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Interactive comment on “MOMBA 1.1 – a high-resolution Baltic Sea configuration of GFDL’s modular ocean model” by H. Dietze et al.

H. Dietze et al.

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We thank reviewer 2 (R2) for his time, effort and encouraging comments. Overall R2 deems the manuscript being worthy of publication. He seems to agree that the manuscript describes a model, which indeed, can be used for interesting research. His major issues are an insufficient description of the lateral boundary conditions at the Danish Strait and "... too many typos and grammatical errors".

As concerns the former: there are no lateral boundary conditions in the Danish Straights. All we do, is to restore temperature (T), salinity (S) and sea surface height (SSH) in North-Sea/entrance to the Skagerrak (T & S west of 9.3E; SSH west of 6.895E). We will clarify this in the revised manuscript and we are sorry for the confusion.

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As concerns the latter: we are especially grateful for any specific comments because we are non-native speakers. We will carefully proof-read and edit the manuscript, as suggested by R2.

— Minor issues —

– R2: The authors talk about the atmospheric boundary conditions without carefully stating what they are. One wonders if there are more fluxes than what they state in the second paragraph (section 2.5 , line 17) plus the SW and LW. The authors do not mention whether the incoming shortwave has diurnal cycle corrections which could be important in the Baltic latitudes. Also they do not mention if any albedo treatment was done on their stated 1353 W/m^2 value. To my knowledge MOM4 and MOM5 accept only a single net downward LW radiation flux. So I do not understand the authors comment about "Long wave radiation leaving the ocean" on page 2074, line 10. Is this added or included in their Eq. 4? section 3.1

–A: We agree with R2 that this is important information and will clarify this in a revised version of the manuscript.

– R2: The authors do not provide a figure for the observed mean circulation in the Baltic but state that their results in Fig4 are consistent with the observation. One wonders how good this consistency is, quantitatively. It would be worthwhile to demonstrate how good this agreement is.

–A: We agree that using a comparison with surface salinity as a proxy for a comparison with observational estimates of the mean circulation in the Baltic is dissatisfactory (and we do state that in the manuscript in line 25 page 2075). The reason for us doing so (and for others having done so in the past) is that we do not have access to quantitative observational estimates of the mean circulation in the Baltic. We would be grateful for hints pointing us to such data (and would, upon reception, be happy to include such a comparison in a revised version of the manuscript).

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–R2: The authors have provided the observation comparison for salinity in Fig 5., which is good , but the units of salinity is missing which could be confusing for readers not familiar with Baltic and the fact that they restore to 35PSU at the western boundary. section 3.2

–A: Agreed. We will add units to Fig. 5 in a revised version of the manuscript.

–R2: The units of the eddy kinetic energy is stated wrong and should be corrected throughout the paper to cm^2/sec^2 . Also because of the large range of this quantity it would be advantageous to sketch its logarithm.

–A: R2 is right, we got the units of energy wrong throughout the paper. We will mend this in a revised version of the manuscript. As concerns the logarithmic scale: We tried that, but feel however, that a logarithmic scale obscures the point we try to make with the figure (but that is, admittedly, subjective).

–R2: It would be advantageous if the nautical mile is defined (1.85 Km) at the onset to eliminate any guess work.

–A: O.K. We will do that in a revised version of the manuscript.

Interactive comment on Geosci. Model Dev. Discuss., 7, 2065, 2014.

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