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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.

Anonymous Referee #2

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general comments:

The authors present a high resolution (~ 2 km horizontal) ocean-ice model to simulate the dynamics of the Baltic sea. They suggest to use this as a basis for a fully coupled model to study the biogeochemistry of the region. They have carefully chosen a particular initial condition for the model and have set themselves the goal to look at a few specific diagnostics in the model to assess its performance. The authors have accomplished to run the model on various computational resources. They have stated both the success and shortcomings of their model for the diagnostics they chose to study. In my opinion this work is worthy of publication as a Discussions paper after addressing a few issues.

specific comments:

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section 2.5

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

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.

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

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.

The hypothesis regarding the presence of Ekman pumping in the absence of a high resolution wind data is very interesting indeed.

section 3.4

My biggest concern is that the authors do not carefully state how they apply the lateral boundary condition at the Danish Straits. Do they apply the flux of incoming/outgoing water mass from observation, or from a global model, and how frequently is this done? Do they apply a corresponding salt flux in that boundary? Or, do they just treat the "restoring" mechanism as the lateral boundary condition? These questions become puzzling when they mention that their model underestimates the deep water salinity.

technical corrections:

The paper has too many typos and grammatical errors and should be carefully proof-read and corrected before publication (the typos are not limited to spelling errors).

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

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