Ocean Science Discussions

Ocean Sci. Discuss., author comment AC3
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Reply on RC2

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-AC3, 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 authors revisit the effects of surface buoyancy fluxes on sea level variability in the tropical Pacific, a topic that has received comparatively little attention in the literature. Following up on Piecuch and Ponte (2012) and others, they use three different numerical experiments with an eddy-permitting (1/4 degree horizontal grid) to separate out the role of surface wind and buoyancy forcing over an extended period (1958-2016). The results are an interesting contribution to the literature, confirming the importance of buoyancy fluxes in several areas of the tropical Pacific, their excitation of Rossby waves and related dynamic sea level signals, and pointing out the influence of both heat and freshwater fluxes at different (interannual to decadal) time scales.

The conclusions of the paper are reasonably well supported by the analyses shown, but there are a couple of issues that need to be discussed in the manuscript. In trying to separate wind and buoyancy effects using the experiments with full and climatological forcing described in section 2, there is always the issue of nonlinearity, as discussed for example by Piecuch and Ponte (2012). Moreover, given the eddy-permitting nature of the runs used, it is also not clear how much of the differences in variability between runs with full and climatological forcing are due to "chaotic intrinsic" eddy-related processes as discussed by Carret et al. (2021) and references therein (not cited in the current manuscript).

Piecuch and Ponte (2012) seem to imply significant nonlinear effects in some of the regions discussed in the current paper. Carret et al. (2021) point to generally weak effects of intrinsic variability relative to atmospherically forced variability in the tropical Pacific at interannual time scales. Although the three runs used by the authors do not permit addressing these isues, the manuscript should nevertheless acknowledge and discuss them explicitly.

[Carret, A., Llovel, W., Penduff, T., & Molines, J.-M. (2021). Atmospherically forced and chaotic interannual variability of regional sea level and its components over 1993–2015. Journal of Geophysical Research: Oceans, â□¨126,

e2020JC017123. https://doi. org/10.1029/2020JC017123]â□¨

We will include the results of a climatological experiment (O025-RYF90), where we used the repeated year forcing approach to replace the entire atmospheric forcing, to address this issue. This experiment allows us to quantify the role of intrinsic variability. We found that the intrinsic variability accounts for less than 5% of the interannual variability in the tropical Pacific and we will include an additional figure to show this. Please see the attachment to this comment for the new figure.

Quantifying non-linear effects is not straight forward. In the absence of non-linear effects and intrinsic variability, the linear superposition of anomalies from O025-B90 and O025-W90 should be equivalent to the results from O025-HC. We can get a rough estimate of the importance of non-linear effects by comparing the root-mean-square error (RMSE(HC-W90-B90)) to the SD of the climatological experiment. We did this for the anomalies shown in Fig. 3, and we found that the RMSE outweighs the SD of RYF90 in the western tropical Pacific but not in the eastern basin. This suggests that non-linear effects might be important in the western part of the basin. We will include this in the manuscript and also discuss our findings with respect to existing literature.

The manuscript contains many typos and careless errors, repeated at places several times. I have tried to point these out in the long list below, although I probably did not get them all. Needless to say, the authors should have proofread their manuscript more carefully and need to do that before submitting a revised version.

We apologize and will carefully proofread our manuscript before resubmission.

Other comments by line number

l11 Delete comma after "both"

Corrected

l16-17 Broken sentence: I suggest a colon, instead of a period after "processes". In addition, "melting of land ice" is not the only reason the ocean's total mass changes. Imbalances in precipitation, evaporation and river runoff can also be contributors, depending on time scale.

We will modify the sentence accordingly.

l24 Sea level change (SLC) normally refers to long term (multidecadal or centennial) variability. Here and elsewhere in the paper, perhaps you want to use the more general term of sea level variability, which can include shorter time scales of relevance to the paper.

We will change this throughout the manuscript.

l29 Define all acronyms on first mention.

Corrected

l48 "…that allow…"

Corrected

l53 "…ocean general circulation…"

Corrected

l63 "…a relaxation timescale…" and remove comma after "correction"

Corrected

l70 "May 1990"

Corrected

l65-71 Not exactly clear what the forcing is and why May 1990 to April 1991 is chosen. In particular, forcing could still contain interannual variability (e.g., if there is a long term trend, it will have a jump at the wrapping date of April 30, which adds energy at most frequencies including interannual). I guess the particularly period chosen is trying to avoid these effects, but there should be more explicit discussion of these issues in the paper.

