We thank the reviewer for the careful reading of the manuscript and the constructive comments. Please find below our point-by-point replies:

Comment 1:

Previous studies suggested that remote forcing from the equator significantly modulated the intraseasonal current and eddy kinetic energy (EKE) in the BoB by the coastal Kelvin waves and reflected/free Rossby waves. Strong Intraseasonal Variability of Currents and large EKE can be found near 5N in the eastern Indian Ocean (e.g., Chen et al. 2017 JPO, 2018 JGR). Obvious salinity anomalies can also be observed here in your Fig. 10.

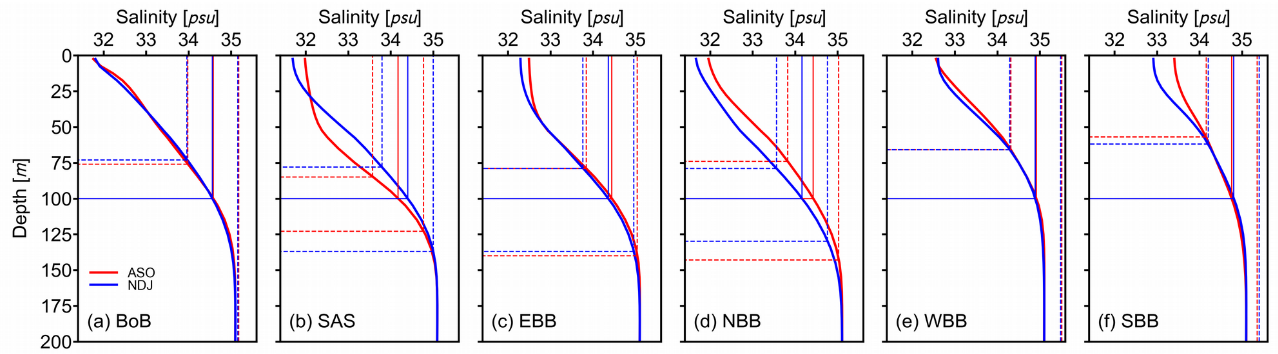
In Fig.10, you chose several sub-regions. How to choose these sub-regions? To clearly demonstrate your points of remote forcing modulating the salinity by waves, I suggest to choose a sub-region near 5N in the eastern Indian Ocean. Furthermore, it would be helpful to compare the differences of these sub-regions, not just in correlation coefficient (Fig. 11).

Reply 1:

Yes, a significant intraseasonal signal can also be observed in the southeast of Sri Lanka in our simulation. However, the choice of sub-regions is based on the general circulation pattern in the Bay of Bengal. Essentially, the purpose of these sub-regions is to better describe the propagation of coastal Kelvin waves and westward moving Rossby waves, and to discuss the different contributions of advection and diffusion processes during the wave propagation.

We agree that a sub-region near 5N could help to give us a more comprehensive view, especially with respect to Rossby waves. However, a sub-region at 5N is very close to the southern lateral open boundary, and as shown in Fig. 5d, the U-velocity is overestimated due to open boundary effects, which may result in unrealistic results especially with respect to the salinity advection. Meanwhile, we think the current 5 sub-regions selected, are sufficient for the purpose of our study.

As requested, we will put a stronger focus on the differences of these sub-regions. Therefore, we plan to add a new figure (insert after Fig. 10) showing the vertical salinity profiles in ASO and NDJ, as well as the vertical displacement equivalent to a 0.6 psu salinity anomaly. A new paragraph explaining this new figure will be added.



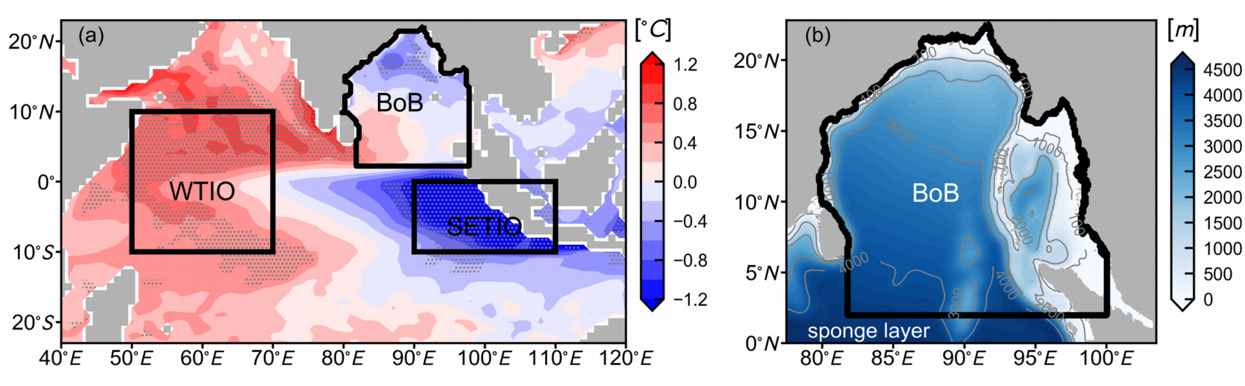
Comment 2:

