The authors are grateful for the careful review received and try here to answer completely the comments of the reviewer. In the following the authors' reply (AR) to reviewer's comments (RC)

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

Received and published: 26 August 2015

RC: 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.

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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 observations, finding an acceptable agreement of model ouputs 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.

AR: The authors agree with the reviewer on the fact that the surface fluxes reproduction is a central point in order to provide a realistic reproduction of the DWF event. For sure using the same approach in simulating the surface heat fluxes would have allowed the construction of a paper more focused on the structural differences of the two models. On the other hand, in this work the main goal was not to test new numerical tools but to state the differences in the state of the art of the two models, clarifying the effects due to different approaches. However this point on the choice of heat fluxes identifies an important aspect that deserves specific investigation in future papers (as surface heat fluxes simulation or turbulence formulation) .

In the following we list our answers to specific comments.

RC: 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.

AR: The authors would like to explain that, due to the use of state of art grids on the study area, the bathymetric information used for the two different models have different origins. The bathymetry used in the SHYFEM implementation is a merge of data from the NURC dataset provided within the ADRIA 02 framework and field campaigns done by ISMAR-CNR within the last 15 years in the area in front of the Venice Lagoon. The bathymetry used by MITgcm is from GNOO (National Group of Operational Oceanography) dataset. The choice to keep the bathymetries used in previous implementations by the two models is to state the difference in state of the art implementations.

Moreover the authors want to stress the fact that, even if the same bathymetric information is used, differences in the implemented bathymetries would arise, just as a result of the interpolation process connected with the use of different grids, which would in turn affect in any case the models' results.

The missing information on the bathymetries used will be introduced in the text.

New text, lines 106-110 new manuscript "The bathymetry used is a merge of data from the NURC dataset provided within the ADRIA 02 framework and field campaigns done by ISMAR-CNR within the last 15 years in the area in front of the Venice Lagoon."

New text, lines 135-136 new manuscript: "The bathymetry used by MITgcm is provided by the National Group of Operational Oceanography(GNOO, http://gnoo.bo.ingv.it/bathymetry/)."

RC -What's the atmospheric model providing forcing fields? What's its time and space resolution? Are analyses or fcst?

AR: Surface forcings (wind speed and direction, air temperature, relative humidity and cloud cover) are provided by means of hourly meteorological forecasts from MOLOCH model (Malguzzi et al, 2006; Ferrarin et al, 2013). The MOLOCH model is a non-hydrostatic atmospheric model running on a horizontal grid with 2.3 km resolution and 54 vertical layers, developed and run at the ISAC-CNR, Bologna, Italy (Drofa and Malguzzi, 2004). The atmospheric model allows the investigation of the effects of local and highly variable atmospheric processes in the coastal area, due to its high resolution.

Information about the surface forcings will be added in the text.

New text, lines 164-170 new manuscript: "Output fields and diagnostics are produced every three simulated hours. Surface forcings (wind speed and direction, air temperature, relative humidity and cloud cover) are provided by means of hourly meteorological forecasts from MOLOCH model (Malguzzi et al , 2006; Ferrarin et al , 2013). The MOLOCH model is a non-hydrostatic atmospheric model running on a horizontal grid with 2.3 km resolution and 54 vertical layers, developed and run at the ISAC-CNR, Bologna, Italy (Drofa and Malguzzi , 2004)."

RC -Do you use AFS fcsts hindcast or analyses for the BC? Please state it in the text

AR: The AFS fields used are forecasts. It will be explicitly added in the text.

New text, lines 171-174 new manuscript: "Temperature and salinity are initialized, interpolating 3D values on the two grids, and forced at the open boundary at the Otranto Strait, from AFS (Adriatic Forecasting

System) data. AFS data are forecasts provided with 2 km horizontal resolution, 3-dimensional fields on a sigma level system, daily means"

RC - p 1626 line 3 -"involve" in place of "involves"
AR: Corrected

RC- 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.

