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Comment on esd-2021-37

Anonymous Referee #2

Referee comment on "Climate change signal in the ocean circulation of the Tyrrhenian Sea" by Alba de la Vara et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-37-RC2, 2021

Review of the manuscript "Climate change signal in the ocean circulation of the Tyrrhenian Sea", by Alba de la Vara et al.

This work is devoted to the analysis of the future evolution of the circulation in the Tyrrhenian Sea (TS) under the pessimistic RCP 8.5 emission scenario. Using the outputs of the regional atmospheric-ocean coupled model ROM, the authors study the projected changes in the seasonal circulation patterns and the exchange through the main straits connecting the TS with the rest of the Western Mediterranean: the Sardinia Strait (SS) and the Corsica Channel (CS). The results show a weakening of the TS main cyclonic circulation and an enhancement of the mesoscale structures of this sub-basin. Authors attribute this changes to a reduced inflow of Modified Atlantic Waters across the SS and to an increase of the kinetic energy transferred by the wind. In addition, the results also show an increase of the water column stratification and a reduction in the water transport to the Liguro-Provençal basoin across the CS, which may impact the deep water formation process at the Gulf of Lions.

For my review, I'll take advantage of the journal's format that allows me to read the comments of my fellow reviewer. I agree with him/her in the interest of this work, but I'm also surprised that the authors have chosen the TS as area of study. Given the potential of the model to reproduce complex dynamics processes in the whole Mediterranean, I coincide with reviewer #1's (R1) opinion that extending the region analyzed to, at least, include key processes like the deep water formation would have substantially increased the impact of the paper. Clarifying the reasons of this choice is important in my view.

Nonetheless, the results obtained are relevant and well presented, and the conclusions are well-argued. However, I have some concerns about both formal and content aspects of the current MS.

Formal aspects:

My main recommendation here is that the differences between the future and present climates would be much easier to understand if they are directly represented in the figures. Instead of only plotting the average for the present and future, if you include a panel with their differences with a positive-negative color bar the reader will be able to rapidly identify the regions where the different magnitudes increase/decrease. I

understand that this is tricky for the velocity vectors, but for the scalar magnitudes it would be of great help.

Contents:

Model set-up: In general, I agree with R1 that the description of the model should be extended, more considering that one of the key results strongly depends on the modification of the MAW properties. It is important to understand how the Atlantic Waters are imported through the Strait of Gibraltar and which are the conditioning factors for their lower salinity. For instance, is the salinity of the GCM used as boundary condition for ROM, MPI-ESM-MR, driving the properties of the inflowing waters?

Validation: Following my previous comment, Parras-Berrocal et al. (2020) show that for a hindcast of ROM forced by ERA-Interin in the present climate the average SSS in the Mediterranean is between 1 and 2 psu higher than MPI-ESM. This result should be commented here or in the previous section to contextualize the results. It means that, for the present climate, the GCM could be underestimating the salinity. This doesn't necessarily mean that the projected freshening of the surface layer is wrong, but it is an important information in order to interpret the results. Comparing with the SSS projected by other regional models of the Mediterranean, particularly if they are forced by different GCMs, would also give more context to the results.

In the validation of the geostrophic currents using AVISO altimetry data, I also agree with R1 that the results shown in figure 2 are not as conclusive as the authors claim, and that the comparison should be made using the same periods for the average. Here a plot showing the difference between model and altimetry data would also be of great help to identify similarities/differences. A quantitative analysis, perhaps computing the spatial correlation in the area of study, would also help.

Another point to be considered is extending the validation to include the seasonal cycle and the interannual variability, not only mean state for winter and summer, and including the SSS (as carried out by Parras-Berrocal et al. (2020) for the SST in the whole Mediterranean). This way if there is a bias in the SSS variability of the simulation in the present it could be identified and considered for the discussion of the results.

Finally, the AVISO interpolated products in the Mediterranean have strong limitations representing mesoscale structures (see Amores et al. 2018, JGR https://doi.org/10.1029/2018JC014140). I would suggest complementing the validation using a reanalysis dataset.

Results, discussion and conclusions: : In my view these sections are complete. Results are clearly explained and argued. My only suggestion is to include some of the referenced results, particularly those related to the previous work of Parral-Berrocal et al. (2020) and to the reduction of the deep water formation in the Gulf of Lions (I agree with R1 that a work in preparation shouldn't be references). Also, in the discussion section, I miss a comparison of the results for the future evolution of the different variables with previous work for the region. Are they consistent with other modelling studies?