We thank the reviewer for his/her careful reading and constructive comments which helped to improve the manuscript (ms). The point-by-point replies to the reviewer's comments are provided below in bold font:

Specific comments

Comment #1 – (Figure 1 and line 149) A reference for GEBCO 2014 should be provided and added to the references list. Are the authors considering updating MCao bathymetry based on the GEBCO 2020 dataset? Although GEBCO 2019 and GEBCO 2020 were not available when the simulations were performed, these datasets were available when the paper was submitted. The authors should then discuss how using a 15 arc-second resolution bathymetry dataset could influence model results.

Web link to the GEBCO 2014 data is included in the revised manuscript. In our future developments of the coupled system, the latest available bathymetric data sets will be used. More accurate representation of the model bathymetry is expected to improve the simulations of tide sea level and also the ocean circulation patterns.

Comment #2 – (line 80 and 81) Please provide references to support this statement ("For instance,. . . Pacific Ocean").

Reference included in the revised ms (L80).

Comment #3 - (line 81 and 82) Please provide references to support this statement ("The north Pacific Ocean. . . tropical cyclone annually").

Reference included in the revised ms (L82).

Comment #4 – (line 156 and 157) Why did the authors decide to change the values of background vertical eddy viscosity and eddy diffusivity coefficients?

We have conducted a few sensitivity experiments and found that reduced background viscosity and diffusivity coefficients has led to an improvement in the model simulations at the model resolution we used. Hence, those reduced values are used in our model simulations.

Comment #5 – (line 167) Could the authors briefly explain how they managed not to have these numerical issues in MCao? This information can be handy for other authors that want to implement similar model systems based on NEMO.

A few steps were taken to overcome this issue from T18 to MCAao. Mainly we have adjusted the bathymetry manually by comparing gebco with the navigation charts.

Comment #6 - (line 168) FES2014b is not in the list of references.

Reference added (L168).

Comment #7 - (line 190) What is the external source for MSLP?

ECMWF IFS. A sentence added in the revised ms (L189-190).

Comment #8 – (line 194) Please provide a reference for Mercator global ocean reanalysis.

Reference added to the data discussion in section 3.1 (L239).

Comment #9 – (line 254) Please provide a reference for the Operational Sea Surface Temperature and Sea Ice Analysis.

Reference included in the data discussion in section 3.1 (L258).

Comment #10 – (Figure 4): Although Figure 3 mentioned that the Bay of Bengal region is excluded from the analysis-domain, results are presented for this region in Figure 4. Could the authors clarify this better in the text? Moreover, I suggest writing the abbreviations of each sub-region in the map of figure 3. This will help readers not familiarized themselves with the Maritime Continent.

In the figure caption, the sentence modified as "The Bay of Bengal region (north of 5^{o}N, west of 92^{o}E) is excluded when analysis is performed for different sub-regions."

The abbreviations are included in figure 3.

Comment #11 (line 295) – What could be the reason for the observed SST bias in the Andaman Sea region?

From the present study, we are not able to completely understand the reasons behind this SST drift. The possible reasons for this SST drift may include the effects of wave-induced mixing, the air-sea heat fluxes and surface wind patterns, etc. More detailed analyses are required to identify the factors responsible for this discrepancy.

Comment #12 (line 366 to line 368) – What could explain this?

