overlaying the three population fits of of Devred 2011, Hirata 2011 or Brewin 2015 onto subfigures a) and

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d) of this figure, ? This would allow a comparison of how the North Sea compares with the rest of the global ocean, as in figure 11 of Holt 2014 or in de Mora 2016. With only 85 data points you may struggle to apply the three population model to your three data sets, but it should be possible to apply the three population model to the full (not subsampled) model datasets. This could be overlayed onto subfigure b), c) e) and f). Is it even necessary to sub-sample the model data to match the in situ measurements? Judging from figure 1, the cruises have done a great job at getting an spatially even distributed data set of the North Sea. Could you comment on the detection limit of the HPLC data? The GETM-ERSEM-BFM model seems to have values much lower than that seen in data. Is that a detection limit effect not present in the model?

Figure 10: Can you please change two of the circular markers to squares and triangles, like in figure 9 so that these plots work in grey scale too?

Figure 11: This plot is the only figure in the work with a continuous colour scale, but it is the figure that would benefit the most from a discrete colour scale. In grey-scale, the legend becomes very uniform. See above comments about an alternative colour scale.

Table 1: Please add a “further reading” or “references” line with at least one reference for each model.

Table 3: In the header line, please replace ERSEM’s internal PFT’s names (P1-P4) with the public facing names used elsewhere in the paper.

Table 3: This table only shows total Chlorophyll, but it would be valuable to look at similar statistics for the rest of the community structure. ie pico, micro and nano in both models vs the IBTS data (but only for 2010+2011, you wouldn’t need both years separately)

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4 Technical Corrections:

Inconsistent use of “e.g.” in the introduction section, especially when citing other works. My preference is to avoid it altogether. p2-L3: “e.g.”, p2-L14: “e.g.”., p2-L21: e.g. p2-L33 e.g. appears twice.

p2-L2: the scientific community,

p2-L5: (MICE) acronym, while clever, is never mentioned again, so is not needed.

P2-L6: remove “though”

p2-L6: ...task at hand – different scientific.... The dash (-) should be an em dash, a semi-colon or even a full stop.

P2-L8: colloquial: “say, global-scale...”.

p2-L14: van de Molen is spelt with a lower-case v in the authors list but with a capital V elsewhere in the document and in the reference list. Is this a conscious choice? Similarly for other names: de Mora, van der Woerd, van Raaphorst, and van Leeuwen are also spelt differently in various places.

p2-L25: I don’t think you need to explicitly define the “(UK)” just as you correctly don’t define USA later in the same sentence.

P2-L28-L30, please re-write this sentence with more clarity.

P3-L13: replace “too” with “as well”.

p6-L2,4 ,6, 25: Are these web addresses the best way to include this information? If so, please cite them instead of in-line using the Copernicus standard styles: http://publications.copernicus.org/Copernicus$Publications_Reference_Types.pdf$

p3-L10, p3-L28: Would it be better to move both copies of the sentence “Details of the model configuration and forcing are given in Table 1.” to the end of the introductory

paragraph at p5-L9?

P6-L22: Please reverse FOAM definition and acronym: “Forecasting Ocean Assimilation Model (FOAM)”

P7-L6-L9: Long sentence, please split into two: “reanalysis period. The biogeochemical...”

P12-L3: Please add a reminder about how the model PFTs were aggregated. Something like: “as described in section 3.4”.

P14-L17: change strange wording: “confidence must be had in”.

p16-L21: The dash (-) should be an em dash.

P16-L27: Rephrase this sentence to re-word “for confidence to be had in them”.

P16-L33: replace “current” with “these two”.

p34 Figure 9: Phytoplankton Size Class instead of PSC in the figure caption.

P39 Table 33 caption: Statistical comparison of log 10 (chlorophyll) against IBTS observations.

5 Literature:

Brewin, R. J. W., Sathyendranath, S., Hirata, T., Lavender, S. J., Barciela, R. M., and Hardman-Mountford, N. J.: A three-component model of phytoplankton size class for the Atlantic Ocean, Ecol. Model., 221, 1472–1483, doi:10.1016/j.ecolmodel.2010.02.014, 2010.

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450, doi:10.1016/j.rse.2015.07.004, 2015.

de Mora, L., Butenschön, M., and Allen, J. I.: The assessment of a global marine ecosystem model on the basis of emergent properties and ecosystem function: a case study with ERSEM, Geosci. Model Dev., 9, 59-76, doi:10.5194/gmd-9-59-2016, 2016.

Devred, E., Sathyendranath, S., Stuart, V., and Platt, T.: A three component classification of phytoplankton absorption spectra: Application to ocean-color data, Remote Sens. Environ., 115, 2255–2266, doi:10.1016/j.rse.2011.04.025, 2011.

Hirata, T., Hardman-Mountford, N. J., Brewin, R. J. W., Aiken, J., Barlow, R., Suzuki, K., Isada, T., Howell, E., Hashioka, T., Noguchi-Aita, M., and Yamanaka, Y.: Synoptic relationships be- tween surface Chlorophyll-a and diagnostic pigments specific to phytoplankton functional types, Biogeosciences, 8, 311–327, doi:10.5194/bg-8-311-2011, 2011

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