To :

Dr. Qiang Wang, Editor of GMD

From:

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Marine Herrmann, Caroline Ulses, Patrick Marsaleix, Thomas Duhaut, Claude Estournel

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To Duy Thai

IO, Nha Trang, Vietnam

R. Kipp Shearman

Oregon State University, Corvallis, United States

December 9, 2023

Dear Editor,

Please find a revised version of our paper entitled "***New insights on the South China Sea Throughflow and water budget seasonal cycle : evaluation and analysis of a high-resolution configuration of the ocean model SYMPHONIE version 2.4***".

We warmly thank the reviewers for their careful reading and their constructive evaluation of our manuscript. We sincerely believe that these remarks helped us to improve and clarify the manuscript. We carefully took their comment into account.

In particular:

- We performed a twin simulation at coarser (12 km) horizontal resolution to further highlight the advantages of the high-resolution and investigate the influence of this resolution on the water budget analysis.

- We investigated more into details the factors for the mixed layer depth underestimation.

- We compared our analysis of the ratios of interocean straits exchanges with previous studies.

Best regards,

Trinh Bich Ngoc and Marine Herrmann

Answer to reviewers' comments

We warmly thank both reviewers for the time and attention devoted to our paper, and for those positive and constructive comments. We have carefully considered all the comments and suggestions in the revised version of our manuscript. In what follows, and in the highlighted version of the manuscript, our answers and modifications are highlighted in blue. Line numbers refer to the highlighted version of the revised manuscript.

Answers to reviewer 1:

The authors have taken great care to address my concerns in the latest round of review, incorporating extensive validations of the simulation. It is compelling that this simulation effectively elucidates the general circulations in the South China Sea. Therefore, I highly recommend its acceptance.

The manuscript's writing and logic remain sound. I have only two minor suggestions.

1. Firstly, in Figure 6, the representation of the SCS basin appears slightly distorted.

   → this was corrected : we adjusted the Figure 6

2. Secondly, in Table 4, it would be beneficial to reorganize the citations, perhaps by including a line before Wyrtki's pioneer work to indicate that the subsequent citations primarily pertain to observational studies.

   → Following this comment, we added a line in the Table 4 before Wyrtki's pioneer work to indicate the citations of observational studies

3. Furthermore, the examination of volume transport in the Taiwan Strait could be further fortified by incorporating references such as Hu et al. (2010) and some more recent reviews. This would be particularly sensible given the better sampling of this shallow strait compared to others surrounding the SCS basin. Of course, an estimate of about 1.2 Sv remains reasonable.

   → Following this comment, we added the review study of *Hu et al. (2010)* and *Isobe et al. (2008)* in the Table 4. These studies are also cited in the discussion part on Taiwan strait (lines 672-680)

References

Hu, J., Kawamura, H., Li, C., Hong, H., and Jiang, Y.: Review on current and seawater volume transport through the Taiwan Strait, https://doi.org/10.1007/s10872-010-0049-1, 2010.

Isobe, A.: Recent advances in ocean-circulation research on the Yellow Sea and East China Sea shelves, https://doi.org/10.1007/s10872-008-0048-7, 2008.

Answers to reviewer 2:

I recommend that the manuscript be accepted for final publication after the authors address the remaining concerns and suggestions from the reviewers. The authors have made significant improvements in response to the initial major comments and have effectively addressed issues related to the title, advantages of their high-resolution approach, restructuring of sections, and comparisons with other models. The addition of a dedicated section on the South China Sea Throughflow (SCSTF) and water budget analysis further enhances the manuscript's content. However, there are a few questions and recommendations that should be addressed in the final revision. The authors should provide more clarity regarding the underestimation of Mixed Layer Depth (MLD) and the role of wind speed, as well as the robustness of lateral interocean exchanges ratios and their validation against observational or previous studies. Additionally, the suggestion to use a coarser version of the same model core for comparison instead of other coarser-resolution models should be addressed if possible, exhibiting the benefits of the higher resolution more directly. Once these remaining points are adequately addressed, the manuscript should be considered suitable for final publication.

Major Comments:

1. MLD Underestimation and Wind Speed: How the authors concluded that all models' underestimation of Mixed Layer Depth (MLD) is attributed to the underestimation of wind speed. Do all models use the same forcing? It's important for the authors to clarify and provide further details on the role of wind speed and the consistency of forcing among the models when discussing MLD underestimation.

All simulations considered in our paper use bulk formulae of *Large and Yeager (2004)*. Our SYMPHONIE simulations, INDESO and COPERNICUS use atmospheric outputs from the European Centre for Medium Range Weather Forecasts (ECMWF) operational forecasts (1/8° ~14 km horizontal resolution and 3 hours temporal resolution), and GLORYS uses the outputs of ERA-Interim reanalysis (80 km and 3 hours resolution) produced from the same ECMWF model. OFES uses the atmospheric surface dataset JRA55 atmospheric reanalysis (~55 km and 3 hours, resolution, *Kobayashi et al., 2015*). This information is summarized in Table 1 of the revised manuscript.

Compared to QuikSCAT, ECMWF analysis and reanalysis indeed underestimate sea surface wind speed (by ~1 $m.s^{-1}$ on average over the region for ECMWF analysis, see Fig. SM1). *Herrmann et al. (2020, 2022)* indeed showed that global and regional atmospheric models generally underestimate sea surface wind speed over the SEA region, and *Wang et al. (2020)* showed that both ERA-Interim and JRA55 underestimate observed wind dataset over China Seas, with a smaller bias in JRA55 (0.22 $m.s^{-1}$ over the period 1988-2015) than in ECMWF product (0.62 $m.s^{-1}$). This underestimation of wind speed in forcing datasets partly explains why all models underestimate the MLD, and why OFES, which uses JRA55, produces the closest MLD to observations (Fig. 10f of the revised paper).

