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Interactive comment on "Recent development of the Met Office operational ocean forecasting system: an overview and assessment of the new Global FOAM forecasts" *by* E. W. Blockley et al.

Anonymous Referee #2

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Title: "Recent development of the Met Office operational ocean forecasting system: an overview and assessment of the new Global FOAM forecasts"

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This paper describes the new implementation of the operational Met-Office Ocean Forecasting system. It describes the upgrades brought to all the elements of the sys-

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tem (model configuration, assimilation scheme, sea-ice etc. . .), and changes of the operational suite. It then shows and discusses statistics of analyses and forecasts over a two-year period. The results show an overall significant improvement of these, with some exceptions for some areas and variables where the new system performs worse than the previous one, especially in the tropics and in the Southern Pacific. The last section of the paper discusses this and suggests leads to improve those points.

1) General remarks

My general opinion is that this paper is a serious, rigorously conducted, documentation of the new system. It is well written, clear and rather easy to read, despite the quantity of technical details about the suite of operational systems at the Met-Office. The results are convincing and well presented. Although the assimilation techniques used in this paper have already been developed in the context of lower resolution models, their application within this version of the NEMO are worth being documented and analysed.

I have two main critics:

- It is claimed that the new operational implementation using a 48 hour window “allows the FOAM system to assimilate considerably more observations” (page 6229), up to 50% more (p 6230) for the window -48h -24h. However the trials presented in sections 3 and 4 use a 24h window. I agree that this does not affect the comparison between systems since it is done in both v11 and v12 trials. But the discussion of the impact of such an increase in available data for the analysis step is not discussed anywhere in the paper. I agree that redoing a full set of analyses/prediction can’t be done, but I am surprised that the authors have not done any test addressing this question.

- My second critic is on the form of the system description (section 2). Lots of details are given there, which is good, but it makes the reading a bit fastidious. Moreover, the main motivations of the design of the v12 system are not exposed clearly enough in my opinion. I therefore suggest placing as much details as possible in one or several appendices, and reorganising the presentation around the main improvements of the system and their justifications. See below for some more comments.

This paper can therefore be published, but addressing these points should strengthen its message in my opinion.

2) Some more specific comments along the text (page and line numbers refer to the pdf version published here: http://www.geosci-model-dev-discuss.net/6/6219/2013/gmdd-6-6219-2013.pdf) :

- p 6221, line 26 and following: “This change from LIM2 to CICE was driven by the need to be consistent with the Met Office seasonal forecasting”. Could the authors be more specific ? What are the inconstancies implied by LIM2 ?

- p 6222, line 14-15: I don’t think Collins et al. 2006 support the affirmation that “the Atlantic meridional overturning circulation at 26.5°N” is “important for the initialisation of the coupled seasonal forecasts”. Its focus is on interannual to decadal forecasts.

- General comment about section 2.1: this section presents lots of details of the model configuration and I am not convinced that all of them are needed for the understanding of the results presented in sections 3 and 4, since NEMO was already in use in v10 and v11 versions. I would suggest placing most of them in an appendix and keeping in this section the major choices made specifically for the v12 version. For example, most parameterizations specifications refer to the Drakkar community choices, which goal is to run the model for climate studies. Focussing more clearly on what has been changed and why for the operational use would make this part stronger.

- p 6226, line 5-7, and last paragraph of section 2.1: see my comment below. I don’t sea the justification of changing LIM2 whilst some inconsistencies still exist with CICE.

- Section 2.2: NEMOVAR is a new feature specific for v12, so it is legitimate to present it extensively. However, as for section 2.1, I wonder whether some features could be put into a second appendix. On the other hand, almost nothing is said about the previous assimilation scheme (OCNASM) (e.g. is it multivariate ?, etc…). This contrasts with

GMDD
6, C2435–C2439, 2014

the previous section about the model. One or two sentences would remind the reader its basic principles and help interpret results in the following sections.

- p 6227 line 2: “Key features of NEMOVAR are the multivariate relationships which are specified through a linearised balance operator”. It is not clear whether there is some balance relationship between sea-ice concentration and the other state variables (none of that is in Weaver et al, 2005). Could the authors clarify this ?

- About the sea-ice assimilations scheme (section 2.2): is there a constraint that the ice thickness is positive within the assimilation scheme, or is this ensured by the model ?

- p 6238, end of section 4.1.2: The formulation “NEMOVAR fails to fully constrain a persistent model bias” is a bit specious. First the model in free mode primarily causes the bias. Second, this implicitly says that OCNASM succeded reducing it in v11. What is the bias of the v11 equivalent free run ?

- p 6239, line 5: typo “salinity”.

- Section 4.1.4: The free run mean sea level drift about 5 times the observed long term trend should be a concern. To my knowledge, there is a NEMO option that can prevent the model from drifting through a water flux correction. Is there any reason why it has not been used here ?

- Section 4.1.4: It is claimed several times in the paper that the NEMOVAR assimilation scheme is more suitable for constraining the eddy variability, but it is not fully demonstrated in the paper in my opinion. A way to illustrate this could be to divide the ocean (outside the tropics) in two regions of high and low variability using a sea-level variance threshold derived from the observations, and to re-compute statistics such as Figure 1e in each region. I think including such a diagnostic in this section would add information.

- Sections 4.2.2, last paragraph p 6247: the problem of overfitting has already been addressed within the NEMOVAR system in Daget et al., 2009 Q. J. R. Meteorol Soc., 135, 1071-1094. They propose a diagnostic that could be used here.

- Section 4.2: This section does not present forecast statistics of sea level anomalies. I wonder why, because sea level is a useful indicator of the upper ocean dynamics. I understand that it is indirectly done in comparisons with the OSCAR product in section 4.3, but this is only qualitative. In my opinion, a direct verification with sea level gives at least as much information.

- p 6254, line 11: typo “will be upgraded”

- p 6255, line 24: typo “ocean”

- p 6256, line 9-10: I don’t understand the argument about increments in the tropics. It is said line 17 of the same page that tests “including the use of a second-order velocity balance” are under way. This implies that this balance is not applied in the present system, and therefore that the velocity increments should be zero in the tropical regions. If this is right, how could velocity increments indicate anything as said line 9-10 ?

- Remark about the edition of the figures: I found the indications/legends of all figures very small and had difficulties reading them even zooming on my screen.

- Remark about Figure 7 b and d: the forecast lead-time information is not obvious to understand. I suggest a better caption text, or a different representation.

Interactive comment on Geosci. Model Dev. Discuss., 6, 6219, 2013.