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

General Comments

Overall, "Decadal sea level variability in the Australasian Mediterranean Sea" is very well constructed, explained, and is an important contribution to our understanding of how climate variability impacts regional sea level. Specifically, the authors use an ocean model to determine how ENSO and PDO impact decadal regional sea-level variability in the Australasian Mediterranean Sea (AMS), determine the contributions of momentum and buoyancy fluxes to sea-surface height (SSH) variability, and further disentangle the steric variability into thermo and saline components. I do not see any major flaws in methodology or reasoning, and as such, recommend acceptance after addressing some minor issues mainly related to presentation.

Specific Comments

1. **It would be very helpful to include an overview map of the study area, with the different seas and currents discussed clearly marked. The labels in Fig. 1a were helpful but not sufficient in this regard.**

We added a map of the region as a new Fig. 1.

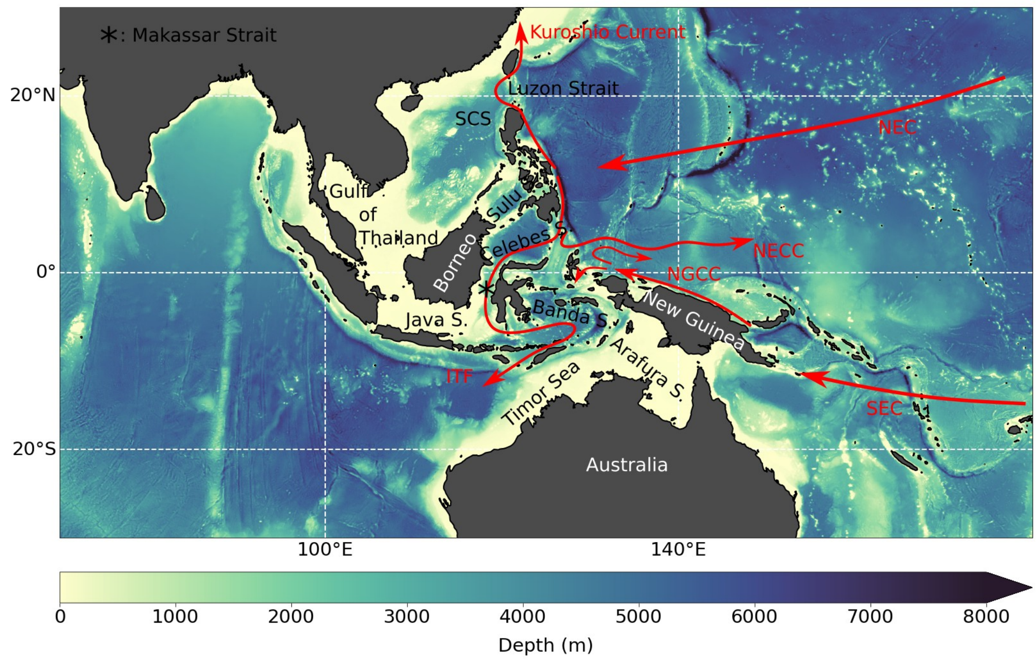

Figure 1: Overview map of study domain, depicting the marginal seas and schematic currents discussed in the text. Colourshading indicates depth. Marginal seas, islands, continents and schematic currents that are mentioned in the text are also marked. SCS: South China Sea, NEC: North Equatorial Current,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

2. **Some of the markings on the figures are difficult to see, e.g. the labels and contours in Fig. 1, current vectors in Fig. 6. Switching grid colors to black and others to white may be one way to remedy this.**

As described above, we included an overview map as a new figure. Regarding Fig. 6, we decreased the number of arrows shown and increased their size.

3. **There are some repeated panels between figures. Perhaps some of the redundancy can be removed (though admittedly, I don't see an immediately obvious way). If not, please clearly mark where repeated.**

Yes, the colorshading in panels a) and d) of Fig. 4 are repeated in the same panels of Fig. 6. We agree that this is not strictly necessary. However, both figures support very different lines of reasoning and it makes it easier for the reader to follow our arguments to have all necessary charts in one figure. Combining figures 4 and 6 into a single figure seems unreasonable as it would result in a multipage figure. We therefore decided to repeat these two panels for the benefit of the reader. As you pointed out, we failed to mention the repeated figure and included a remark in the caption of Fig. 6.

4. **As you indicate they are not the same, what time step do the coarse and nested grid run at?**

The base model and the nest were integrated with time steps of 15 and 5 minutes respectively. We included this in the model description.

5. **I'm glad you talk about the uncertainty in the atmospheric forcing product at the end. Perhaps you could add a justification as to why JRA55-do was chosen relative to another reanalysis product?**

Unlike other reanalysis products, JRA55-do is specifically designed to be used as an atmospheric forcing for OGCMs. It is of course based on an atmospheric reanalysis, but its surface fields are adjusted with respect to observations to resemble reality as close as possible. It also aims to provide closed heat and freshwater budgets (with respect to a given set of bulk formulas) to avoid model drift. We therefore favoured JRA55-do over other reanalysis products. We also acknowledge that this choice is somewhat arbitrary. We extended the model description to include these points.

