We thank Dr Michael McPhaden and Reviewer 2 for their time and thoughtful reviews of our revised manuscript. The manuscript is very much improved as a result of their efforts.

We have responded to each comment below. The reviewers' original text is in black and our response is in blue. Where line numbers are mentioned these refer to lines in the revised manuscript, which are different from the track change version submitted.

Referee #1: Michael McPhaden, michael.j.mcphaden@noaa.gov

The authors have done an excellent job to address my concerns on the original submission of this paper. The revision is much improved both in terms of content and organization. I recommend publication subject to a few minor adjustments as described below. I do not need to see the final revised version of the paper.

General: Since the paper is so long, it might be helpful to include a table of contents at the front end with the various sections and subsections listed. That way the reader can orient with a big picture overview and, if they are interested in only a subset of topics, can navigate directly to the relevant sections.
We found it useful to have a table of contents during the writing of the manuscript and appreciate your suggestion that it might also be useful for the reader. We have included this now.

Specifics:

Line 65. "topography". Do you mean "geography"?
We used the word topography because that is used in the IIOE-2 science plan in the list of questions being addressed in Science Theme 4. There is no need for us to follow the exact wording and we did not do so for the third science question. Geography would fit equally well, or better, and we have changed to this (Line 143).

Line 298. Kessler et al, 1995 and McPhaden 1999 proposed this connection before Bergman, so they should be cited as well.
Thank you. We have added these references.

Line 362-64. The juxtaposition of semi-annual with intraseasonal variability here is a little abrupt. Maybe move the reference to semi-annual variability to the section on the Wyrtki jets or provide a smoother connection at this point.
We agree that this mention of semi-annual variability is a little abrupt here. We removed it from the MJO section but found that it did not fit well in the Wyrtki Jet section either and so have deleted this text.

Section 3.2.2. Please point out that time scales for the MISO as for the MJO. The MJO is classically characterized by 30-60 day periods but MISOs can also occur on 10-20 day time scales (Goswami et al, 2016) and there are studies that have identified a 3-7 day time scale MISOs (e.g. Roman-Stork et al, 2020).
We have added this information about MISO timescales to the start of Section 3.2.2 and have included the suggested references.

Section 4.4.2.1. A new paper has appeared online by Zang et al (2021) on the Somali Current/Undercurrent that might be good to cite in this section.
This is a great addition. We have added a paragraph on this paper to the end of Section 4.4.2.1 (Line 1250).

Lines 2081-82. There has been progress on understanding circulation at intermediate and deeper depths, at least in the equatorial band. Huang et al (2018) summarize Indian Ocean studies of deep ocean propagating features and present some new results. You could possibly fold a short discussion of this topic into section 4.3.3 if you felt is would be of benefit. But it's not necessary given how much territory you've covered already in the paper.
Thank you for pointing out this reference, which we have now added. We have referred to it in a sentence at the end of Section 4.1, the overview of the circulation section (Line 565). Given the length of the paper already, we decided not to add more on this topic.

Line 2113-14: It might be helpful to mention marine heatwaves and their ecosystem impacts here.
Thank you for this suggestion. We have added a sentence about this at line 2208.

A final suggestion (though I'm guessing the authors have already considering this)--some kind of acknowledgement or maybe even a dedication to the recently deceased co-author Satya Prakash.
We have been considering how best to acknowledge Satya's contribution. We have been touched by the many fond messages from his colleagues and have included our message in a dedication just before the abstract of the paper.

References
These references were very helpful and are now included in the manuscript.

Goswami, B.N., S.A. Rao, D. Sengupta, and S. Chakravorty. 2016. Monsoons to mixing in the Bay of Bengal: Multiscale air-sea interactions and monsoon predictability. Oceanography 29(2):18–27, http://dx.doi.org/10.5670/oceanog.2016.35.

Huang, K., McPhaden, M. J., Wang, D., Wang, W., Xie, Q., Chen, J., et al. (2018). Vertical propagation of middepth zonal currents associated with surface wind forcing in the equatorial Indian Ocean. Journal of Geophysical Research: Oceans, 123, 7290–7307. https://doi.org/ 10.1029/2018JC013977

Kessler, W.S., M.J. McPhaden, and K.M. Weickmann, 1995: Forcing of intraseasonal Kelvin Waves in the equatorial Pacific. J. Geophys. Res., 100, 10,613–10,631.

McPhaden, M.J., 1999: Genesis and evolution of the 1997-98 El Niño. Science, 283, 950-954.

Roman-Stork, H. L., Subrahmanyam, B., & Trott, C. B. (2020). Monitoring intraseasonal oscillations in the Indian Ocean using satellite observations. Journal of

Geophysical Research: Oceans, 125, e2019JC015891.
https://doi.org/10.1029/2019JC015891

Zang, N., Sprintall, J., Ienny, R., Wang, F., Seasonality of the Somali Current/Undercurrent System, Deep-Sea Research Part II, https://doi.org/10.1016/j.dsr2.2021.104953.

Anonymous Referee #2

I appreciate the authors' efforts to respond to the reviews. The resultant revision is improved. Some of issues remain as highlighted below. I hope they can consider these comments in finalizing the paper.

L144-147. I agree that the Indian Ocean is important for climate but its role should not be overstated. The just-released IPCC AR6 does not support the claims here, for example.

Box 9.2, Figure 1 (AR6). The Indian Ocean is small in area. I don't see how to justify the claim "the western Indian Ocean… has been the largest contributor to the overall global SST trend."

"The Indian Ocean accounts for 50-70% of the total ocean heat uptake in the global upper (700 m) ocean over the last decade, associated with anthropogenic warming." Figure 9.6 of AR6. does not support the statement for "anthropogenic warming." If the 50-70% figure is valid during the "last decade" (specify please), is it meaningful in terms of the climatic effect?

Thank you for your attention to this point. We have revised the statement about rapid warming in the Indian Ocean and now refer to Chapter 9 of the IPCC AR6. The text starting at line 221 now reads
The tropical Indian Ocean sea surface temperature (SST) has warmed faster over the period since 1950 than either the tropical Pacific or Atlantic (Han et al., 2014, Fox-Kemper et al., 2021), with implications for primary productivity (Roxy et al., 2014, 2016).

L1475. The strong IOB-ENSO correlation is not limited to the post-1975 epoch but was high "during the decades in the late nineteenth–early twentieth century" as well (Chowdary et al. 2012, J Clim).

This sentence has been modified and the Chowdary et al (2012) reference has been included (Line 1563).

L1582. The 2019 IOD "caused extreme rainfall and floods over Japan and China" not directly but by exciting downwelling Rossby waves in the South Indian Ocean that triggered the strong IPOC of 2020. See the discussion in the cited references (Takaya et al. 2020; Zhou et al. 2021). This is an opportunity to integrate with section 6.1.

This sentence has been expanded and climate impacts associated with the 2019 IOD event put into a broader context. The modified text begins at Line 1670.

Saji et al. (1999) was cited in text but not listed in References.

There was a formatting error and this reference was hidden in the reference list before. It now shows clearly in the references.