We would like to sincerely thank the referee for the time and effort he or she put into this review and the helpful suggestions improving our manuscript. We address the issues raised by the referee below.

General comments:

1. **A study area map, with the name of the main places (which come up a lot during the text) would help to locate the reader. Just as presented in Figure 1 is not enough.**

We added an overview map of the region with additional labels as a new Fig. 1.
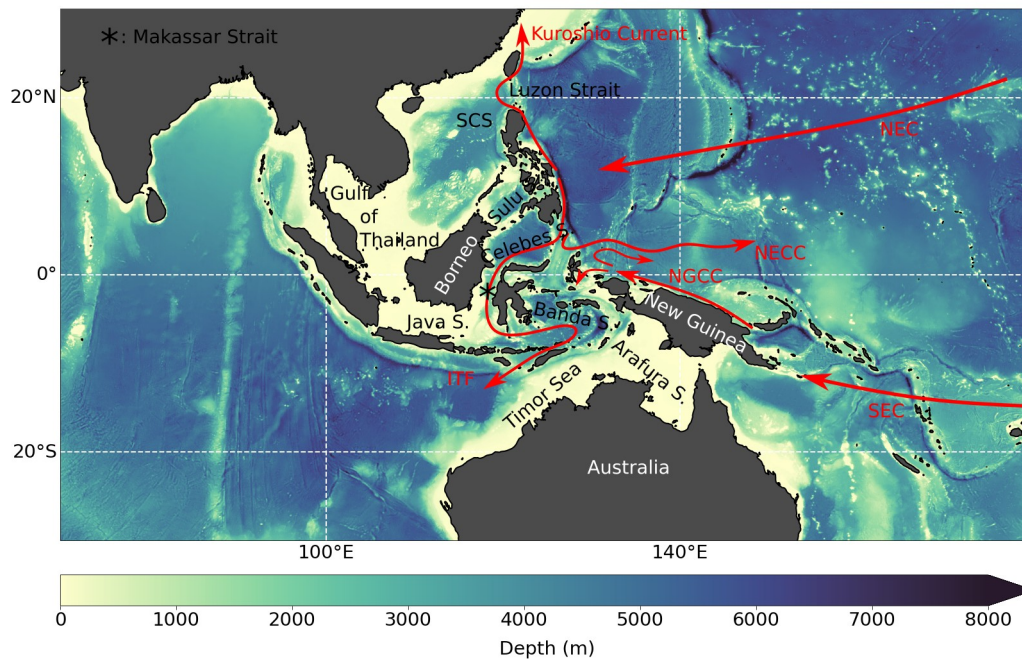

Figure 1: Overview map of study domain, depicting the marginal seas and schematic currents discussed in the text. Colourshading indicates depth. Marginal seas, islands, continents and schematic currents that are mentioned in the text are also marked. SCS: South China Sea, NEC: North Equatorial Current, SEC: South Equatorial Current, NECC: North Equatorial Counter Current.

2. **On line 89-91, the authors present the different nomenclatures for the region, and explain why they use 'Australasian Mediterranean Sea'. I must admit that I had never heard of such nomenclature, and found the word 'Mediterranean' in specific a bit puzzling at first. I wonder if using a more common nomenclature for the region, such as 'Tropical Asian Seas' and 'Southeastern Asian Sea', won't attract more readers.**

There is a broad variety of terms for the region but all lack precise definition. The term AMS seems to be used by a broader community and is also found in established textbooks (e.g. Tomczak and Godfrey, 2003). We also choose AMS in favour of other options because it includes the term "Mediterranean", which we consider an appropriate characterization of the region. Nevertheless, we agree that recent papers tend to use terms like "Tropical Asian Seas", "Southeastern Asian Sea" or "Maritime Continent" which is why we added the paragraph on the nomenclature in the first place. We also acknowledge your argument that readers that are used to alternative terms might not be

attracted by the term AMS. We therefore suggest including the terms "Tropical Asian Seas" not only in the introduction but also in the abstract.

3. **The results section is relatively long, and includes different things. I recommend the authors to create subsections to guide the reader better. For example, on Line 157 and L221 would be good places to start subsections.**

Agreed, we split the results section into three subsections.
    3.1 Impact of buoyancy fluxes on SSH
    3.2 Linear regression of SSH on ENSO and PDO indices
    3.3 Decomposition of SSH variability into thermosteric and halosteric contributions

4. **Section 4 (Summary and conclusion), is more of a discussion section. The main findings are very clear in the abstract, but not so much in Section 4 (which is very long for a summary/conclusion). Some re-structuring of this Section would improve the text.**

Thank you for pointing this out. We renamed the section into "Summary and discussion" and restructured it into two subsections.