AR: We have now reorganized the plots separating the plots of surface and bottom temperature into different panels in order to make the graphs more distinguishable. New figures will be introduced in the text.

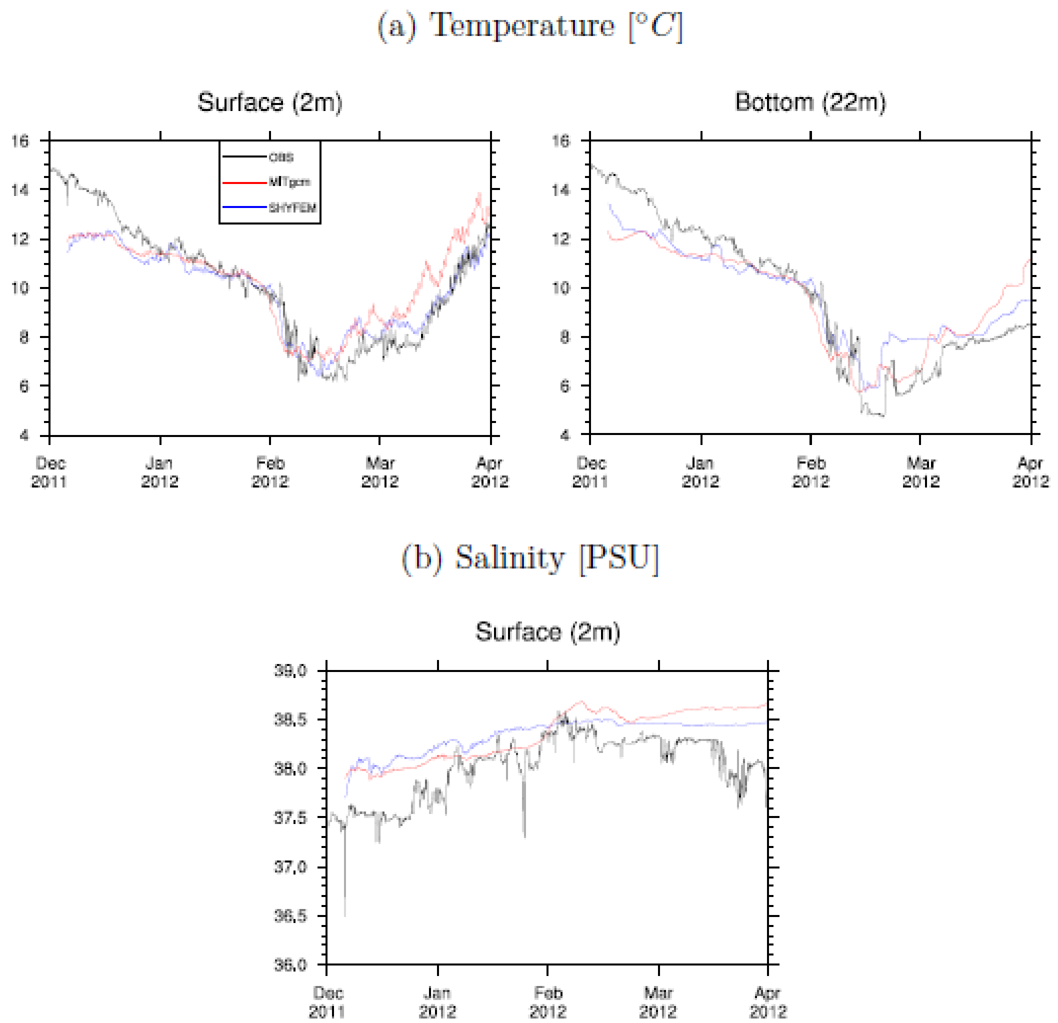

Figure 3. Timeseries of (a) surface (left panel) and bottom (right panel) temperature and (b) surface salinity for SHYFEM (blue), MITgcm (red) and the Vida buoy observations (black).