I am surprised that the amplitude of correlation coefficient for the BoB (Fig. 9) is comparable with that for the sub-regions (Fig. 11). Which region was chosen when calculating the correlation coefficient for the BoB.

Reply 2:

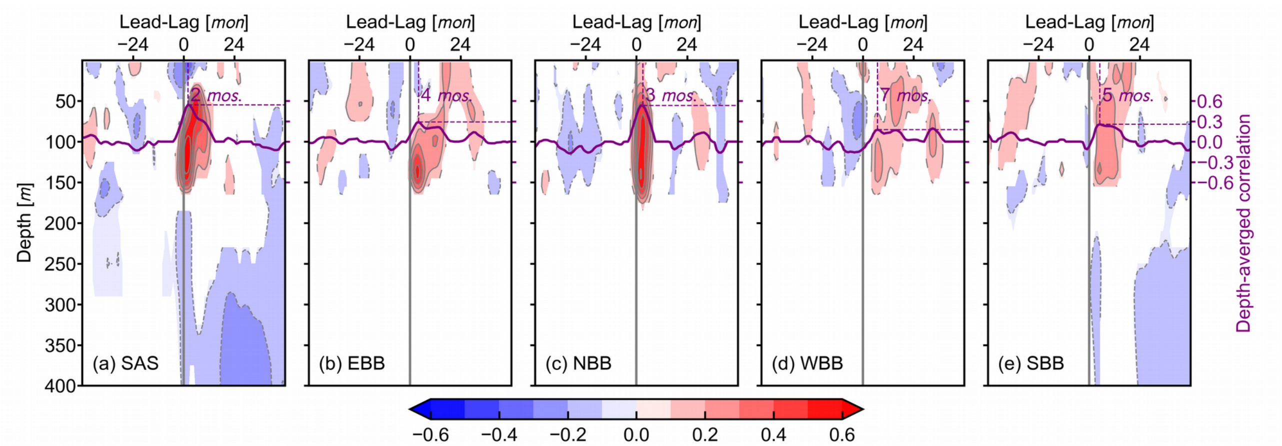
The region of the BoB is indicated in the Fig. 1, but we admit that this figure is confusing because the BoB is marked on the map from the MPI-ESM-MR. We plan to add a new subplot to Fig. 1

presenting the model bathymetry and our research domain. The exact area, which was used to calculate all BoB-related parameters is also marked in this new Fig. 1b.



Comment 3:
Could you clearly demonstrate the lagged period for each sub-region? It's not easy to identify this information in Fig. 11.

Reply 3:
We plan to replace Fig. 11 by the new figure shown below. The new added purple line give the depth-averaged correlation from 50 m to 150 m. The lagged period for each of the sub-regions is also indicated. Of cause, we will also discuss these correlations and lags in the updated manuscript.



Comment 4:
Lines 164-173. Evidences are needed here.

Reply 4:
We agree. We will rephrase this part making our line of thinking given blow more clear.

The term "degree of freedom" as it is used in this context describes the complexity of a data set (or a system). Results from a general circulation model show a lower degree of freedom than an observational data set because a model is not able to resolve all processes that exist in the real Earth system. Similarly, pure ocean model results show a lower degree of freedom than an atmosphere-ocean coupled model because air-sea feedback processes are not included. Therefore, we think the term "degree of freedom" helps to explain the correlation differences shown in different data sets. And as also mentioned in section 5, lines 302-306, when investigating the principle physical processes behind the correlations we found, the analysis of our HAMSOM simulation results is superior over other data sets, because our model results shows a relatively low degree of freedom.

Comment 5.1:
"The distribution of composited SSTA presents a dipole mode in the tropical Indian Ocean, which indicates the MPI-ESM-MR can reproduce IOD events." It would be better to show more evidences here.

