Ocean Science Discussions

Ocean Sci. Discuss., author comment AC2
https://doi.org/10.5194/os-2021-31-AC2, 2021

Reply on RC1

Patrick Wagner et al.

Author comment on "Contribution of buoyancy fluxes to tropical Pacific sea level variability" by Patrick Wagner et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-31-AC2, 2021

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.

The experiments and results conducted by the authors to estimate the effect of buoyance fluxes on the tropical Pacific sea level were interesting and well supported the conclusions of the claims. However, it is necessary to explain some terminologies and experiments further and use a more quantified language to interpret the experimental results.

Major comments

1. There is not enough description of the buoyancy flux in the title. Readers without prior knowledge cannot understand at all. It is necessary to introduce what buoyancy flux means.

We agree that an introducing of buoyancy fluxes is missing. We will extend the introduction accordingly.

2. The results of O025-W90 and O025-B90 represent momentum and buoyancy flux effects, respectively. However, there is too little explanation for each of these cases. Authors need to explain to both models so that readers can get a rough understanding of what each of these results means without reading the bibliography.

Point taken. In addition to a proper introduction of buoyancy fluxes, we will extend the model description of the sensitivity experiments and clarify the purpose of these experiments, add specifics on the method and point out limitations of this sort of experiments.

3. There are more strong ENSO events 1982/83, 2015/16. If the authors provide one more analysis, the authors will be able to convey the claims more clearly. There is a limit to reaching generalized conclusions through a single case study.

We agree that it is useful to check if the mechanism identified for the 1997/1998 El Nino are also active during other events. In general, we find this to be true as buoyancy flux

forced anomalies appear during most ENSO event in the central, tropical Pacific. The relative contributions of halosteric and thermosteric anomalies varies over time, but this is also visible in figure 6 and has been discussed there. We failed to mention this in the manuscript and will extend it accordingly.

4. Figures 6, 7, and 8 are very important. However, it is confusing because the pictures are not properly marked. Add lines and boxes to make the pictures easier to read.

We will add additional labels to make them easier to read.

5. I generally understand and agree with the authors' claims. By the way, the language used for comparison is not clear (especially section 3 results). It is necessary to quantify the comparison, and if it is difficult to quantify, please provide more details in what respects they are similar or dissimilar, or triggered.

We see the point and suggest three modifications in this respect:

1. Quantify the change of variability shown in figure 3 and give numbers in the text. Also modify figure 3 to show a discrete colormap that enables the reader to infer values from the figure and follow our argument. This also relates to your comment below.

2. Quantify the impact of buoyancy fluxes by giving the relative change in variability between O025-HC and O025-W90. We already included this for the low-frequency variability in the western box and will extend it to both boxes and also to the interannual variability.

3. We already compared the amplitudes of variability for halosteric and thermosteric SSH shown in figure 5. We suggest extending this comparison by giving correlation coefficients.

Miner comments L21: Add a description of "Ocean atmosphere buoyancy fluxes." before using this term.

We replaced the term buoyancy fluxes at this early stage by "heat and freshwater fluxes" and introduced it properly in the second to last paragraph of the introduction.

L24: Define SLC before using.

Following a suggestion by referee #2 we avoided the term SLC altogether as it is commonly used for decadal to multidecadal variability and trends. We used the term “sea level variability” in this case.

L76: Is the meridional dipole right what the authors are trying to explain? It seems to be explaining the zonal dipole. If the authors try to explain the meridional dipole, please make it more clear.

Thanks for catching this error. It should read “zonal dipole”.

L84: "In all cases, the correlation coefficient is over 0.95", but in the case of "3", 0.95 is unreasonable. Please check it.

We double-checked the correlation and can confirm that correlation for box “3” is at 0.98. However, the correlation for box 2 is only 0.93. We will correct the sentence accordingly.

L91: it is helpful to show SPCZ on the map.

We would like to keep the figures as simple as possible and avoid adding additional lines. Instead, we suggest removing the reference to the SPCZ in the text and refer to the region as “southwestern tropical Pacific”. This would also be consistent with the preceding paragraph and the naming of the boxes in figure 1.

L97-98: A bibliography is needed.

Yes, we will include references.

L115-116: "Changes are mainly limited… in O025-W90." It is difficult to agree with the argument by judging by the colors only.

We will change the colormap to a discrete colormap and mark individual steps with contour lines.

L133-137: It is difficult to accept the argument from a comparison of two temporal windows only. It is recommended to make a moving calculation window and show the change of SD.

A moving windows analysis gives a decline of SD of thermosteric SSH of about 0.1cm/10years since around 1985. We will include this in the text. Because this is only a minor finding which we do not pursue any further in this study, we suggest to not add another plot.

L139-141: Please provide a visual comparison with the ENSO index (simply just add up any ENSO relating indices). There is a limit to generalizing to only one event.

We agree that this would be helpful. We will add the Nino34-index computed from the model to figure 6b)and point out the correlation in the text.

L167-168: "These anomalies… . (Fig. 7b)." I don't understand. An additional explanation is required.

Sorry for the poor phrasing. We will reformulate this sentence to be more concise.

Figure 1: "SD" needs to be predefined before use.

Corrected