RC- do the salinity and temperature are shown at the same time frequency as they are acquired?

AR: The timeseries are shown sub-sampling the dataset every 6 hours in order to make the images more readable.

RC- 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

AR: The presented statistics are computed on the 6 hours time series as presented in fig 3 and 4, extracting the measured steps corresponding to the model output. The authors think that this time step is enough resolved temporally to identify the processes that are the focus of the paper.

RC- squeeze as much as possible Y axis to the actual range of values.

AR: Done.

RC- 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.

AR: The CTD profiles are extracted from the closest modeled nodes/cells for the two model.

The authors will add further explanation within the text in the Methods section.

New text, lines 2002-2002 new manuscript:" For a direct comparison with the CTD transects, modelled data profiles are extracted from the nodes/cells closest to the measured data."

RC- 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 Schluessel 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.

AR: The authors agree with the reviewer, the case study conditions were different from the ones discussed in Cervone (2013) and they were not "under weak winds and high amounts of incoming sunlight". However, referring to Donlon et al. (2002), surface thermal stratification can induce differences of some degrees between the skin and the bulk temperatures. In the western Adriatic Sea shelf, where the majority of river discharges occurs, the buoyancy flux due to river runoff at the sea surface causes a significant increase of the difference between the skin and the bulk temperatures.

As not mentioned previously in the text, we want to stress how also water turbidity due to river runoff can affect the SST: a modeling implementation in the Black Sea, done by Kara et al. (2005) demonstrated that high turbidity affects the depth corresponding to solar radiation extinction and consequently the calculation of SST. Kara et al. (2005) demonstrated that using a clear-water constant attenuation depth assumption (as done also in the modeling work here proposed), as opposed to turbid water type values in the modeling implementation, produced monthly SST biases as large as 2°C in the winter period in the Black Sea. Not being possible to apply different values of depth corresponding to solar radiation extinction, based on the presence of sediments (dynamics not simulated in the models), we had to take into account a possible bias in simulating SST close to river inputs of 2°C.

New text to clarify this point will be introduced in the paper.

New text, lines 340-360 new manuscript: “To correctly interpret the outcomes from the model-satellite comparison, we should highlight that there are several factors which might affect the performance of SST-satellite derived results. Satellite derived SST is the skin layer temperature and it provides information on only a few microns of the sea surface. SST measured by buoys or derived by models are generally collected at depths from 0.5 to 5m below the sea surface. These SSTs are called bulk SSTs. Therefore, the skin SST can be significantly different from the bulk SST. Referring to Donlon et al (2002), surface thermal stratification can induce differences of some degrees between the skin and the bulk temperatures. In the western Adriatic Sea shelf, where the majority of river discharges occurs, the buoyancy flux due to river runoff at the sea surface causes a significant increase of the difference between the skin and the bulk temperatures. In addition, spatial variations in the near-coast surface winds might induce different levels of heating in different areas and generate spatial gradients in SST (Otero et al , 2009). It has to be stressed that also water turbidity due to river runoff can affect the SST: a modeling implementation in the Black Sea, done by Kara et al (2004) and Kara et al (2005) demonstrated that high turbidity affects the depth corresponding to solar radiation extinction and consequently the calculation of SST. Kara et al (2005) demonstrated that using a clear-water constant attenuation depth assumption (as done also in the modeling work here proposed), as opposed to turbid water type values in the modeling implementation, produced monthly SST biases as large as 2°C in the winter period in the Black Sea. Not being possible to apply different values of depth corresponding to solar radiation extinction, based on the presence of sediments (dynamics not simulated in the models), we had to take into account a possible bias in simulating SST close to river inputs of 2 °C .”

RC-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.