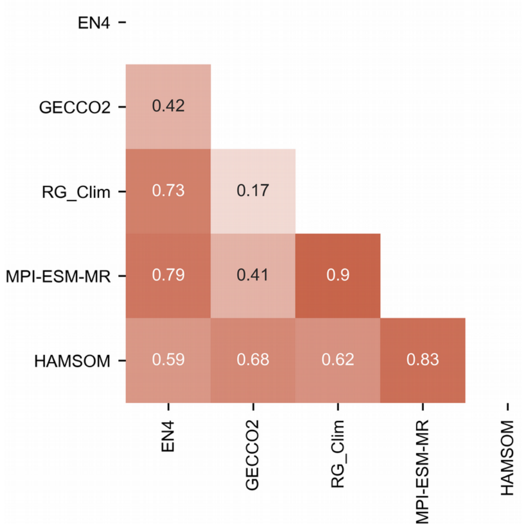
Reply 5.1:
In the updated manuscript we will cite Webster et al., 1999, Deser et al., 2010. These two papers show similar pIOD SSTa pattern. Intended new text: "This distribution of the composited SSTa (Figure 1a) shows a significant dipole mode in the tropical Indian Ocean, which is in good agreement with previous studies (Webster et al., 1999; Deser et al., 2010). The corresponding DMI time series (Figure 7d) exhibits reasonable interannual variation characteristics. These indicate that the global model used in this downscaling study can realistically reproduce IOD events."

Comment 5.2:
The salinity of HAMSOM is obviously lower than that of the other datasets (Fig. 2). Dose this affect the results in this study?

Reply 5.2:
We think this doesn't affect our results very strongly. The reason is that the coastal Kelvin waves and the associated Rossby waves appear as eigen-modes of the stratified ocean. Therefore the characteristics of these waves are mainly determined by the vertical density gradient. And since salinity gradient simulated by HAMSOM are consistent with other third-party datasets, as shown in the model validation section 2.3, we are confident that our final results are reasonable.

Comment 5.3:
The salinity in the subsurface layer is less correlated with each other (Fig. 3b). Are we confident enough in the results?

Reply 5.3:
We calculated the correlation metrics (as shown below) for lines shown in Fig. 3b. From this metrics, we are confident that the seasonal change of the subsurface salinity simulated by HAMSOM is reasonable, which also provides confidence in our final results. We will provide the averaged correlation coefficients (EN4: 0.63; GECCO2: 0.42; RG_Clim: 0.60; MPI-ESM-MR: 0.73; HAMSOM: 0.68) of each datasets in the updated manuscript.



Comment 5.4:

Why is the RC-CLIM not used in Fig. 2? Why choose EN4 as the "standard" in Fig. 6? How to obtain the conclusion "Overall, the standard deviations of HAMSOM and other data sets are in close agreement" according to Fig. 6?

Reply 5.4:
The RG_Clim is an Argo-based dataset, which extends from 2004 to present. The reason why we didn't use RG_Clim in Fig.2 is that the mean state of RG_Clim from 2004 to 2016 presents a different climate state compared to the climate state from 1971 to 2000, especially in light of an increasing global warming.

When plotting a Taylor diagram, the ideal situation would be to have a reference dataset based entirely on observations. Unfortunately, there is no such a dataset available. However, the EN4 dataset shown in Fig. 6 is based on an objective analysis using a global quality control, while other three datasets are model-based. Hence, the EN4 dataset is obviously the best choice, when looking for a reference state.

With regard to the last question, we think the results shown in Fig. 6b are acceptable. To make use of the advantage that HAMSOM results have a high resolution, we employed the HAMSOM grid when calculating the Taylor-diagram-related parameters. Hence, for other datasets, except for HAMSOM, a linear interpolation to the HAMSOM grid was necessary, before performing the Taylor calculation. In particular, for the subsurface salinity, the HAMSOM results are supposed to show more mesoscale features due to HAMSOM's high resolution. In contrast, for the surface salinity simulated by HAMSOM, the spatial patterns are largely determined by the low resolution atmospheric forcing fields. This explains why, compared to the EN4 data, for the sub-surface (Fig. 6b) HAMSOM results show a large standard deviation, whereas for the surface (Fig. 6a) both data sets are in close agreement. We thank the reviewer for pointing out that the explanation of Fig. 6 was not clear enough. We will modify the discussion of Fig. 6 along the lines of the explanation given above.

Comment 5.5:
Could you give more explanation of the grey lines in Fig. 6?

Reply 5.5:
Yes. The grey lines indicate the axis of the centered Root-Mean-Square (RMS) difference, which is often used to quantify differences between two fields. We will improve the explanation of Fig. 6.