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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

Anonymous Referee #1

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General

The author conduct two companion integrations of a regional model, one with the surface current included in the wind stress computation and another without. The main conclusion of the paper is that accounting for current-wind effects inhibits the total vertical exchange over the model domain, but enhances vertical exchange at major upwelling sites.

The overall approach and methods here seem generally sound. 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. The biggest problem I see is the notion that these results are inconsistent

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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.

Detailed comments

Introduction

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.

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.

Method

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

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

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?

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)."

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What does that mean?

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 modeling—it is not necessary to characterize the approach this way.

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

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

Results

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

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.

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.

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

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 text—again, 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.

pg 6, ln 9: “supply” –> “transfer”

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

Fig 6: how is persistent defined? It’s okay to provide a reference, but we should at least be provided with some minimum information to interpret what’s plotted.

pg 7, ln 13: “Consistently, ” –> “Consistent”

Discussion

pg 8, ln 5-7: Martin and Richards (2001) point to Ekman pumping in the interior of eddies. You only have an inconsistency, then, if you posit that the ocean surface is wholly dominated by coherent vortices. I am not sure that the Martin and Richards (2001) result can really be extended to make inferences about net momentum transfer with and without the surface-current effect on wind stress.

pg 10, ln 5: “...prevails [over] the effects....” It’s not clear what is meant be “increase horizontal inhomogeneity.” Please remind the reader of this concept as discussed in the introduction.

pg 11, ln 4: I don’t think you have demonstrated this. You have presently only mean quantities, leaving open the possibility of effects with cancelation.

Summary and Conclusion

pg 12, ln 18: I don’t see how this is consistent; you are talking about coastal upwelling and Martin and Richards (2001) discuss Ekman pumping within coherent vortices.

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