AR: The authors understand the reviewer comment and here want to explain our choice not to show the NH results in the validation phase. The idea was to allow the validation of the two different models using the same physics (both introducing the hydrostatic approximation), in order to be able to provide an overall assessment on the relative accuracy of the two models. Then after this, in order to deepen the knowledge about the models performances, to add the NH run, just for the model that has this option, the MITgcm. In fact, the HY and NH runs for MITgcm do not differ greatly in the specific points considered for the validation (Vida Buoy and AA Platform). The differences between the two runs are more identifiable considering wider areas, therefore, we think that Fig.7 can add more information on this aspect. The choice to show non hydrostatic results for the North Adriatic, as an average, should help to identify whether on the sub basin scale, more than on specific areas, the effect of vertical acceleration properly reproduced can be detected. Even if differences are small, some aspects arose, therefore the authors prefer to keep this approach and new, more readable images are proposed.

RC- 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.

AR: We have re-organized the panels to maximize the figure size and increase the fonts. Fig.8 was split in two different figures (8 and 9), for a better readability of the images. New images will be introduced in the text. As an example here is a screenshot. The original images have higher resolution.

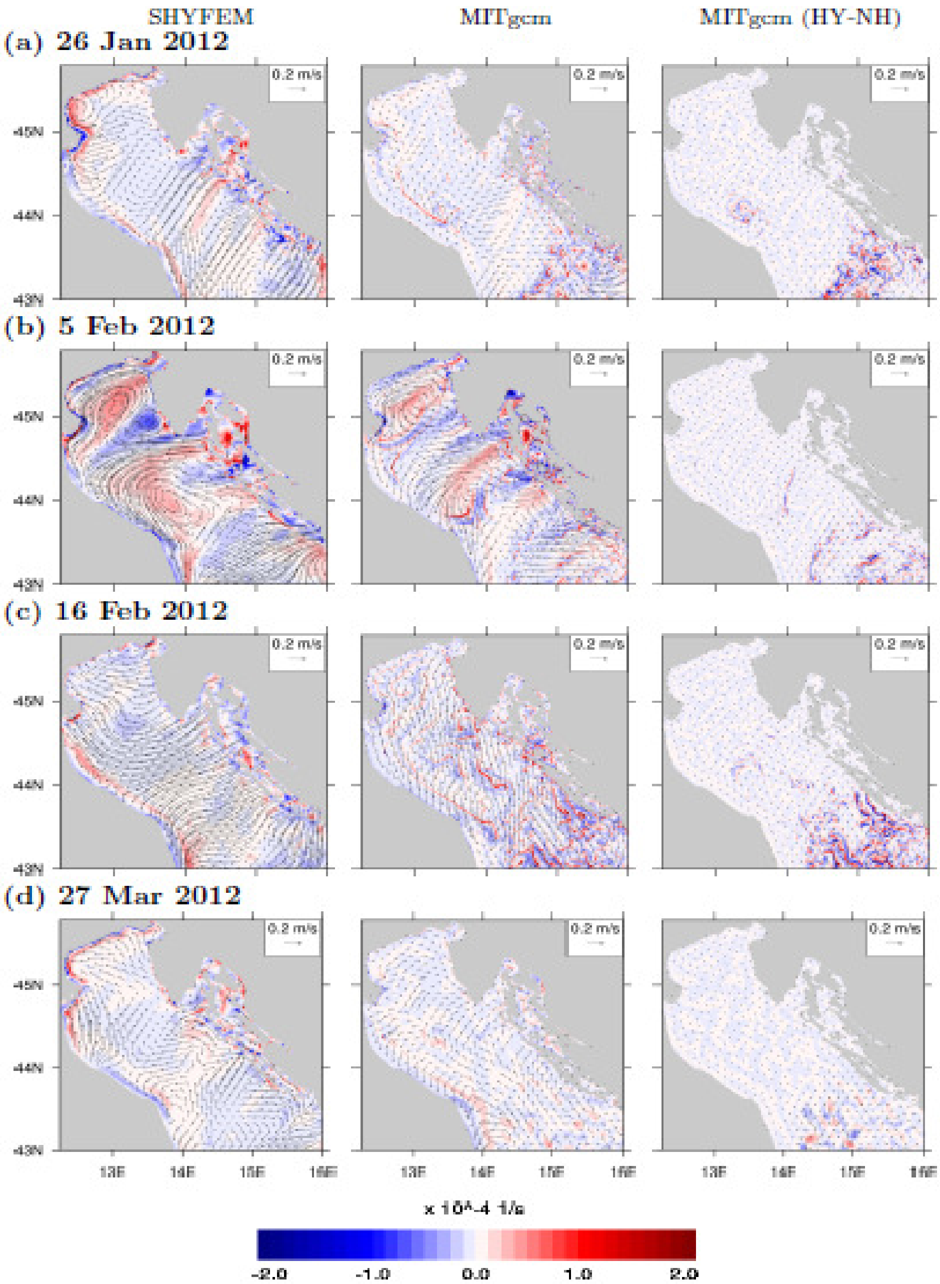

Figure 8. Maps of surface vorticity with surface current overlaid for SHYFEM (left panel) and MITgcm (central panel), in the hydrostatic implementation, and differences in vorticity between MITgcm hydrostatic and nonhydrostatic implementations (HY-NH, right panel), for the dates indicated.

