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Discussions

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

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Detailed response to comments of referee#2:

We thank the reviewer for her/his comments, which have been useful to improve the manuscript. Below we have responded to each of the specific comments, hoping that these clarifications and amendments meet his/her approval.

Reviewer comment: 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.

Response: the simulation runs used in this paper are those described in Sannino et al. (2015). We have better explained this fact in the new version of the manuscript. As a consequence, we refer to that paper when describing the model configuration and the validation of the tidal amplitude and phase done by Sannino et al. (2015). This has been clarified in the manuscript in order to avoid confusion with the validation of the 3D tidal currents performed here and the results obtained (see comment below).

Reviewer comment: 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.

Response: in section 4.3 we first present the empirical relationships based on model outputs that can be used to compute, in a simple way, what should be the transformation of water properties along the Strait. This could be used to assess whether the coarse resolution model is representing in a realistic way the water transformation. However, as those models have not enough spatial resolution is very difficult for them to do that job. The only way is to parameterize somehow that process. Therefore, in the second part of section 4.3 we propose a simple way to "force" the coarse resolution models to transform the water properties in their path along the Strait.

Reviewer comment: 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.

Response: Most of these general comments have been raised in the specific comments so we provide the clarifications and amendments below. Related to the last general comment, we have included an introduction to all the figures in the new version in order to clarify what is shown in each of them.

Reviewer comment: 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.

Response: Sannino et al. (2014) compared two twin simulations reproducing the Strait of Gibraltar dynamics differing only for the hydrostatic/non-hydrostatic formulation. When comparing the two model outputs, it was found that the main difference between the two simulations was the presence of an eastward propagating bore in the non-hydrostatic simulation. On the contrary, no relevant differences were found in the simulated hydraulic control, volume transport and tracers vertical profiles. Considering that the non-hydrostatic simulation increases a lot the computational time (about 8 times), and that not relevant differences were found by Sannino et al. 2014, we decided to use the hydrostatic formulation in the model configuration used in this work, and not to rerun the model in its non-hydrostatic version.

Reviewer comment: 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.

Response: we have now clarified in the manuscript that we use the model implementation and the two model runs described in Sannino et al. (2015). Moreover, we have included a new subsection within the model description to give the details of the simulation setups. Finally, the validation of the model has been moved to the first subsection of the results as suggested by the reviewer; we have made more clear that the validation of the tidal amplitude and phase was carried out by Sannino et al. (2015), while in our work we have performed a new model validation for the 3D tidal currents, namely at the Espartel and Camarinal sills.

Reviewer comment: Page 4, line 11. Probably 1/16âŮę 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.

Response: 1/16° is now a standard for Mediterranean modelling. For instance, most of the MedCORDEX climate simulations are run at 1/12° (www.medcordex.eu) while the CMEMS official operational product for the Mediterranean is run at 1/24° (http://marine.copernicus.eu/services-portfolio/access-to-products/?option=com_csw&view=details&product_id=MEDSEA_ANALYSIS_FORECAST_F
. Thus 1/16° seems a reasonable option. Moreover, as we focus on the local processes at the Strait of Gibraltar, the influence of the dynamics inside the basin is minor. The most important issue is to have an appropriate resolution around the Strait of Gibraltar, and there we reach 500m following the suggestions of Sannino et al. (2014). We have modified that paragraph as follows to clarify this point:

"The grid has a nonuniform horizontal spacing: over most of the model domain it is 1/16° x 1/16° , which is a standard in present state-of-the-art Mediterranean climate simulations (e.g. www.medcordex.eu). In between the Alboran Sea and the Gulf of Cadiz, following the recommendation of Sannino et al. (2014), the resolution increases up to a maximum value of 1/200° (∼500 m) at the Strait of Gibraltar (Fig. 1b)."

Reviewer comment: 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.

Response: the model used in this study is a z-level model with partial cell on the bottom. The 300 m vertical resolution refers to the maximum model depth (about 5000 m). Within the Strait the maximum depth does not exceed 800m, and so the vertical resolution on the bottom of the Strait of Gibraltar is only a few tens of meters. We agree with the referee that a better explanation on the vertical resolution is needed. We have added the following text in the new version:

“The layer thickness ranges from 3 m at the sea surface to 300 m at the maximum model depth (5000 m). The partial cell formulation is used for the near-bottom level. Thus, the thickness of the bottom layer will vary according to the bathymetry. The Strait of Gibraltar has a maximum depth that does not exceed 800 m, and the vertical resolution there is only of a few tens of meters. This allows the model to properly reproduce the dynamics linked to the hydraulic control of the exchange flows. “

Reviewer comment: 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 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)

Response: in the model configuration described in Sannino et al. (2015), the Black Sea net flow through the Dardanelles Strait was imposed according to the results described in Stanev et al. (2000). The more recent findings on the Black Sea net flow reported by Stanev et al. (2017) have not been used in the two hindcast numerical simulations used here. The geographical scope of this paper is the Strait of Gibraltar and not the Black Sea or the easternmost part of the Mediterranean basin. Thus, we honestly consider that further information on this topic is not particularly relevant for our work and, moreover, it could add confusion to the reader.

Reviewer comment: 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.