5. **The final 'general' conclusion of the paper is about the models' resolutions (from L332). However, while the effect of the resolution is clear for the temperature and salinity, it didn't seem to me so strong on the SSH variability itself. I don't think that these results have enough evidence to back the statements on L335,336.**

You are right that there are only minor differences with respect to SSH and we added this point to the last paragraph. However, we think that our results regarding temperature and salinity biases and the fact that many climate models are unable to resolve dynamically relevant straits and passages (e.g. Makassar strait or Mindoro-Sibuto strait) warrants our rather general warning. Additionally, a recent review on coupled ocean-atmosphere modelling over the region (Xue et al. 2020) support this with respect to ocean dynamics. We included the reference in the text.

6. **The observational dataset and variables used should be presented before the Results Section. Furthermore, it's important to mention that you use altimetry observations to validate the model, and that they are only available from 1993 (in contrast with your model that runs since 1958). Also how was the thermosteric and halosteric components computed? Or are they output of the model? This should be clarified. Also, where did the ENSO and PDO indices come from (from figure A1 it seems you computed it based on the model results, so would be good to say how you computed them. And does it match with the observations indices?)**

We computed the steric sea-level changes from the stored model output. We added another subsection on the diagnostics to the methods section. Another additional subsections now describes the observational data. Both climate indices were computed from the model output and compare well with observational estimates. We included a description regarding the computation and also added observations to figure A01.

7. **Did you do any analysis looking at 'specific' ENSO and PDO events? Does the explained variability change regarding an El Niño instead of a La Niña? Or does the position of the ENSO have an influence on your results? Maybe such analysis would enrich the paper even more.**

Regarding the symmetry between El Niño and La Niña:

We agree that this is a valid point. We did a composite analysis to check for differences between El Niño and La Niña events. We found the response to be symmetric with only minor difference in most seas except for two regions (see Figure below). Both, El Niño and La Niña trigger positive sea level anomalies in the western coastal regions of the SCS, which is not representative for the rest of the region. The total sea-level variability is also very weak in that area (see Fig. 1 in the manuscript). The second region is the Arafura Sea where La Niña produces much stronger anomalies than El Niño. However, we did not investigate this further but included a sentence to this end in the manuscript.

Regarding different ENSO flavours:

We did not test this but used the Nino34 index to capture both flavours of ENSO. Judging from SST regressions, the difference between Central Pacific and Easter Pacific El Ninos is most pronounced in the central to eastern Pacific (Ashok et al. 2007) and available observational data suggests that the impact in the region of interest is rather small. For example, Liu et al. 2015 showed the ITF transport anomalies during both ENSO types are rather similar. We therefore suggest to not address this aspect in the manuscript.
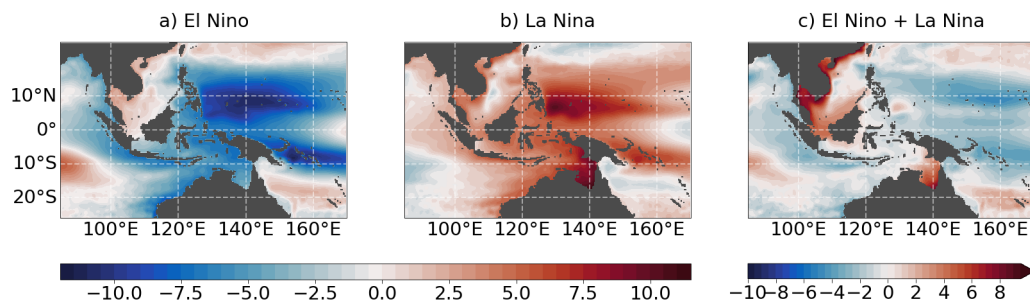

Figure 2: Composites of SSH anomlies during (a) El Nino and (b) La Nina events and (c) the sum of both composites.

Technical comments:

1. **Sea level should be hyphened when used as an adjective (sea-level change Vs. change in sea level).**
Corrected

2. **Verbal times: In the introduction the authors use present (''the objectives are " (L75)); they use past tense in Section 2 ('we used a global ocean model" (L97)); future tense when presenting what is showed in the results ("First we will compare" (L139)); and back to present tense in Section 4. I recommend the authors to either keep it constant (i.e., using always present or past tense), or change the tenses accordingly to the Section, but keep it constant throughout the Section (i.e., present in the introduction,**

past in the methods (since it's something they already did), and present in the results and discussion, for example).

