Weather Clim. Dynam. Discuss., referee comment RC2
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Comment on wcd-2022-51

Anonymous Referee #2

Referee 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-RC2, 2022

The study applies multivariate empirical mode decomposition (MEMD) to analyze four variables associated with ENSO. Two intrinsic mode functions (IMFs) derived from the MEMD have their characteristic time scales of about 3 years, that match with ENSO variability. The time series of IMFs indicates a quasi-periodic ENSO variability and is consistent with oscillators. The authors demonstrate a novel pathway to explore the intrinsic dynamics of ENSO, and I believe that the article could be published. However, I have some concerns about the details of the methodology and suggestions for writing.

Major comments:

1. The author clearly introduces the MEMD method in the appendix. However, in section 3, the author jumps to IMF12 and IMF13, and I am a little bit confused. Hence, I suggest inserting a section (or a section in the appendix) to summarize more details about all the IMFs. The authors should present the time series of all the IMFs, and enlist a table with characteristic frequencies for each IMF before the discussions of IMF12 and IMF13. Also, the author needs to clarify more about Fig. B1. For example, given the doubling periodic of each consecutive IMF, it is reasonable to have 10 IMFs using the EMD for ~100-year monthly mean data (Fig B1a, B1b). However, why are there 22 IMFAs from the analysis of PC1 (Fig B1c) and 21 IMFAs from the Nino3 (Fig B1d)?

2. I also suggest that the authors could clarify more about the “multi-variable” parts of the

EMDs. For example, it is well-known that the sea-surface temperature is generally more "smooth" than wind stress. It is more likely that the IMF1 from the wind stress data carries more high-frequency noises than the IMF1 from the SST. Hence, the IMF1 from the wind stress and SST might have different characteristic frequencies. Again, the authors could present some figures of raw data and IMFs to clarify if each IMF function from different variables have similar frequencies.

3. One more concern about the IMFA forms by the addition of s'th IMFs of the 20 PCs(with EOFs). However, what if the s'th IMFs have different characteristic frequencies for different PCs? It is very likely that there is a big difference between the frequencies of IMF(s, m) and IMF(s, m+1). This issue might enhance the mode-mixing and further jeopardizes the analysis. Hence, I suggest presenting the EOFs/PCs results (in the appendix or before section 3) with a least a table of PCs. In this case, readers can judge how PC1 dominates so that we don't have to worry too much about the issue.

Minor comments:

1. (L126-129) I am a little confused about the "30-year running mean seasonal cycle". Do you take the 30-year climatology of seasonal cycle, and then make a moving window of this climatology? Need clarifications.

2. (L162) "Where timescales are not clear" I suggest giving a range of frequencies.

3. (L197) "standard deviation" Needs to clarify how whether the standard of each variable or four variables.

4. (L232) Would IMF11 and IMF14 be involved with the ENSO? Need clarifications.

5. (Fig 3,4,6) The legend line styles are hard to discriminate; suggest rearranging the size.

6. (L359) Need more details about why the time scale is about three years, or insert a reference.

7. (Fig 5) Hard to tell the line. I suggest using different colors or line widths.

8. (L521) I suggest giving meow descriptions about the linear-regression models.

9. (L675) “filed” is a typo.

10. (L679) Replace “=“ by “\simeq”.

11. (L682) Same as (L679).