

Vaenersborg, 3-July 2014

Dear editor,

as put forward in the open discussion we found many of the issues raised by the reviewers constructive and very helpful. Below, we present the list of changes applied to the original manuscript. Note that we have marked major changes in bold.

We hope that the revised manuscript is now suitable for publication in GMD. In any case, thank you for your time and effort!

Yours sincerely,

the authors

Concerning R1:

We added a paragraph explicitly stating that there is no tidal forcing and that there are no open boundaries (pg. 5, ln. 6). We do now state in the caption of Fig. 4 that the actual model domain is bigger than what is shown, since we think that this triggered the question about open boundaries.

Concerning 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.

We modified the explanation (pg. 10, ln. 8 - pg. 11, ln. 15) and think it is now much clearer.

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/m² value.

We added an extensive explanation (pg. 10, ln.15 - pg. 11, ln. 10).

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?

We clarified the respective sentence (pg. 12, ln. 4 - 6).

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

We added units to Fig. 5. Further we explicitly state now that the western boundary is further west than shown in Fig. 5.

The units of the eddy kinetic energy is stated wrong and should be corrected throughout the paper to cm^2/sec^2 .

We corrected the EKE units throughout the manuscript.

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.

After carefully rereading the manuscript we think that the reviewers confusion about lateral conditions came from the fact that all our result figures show only a fraction of the actual model domain. We do now state in the caption of Fig. 4 and Fig. 5 that the actual model domain extends further to west than shown in the figures (since the focus is on the Baltic and not on the North Sea). Further we added two clarifying sentence right at the start of the model description (pg. 5, ln. 2 - 3).

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).

We found (and corrected a couple) and hope that the copyediting will root out the remainder.

It would be advantageous if the nautical mile is defined ($\sim 1.85 \text{ Km}$) at the onset to eliminate any guess work. ! we do tha

We do that now in the second sentence of the abstract and on pg. 4, ln. 10.