Response: we thank the reviewer for drawing our attention to this issue. As stated above, the validation of the model has been moved to the results section to avoid any confusion. We have also clarified that the 2D validation of the tidal amplitude and phase was carried out in Sannino et al. (2015), whilst here we have conducted the 3D validation of tidal currents at the main sill of Camarinal and at the secondary sill of Espartel (see the response to a previous comment). Regarding the different runs, in this study we have used two runs, both with 3-hourly data outputs, and only differing in the inclusion (‘tidal run’) or not (‘non-tidal run’) of the tidal forcing. These runs are those described in Sannino et al. (2015) and in the revised version of our

work they are described in a new subsection (simulation setups) of section 2.1 for the sake of completeness. Additionally, we have computed monthly-mean values from the 3-hourly original outputs (i.e., both for the tidal and the non-tidal runs). This has been highlighted in the new version, in order to avoid confusion.

Reviewer comment: 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.

Response: The original idea was to simulate the entire ERA-Interim period. However, the simulation was stopped after few years due to the unavailability of the HPC cluster. Moreover, as we focus on describing the mechanisms acting on the Strait at high frequency, the period considered is unimportant.

Reviewer comment: Page 5, line 26-26 " however, the vertical ...of the basin" this sentence need to be proved.

Response: what we mean here is that the salinity drift is really small compared with the sharp salinity gradients observed at Gibraltar. Thus, a long-term salinity drift of less o(10-3 psu/yr) will certainly affect climate studies, but it can hardly affect the recirculation of water in a region (the Strait) where water masses differ in more than 2 salinity units.

Reviewer comment: 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.

Response: in the new version we have clarified that the fifteen days showed in Figures 3 and 5a cover a spring tide subset of the 10-year time series. We chose that period because in a spring tide period it is easier to display the discrepancies between the incoming (outcoming) flows at the outer sections of the Strait. We have added the

following sentences to the new version of the manuscript:

"Figure 3 presents an example of the 3-hourly tidal run; namely, it shows the exchange flows computed at the outer limits of the Strait during a 15-days period covering a typical spring tide (days 16 to 22). The spring tide period has been chosen to better display the discrepancies between the inflow and the outflow computed at both sections"; and: "The upper panel of Fig. 5 displays an example of the vertical transfer of water computed from the tidal run; the 15-days time period is the same one displayed in Fig. 3."

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

Response: this value has been computed over the 10-year time series. It has been clarified in the new version.

Reviewer comment: 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.

Response: we thank the reviewer for drawing our attention to this. The sentence has been moved to the last paragraph of section 4.1, that is, within the discussion section.

Reviewer comment: 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.

Response: merging the two tables is a difficult task due to the amount of information provided in each of them. Also, for completeness we have decided to keep the information in the outer sections in Tables 4 and 5 to ease the interpretation for the reader.

Reviewer comment: Page 14, line 10: somewhere you should describe what we see in Fig.8, from what simulations, before discussing them.

Response: we have added the following sentence to page 14 of the new version (before the discussion of Fig. 8): "At low frequency the picture is quite similar. Fig. 8 shows a two-layer sketch to summarize the low-frequency transport divergence associated with tides in each layer. Comparing Fig 8a (tidal run) with Fig 8b (non-tidal run) the role of the tides in the low frequency transports can be assessed (see also Table 5). There is a steady decrease of transports from both the Gulf of Cadiz and Alboran sections to the main sill of Camarinal, where minimum values of the exchange flows are obtained (0.48 Sv and -0.41 Sv for the inflow and outflow, respectively). "

Reviewer comment: Page 15, line 30: the mention and the description of the non-tidal run setup should be added in the methods section.

Response: in the new version we have added subsection 2.1.1 (simulation setups) to the methods section and included the descriptions of the tidal and non tidal runs.

Reviewer comment: Page 17, line 26: net water fluxes from simulation data as well?

Response: Yes, if model is well configured, the net transport through Gibraltar will be equal to the net water flux through the sea surface inside the basin.

Reviewer comment: 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?

Response: it is not a typo; the value is correct. The Espartel section has two channels: the secondary northern channel, with a maximum depth of 250m, and the southern main channel, with a maximum depth of 360m. Nevertheless, the reviewer is also right when considering the CS as the shallowest point of the Strait: this assumption is true when considering the main path followed by the exchange flows along the main axis of the Strait. Specifically, in the Espartel section 80% of the Mediterranean outflow flows throughout the southern main channel whilst the remaining 20% flows throughout the secondary northern channel (see e.g. Sánchez-Román et al., 2009). Thus, from the mean exchange point of view one can assume that the Espartel sill is that of the south-

ern channel and has a maximum depth of 360m. On the contrary, at the Camarinal section 100% of the exchange flows throughout its single channel, which has a maximum depth of 290m and is referred to as the Camarinal sill (CS); this is why the CS is considered as the minimum depth for the exchange.

Reviewer comment: Page 13, lines 24-27: some problems with this sentence. Any word missing?

Response: the reviewer is right. We have reworded the sentence as follows:

"For the tidal induced variability, and from west to east, the mean exchange (see Table 4 and Fig. 7a) increases from the Gulf of Cadiz to the ES and then decreases to a minimum at the CS. East of CS, the exchange increases again reaching a maximum at the easternmost section. This behavior is in good agreement with previous studies (Garcia-Lafuente et al., 2000; Garcia-Lafuente et al.,2013) that already showed stronger tidal currents in the eastern part of the Strait."

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