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Interactive comment on "Effects of surface current/wind interaction in an eddy-rich general ocean circulation simulation of the Baltic Sea" *by* H. Dietze and U. Löptien

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Answer to Anonymous Referee 1 The referee's comments are typed in **bold**.

I find the narrative to be less than perfectly clear, however, mainly due to distracting phrases sprinkled throughout, a bit of a muddled notion of equivalence, and some clumsiness in crafting the storyline.

We acknowledge the referee's time and effort. We are especially thankful for his many constructive comments and suggestions!

The biggest problem I see is the notion that these results are inconsistent with Martin and Richards (2001), who discussed Ekman pumping within coherent vortices, not the general impact of surface-current-wind interactions on vertical exchange. Here the authors seem to indicate that regions where vertical exchange is enhance with current-wind turned on are consistent with Martin and Richards (2001), even though these regions are not operating with the same physics; i.e., coastal upwelling south of Sweden versus coherent vortices.

The main topic of the paper are "effects of surface current/wind interaction in an eddy-rich general ocean circulation simulation of the Baltic Sea". It is motivated by (1) published theoretical considerations which hint towards a strong effect of surface current/wind interaction, (2) by the fact that, only very recently, the spatial resolution of Baltic Sea model configurations allows for a fairly realistic representation of surface currents. Combining (1) and (2) raises the question if and to what extent previous (coarser resolution) model configurations are flawed - a question which is highly relevant in the Baltic as all projections into a warming future are based on (coarser resolution) models that do miss most or even all of the current/wind effect.

The referee is right in pointing out that we did not prove Martin and Richards (2001) wrong. The referee is also right in pointing out that coastal upwelling off Sweden is not to be confused with coherent vortices. We will revise the manuscript rephrasing all respective clumsy passages with the aim to make the following line of thought much clearer: (1) theoretical considerations suggest that surface current/wind interaction may give rise to substantial vertical upwelling and downwelling. (2) This raises the question if and to what extent previous (coarser resolution) model configurations are flawed in terms of their vertical transports of heat and nutrients. (3) To explore this we compare two simulations: one comprising the surface current/wind effect with one neglecting the effect. (4) We find that vertical exchange is, on average, damped.

Locally, however, as e.g. south of Sweden, the vertical exchange is increased.

pg 2, ln 9-10: the implication here is that adjoint methods are correcting stress estimates because of uncertainty in the stress formulation. Is that really the case? This methods is correcting for all source of uncertainty in the forcing, including the data describing the wind fields themselves.

The implication is not the case. Rather: the wind stress formulation is so uncertain that modifications within is substantial uncertainty can "compensate" most of the other sources of uncertainty. We will clarify our reasoning in the revised version of the manuscript or delete the respective paragraph.

pg 2, ln 21-22: I don't understand the sentence, "It is based on the success of the concept Ekman Pumping." Martin and Richards (2001) describe how eddy-wind interaction results in Ekman pumping within eddies.

We will delete " ... the success of ..." in the revised version of the manuscript.

pg 3, ln 29: The word, "competitive" is not appropriate.

We will rephrase to " ... competitive compared to other Baltic Sea configurations ... ".

It seems odd to report the resolution in nautical miles with the relevant metrics about scaling (Rossby radius) are in km. I would report the resolution in km.

O.K.

pg 3, ln 32: KPP –> K-profile parameterization (KPP)

O.K.

pg 4, ln 14: I find the phrase, "REF is identical to MOMBA 1.1" confusing. REF is a simulation and MOMBA 1.1 is a model? What "earlier Baltic Sea models" are you referring to?

MOMBA 1.1 is a configuration of GFDL's Modular Ocean Model. We will clarify the issue in the revised version of the Manuscript. As concerns "earlier Baltic Sea models" we will add respective references - among them:

Meier, M. H. E., Döscher, R., Coward, A. C., Nycander, J., Doeoes, K. (1999). RCO-Rossby Centre regional ocean climate model. Model description (version 1.0) and first results from the hindcast period 1992/93. SMHI Reports. Oceanography (Sweden).

