Ocean Sci. Discuss., 9, C301–C305, 2012
www.ocean-sci-discuss.net/9/C301/2012/

Ocean Science Discussions

Interactive comment on "Validation of the NEMO-ERSEM operational ecosystem model for the North West European Continental Shelf" *by* K. P. Edwards et al.

K. P. Edwards et al.

karen.edwards@metoffice.gov.uk

Received and published: 11 May 2012

The reviewer's main concern is that "the paper only focus on the biochemistry and that some of the results are impossibel to discuss without saying anything about the physics." As we say in paragraph 1 on Page 749, the purpose of this paper is to validate the ecosystem component of the operational system which is run as part of an automated suite of operational models and serves the MyOcean community as well as other customers. We believe that the paper as presented (with some clarification and changes) is suitable as a validation paper for the operational ERSEM system and represents a continuation from O'Dea et al (2012) which focussed on the physics in

the new operational system. However, as we mention below and in the Reply to Reviewer 3, we plan to extend our discussion of the model physics as suggested by both reviewers.

The reviewer states "when doing a validation a two year hindcast is not enough. The present ms. only show that the new model is able to reproduce the seasonal signal. I would like to see that the model is able to reproduce the inter annual variability. To do this at least 20 years are needed."

There has been and continues to be a lot of validation on the ERSEM model itself with much of this work being cited in our paper. Given this ongoing effort, the focus of this paper is to validate the ecosystem component of the upgraded operational system. For this, we believe that a two-year hindcast is sufficient. We realize that we can't yet say anything about the interannual variability, but this analysis does highlight which parts of the overall domain ERSEM does well in (and which parts it does not) but also how well the system does in reproducing the seasonal and shorter cycles, a key ingredient in the operational products. As the model set-up is being used to address short-term daily analysis to 5-day forecasts rather than climate change, the interannual variability is of less importance than an appropriate representation of the seasonal cycle. In this case, it is important to show the model skill with data pertinent to a particular year, otherwise the forecast becomes redundant with respect to a reanalysis climatology.

In Paragraph 2 of the review, the reviewer asks about the initial fields for the nutrients between the two model runs which result in large differences in the results. To clarify, the two hindcasts do start with slightly different values for many of the ERSEM variables although the nutrients have been reset in the MRCS-PE run. We have added the following to the end of Paragraph 1 in Section 2.2:

"For the MRCS-PE hindcast, the POLCOMS fields were taken from the operational restart at the end of 2006. A one-year spinup for this system, similar to the MRCS-PE system was performed with the initial bed sediment as above and with the ERSEM fields from the operational restart with the exception of a reset of the nutrient fields to remove the nutrient problems present in the operational system. From this, a two-year hindcast was produced. "

Additionally, the reviewer hypothesizes that there is either a bug in the POLCOMS code or that the physics between NEMO and POLCOMS are very different. We do discuss the nutrient problem and possible causes on page 762 paragraph 1: which points to a change in the domain size along with the boundary conditions used in ERSEM in the POLCOMS-ERSEM model. The MRCS-PE system was presented in Siddorn et al., 2007 along with a discussion of some of its short-comings and is used here to provide a comparison to the new operational system. The transition from POLCOMS to NEMO began several years ago in the Met Office and, since that time, the POLCOMS-ERSEM system was not further developed. As this paper is providing a comparison between the operational systems for both the MRCS-PE and AMM7-NE, the code for both POLCOMS and ERSEM in the MRCS-PE system is from the date the transition began and, as we mentioned above, several of the deficiencies in the older system were successfully addressed in the new AMM7-NE operational system and led to the improvement in the nutrient fields on the shelf.

To reply to the specific comments by the reviewer:

Page 748: we mention operational systems in the Mediterranean, Baltic and Adriatic Seas. If the reviewer had other systems in mind, we would certainly include them, but this is the list we found in a Web of Science search for operational modelling and, as this is not a review paper, we have not claimed that we have provided a complete list.

Section 2.1.2: As mentioned above, the purpose of the paper is to compare the operational systems, so we have clarified the two changes to the parameter set between the systems with the following: The parameter set used in the MRCS-PE operational system is from Blackford et al. (2004) while the new AMM7-NE system includes modifications as described in Butenschön et al. (in prep).

Section 3.1: We have decided to address this more clearly in the Discussion section and have added the following in Paragraph 1 Page 762: This is apparent in both the winter nutrient levels and the draw-down of the nutrients in spring and summer. In Figure 6 of this paper along with Figures 4c and 5c from Siddorn et al (2007), it becomes apparent that there was an underestimation of the nitrate depletion in summer months in the MRCS-PE system that is no longer apparent in the AMM7-NE system. This may have contributed to the MRCS-PE system problem of the accumulation of nutrients throughout the domain over time.

Page 757, line 5: true.

Page 757, line 3, 18, etc.: while we agree that the correlations are strongly influenced by the seasonal cycle, it is an important part of the operational model that we manage to reproduce that cycle. The correlations provide an indication of how well the model reproduces the data both in time & space – which is very interesting. This is highlighted through the use of the Taylor plot with the subregions, so we believe that their use is justified.

Section 3.2 and further on: we believe that the use of log10 chlorophyll is justified and is actually providing useful information to the reader. Because of the wide range of values within the chlorophyll field, any plots that do not use log10 chlorophyll are dominated by the peak values and obscure any information about the variability at the low end of the range. This is especially true in Figures 10 which provides time series comparisons of log(chlorophyll) at Gabbard. From this it is apparent that the satellite data is limited at the lower end (the minimum value in the satellite observations is 0.1 mg chl m-3) while the peaks in both are similar. At this particular location, the model does a fairly good job of reproducing the observations at the lower end of the scale but not the upper. Using a normal scale would make this detail very difficult to pick out.

Page 760: thank you. We are looking into further analysis of this data set along with other in situ data that is slowly becoming available for these years.

Page 762, line 1: We already provide some general results from both O'Dea et al (2012) and O'Neill et al (2012) in this paragraph and believe that any more specific results are not necessary and outside the scope of this paper. However, in response to this point and your earlier point about the importance of physics (as discussed in Skogen and Moll, 2005), we have added the following: As discussed by Skogen and Moll (2005), the choice of the physical model on the biogeochemistry is very important. For our new operational system, O'Dea et al.

Interactive comment on Ocean Sci. Discuss., 9, 745, 2012.