The period from May 1990 to April 1991 (rather than January to December) is indeed chosen to minimize sudden changes in the forcing that might introduce spurious transients. The year is chosen because it resembles a "neutral" year with respect to several climate indices such as SOI, NAO and SAM. Stewart et al. 2020 tested three different periods (1984, 1990, 2003) with three different model configurations. They find that inter-model differences are much larger than inter-forcing differences between these three periods. They conclude that the choice is therefore not critical but give a general recommendation for the 1990-1991 period. We followed this recommendation.

We would like to avoid a detailed description of the procedure and its motivation but refer to Stewart et al. 2020 instead. However, we agree that the paragraph is difficult to understand for someone not familiar with the cited references. We will rephrase it accordingly and give more details.

l74 Capital B on Boussinesq

corrected

Figure 1 The reader needs to be told what altimeter data is used (the link to CMEMS is not enough), and whether the model results in (b) are calculated over the same altimeter period. It is also awkward to say "interannual SD of SSH". What you have is SD of SSH series that have been smoothed with 12-month running mean.

We will include a complete reference to the altimetry data and provide additional information in the caption of figure 1.

l76 "Meridional dipole" should be "zonal dipole" as used in the rest of the paper.

Corrected

l81 Any criterion for choosing these particular boxes, other than being generally over the regions of enhanced variability? Are the results sensitive to box boundaries? This could be discussed in the text.

We chose these boxes as we consider them representative for the variability in the region, but we acknowledge that the choice is somewhat arbitrary. However, the result is not sensitive to the exact choice of the boxes, as long as they do not cover the boundary current regions with high mesoscale activity. There, the model performance is reduced due to an insufficient spatial resolution. We will include this in the text.

l99 Cite relevant works.

Corrected

l100-105 What about the maximum values seen in the northern most latitudes of the domain shown in fig 2?

We neglected this region in our analysis because of intrinsic variability. The new figure mentioned above illustrates this. We will point this out in the manuscript.

l108 "assess"

Corrected

figures 1,2,3 The color of land is rather similar to actual values being plotted. The land could use some other less confusing color. I assume all the plots are based on 12-month smoothed series as in fig 1, but this should be made clear in the text or captions.

Changed accordingly

l109 Why "absolute change"? Not clear what is meant by "absolute".

Absolute changes in contrast to relative/percental change. However, we agree that the attribute is not needed and might cause confusion. Will be removed.

figure 3 Caption should state SD of x minus SD of y. This way the reader can be clear on what the sign of the values means.

Changed accordingly

l110 Move "is removed" after "forcing" on l111.

Corrected

l113 "...E on both sides..."

Corrected

l119 "...effect of halosteric and thermosteric SSH..."

Changed accordingly

l124 "In phase" means correlation. If they are anticorrelated, you should use out-of-phase.

Corrected

l127-128 In this case, the phase/correlation statement is redundant and should be rephrased.

Changed accordingly

figure 4 caption Not 12-year but 12-month running mean.

Corrected

l130 I would say figure 2 only suggests this interpretation. You have not done the experiments to strictly separate the effects of heat and freshwater fluxes and determine if both play a role.

We will change “indicates” to “suggests”.

l136 Should be “0.75 cm and 0.59 cm”

Indeed

figure 5 caption Again you mean “12-month” running mean?

Corrected

l139-140 Again correct the numbers.

Corrected

l162 Somewhat odd numbering of one section 3 with only one subsection 3.1. I would number this section 4.

Changed

l156-158 Refer to the relevant figures behind this summary statement for the benefit of the reader.

Changed accordingly

l164 Text should clarify how the “anomalies” are defined.

Anomalies are deviations from the seasonal climatology. We will include the definition.

l168-171 There is an implicit assumption here that freshwater flux is the only way to generate halosteric anomalies, but that is not necessarily true. For example, heat flux could drive flows that advect both temperature and salinity fields and generate salinity anomalies. In fact, the observed compensation between halosteric and thermosteric anomalies suggests some adiabatic advective mechanism along isopycnals.

This is of course true and we will clarify this.

l176 Refer to Fig. 8b, not 8c?

Corrected

l182-183 This seems to be the first mention of monthly output used for the analyses. The information should be provided much earlier in the paper (section 2).

We will include this in the model description.

l210 “varies in phase”…see comments on l124.

Corrected

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2021-31/os-2021-31-AC3-supplement.pdf