Meier, H. E., Döscher, R., Faxén, T. (2003). A multiprocessor coupled iceâĂŘocean model for the Baltic Sea: Application to salt inflow. Journal of Geophysical Research: Oceans, 108(C8).

Funkquist, L. and Kleine, E.: HIROMB - An introduction to HIROMB, an operational baroclinic model for the Baltic Sea, Tech. rep., SMHI, 2007.

pg 4, ln 23: I have difficulty parsing this text: "...detailed in Large and Yeager (2004); Large (2006) which has matured to a reference in the field (e.g. Griffies et al., 2014)." What does that mean?

We will delete " ... which has matured to a reference in the field ... " in the revised version of the manuscript.

pg 4, ln 24: This sentence is unnecessarily complicated: "The setup noCW is identical to REF except for that the traditional (similar to, e.g., Meier et al., 1999, their Eq. 30), physically less plausible way to force an ocean model, which neglects the effect of surface currents on the wind stress, is applied." Also, use of the word "traditional" may be confusing to some readers with different levels of experience with ocean modelingâĂŤ it is not necessary to characterize the approach this way.

Agreed. We will rephrase in the revised version of the manuscript accordingly.

pg 4, ln 30: What does "...to an apparently especially realistic model behaviour" mean? A period where the model compares especially favorably to observations?

Yes. We will rephrase this clumsy sentence.

pg 4, ln 31: why is bit-reproducibility relevant here?

This is just for the sake of completeness. We will add some explanation here.

Fig 1. add panel showing SST and heat flux time-series?

Very good idea.

pg 5, ln 10-17: I find this explanation hard to follow. What is the change in mean SST? Does stratification increase in REF relative to noCW? What happens to

MLD? It seems that this is an obtuse angle from which to attack the differences in the simulations.

We will avoid the expression "thermal momentum" in the revised version of the manuscript. We believe that this inventing of new terminology made it hard to follow our argument. Here is what we were trying to say:

We find:
(1) A damped amplitude of the seasonal cycle of air-sea heat fluxes.
(2) An increased amplitude of the seasonal cycle in sea surface temperature.

We conclude that a reduced seasonal flux variability drives an enhanced surface temperature variability. Thus the water column subject to seasonal heating and cooling from the surface must be shallower.

We will clarify this in the revised version of the manuscript. We will also add the information that the basin-averaged MLD (defined by a bulk-Richardson number following Large et al .1994) gets shallower - in line with our reasoning.

pg 5, ln 19: This sentence, "A gedankenexperiment reveals that by accounting for the ocean's movement in the calculation of wind stress exerted on the ocean's surface –overall – less energy is transferred to the ocean: winds and surface currents can – in addition to having a perpendicular component to one another – either oppose one another, or run along into the same direction" is confusing: why resort to a thought experiment when you have actual numerical experiments? What are you actually saying? Perhaps present Fig 2 first, then describe the mechanisms operating to cause this change.

We will reformulate the corresponding sentence.

pg 6, ln 1: Fig 2 confirms that there is less net energy transferredâĂŤif you rely on the reader to spatially integrate the difference field. Maybe point out that this is what you really mean.

O.K.

pg 6, ln 5: Fig 3 looks like it has some mesoscale variability retained in the climatological field. Are the wintertime difference really the same sign everywhere? This figure indicates that mixed layers are not shoaling everywhere. This is not reflected in the textâĂŤagain, you are leaving out a step, it is the spatial integral of this map, not the map itself, that indicates net shoaling over the domain.

We will clarify that we refer to spatial averages and we will add an explanation We regarding the "remaining mesoscale variability". The explanation will state that the respective variability is the result of differences in persistent current features which are correlated with topography.

pg 6, ln 9: “supply” –> “transfer”

O.K.

pg 7, ln 9: I would have said these winds are southwesterly.

We will change that to: " ... the winds' persistency decreases as they travel in a north eastward direction"

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