Corrected

3. **The terms 'i.e.' and 'e.g.' are usually followed by a comma.**

This seems to be handled differently by different style guides. American guides tend to suggest the comma while British styles tend to omit it. We followed the British style and therefore don't put a comma beyond both abbreviations.

4. **Use of one-sentence paragraphs (for example, L258-259, L289-290) or very short paragraphs (e.g., L71-73, L84-86) should be avoided.**

Corrected

Specific comments

1. ENSO and PDO are not defined (neither in the abstract or the main text). Although they are well-known acronyms, it should be defined.

Corrected

2. Whenever talking about sea-level trends, it is important to mention the period that it referred to. For example, L24 a trend is given, but no period. Also the first sentence of the paper is 'over the last three decades', but then the older reference is 2013, so I assume you mean 1980-2010? Not including 2010-2020? Adding the period to the sentence would clarify this.

Clarified

3. L22 you mention that some areas in the western tropical pacific have received a lot of attention. Maybe adding a more specific example would be nice.

We refer to the "western tropical Pacific" and don't see where specific subregions are mentioned. Examples for studies are given after the first sentence of the paragraph.

4. L34: You mention the 'Luzon Strait'. I have no idea where it is. Having a study are map, with such names would help.

Please see response to your general comment No. 1.

5. L35 'might amplify the signal'. Which signal?

We refer to the ENSO-related SSH anomalies mentioned in the preceding sentence. We rephrased the sentence accordingly.

6. L43-44: is the ITF variability governed large-scale climate events (like ENSO and

PDO), or by the climate indices (like Niño3.4)?

We changed the sentence to:

The ITF variability on interannual time scales is mostly a response to variations of the Pacific trade winds and governed by large-scale climate modes.

7. L52: Wijffels and Meyers showed (instead of 'could show'), based on XBT observations, that sea-level anomalies... Would also be good to mention which period of observations they used.

1984-2001. Corrected

8. L57: You mention 'mass and steric components'. I think it would be good to define to the reader what exactly are these components (not everyone will know). And mainly the steric contribution could be better explained, as you discuss it further on your results.

Please see our reply to your general comment No. 6.

9. L61: are you talking about sea-level trends? ('reconstructed decadal trends during the...'). Make it clear to the reader.

Yes, sea-level trends. Clarified.

10. The term halosteric appears for the first time on Line 67, and then again with the term thermosteric in the objectives of the paper (Line 83). But you didn't introduce what thermo and halosteric changes are. Although the readers of Ocean Sciences might be familiar with it, in my opinion you could briefly define thermo and halosteric changes before having it as one of the objectives of the paper.

We rephrased the third objective (line 83) to avoid the term "halosteric" and introduced it in the following section.

11. Line 75 should be connected to the previous paragraph.

Corrected

12. Line 104 should be connected to the previous paragraph.

Corrected

13. Lines 104-106: Won't this freshwater budget correction affect then your halosteric analysis?

Yes, this could affect halosteric sea level. We have the freshwater flux of both corrections available and find their variability in the region at least an order of magnitude smaller than the net freshwater flux. Even in the coarse resolution experiments that employ a stronger restoring than the high resolution experiment. We therefore assume the effect on halosteric sea level to be very small.

14. L122: is there a specific reason for using the years 1990-1991 as your climatological forcing?

Yes, that is the period recommend by Stewart et al. 2020. We clarified this in the text.

15. L129: Add 'respectively' at the end of the sentence: 'isolate the momentum forced and buoyancy forced variability, respectively'.

Corrected

16. L134 should be connected to the previous paragraph.

Corrected

17. L139: Here you start about your validation with observations. It's important to highlight that you are using satellite altimetry observations, and that they only start in 1993

Please see our reply to your general comment No. 6.

18. L143: Here and in the rest of the paragraph I think you meant figure 1?

Yes, corrected

19. L149: biases in relation to what? Model biases? Make it clearer

Clarified

20. L151: What is the reference period of the mean fields? Is it the same as the model biases (which I am assuming is for the entire period?)

WOA18 Data is available from 1955 to 2017 and the mean field show a long-term average over this period. Model biases are computed over the full integration period of the models (1958-2016). Details are added to the figure caption.

21. Figure 2: Here locations where you don't have data (which is a lot of the AMS actually) has been plotted with the same color as land, which was confusing. So I suggest you to plot it in a different color. I was also left wondering why you don't have data there (I can understand from the observations, if are areas that are too shallow... but I would expect the model to have values there as well). So it might be worth adding one sentence about it in the main text and/or in the caption of the figure.

Figure 2 shows an average over the upper 400 m and data is only shown where the water is at least 400 m deep. All other areas are marked as land. We clarified this in the caption.