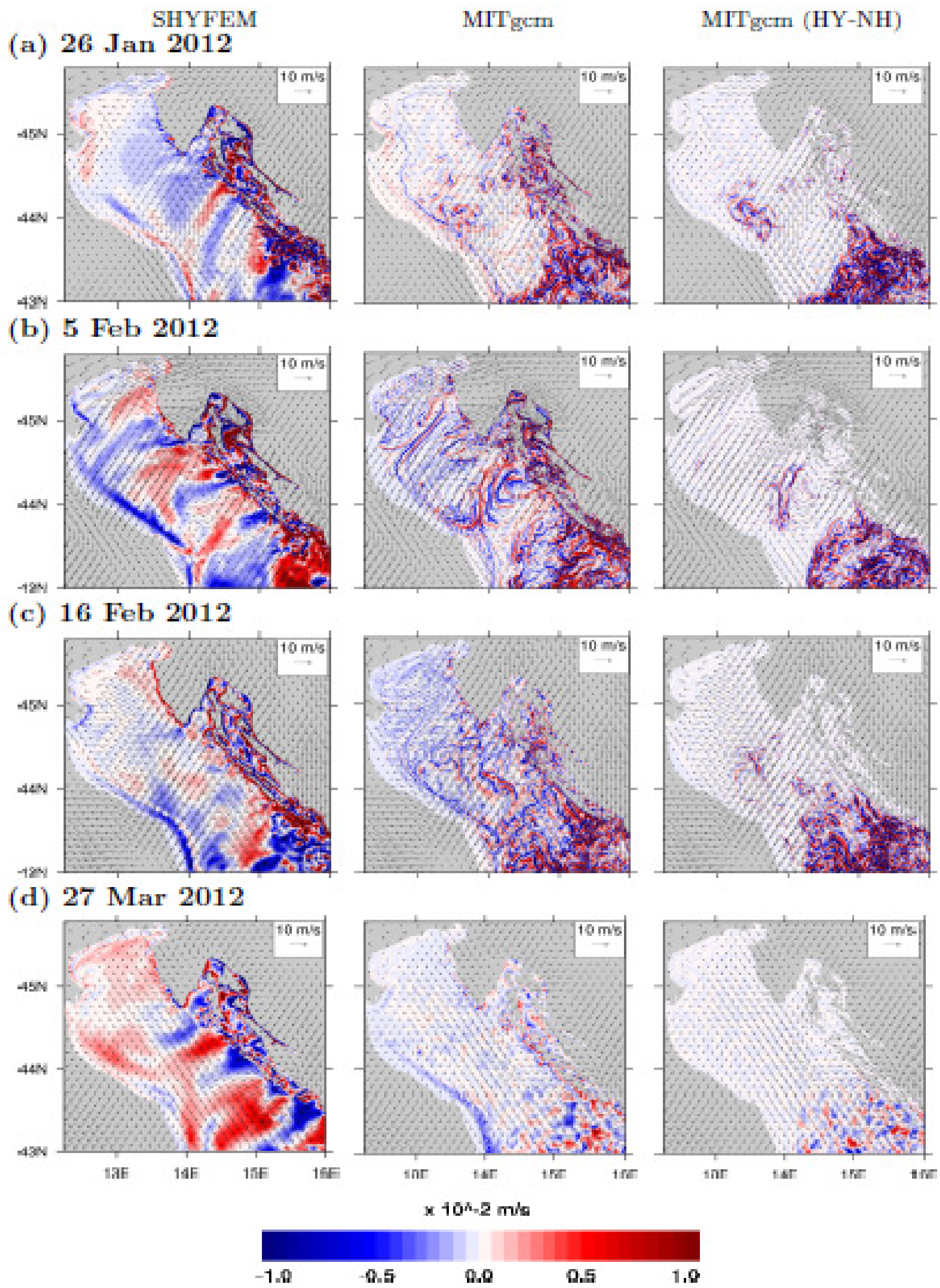

Figure 9. Maps of net vertical velocity with wind vectors overlaid for SHYFEM (left panel) and MITgcm (central panel), in the hydrostatic implementation, and differences in net vertical velocity between MITgcm hydrostatic and nonhydrostatic implementations (HY-NH, right panel), for the dates indicated. Red and blue colors in the net vertical velocity maps indicate upward and downward motion.

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

AR: The text related to Fig.9 (from now Fig.10) has been changed according to the reviewer comment.

RC- 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.

AR: We agree on the references to “Ekman transport” and have now changed this in both instances in this paragraph as to avoid confusion. The changes are introduced in the text.

New text, lines 460-469 new manuscript:” This upwelling is the result of the net Ekman suction induced by the Bora wind while there is a general DW sinking in the rest of the Gulf of Trieste. This is evident in Fig. 11b, where we show the comparison between the daily maximum Ekman induced vertical velocity (estimated from the two different wind-stress formulations used in the two models) and the daily maximum net vertical velocity. Both models show an Ekman induced upwelling during the DW event, though the magnitude of the vertical velocity in SHYFEM are more comparable to the Ekman values, whereas MITgcm shows a lower net vertical velocity in the same period. The small differences in the Ekman velocities computed by the two models are connected with the different formulation of wind drag coefficients, though overall they are very similar.

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

AR: The Fig. 11 (from now Fig.12) has been rearranged accordingly.

As an example here is a screenshot. The original image has higher resolution.

