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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 #2

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General Comments:

The manuscript investigates the phytoplankton community structure and distribution in the North Sea (NS), employing two versions of ERSEM and a set of observations including remote-sensing and in-situ data. The study demonstrates a good match between different measurement techniques and narrates an extensive performance assessment of 2 model versions, and concludes that one of the ERSEM versions - the one which is used more for coastal applications, show higher skill then the other, which is usually used for a larger domain. Manuscript is clearly written and easy to follow. The study exhibits a high degree of technical sophistication: both versions of ERSEM provide detailed descriptions of numerous processes driving the highly com-

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plex ecosystem under investigation, and the observation dataset, especially regarding the HPLC-derived PFT's provide an excellent base for model evaluation. However, despite these ample resources and recognized expertise of authors, the manuscript fails to deliver a comparably significant outcome or a crisp message in its current form.

Specific Comments:

1) The title is not representative:

a) The title suggests this work is about biodiversity, but actually it's not. Although the 'relative abundance' of PFT's (Fig. 2,10 and 11) is an index relevant for biodiversity, the results are not at all discussed or framed in the context of biodiversity (in fact only the very last short paragraph of the manuscript refer to biodiversity again after the introduction, and replacing 'biodiversity' with 'phytoplankton community structure' would probably improve this paragraph already).

b) Again, at the first glance 'ERSEM-type models' looks like more general than it is, whereas the manuscript is literally about 2 ERSEM versions, which could be more clearly indicated in the title. Accordingly, I invite co-authors to consider a more representative title (as changing the content according to the current title would basically result in a new manuscript)

2) Validation of PFT estimates is not a new endeavor:

A recurring notion in the manuscript (abstract, introduction, summary and discussion) is that 'model estimates of PFTs are not validated', which is misleading. Quite a few attempts have been made previously to assess model performance with respect to reproducing the spatial distributions of PFTs, such as those making use of the data from continuous plankton recorder surveys, monitoring data obtained at stations located at different sites and estimates from satellite images, which should be acknowledged (examples are too many that pointing to specific studies would be unfair to others).

3) 'Observations' section is incomplete:

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In this section, only the procedures relating to the HPLC data are provided. Other data sources, like the 'in-situ quantification of chl-a', OSTIA-SST, OC5-chlorophyll/SPM and WOA data should be introduced also in this section, and not elsewhere (e.g., as currently done in sections 4.1 and 4.2, see also the 'Technical Corrections')

4) 'Models' section is inadequate:

a) 3.1 GETM-ERSEM-BFM: each item in the list of additional processes to 'make [ERSEM as presented by Vichi et al. (2007)] suitable for temperate shelf seas' need a reference to previous literature if possible (like in the list items iv and v), or if not, an adequate description.

b) 3.2 NEMO-ERSEM: page(P)6,line(L)15-16: the employed version of NEMO appears to be not described previously (although a full description is foreseen to be provided by O'Dea at al., in prep), so here at least some hints should be provided

c) 3.3 Comparison: a figure showing the complete domains of the two models would be helpful

5) Fig.11 is difficult to perceive:

although I like the idea of overlaying measurements on the contour-plots using a ternary color map, I found it difficult to distinguish the resulting colors. Although I admit distinguishing colors is not my strength, I wonder whether using a more saturated and bright color scheme would help (eg., as in Fig.4 of Arteaga et al. 2014). The authors may also want to consider plotting the percentage histogram of each group across total chlorophyll as in Fig. 3 of De Mora et al. (2016), which does not rely at all on the color-perception abilities of the reader.

6) How to reproduce the spatial distribution of PFT's? :

as mentioned above, the results suggest that the ERSEM version previously tuned more for coastal applications (GETM-ERSEM-BFM) performs better than the one used for simulating the margin of the Atlantic Ocean (NEMO-ERSEM). But considering that

the performance difference wrt the PFTs basically boils down to the reproduction of the diatom blooms at the coast by GETM-ERSEM-BFM and not by NEMO-ERSEM, the obvious follow-up question is why the latter cannot reproduce the Diatom blooms at the coast, which I believe should be straightforward to figure out. It is hinted in section 3.3 and later in P15,L1-7 that the parameterization of the PFT's is likely to be the reason, but, it should be straightforward to test whether this explanation really holds. And I believe this would be a rather useful outcome of this study with a potentially wider relevance: can an open-ocean model transferred to a coastal system just by a reparameterization of the PFTs?

7) What drives inter-annual differences?:

Figures 4-11 suggest considerable differences between 2010 August and 2011 with respect to physical, chemical and biological characteristics. Although these differences are acknowledged throughout the manuscript, not much is done to elaborate the mechanisms driving these differences, apart from hinting 'a set of physical drivers' to be responsible for the difference (P15, L21). Again, I feel like more could be done with the available tools and data.

Technical Corrections:

- P6, L34: sediments=? SPM?

- P7, L3: nutrients =? of all forms, or dissolved inorganic form only?

- P7, L28,L30: first occurrence of the 'PML-ERSEM'. Although what this means should be clear to the careful reader, but better stick with the previous definitions.

- P9, L11: first occurrence of MODIS (also consider my specific comment (SC)-3)

- P9, L26-27: 'Picophytoplankton...' reference needed, and consider SC-3

- P10, L9-14: consider SC-3

- P10, L33-34: 'The largest PFT...' reference needed and consider SC-3

- P12,L15-21: belongs to section 4.2?

- P13, L24: NEMO-ERSEM chlorophyll is not so good really?

- P16, L6: specific issues =? please expand

- Table 2: are the question marks in the NEMO-ERSEM parameters typographic errors?

- multiple occurrences of 'less good': not wrong, but wouldn't 'not as good' be more natural?

- P24, L9: reformat Thorpe et al.

References:

Arteaga, L., Pahlow, M., Oschlies, A. Global patterns of phytoplankton nutrient and light colimitation inferred from an optimality based model. Global Biogeochem. Cycles, 28, 648-661, 2014

De Mora, L., Butenschön, M., 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, 2016

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