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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.

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The paper reports on the validation of Validation of the NEMO-ERSEM operational ecosystem model in the NE Atlantic.

Although the paper is well-written and contains some interesting information on details of the difficult exercise of setting up an ecological model forecast, I think the results are in an early stage of elaboration and the paper does not contain substantial developments in modelling or skill assessment of the model nor provides insight in the ecosystem in the area and therefore I recommend rejection although I'd encourage authors to focus their presentation of the results on providing new info on the system or

more novel methods on the skill assessment of the model.

My major concerns are: - It seems that the main conclusion is that the new model configuration outperforms the old one, which aparrently had some major deficiencies. However, the model only compares with surface data values and mainly at seasonal scales. I do not see the gain in running a forecast model which has not been evaluated in its response to events. On the other hand, the authors only compare with surface values. The authors are aware of it and report that they will perform more validation with other data (page 761), and I feel that some comparisons to subsurface data are required (see below) - It is clear that the model has evident problems on the slopes. The authors claim that "This may be partially caused by excess upwelling in the NEMO model along the steep slopes in these regions (John Siddorn, personal communication) and is also evident, but to a lesser extent, in the other ERSEM variables (see Figs. 2, 3 and 9)." but to me the unrealistic excess nutrients on the slopes is a clear sign of excess spurious diapycnal mixing on the slope, which is likely to come from the physical model configuration. Although spurious diapycnal mixing is a characteristic of fixed level models, and cannot be eliminated, it can be reduced and it seems it is not the case in this configuration - Other clear major problem is the unrealistic winter chlorophyll maximum west from Iberia. In fact the authors report that "The AMM7 domain appears to rely on nutrient influx from the North East Atlantic Ocean" but do not show enough evidence that the nutrient values in the NE Atlantic and its interannual variability are correctly captured in the winter mixed layer and in the subsurface central waters. Given the likely problems of spurious diapycnal mixing in the model, I doubt that subsurface values of nutrients are realistic, and these nutrients are of high biological relevance since central waters reach the surface with upwelling, especially to the south of the domain

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