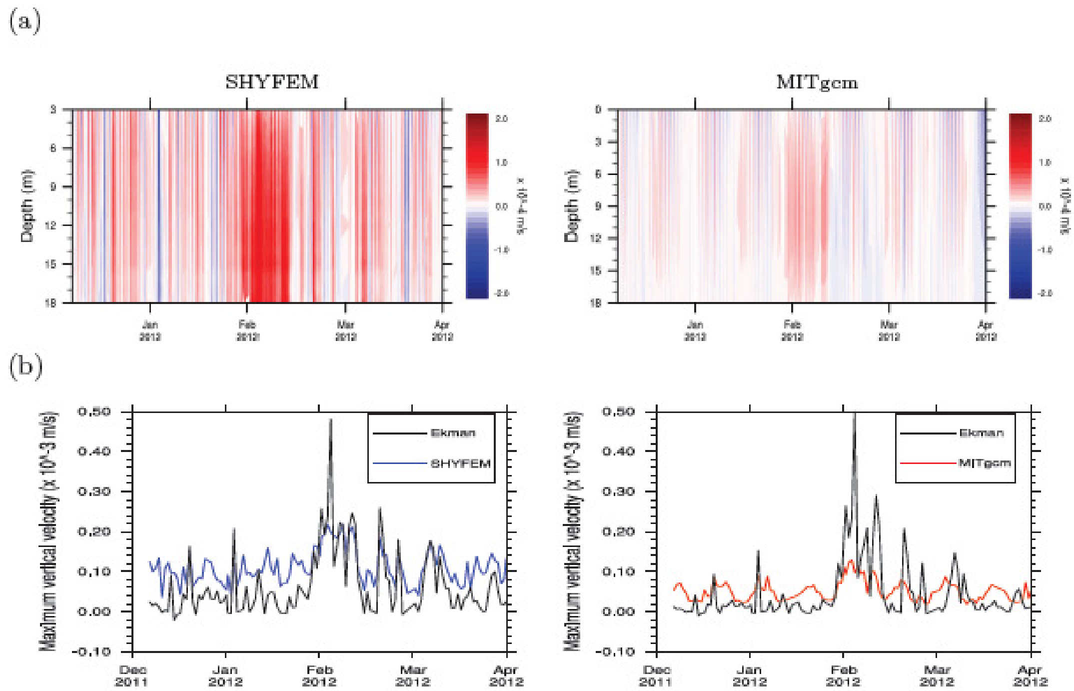

Figure 11. (a) Timeseries of depth profiles of vertical velocity averaged over the coastal area of the Gulf of Trieste, with depth lower than 18 m, and (b) timeseries of maximum daily Ekman wind curl and daily maximum of net vertical velocity for SHYFEM and MITgcm.

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

AR: Previous tests were performed in order to identify the proper spin up time for the implementation, in order to avoid the influence of initial conditions. The well mixed thermo-haline structure characterizing the North Adriatic basin, where the deep water formation take place, during winter time, justifies just one month of spin up. This aspect has been tested by both models by means of a sensitivity analysis (not shown).

This information will be added in the text.

New text, lines 499-502 new manuscript: “The well mixed thermo-haline structure characterizing the North Adriatic basin, where the deep water formation takes place during winter time, justify just one month of spin up. This aspect has been tested by both models by means of a sensitivity analysis (not shown).”

The authors wish to thank the referee for the careful review received. We considered the suggestions proposed by the reviewer and in the following we try to address all the comments made, stressing, on one hand, the analysis limits due to the lack of measured data, on the other, the possible outcomes that make us confident about the results shown. In the following authors’ reply (AR) to reviewer’s comments (RC).

Anonymous Referee #2

Received and published: 28 September 2015

RC - 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.

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This paper describes a model intercomparison for the Adriatic Sea. The authors present the results of two regional model simulations. The presented skill assessment is only partially useful as no velocity comparison is provided. A more complete, extensive, and quantitative assessment is suggested. However, the paper provides interesting results that could help understand the dominant processes in the formation of dense water in the region.

AR: the authors agree with the reviewer about the need of large and useful measured dataset in order to provide a complete picture of the studied processes, particularly concerning vertical velocity. However, the lack of these data should not suggest to avoid trying to infer some physically relevant aspects about vertical processes and dense water formation, but push the authors towards handling other available datasets (first of all temperature and salinity) as evidence of aspects directly connected with vertical motion.

RC- Major points:

It seems odd that the wind stress formulations are different for both models. Not only wind stress, but also parts of the heat flux computation are going to be different and results like the ones in Figure 9 could be affected. You are introducing differences in the model behavior even at the forcing stage. Please evaluate the resulting difference in forcing. Also, why don’t you conduct the simulations with the two models in similar horizontal resolution? While I understand the benefit of the finite element approach for avoiding excessive resolution in the deeper areas, the process you are trying to characterize is occurring in regions where the horizontal resolution of the FE grid might not be sufficient.

