

Ocean Sci. Discuss., referee comment RC1
<https://doi.org/10.5194/os-2021-63-RC1>, 2021
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Comment on os-2021-63

Brett Buzzanga (Referee)

Referee comment on "Decadal sea-level variability in the Australasian Mediterranean Sea"
by Patrick Wagner and Claus W. Böning, Ocean Sci. Discuss.,
<https://doi.org/10.5194/os-2021-63-RC1>, 2021

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

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

- 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.
- 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?
- 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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Technical Corrections

- When used as an adjective, e.g. "sea-level variability", sea level should be hyphenated (sea-level).
- In the first paragraph of the results, references to Fig. 1 are incorrectly marked as Fig. 3.
- On line 178, I think you mean sea-level variability (not just sea level).
- L. 247, missing parenthesis before Fig 3.