Ocean Science Discussions

Ocean Sci. Discuss., referee comment RC2
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# Comment on os-2021-31

Anonymous Referee #2

Referee 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-RC2, 2021

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□¨

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.

Other comments by line number

l11 Delete comma after "both"

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.

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.

l29 Define all acronyms on first mention.

l48 "…that allow…"

l53 "…ocean general circulation…"

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

l70 "May 1990"

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.

l74 Capital B on Boussinesq

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.

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

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.

l99 Cite relevant works.

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

l108 "assess"

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.

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

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.

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

l113 "…E on both sides…"

l119 "…effect of halosteric and thermosteric SSH…"

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

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

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

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.

l136 Should be "0.75 cm and 0.59 cm"

figure 5 caption Again you mean "12-month" running mean?

l139-140 Again correct the numbers.

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

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

l164 Text should clarify how the "anomalies" are defined.

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.

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

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).

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