EGUsphere, referee comment RC1
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Comment on egusphere-2022-1274

Anonymous Referee #1

Referee comment on "Multi-model analysis of the Adriatic dense-water dynamics" by Petra Pranić et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1274-RC1, 2023

The MS presents a helpful analysis of the formation and collection of dense water in the Norther Adriatic during the bora (cold strong winds) events, and some insight into the transport of the newly formed dense bottom water to a greater depth. The authors post-processed previously obtained outputs form four models and calculated basic statistics in order to validate the models against archived observations in relation to the cascading process. They also used a combination of model outputs and observation to analyse the physics of bora-driven cascading.

The structure of the MS is slightly misleading. The Method section describes in much detail the methodology used in previous research, such as model set ups and parameters, which may give a false impression that the models were configured and run as part of this particular study. At the same time, the methods used in the present study are described only embryonically. The MS could be published after major revision to address the comments in this report.

Specific comments

The transport of dense water is the key element of dense water cascades. In the abstract, the authors claim 'Finally, we analyse in detail the numerical reproduction of the dense water dynamics as seen by the four simulations.' However, the dense water transport is described only qualitatively in the main text, and the assessment of the transport only appears in the appendix as 'additional information'. The transport should be given much greater focus.

Lines 18-20 'This study confirms that … are prerequisites for appropriate modelling of the ocean circulation in the Adriatic basin' . This is an overstatement. The study only reveals that one of the four models represents better some variables while other models are better in some other aspects. The paper does not proof that the named parameters are

prerequisites for future research. Please re-word.

Introduction. The authors concentrate entirely on the Adriatic Sea. In order to make the results helpful for a wider oceanographic community the authors are advised to place the bora-driven cascading in a broader context. For example to compare briefly with cascades from other shelves, e.g. as discussed in (Ivanov et al, 2003, https://doi.org/10.1016/j.pocean.2003.12.002; Garciaâ□□Quintana et al, 2021. https://doi.org/10.1029/2020JC016951)

Lines 69-70. ' ..the river climatology used in previous studies … has been replaced by a new climatology'. Please clarify, what is the difference between 'old' and 'new' and give a reference. It seems from the text in Line 71 that the 'new' climatology was used in some 'previous studies'.

Lines 78-79. 'the most advanced variational scheme, the Four-Dimensional Variational…'. Which of the many versions and sub-versions of DA schemes is 'the most advanced' is a matter of discussion. Please re-word.

Line 81. 'a 31-year evaluation simulation' . Please define what is 'evaluation simulation'. Was it run with or without DA?

Line 85 'the newest reanalysis product for the Mediterranean Sea' . Please give a reference.

Line 95 and Line 98 '…between late autumn 2014 and summer 2015' . '…between late November 2014 and mid-August 2015' . Please exact dates as you do in Line 100.

Line 109. '..of various types of models'. This statement is too wide. Based on the results presented in the MS, the authors could only assess, contrast and compare the specific models they used, not the 'types of models'.

Line 113.' reanalysis product for the Mediterranean Sea'. Please give the exact product ID. Is it available from CMEMS catalogue? Is it MEDSEA_MULTIYEAR_PHY_006_004 ? If yes, please use the name for product given by the originators, namely 'Med MFC' to avoid confusion. This product is generated not just by NEMO as stated in Lines 115-16 but 'The Med MFC physical reanalysis product is generated by a numerical system composed of an hydrodynamic model, supplied by the Nucleous for European Modelling of the Ocean (NEMO) and a variational data assimilation scheme (OceanVAR)' (https://data.marine.copernicus.eu/product/MEDSEA_MULTIYEAR_PHY_006_004/description)

Lines 113-200. The description of models from previous studies is too extensive, it should be reduced in size and moved from Material and methods to Introduction. Material and methods should present methodology used specifically for this study and in much greater detail than it is now.

Line 137. Please give a reference to the ALADIN/HR atmospheric model.