AR: the authors understand the referees concerns on the heat fluxes as well as the wind stress differences. This comment was proposed also by the first referee and the authors agree on the need to clarify the approach adopted. In the paper we are working with state of the art models, already applied in the past in the study area. Therefore a major interest is in identifying the models' skills in their state of the art configuration. We have stated in the text the differences in the approaches of the two models. For sure using the same approach in simulating the surface heat fluxes would have better clarified the structural differences of the two models. On the other hand, in this work the main goal was not to test new numerical tools but to state the differences in the state of the art of the two models, trying to distinguish the effects due to different approaches. However the referee's point on the choice of heat fluxes identifies an important issue that deserves specific investigation in future papers.

On the resolution issue, we should point out that in the Northern shelf of the Adriatic the finite element grid of SHYFEM is highly resolved, particularly along the coast, and though, generally, it is not as well resolved as the MITgcm model, in fact the two models are both able to capture the dense water event, when compared with the available observations. This is one of the key findings in the paper: it appears that resolving the coastal shelf is crucial for reproducing this process.

RC- The model solutions are only assessed against temperature and salinity observations. The fact that no velocity observations were available (or used) makes parts of the analysis questionable. As it stands, the paper seems like a model intercomparison. The vertical velocity, being such a fine scale result, requires the horizontal flow to be properly characterized. Without appropriate assessment, the vertical estimates seem an exercise in model behavior, rather than a characterization of the vertical velocities during dense water formation events. While the title of your paper is “Investigation of model capability in capturing vertical hydrodynamic coastal processes”, the results presented do not answer whether the model is capable of capturing vertical motions in a realistic way.

AR: for sure the paper is focused on modeling skills in reproducing specific coastal processes, connected with the dense water event of 2012. Therefore, it is clear that a part of the work is devoted to the models' comparison. However, the final goal is to assess evidences on the spatial and temporal evolution of the processes, considering the typical scales involved. All the available data were used for the models' validation but, as happens also for other studies, datasets containing vertical velocity data were not available. Ideally, we would like to have observations of the real vertical velocity fields for this period in order to really measure the degree of realism of the model velocities. However, failing this, we believe that the comparison of the model vertical velocities is still of interest to understand the differences in the model physics, that manifestly impact on the other components of the system, such as the temperature and salinity, which we can compare with observations.

RC- Have you consider comparing the vertical velocity from the two models with results from observations? While direct vertical velocity measurements are lacking, I suggest considering indirect estimates such as the Klein et al. (2009) formulation. References: Klein, P., J. Isern-Fontanet, G. Lapeyre, G. Roullet, E. Danioux, B. Chapron, S. Le Gentil, and H. Sasaki (2009), Diagnosis of vertical velocities in the upper ocean from high resolution sea surface height, Geophys. Res. Lett., 36, L12603, doi:10.1029/2009GL038359.

AR: this is an interesting approach for estimating the vertical velocity, but requires spatially distributed field measurements of the sea-surface height, as well as knowledge of the typical stratification – both of which are lacking in our case particularly during the dense water event itself. Even if the approach is interesting, the authors have the doubt that, given the assumptions defined for the vertical velocity reconstruction in Klein et al. (2009), this approach would be misleading for the present study. Also, we doubt that the approach is applicable in our case given that the difference in the spatial scales dealt with in our paper (the sub-meso scale, while in Klein et al.(2009) the method seems more referred to the mesoscale). Moreover the case we are investigating represents unstable stratification during the dense water event. The algorithm described in Klein et al. (2009) needs to provide information about the stratification characteristics, that is precisely investigated (and not assumed) in this work. Our condition seems far from the assumption of Klein et al.(2009) on typical climatological surface stratification.

Definitely future work dedicated to obtaining accurate assessments of vertical motions is badly needed for gaining greater insight into the physics required in models, but we are forced not to implement this technique in the present paper.