6. **I agree that the linear separation of WIND and BUOY is justified on the spatiotemporal scales you are looking at, but a further note clarifying what scales you expect this to break down would be useful to a more general readership (which this manuscript attracts, through the implications for sea-level projections).**

The manuscript is concerned with interannual to decadal variability where we assume the linear separation to be valid. We do not suppress variability on timescales shorter than that so that the assumption is not valid on timescales shorter than one year. Furthermore, we would expect non-linearities to be of increasing importance with decreasing spatial scales and that the contribution of non-linear dynamics is not negligible for the mesoscale and below. We add a sentence to this respect to the methods section.
The length of our experiments prevents us from extending our analysis beyond the decadal to multidecadal scale. To which degree a quasi-linear superposition of wind- and buoyancy-driven

signals remains approximately valid on longer timescales, must remain speculative: for example, wind-induced trends in SST could have increasing ramifications for surface heat fluxes, and effects of non-linearities could lead to amplification of small initial errors over the course of multi-century integrations, eventually obscuring the interpretation of individual sensitivity experiments.

7. **Along these lines, I find the CLIM experiment quite interesting, and a nice way to capture the remaining variability. What's not clear to me is the relative role of the seasonal cycle vs nonlinear interactions in 'intrinsic variability. The discussion of eddy kinetic energy seems useful to this end, and I wonder if it could be quantified in the important regions (e.g., the central South China Sea). I freely admit that this discussion is outside my area of expertise, so apologies if this is not a well-posed comment.**

We would like to point out that the CLIM experiment does include a (repeated) seasonal cycle to avoid a possible misunderstanding. The seasonal cycle drives oceanic variability, and some of it might be subject to an upscale energy transfer and energize variability on longer timescales. Independent of this is the generation of intrinsic variability in CLIM related to non-linear ocean dynamics (e.g., the energy cascade of quasi-geostrophic turbulence). However, we can not quantify the individual contributions. Even the EKE, if based on the usual definition, includes both the contributions from forced intra-seasonal variations and dynamic instability processes, and hence appears of limited value for rigorously identifying their relative roles. Apart from that, the EKE (especially in the more energetic areas) represents a useful measure, and we already included this in our discussion about possible connections between the SCS and the Kuroshio boundary current region. We are unsure how our analysis could benefit further from a quantification of EKE in the SCS. Please feel free to clarify your remark.

8. **Can you elaborate on the methodology around line 175? Why did you not just calculate the variability contribution from BUOY? If you do that (assuming you can), does it compare well with the REF025-WIND contribution?**

Assuming, REF025 is a linear superposition of WIND and BUOY, both ways should yield the same result. This is not strictly true because all experiments include intrinsic variability. We therefore looked at the difference between REF025 and WIND rather than BUOY to estimate the effect of buoyancy fluxes. When using BUOY directly, we find a much larger contribution of 24% which is likely due to intrinsic variability that is not negligible, in particular in the SCS (Fig. 3e, f).

9. **In your concluding sentence, you comment on the resolution of coupled circulation models. My initial thought would be that yes, coupling would indeed be important for sea-level projections as ENSO/PDO are subject to change, but that your findings would be largely robust. A comment to this effect would be good to see.**
   - **It would also be nice to wrap the paper up on a positive note, rather than the warning.**

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We agree that a climate model would be more suited to analyse coupled ocean-atmosphere dynamics, and it would be interesting to repeat the regression analysis with output from a historical climate model run that provides sufficient resolution and compare the results. A possibility to generate high-resolution sea level projections without the need to run a climate model could be downscaling. Essentially, an uncoupled model is forced with the combination of the atmospheric long-term clime change signals (obtained from a climate model) and the high-frequency part of the historical forcing. Examples are Sun et al. 2012 or Feng et al. 2017. We used our last sentence to point to this approach.

Technical Corrections

- **When used as an adjective, e.g. "sea-level variability", sea level should be hyphenated (sea-level).**

Corrected

- **In the first paragraph of the results, references to Fig. 1 are incorrectly marked as Fig. 3.**

Corrected

- **On line 178, I think you mean sea-level variability (not just sea level).**

No, we refer to the positive trend between the early 90s and 2010 that constitutes a strong rise in sea level.

- **L. 247, missing parenthesis before Fig 3.**

Corrected

<u>References</u>

Feng, M., Zhang, X., Sloyan, B., and Chamberlain, M. (2017), Contribution of the deep ocean to the centennial changes of the Indonesian Throughflow, *Geophys. Res. Lett.*, 44, 2859– 2867, doi:10.1002/2017GL072577.

Sun, C., Feng, M., Matear, R. J., Chamberlain, M. A., Craig, P., Ridgway, K. R., & Schiller, A. (2012). Marine Downscaling of a Future Climate Scenario for Australian Boundary Currents, *Journal of Climate*, *25*(8), 2947-2962.