Lines 143-144. 'In this model, the horizontal ROMS grid resolution is 2 km and there are 20 vertically spaced sigma levels controlled by the following parameters …'. Ocean model results are strongly dependent on the vertical resolution of a model. This is hinted by the authors in Lines 622-623 ' In addition, the lack of vertical resolution in the ROMS-full model probably contributes to the improper representation of the dense water dynamics'. Has the sensitivity study been performed by the authors or other researchers? The authors attempt to compare 'the types of the models' however the skill of the same model can change significantly when model governing parameters or boundary conditions (e.g. river discharge) are changed. The authors should demonstrate that parameters of the models used for comparison provide the best results within the limitation of the specific 'type' of model in order to suggest which 'type' of the model is the best.

Lines 201-206. Have you noticed the 'double penalty effect' (see e.g. https://doi.org/10.5194/os-16-831-2020) in the higher resolution models? If yes, how it impacts on the final results?

Line 202-203. 'In order to compare different simulations, model results with grid resolution coarser than 1 km are interpolated to the AdriSCROMS 1 km grid…' .Does it mean that all model outputs were also interpolated in the vertical to the AdriSCROMS sigma-coordinate grid with 35 levels? Please clarify.

Line 204-206' For the ocean simulations, MEDSEA, ROMS-hind and ROMS-full results are regridded to 1 km resolution, while for the atmosphere, ERA5, ALADIN/HR-hind, ALADIN/HR-full and AdriSC-WRF results are all regridded to 1 km resolution.' The use of the word 'while' is strange as all model outputs were regridded to the same scale. Please re-word.

Line 214-215. ' probability density functions of the biases (i.e., differences) between the results of the simulations and the in-situ temperature and salinity observations' . This is the core component of methodology and it has to be described in much more detail. How the bias is calculated? If it is the average of all differences at all locations and all times then it would be impossible to calculate the PDF. Or is it an average of daily differences? Or something else?

Line 218. 'The probability density functions are obtained with a kernel-smoothing

method..' Which kernel was used? What was the size of the smoothing window? The results may be sensitive to these factors. Please give more details here (including a reference) as otherwise your results cannot be replicated. An estimate of the sensitivity of results to the size of the smoothing window will be helpful.

Line 223. 'minimum turbulent heat fluxes in the atmosphere'. Do you mean 'minimum downward turbulent heat fluxes in the atmosphere at the sea surface' Please clarify.

Line 230. 'bottom PDA time series is presented without the seasonal signal which is removed from the series using the least-squares method.' Please give more details of how PDAs are calculated otherwise the statement in Line 232 'the time evolution of the spatial distributions of the bottom PDAs' is difficult to comprehend.

Lines 233-234. 'An additional analysis (only presented and commented in Supplementary Material) quantifies the total daily volume transport of the outflowing dense waters..' The near-bottom transport of dense waters is a major parameter quantifying the intensity of dense water cascades. Therefore it has to be included in the main text (both results and discussion) in sufficient detail.

Lines 238-248. The validation of the four models against CTD casts is very helpful. In order to help a reader to interpret the figures given in this section, the methods of calculating biases given in the previous section have to be presented in much more detail. It is advisable to extend the basic stats (mean and standard deviation) to include more advanced tools of model validation, e.g. Pearson correlation, Willmott skill parameter or Taylor diagram.

Lines 238-255. The four models have different resolutions and some of them may not resolve the processes of the scale of the baroclinic Rossby radius. Please provide a map of Rossby radius for your area. You may wish to use a simplified method presented in Chelton et al . 1998: Geographical variability of the first-baroclinic Rossby radius of deformation. J. Phys. Oceanogr., 28, 433-460.

Lines 301-302. 'Overall, for all models, maximums of wind stresses are associated with bora events, while downward turbulent heat fluxes seem to be more influenced by the seasonal variations of the sea surface temperature' . Please clarify the second part of this statement. From the qualitative point of view, stronger and colder winds (the bora) should have a greater influence on downward heat fluxes from the atmosphere to the ocean than smooth and therefore weaker seasonal variations.

Lines 345-416. The presentation of results is mostly concentrated on the atmosphere-ocean heat fluxes and the processes of formation of dense water. However, in contrast to CTD observations the models provide an opportunity to calculate dense water transport,

which is a key component of cascading. This sections gives a good quantitative description of heat fluxes, while it describes the transports only qualitatively. This omission has to be rectified.