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Interactive comment on “Investigation of model capability in capturing vertical hydrodynamic coastal processes: a case study in the North Adriatic Sea” by W. J. McKiver et al.

Anonymous Referee #1

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Review of "Investigation of model capability in capturing vertical hydrodynamic coastal processes: a case study in the North Adriatic Sea" by W.J. McKiver et al.

***** General Comments: The paper is focused on the evaluation of the ability of two different models in capturing coastal and shelf processes in North Adriatic during the extreme DWF event in 2012. Authors point out the differences between the two models and attribute the observed differences to different model characteristics and/or to boundaries (surface and lateral) forcing. The models, based on different grid architectures (unstructured and structured grids), are different from many point of views including the choice of a different formulation of the surface fluxes (momentum and heat). Validation is done with satellite (even if limited to some snapshot) and in situ ob-

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servations, finding an acceptable agreement of model outputs with observational data. Discussion and conclusions are coherent with findings. I only have a single concern that the Authors should respond: -Considering the relevance of surface fluxes on the DWF event, I would have preferred that Authors had used the same formulation for heat and momentum fluxes in the two models. This would have confined the interpretation of the observed differences to other "structural" aspects as turbulence and advection scheme, viscosity parameterizations and architecture/resolution issues. In the present comparison it is hard to understand if differences are more due to surface forcing or to the other above cited structural aspects of the models.

Said this, in my opinion the paper would deserve to be published after minor revision.

Specific Comments and Technical corrections

MEthods:

-Missing bathymetry information for both models. What's the source bathymetry and its resolution. I would expect that the same bathymetry is used for both models. -What's the averaged horizontal resolution of MIT-gcm? please state it in the text. -What's the atmospheric model providing forcing fields? What's its time and space resolution? Are analyses or fcst? -Do you use AFS fcsts hindcast or analyses for the BC? Please state it in the text

p 1626 line 3 -"involve" in place of "involves"

Fig.3: 1)enlarge the subpanels. dashed lines, especially in their high frequencies, can be hardly discerned in temperature panel. It is also hard to distinguish shyfem 22.5 from OBS 22.5. 2)do the salinity and temperature are shown at the same time frequency as they are acquired? In performing statistics (correlations for instance) reported in the text did you average the hi-freq observation the 3-hourly model output freq or did you extracted the steps corresponding to the model output 3)squeeze as much as possible

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Y axis to the actual range of values.

Fig.5 and related text: How do simulated profiles are extracted for comparison with CTD? Are they interpolated to match the exact CTD location from the N surrounding meshes or whatelse? Please explain it in methods section.

p 1639 line 14 and following: The -2 +2 range of uncertainty for MODIS measurements it is definitely unrealistic for your area and period. As you correctly point out "the skin SST can be significantly different from the bulk SST especially under weak winds and high amounts of incoming sunlight", which means substantially during summer. In facts in this period skin and bulk SST show the largest differences. Even in the original figure of Cervone it is clear that the largest part of values are inside -1+1 range and that larger differences are referred to SST values quite large. Further they are in a subtropical context and for supposedly large values of solar irradiation. I would suggest to remove or rephrase the entire period referring to the MODIS uncertainty based only on Cervone and also refer to other studies, for instance Castro et al 2003 (<http://onlinelibrary.wiley.com/doi/10.1029/2002JC001641/full>) and Schluesel et al (1990) (<http://onlinelibrary.wiley.com/doi/10.1029/JC095iC08p13341/abstract>) even if not specifically devoted to modis but more in general to skin-bulk differences. Finally, I'm quite confident that in your specific case (period/area) MODIS observations represent a really good proxy even for bulk SST, let's say within a range of few decimal points.

section 3.2 and Fig.7: It is not completely clear why did you insert only from this point the comparison with nonhydrostatic MIT, while it is not present and even not commented in the validation section. Did you observed no differences in the validation between the two MIT implementation? As you pointed out in the Intro some Author observed changes in horizontal patterns when using nonhydro. It would be interesting to see if some temperature structure as seen from modis is differently matched by the non-hydro, if it is the case. If there are no difference between the two MITs in validation terms, I would suggest to remove nonhydro subpanels even from Fig. 7 where I can

see very small differences between MIThydro and MITnonhydro (You could just cite it in text as "not shown"). Further in the present form tick and colorbar labels, axis titles and other texts in figure 7 are hardly readable. As you have 3 subpanels in the same row you have to increase the size of the figure's font.

Fig.8: same problem as previous fig. Font size is too small for the present size of the panels. In this case even the features inside the figs can be hardly distinguished. You should split Figure8 a-b in two separate figures (8 and 9) with two captions. And/or remove MITnonhydro panels, even if in this case differences between the two do exist (but as you state in conclusions not so relevant for the aim of the study concerning with DWF). In any case, please adopt some solution to make fonts and structures visible.

Fig 9 and related text: change "total heat flux" to "net heat flux".

Fig.10 and related text: - in the text you spoke of "ekman transport" (which with no specification is assumed horizontal) while in caption you refer to "ekman wind curl". Please change to Ekman pumping or Ekman vertical velocity if you are referring to this process. - Only in the discussion (line 3 page 1646) you explain that "The two models apply different formulations in treating the wind stress, inducing slightly different Ekman transports". This explain correctly the difference, but at THIS point of the paper it is not clear at all how did you computed ekman reason why the two estimates are slightly different. Please state here clearly that you used analytical solution for computing ekman using directly wind stress estimates coming from the two models, which in turn use different wind stress formulations. - pag 1643 l 27: rephrase " Ekman transport simulated in the two models" to "Ekman vertical velocity estimated from the two different wind stress formulations used in the two models" or something like that.

Fig.11: Once again, subpanels, features and fonts are too small. Rearrange subpanels (2 per row).

DISCUSSION -please devote some line to discuss the potential effect of the inzializa-tion which is temporally quite close to the event of interest. Do the models have time

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enough to develop their own dynamics even in the "deep" layers? Did you perform any sensitivity test about the "distance" of the initialization from the event?

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