Weather Clim. Dynam. Discuss., author comment AC3
https://doi.org/10.5194/wcd-2022-51-AC3, 2023

Reply on RC1

Lina Boljka et al.

Author comment on "Identifying quasi-periodic variability using multivariate empirical mode decomposition: a case of the tropical Pacific" by Lina Boljka et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-51-AC3, 2023

We thank the reviewer for careful consideration of our manuscript and their constructive comments that have helped improve the original manuscript. Note that line numbers below in our responses refer to the revised manuscript.

Major comments:

Sect. 3.1 and Fig. 2: Why do you only show IMF13 in Fig. 2 and not IMF12? Maybe IMF12 represents the nonlinearity of ENSO as EOF-2 does? Please show the same analysis for IMF12 as shown for IMF13 in Fig. 2, at least as supplement so that one gets a feeling how these two modes interact with each other to represent ENSO.

We have now put IMF12 in the supplement (see Fig. S2) and referenced it in the main text (e.g., l. 328). The main reason for not showing IMF12 initially was because it looks virtually the same as IMF13 (as stated in the manuscript; see l. 252-253, 345-346) but has smaller amplitude and much smaller region of significance (now further emphasised; e.g., l. 854-858). The issue is that both modes are composited over the Nino3 index (virtually the same as EOF1/PC1), which is of interest in the present study. Thus, we pick up East-Pacific (EP) ENSO, not, e.g., Central-Pacific (CP) ENSO or EOF2/PC2 (i.e., we do not pick up different flavours of ENSO). However, if one looked at the temporal (spatial) evolution of IMF12/13 (i.e., not composite) we believe we could see some interesting (spatial) evolutions, since the method is non-stationary and should be able to identify different patterns for each ENSO event.

Sect. 4.1 Model evaluation: Please show same analysis as shown in Fig. 1 & 2 for your climate model so that one can see how well these modes represent ENSO in the climate model and to highlight similarities and differences!

The analysis has been added in the supplement (see Figs. S3-S5). We also provided an equivalent of Fig. 5 in Fig. S6. The main text has been amended accordingly (see l. 516-518, 520-521, 536) and further references to these Figs. have been added where relevant (see track changes). There are differences in the model that primarily stem from model biases (e.g., ENSO SST warming extends further west; amplitude is too strong likely due to too strong periodicity; etc.), but overall it looks similar (ENSO-like).

Minor comments:

L47-58: "This feedback alone would result in continuous warming (cooling) in the eastern tropical Pacific, therefore negative feedbacks are necessary for quasi-oscillatory behaviour in the eastern tropical Pacific ..." I am missing here the discussion of the contribution of the seasonal cycle for the growing and decaying of ENSO events, as positive feedbacks are in general stronger in boreal autumn and the negative feedbacks are stronger in boreal spring (Wengel et al. 2018). Is this seasonal forcing included in these conceptual models? The seasonal cycle is a very important contributor of growth and decay of ENSO.

We agree that in general the seasonal cycle matters for phase-locking of ENSO. However, these oscillator models are very simple and do not (to our knowledge) include seasonal cycle, though one could imagine a more complex model of ENSO that includes it (e.g., Stein et al 2010). Alternatively, the seasonal cycle may still be present indirectly as it helps phase-locking the ENSO signal, which peaks in winter in data with the seasonal cycle removed as well.

In this study, we removed the seasonal cycle from the timeseries analysis and as such we do not really consider it (see l. 135-142). We further emphasise this on l. 142-144; we have also added a note on the importance of the seasonal cycle in the introduction (see l. 60-62).

L103 & L106: I do not understand why you state here "not shown" in the introduction. What don't you show? Please make clear or delete the "not shown".

Any reference to NAO, MSLP etc. has been removed (except for conclusions/future work).

Fig. 3: Please improve the legend in Fig 3, as it is hard to distinguish solid, dashed and doted lines in the legend.

We have updated Figs. 3,4,6 with clearer legends.

L302f: "and deeper thermocline in Niño6 region" In Fig. 2a the thermocline is deepest at the equator in the western Pacific and not north of the equator. Please change.

In this phase, thermocline reaches its deepest "phase" across the western Pacific (including Nino6). We have amended the text accordingly (see l. 336, 647).

Fig. 4 caption: Why do you give a range for the high-pass and low-pass filter? Normally you just give one cutoff period.

The reviewer is correct. Here, we merely take a range of higher and lower frequencies around the intermediate quasi-periodic band to show how relationships between variables can change. This means that we do not take a high/low-pass cut-off, but rather we use 2 additional band-pass filters of different (higher and lower) frequencies (consistent with IMF11 and IMF14). We have clarified this in the caption of Fig. 4 and we used "higher"/"lower" frequency or "longer"/"shorter" period instead of "high"/"low" frequency (e.g., l. 444, 458).

L516f: "Similarly, predictions from tropical Pacific mean thermocline depth are not considered here as they did not improve predictions (not shown)" This sounds strange to me as the mean thermocline depth gives us the predictability of 6 months in climate model predictions. Please explain why you think that it reduces the prediction skill here!

L546f: Similar as above: Why is prediction with only SST better than with all variables in a)? This sounds strange to me. Please explain!

We address the two comments together: This was surprising to us as well. Perhaps it is because other variables can have other processes embedded in them - like deep-ocean dynamics (thermocline) or high frequency atmospheric processes (wind stress). These can then interfere with a statistical prediction of SSTs, which is computed on raw (unfiltered) data. It may also be that wind stress and thermocline are better represented as red noise even though SST (Nino3) is periodic, which may then act to weaken the predictability. Also, we use a very simple linear statistical model and as such it is rather hard to compare it to a dynamical model. Note, however, that the main point here is that smoothing of the predicted signal within the 2-4.5 year band yields good prediction-skill due to quasi-periodic nature of ENSO on those timescales (regardless of variables used).

We have added clarifications along those lines (see l. 589-596).