Moreover, as shown by *Tréguier et al. (2023)*, MLD biases as well as their differences among models may also be due to the models formulation, parameterizations and resolution, whose shortcomings vary between models: horizontal and vertical resolutions, inclusion of tides, vertical mixing parameterisation, advection schemes, etc. *Gaube et al. (2019)* for example showed that mesoscale eddies, whose representation depends on those formulations, modulate the MLD. Indeed, though SYMPHONIE simulations at 4 km (SYM4) and 12 km (SYM12, see answer to comment 2 below) resolutions and COPERNICUS (which provides the initial and lateral oceanic boundary conditions to SYMPHONIE) use the same atmospheric conditions, the MLD underestimation is lower in SYM4 than in SYM12 and COPERNICUS. This suggests first that the MLD underestimation in SYMPHONIE can also be explained by the MLD underestimation of the COPERNICUS initial profiles and those profiles entering the domain, and second that SYMPHONIE, due to different formulations in particular its highest resolution, is able to partially correct the stratification of these initial and entering profiles.

→ following this comment, we added this analysis in our revised paper (Part 4.3, lines 598-617)

2. Use of Coarser Configuration: It is recommended that the authors consider using a coarser version of the same model core for comparison, in addition to comparing their high-resolution model with other coarser-resolution models. Including a coarser configuration with the same model core will provide a more convincing basis for highlighting the benefits of their new setting, especially in terms of finer resolution and other relevant factors. This approach can offer a more direct and robust assessment of the advantages of their high-resolution model.

We agree that running a simulation with the same model and choices of parameterizations, but a coarser resolution, is indeed a rigorous way to highlight and quantify the added-value of the high-resolution. We therefore performed a simulation, called SYM12 in the revised version of the manuscript, using exactly the same configuration, forcing etc as the simulation at 4 km (now called SYM4), but with a 12 km horizontal resolution. In order to avoid any confusion, the simulation at 4 km is now referred to as SYM4 in the whole document.

→ SYM12 simulation is presented in Part 2.4 (lines 314-317) and Table 1 of the revised manuscript.

We then examined the differences between SYM4, in terms of performance and results.

First, SYM4 performs better than SYM12 for all the diagnostics performed here : tidal amplitude (Figure A3 of the Appendices), spatial (Figure 7 and Table 3 of the revised manuscript) and temporal (Figure 5) variability of SST, SSS and SLA, and TS profiles and MLD (Figure 10). Moreover, biases for SYM12 are in the range of the biases obtained for the other simulations at similar resolution, without (INDESO, OFES) or with (GLORYS, COPERNICUS) assimilation. This shows that the SYMPHONIE model at 12 km resolution shows performances similar to those of models with similar resolution, and that using a higher 4 km resolution significantly improves this performance. This can be attributed to the better representation of (sub)mesoscale processes, of tides, but also of topography, in particular of straits. This is particularly obvious when visualizing the bathymetry for the 4 km and 12 km grids in Figure A1, A2 of the Appendices: deep narrow passages are sometimes represented with no more than 1 grid point, in particular at Luzon, Mindoro and Balabac straits.

→ SYM12 is integrated in the comparison with other datasets in the revised manuscript : for sea surface elevation, temperature and salinity (Table 3, Parts 4.1.1 lines 407-414 and 4.1.4 lines 513-

521, Figures 5 and 7), and water masses characteristics and MLD (Parts 4.2 lines 543-555 and 4.3 lines 595-617 and Figure 10).

Second, the resolution of the model also impacts the results on water fluxes at interocean straits (vertical structure (Figure 12), yearly cycle (Figure 11b) integrated estimates (Figure 11a)) as well as the water budget over the area (Figure 11a,c).

→ this influence on water fluxes estimates is integrated in Part 5.3 (lines 805-823) of the revised manuscript, and SYM12 results are included in Figures 11 and 12.

Last, we added a few sentences in the Abstract (lines 23-25 and lines 37) and Conclusion (lines 840-842 and lines 892-893) to mention this study of model resolution.

3. Robustness of Lateral Interocean Exchanges Ratio: The ratio of different lateral interocean exchanges is interesting. I am also wondering if it is robust. If there is any observational or previous study support for this ratio. It's essential for the authors to provide information on the robustness and potential sources of validation for the ratios presented in the study.

The spatial and temporal coverage of the available in-situ observations, reported in Table 3, does not currently allow these ratios to be estimated. An in-situ estimate would indeed require simultaneous measurements to be taken in all the straits over a long period, which is fairly costly and technically complex. The idea is therefore to use observations to assess the quality of the circulation and water masses produced by the models, and then to use the models to 'fill in the gaps' in the observations and compute ratios. Moreover, it would still be extremely relevant to put in place a strategy for measuring these ratios in situ, which would make it possible, among other things, to assess the robustness of these numerical estimates.

→ following this comment, we added, when possible, the values of ratios between water flows through Luzon, Taiwan, Mindoro and Karimata straits estimated from numerical results in Table 4, and discussed this question in the Conclusion, lines 861-872.

Minor Comments:

1. Title Prolixity: The new title appears somewhat lengthy. It might be advisable to streamline the title by removing some of the detailed configuration information, thus achieving a more concise and reader-friendly title. The authors should contemplate this suggestion and determine if certain elements of the title can be omitted while retaining the core information.

→ Following this comment and the suggestion of the editor, we propose the following title : ***New insights on the South China Sea Throughflow and water budget seasonal cycle : evaluation and analysis of a high-resolution configuration of the ocean model SYMPHONIE version 2.4***

2. Legend of Figure 2: The legend of Figure 2c is overlapped with the graph.

→ this was corrected, we adjusted the Figure 2

References

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