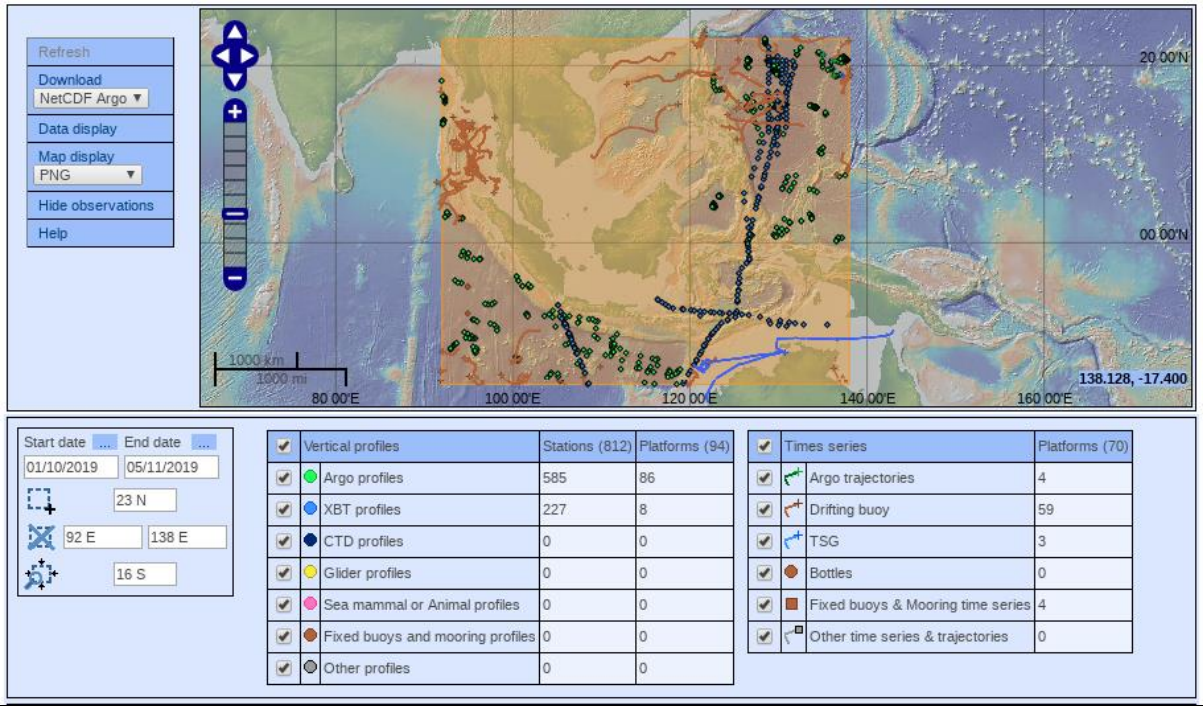
Comment #13 (Table 4) – Can the author elaborate on the reasons for the decrease of SST Bias overtime (Bias decreases for higher forecast lead times). This is a general trend for all the sub-regions (exception for ASMS). In general, it is expected that the accuracy of SST decreases with increasing lead times. However, it seems that Bias is not showing that.

Comment 12 -13: As pointed out the reviewer, it is expected that the quality of model simulations decreases with higher forecast lead times. The decrease in SST correlation with increase in lead times is in accordance with the above notion. Meanwhile, decrease in the SST bias or RMSD as seen in our study is not consistent with the above discussion. Since the present study focuses mainly in the development of an operational coupled modeling system and evaluation of ocean forecast fields, detailed analysis to isolate the mechanisms behind the drifts in forecast fields are not undertaken. Our ongoing atmospheric forecast analysis may provide useful insights in identifying the factors contribute to such variations/discrepancies.

Comment #14 - In Figure 9a, results are presented for Mooring M1(95E, 8S). However, in Figure 9b, 9c and 9d results are presented by MCO_ao at 95E, 5S. Is this a typo? Based on the text in the manuscript, I believe this is a typo. However, the authors must double-check if they compared observations and model results at the same lat/lon.

Corrected the M_1 location as 95E, 5S.

Comment #15 (line 424 to line 435, and Table 6) – The authours say "the analysis mainly demonstrates the model performance in the domain excluding the SCS region". This is understandable. However, it is impossible to understand if Argo and XBT profiles available from 1 October to 5 November 2019 represent the other sub-regions. Did the author calculate RMSD for all the sub-regions (excluding SCS)? Does RMSD change from sub-region to sub-region?



Number of observations available during 01/10/2019 to 05/112019/. Argo, XBT and CTD profiles are used for the subsurface temperature and salinity analysis.

The Argo/XBT/CTD profiles used for the subsurface temperature/salinity forecast evaluation are shown in the figure. As seen, the profiles are mostly confined to the Tropical eastern Indian Ocean and tropical western Pacific Ocean. A few profiles are present in the Timor-Arafura and Banda Sea regions. The temperature and salinity RMSD variation between these sub-regions are less than 0.3 deg C and 0.025 psu, respectively.

Comment #16 (Table 6) The legend of Table 6 indicates "observation during October 2019". However, in line 424 the authors mentioned: "for the period 1 October to 5 November". Please clarify this point.

Corrected as "Summary of temperature and salinity RMSD statistics between coupled ocean forecasts and in situ (Argo profile and XBT) observations during 1 October to 5 November 2019."

Comment #17 (table 7) - The 19 tide-gauge location used to evaluate the model performance to simulate SSH should be presented in Figure 3. Although the lat/lon of each tide-gauge is provided in Table 7, its location in a map will make it easier for readers to understand better where the authors evaluated SSH performance.

Technical corrections

Line 117 – In line 111 MC_ao is defined as MCao. Please use only one nomenclature for the MC atmospheric-ocean coupled model.

Corrected

Line 143 – Do you mean Parallelise instead of PArallelise?

We follow the naming convention used for the OPA model, hence it remains as PArallelise

Line 269 – Replace “set of variables an d” by “set of variables and”

Corrected

Table 5 – Please delete the space in "observation s"

Corrected