22. L162: I found the word 'confirming' a bit of a bold statement, I suggest replacing it with 'indicating'.

Changed "confirming" to "indicating".

23. L164 and Figure 3: Here you sat that you are using different colorbar for the same figure. I can understand why you want to do this, since if you keep the range of panel d and e to 0-4 we won't be able to see the patterns... But I still find it a bit misleading. Even though the colorbars are indicating that they range only up to 1, I find it harder to compare the panels, which is a bit the point of the plot. So it's up to you, but I would suggest fixing the colorbars throughout the figure.

We agree that the different colourbars can be misleading. However, as you state correctly, using the same colourbars in all panels makes it impossible to distinguish any patterns in panels d) and e). In particular, the pattern in panel e) is an important result. We deliberately choose the ranges of 4 and 1 to facilitate the comparison (same colour means difference by a factor 4). We therefore suggest keeping the different colourbars.

24. Both paragraphs at L167-179 are just talking about REF025 experiment. What about REF005?

The paragraphs are on the sensitivity experiments that are only available for the REF025 experiment.

25. L185: 'e.g.' should be followed by a comma. And the first name of the authors of the reference are appearing (Thomson, R.E.)

Please see our reply to your technical comment No. 3 regarding the comma. We fixed the reference.

26. L190; Bring the reference to Figure 4 here ('REF020 and REF005 show a similar response to positive ENSO cycles (Figure 4.a,b)'.

Changed

27. Figure 3: Why are the grid lines here marking only 10˚N, 0˚ and 10˚S? And the longitudes are on a different spacing than in Figure 1 and 2. This is very small detail, but you could use always the same gridline spacing.

Changed

28. L200: Bring the reference to Figure 4d.e. here: 'the linear response to the PDO index (Figure 4.d.e.)...'

29. L200: I think you meant a 'and' here instead of 'to': 'strong amplitudes of 3 and 4 cm'.

Indeed

30. L212: Figure 5d is about PDO... I thought you were talking about ENSO, so figure 5c?

Changed

31. L2017: 'PDO does not drive any buoyancy flux driven variability', in the AMS right?

Because I can seem some variability on the surrounding seas around the AMS.

Changed accordingly.

32. L224: How do you know that the mass fluctuations are small? Is this based on previous research, if so, then reference it.

This statement is based on our own research. But the claim is also in line with previous research. We added a reference.

33. Figure 5: Add variable name to the colorbar (R-squared), and also mention in the caption which coefficient you are showing (R-squared)

Corrected

34. L233: halosteric anomalies complement (not amplify) the thermosteric signal (it is not because of the halosteric variation that the thermosteric signal will be higher, but you will have a total steric change that is higher).

Corrected

35. L245: Remove 'does': changes during PDO cycles also manifest in the vertical profiles'.

Corrected

36. Figure 6 caption: linear regression with (instead of to); move the 'of upper ocean currents' to the end of the sentence or to another sentence (right now it seems you made the correlation with the currents, but I believe you are just showing the currents). And are those the mean current velocity? Make it clearer.

The figure shows a regression of currents and SSH with ENSO and PDO index.

37. First paragraph of 'Summary and conclusion' should be connected to the second one.

Corrected

38. L269: pressure gradient between the Pacific and (not in) the Indian Ocean.

Corrected

39. L270: Remove 'again'.

Corrected

40. Line 289 should be connected to previous paragraph.

Corrected

41. L306: Add 'current' with Kuroshio here and in the following mentions of the current (Kuroshio current flows...)

Corrected

42. L324: 'McGregor demonstrated (not could demonstrate).

Corrected

43. General conclusion statement: The resolution effect is clear for the biases, but it doesn't seem to have had a significant impact in your SSH variability analysis. I'm not sure if your final and general conclusion should be about this.

Please see our reply to your general comment No. 5.

References

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Liu, Q.-Y., Feng, M., Wang, D., and Wijffels, S. (2015), Interannual variability of the Indonesian Throughflow transport: A revisit based on 30 year expendable bathythermograph data, *J. Geophys. Res. Oceans*, 120, 8270– 8282, doi:10.1002/2015JC011351.

Tomczak, M. and Godfrey, J. S.: Regional Oceanography: an Introduction, Daya Publishing House, Delhi, 2 edn., 2003.

Xue, P., Malanotte-Rizzoli, P., Wei, J., & Eltahir, E. (2020). Coupled Ocean-Atmosphere Modeling Over the Maritime Continent: A Review. Journal of Geophysical Research: Oceans, 125(6). http://doi.org/10.1029/2019JC014978