

Interactive comment on “Modelling study of transformations of the exchange flows along the Strait of Gibraltar” by Antonio Sanchez-Roman et al.

Anonymous Referee #2

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The manuscript deals with the modeling reproduction of the Gibraltar Strait fluxes exchange. The authors investigate and quantify the vertical transfer of properties between the inflowing and outflowing waters using a velocity criterion, not done before. Moreover one interesting evidence is the establishment of the limited action of mixing processes, compared with vertical advection forces. There is an interesting approach to the characterization of processes in order also to provide tools for a more realistic implementation of coarser models that parameterize the vertical processes tuning mixing. The manuscript jointly investigate the physical processes affecting the water exchange at the Gibraltar Strait and provide suggestion on the use of the main results for operational modeling. The immediate support to a large modeling community is

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evident and despite some doubts on the overall reasoning presented in the paper, it represents an added value for researchers in oceanography.

The paper is linked to a large literature that tackled in the past the same topic, it appropriately quote it, probably sometimes relying on previous evidences a little bit too much (i.e. the reference to Sannino et al 2015 and the results of that paper are mentioned too broadly and there are parts of the text in which the reader is not sure whether some info (on validation, for example) come from the present manuscript or are mentioned from that one. In the discussion section there are two approaches described on how to translate the paper results to improve modeling of the strait for, as example, climate runs. The first one, that link the recirculation flux to net fluxes and provide a relation between them is supported by evidences (fig.9), the other one, just drafted in the last rows of the discussion (from line 9 page 19 on), seem more a speculation and some doubts on the opportunity to mention it are raised.

The paper is structured in a coherent way, even if can be slightly improved. It is written in a good English. The number of tables, probably, can be reduced (suggestion: keep in the same table Tab.1 and Tab.4, where some info are repeated). It has to be clarified the level of generality of the 15 days graphs shown in Figs. 3 and 5a because they are discussed mentioning the max-min values but not clarifying if they represent a typical behavior of the 10 simulated years or not. As a general comment, evidences from figures are discussed, not always introducing to the reader what is shown in that figure. Therefore the reader sometimes jump from one figure to the other but is not helped by the text (example Fig.8). An overall check of this aspect should be done throughout the paper. Even if there is the need to improve the clarity of the presentation of the work done, being more systematic in the presentation of methods (runs done, differences in setups), introducing figures before describing their evidences, I consider these more formal than substantial changes, therefore I suggest to consider the paper for publication after minor revision. Specific comments to support this suggestion are listed below.

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Specific Comments:

- Page 3, line 23: since there is an interest in vertical processes, faster than convection, is the choice of a hydrostatic version of the model suitable for investigation? If so, please infer on the added (or not) value of a proper reproduction of the non-hydrostatic component for these specific dynamics. This comment considers that the MITgcm allows also this option, if needed.

- Section 2.1: I would expect in this section just the description of methods and simulation setups. Why not to keep a separate subsection for validation information, probably the first of Results section, instead of mentioning it here, mixed with methodological aspects? It is quite hard to fully understand to what extent the model implementation of Sannino et al., 2015 was the basis of this study, what are the new runs, what are the differences, what was considered validation done in that previous paper and how much is directly validated here. Therefore the request is to dig into the section and try to clarify these points.

- Page 4, line 11. Probably $1/16^\circ$ resolution for the majority of the Mediterranean basin, increasing it in the strait, is sufficient for a correct reproduction of tidal dynamics and, more generally, of circulation. However, I would appreciate a comment, or references to other works dealing with different, variable resolution applications, inferring the effect of resolution on process reproduction.

- Pag. 4, line 14: it is stated that the vertical discretization is in variable thickness layers, from 3 m on the surface, to 300 m at the bottom. Given the specific focus of the present work, the reproduction of bottom layers with 300 m thickness, to reproduce correctly bathymetry and the dynamics linked to the hydraulic control, is appropriate? How is the last layer set? Variable thickness for the last layer in order to reproduce the correct bathymetry? Please spend some words on this aspect.

- Page 4, line 28. There is the mention to Stanev et al., 2000. Is there the possibility to add info on more recent findings connected with the topic, considering, for example the

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work presented by Stanev et al., 2017 (Cascading ocean basins: numerical simulations of the circulation and interbasin exchange in the Azov-Black-Marmara-Mediterranean Seas system- OCEAN DYNAMICS)

- Page 4, from line 31 to the end of section: this part mixes the description of datasets with validation aspects. Does this mean that validation is just mentioned but done in other papers, like for tidal signal in Sannino et al., 2015 or are there aspects directly validated with the new runs (i.e. temperature and salinity)? Going through the paper, am I right saying that three are the runs performed, the first with 3 hourly forcing, the second with monthly mean forcing and a third without tide (the one just mentioned in the discussion and in figure 8 that should be described, as well in the methods, I guess)? Or just the 3 hourly run is done for this paper and the others were accessible from available datasets? Please, help the reader in understanding these points, clarifying and perhaps splitting subsection 2.1.

- Page 5, line 18: why to choose such a not recent period for these simulations? Certainly there is a reason that should be explicitly stated, because the reader would ask why not to consider a recent, well documented by measured data period.

- Page 5, line 26-26 “ however, the vertical ...of the basin” this sentence need to be proved.

- Fig. 3 and 5a: clarify why to choose 15 days, if they are a real period taken from the dataset or an average and what are the tidal condition in that period.

- Page 9 , line 11: 2.5 Sv: is that computed as max value on the period or does it refer to fig.3?

- Page 12, lines 10-17: I would move these sentences in the discussion section. There are also other parts, in the Results section that mix the plain presentation of results with the discussion. It could be fair but this, in my opinion, sometimes affects the clarity of results presentation.

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- Table 4: in Table 1 and 4 there are some repeated info. I would suggest either to merge the two tables or to express the info just once.
- Page 14, line 10: somewhere you should describe what we see in Fig.8, from what simulations, before discussing them.
- Page 15, line 30: the mention and the description of the non-tidal run setup should be added in the methods section.
- Page 17, line 26: net water fluxes from simulation data as well?

Technical Corrections:

- Page 1, line 27: 250 m. is this value correct? Shouldn't be the CS the shallower point of the strait? Is this a typo?
- Page 13, lines 24-27: some problems with this sentence. Any word missing?

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