

Earth Syst. Dynam. Discuss., referee comment RC1
<https://doi.org/10.5194/esd-2021-37-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on esd-2021-37

Anonymous Referee #1

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-RC1>, 2021

General comment on "Climate change signal in the ocean circulation of the Tyrrhenian Sea" ms by Alba de la Vara et al

The manuscript addresses future changes that could take place in the Tyrrhenian Sea (TS) circulation by the end of the current century under the "business-as-usual" high-emission RCP8.5 scenario. Authors use the regionally-coupled climate model ROM (from REMO-OASIS-MPIOM) to conclude that surface mesoscale patterns in the basin are slightly modified, which authors ascribe to changes in water transport across Sardinia Strait and mechanical energy transfer from wind field to the ocean in the vicinity of Bonifacio Strait. More interesting is the conclusion that the flow from the TS into the Liguro-Provençal basin will weaken and the advected waters will be more stratified, speculating about the possibly hampering of winter deep water formation in the Gulf of Lions by the end of the century.

While the objective of the manuscript is of interest, a question that arises is why authors made up their mind to select the TS instead of the critical areas of intermediate and deep water formation, focusing on these important processes. Such a study would provide a deeper insight on the future evolution of the Mediterranean Sea circulation. I would like to have read a sentence explaining the reasons and the pros of their selection against the other alternatives.

In its present form, there are some points that should be considered and revised or completed. My objections refer to the model set-up, mainly the (boundary?) condition at Gibraltar, and to the validation. The review only addresses these two aspects and it does not include specific comments, technical corrections or typing errors.

MODEL SET-UP. One of the ms main conclusions is the future enhanced stratification in the TS, which is the joint result of the expected SST increase in a warming ocean and the freshening of the surface water. Whereas the SST increase is an undisputed fact, the freshening is not. Obviously, it must be the result of a fresher Atlantic inflow through the Strait of Gibraltar, which has consequences on the whole Mediterranean Sea, not only in the TS. And obviously again, all the results of the modelling (not in the TS uniquely) rely critically on the validity of this boundary condition. Except for a vague sentence on lines

104-105 ("the water exchange at Gibraltar and Dardanelles in ROM is not parametrized and Atlantic water properties are not relaxed towards climatological values in the areas adjacent to the straits"), nothing else is said about this critical point, as if the future freshening of the Atlantic inflow were a proven result or another indisputable fact. Does this result come from MPIOM global model? Authors must be much more explicit about this point, discuss its validity and assess how sensitive their conclusions are against small changes of Atlantic inflow salinity . Similar considerations can be extended to the strength and size of the exchanged flows through the Strait of Gibraltar, which should be notably increased if the density contrast between inflow (warmer and fresher) and outflow increases. Could authors show the ROM forecast for these variables in the "Model setup" section and extend the section by addressing the conditions that hold at the western boundary of the Mediterranean. It is necessary in order to support adequately the conclusions of the paper and to discuss how robust they are against changes of those conditions.

VALIDATION is addressed in a very light way in Section 3 (RESULTS), where it appears in different places along with the interpretation of the model outputs. AVISO geostrophic currents is the reference for validation, which is carried out by comparing the thirty-year period 1976-2005 averaged circulation from ROM with the thirteen-year (1993-2005) period of AVISO. From Figure 2, it is difficult to conclude that both patterns agree satisfactorily, partially because of the poor resolution of the figure. Rather, the agreement seems to be only moderate if not arguable. But, even if it was satisfactory, the question that arises is why different time periods are used to average both data when ROM can use exactly the same period as AVISO. Why this mismatch of periods? Considering that the longer the averaging period the smoother the resulting pattern, the, for instance, disagreement between the mesoscale-rich circulation summer pattern in the TS from AVISO (Figure 3D) and ROM (Figure 3E) could be partially explained. In any case, the right way of comparing results is to make use of identical periods if possible.

Reasons provided in lines 136-140 to focus on the surface geostrophic circulation for validation purposes are strange. Actually, using surface geostrophic circulation and/or sea surface height (SSH) from AVISO is out of necessity: they are the only available variables.

The mention to the in-preparation paper by Parras-Berrocal et al. in order to justify the realistic representation of the main features of the surface circulation (lines 156-157) should be removed. If ROM is successful in doing this, the results should be either shown in this paper or referred to an already published paper, not to an in-preparation work.