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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.

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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

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. This

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Interactive Discussion

Discussion Paper

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

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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

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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

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are forced not to implement this technique in the present paper.

Interactive comment on Ocean Sci. Discuss., 12, 1625, 2015.